THE

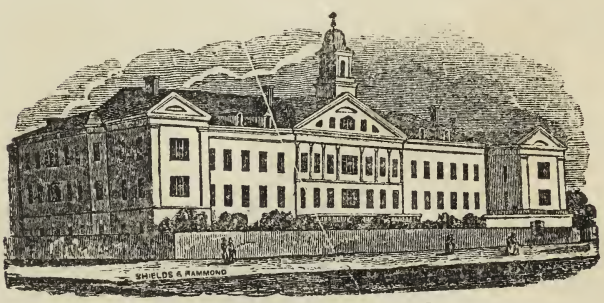
NEW ORLEANS MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE AND THE COLLATERAL SCIENCES.

EDITED BY

J. HARRISON, M. D.
W. M. CARPENTER, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

VOL. IV.—FOR 1847-48.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1848.

INDEX

To Vol. IV. of the New Orleans Medical and Surgical Journal,
From July 1847, to May 1848.

Address delivered in Med. College of Chemistry, Hanover square, by Gardiner, 369
Air passages, treatment of diseases of, 69
Alabama, Medical History of, by Lewis, 151, 459, 318, 3
Alabama, proceedings of Medical Society of, Mobile, 674
Alabama, review of Medical history of, by William M. Boling, 601
Aneurism, compression in the treatment of, 235
Anatomy, general and special, 91
Animals, their constituents and their food, 103
Arsenic, Medical evidence in poisoning by, 389
Artery, carotid, ligature of, 107
Asthma and pertussis, ether in, 266

Bartlett, on fevers of U. S., 759
Beck, on adulteration of Medicines, 757
Bischoff, on maturation and discharge of ova, 757
Blood, speculations on coagulation of, 34
Boling, W. M. his review of Medical History of Alabama, 601
Breast, abscess in, by Velpeau, 667
Burrows, on cerebral circulation, connection between disease of heart and brain, review of, 753

Calcus, extraction of from female, 47, 110.
Carotid artery, ligature of, 107
Cerebrum, disordered circulation of, 753
Cerebro-spinal meningitis, Chester on, 314
Cenas, obstetrical memoranda by, 312
Cholera, nature of the secretions from the alimentary mucous membrane in, 668
Cholera, its history, progress, &c., 768
Chester on cerebro-spinal menigitis, 314
Charleston, value of life among children of, 270
City, health of, 405, 682, 535, 133, 272 797
Country, health of, 406, 542, 685, 135 275, 805
College of Physicians, transactions of, 88
Compression, use of in treating aneurism, 235
Convention, national medical, proceedings of, 231
Chloroform, a substitute for Sulphuric Ether, 794

Day, Dr. cases of obstetrics, by, 223
Diseases, causes of, in St. Louis, 269
Dowler, Dr. his criticisms, &c., in relation to muscular and nervous systems, 279
Dowler, Dr. his meterological researches, 411
Dropsy, treatment of after scarlatina, 670

Education, elementary principles of, 87
Electricity, influence of in producing diseases, 666
Electro-galvanic machine, by Moorhead, 547
Epidemic, a new and fatal kind of, by Taylor & Hicks, 49
Epidemic, Cerebro-spinal meningitis, cases of, 106
Epidemic, a peculiar kind of, 689
" History of same, 689
" Prevailed in Vicksburg, 689
Ether, the vapour of in asthma and pertussis, 266
Ether, the inhalation of, by a new apparatus, 265
Ether, its use and value, in Surgical, medical and obstetrical diseases, 256
Ether, inhalation of in obstetrics, 145, 112
Eye, a treatise on the disease, of, 83

Fenner, Dr. on retained placenta, 177
" withdrawal of, 681
Fever, scarlet, cases of, 560
" yellow, a disease *sui generis*, 563
" " animalcular origin of, 563
Fevers, their peculiarities in U. S., 759
" description of, by Bartlett, 759
" review of, by Harrison, 759
Fever, typhoid, treatment of, 669
" " black sulphuret of mercury in, 669
" " causes, prevention and
" " treatment of, 571
Fever, congestive, speculations on, 36
" Yellow, a memoir on, 237
" a report on, at Boa Vista, 365
Fermentation, causes of, 376
" ascribed to growth of fungi and infusoria, 376
Fever, yellow, 536
Fistula, urinary, a case of, 694

INDEX.

Fistula, vesico-vaginal, 396
Foreign Body, lodged in the Trachea, 785
Functions of the Red Corpuscles of the Blood, by G. O. Rees, 787

Gardiner, his address, &c. 367
Generation, tracts on, a review of, 764
Generation and development, 99
Graham, Dr. his address before the Physico-Medical Society, 448

Health, an act to establish a Board of, 798
Harrison, John Dr., on Coagulation of the blood, 34
Harrison, John Dr., lecture, introductory, 439
Harris, Dr. *versus*. New Orleans Medical and Surgical Journal, 371
Hernia, strangulated, 695
" operation for cure of, 695
Hippocrates, oath of, 403
Histology, history of, by Henle, 353
Hospital, reports from, 410–687–543–142–278–801
Hort, W. P. Dr. on Congestive Fever, 36
Hydriod. Potassa in Secondary Syphilis, by Dr. Brickell, 801
Hysteria, its pathology and treatment, 528
Hypochondriasis, clinical lecture on, 382

Ingalls, on Malig. fever and Black vomit, 349
Iodine, poisoning from use of, 535
Irish Emigration Society, 144

Lectures, introductory, by Mitchell, 641
" " by Muller, 641
" " by J. P Harrison, 641
" " by J. Harrison, 439
" " by Geo. R. Grant, 641
by O. W. Holmes, 641
" " by S. H. Dickson, 641
Letheon, its discovoer, who 233
Lectures on the Phenomena of living beings, by Matteucci, 643–496
Louisiana, University of, 147–404 800

Matteucci, his lectures on the phenomena of living beings, 643–496
Materia medica. a work on, review of, 78
Magruder, Dr. on epidemic, 689
McWilliams, Dr. his report, 275
Medicines, communication on the adulterations of, by J. Tickell, 796
Medical ethics, a code on, 246
Medical science, Ranking's Abstract of, 236
Medical Botany, description of, 232
Medical Society, State New York, 88
Meteorogical Tables, by Lillie, 688–278–547–149–410–807
Medicines, adulteration of, 757–396
Medical Colleges, appointments in, 404
Meteorology, researches on, by Dowler, 411
Medicine, a treatise on practice of, a review of, 766
Medical Society, proceedings of, in Mobile, 674
Milk of the Carnivora, on the presence of layer of milk in the, 788
Midwifery, theory and practice of, review of, 766
Morehead, Dr. his Electro-Galvanic machine, 547
Muscular and Nervous System, criticisms on, by Dowler, 189
Mucous membranes, the nature of their secretions in cholera, 668

National Medical Association, 126
National Medical Convention, proceedings of, 231
New Orleans, mortality in, for 1847, 540
" " List of interments in, 687
" " Scientific miscellany of, 351
New Medical Journal, 272
New York, medical society of, 88

Obituary, Dr. J. B. Slade, 548
Obstetrics, cases of, by Cenas, 312
" use of belladonna in, 312
Obstetrics, cases of, by Day, 223
" use of Ether in, 145–112
Oil, castor, new method of using it, 266
Osteo-Sarcoma, operation for, 400
Ova, enlargement of, 457
" treatment of by iodine, 457
Ova, their maturation and discharge, 757
" " proofs of, without coition, 757

Parotid gland, removal of, 395
Parturition, effects of, 402
" to early rising after, 402
Physiology and Pathology, mutual relations between, 92
Physico-medical Society, New Orleans, address before, by Graham, 448
Physic, lectures on principles and practice of, review of, 495
Physicians, compliment to, 264
Plaster, discovery of the new Liquid Adhesive, 790

INDEX.

Placenta, remarks on retention of, 177
Poisons, medical jurisprudence, 641
" their relation to each other, 641
Poisoning, from arsenic, evidence of, 389
Prussic Acid, new test for, by Liebig, 789

Quinine, poisonous properties of, 119
Quinine, large doses of, in Fever, by Dr. Brickell, 803

Ranking's Abstract of medical science, notice of, 236
Report of Dr. McWilliams, 361
River quarantine, 275

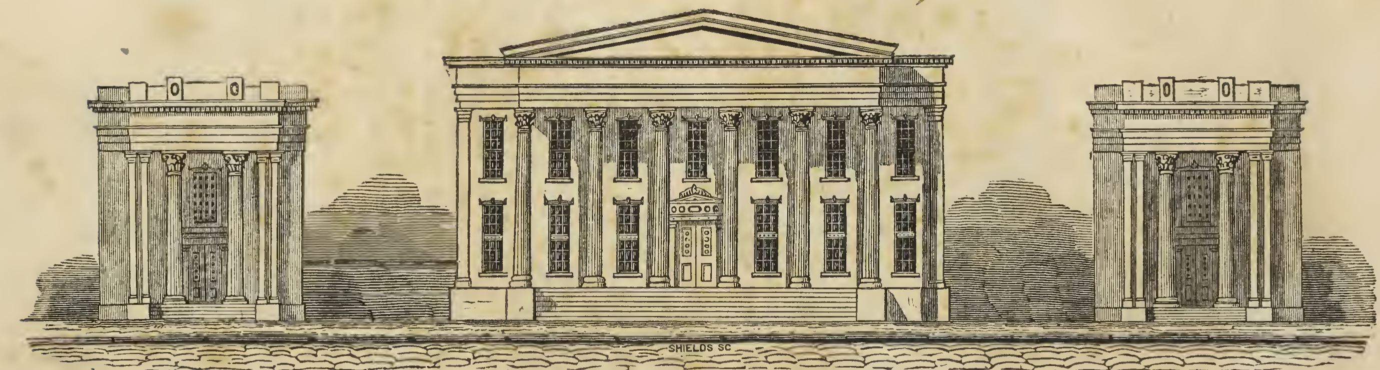
Scarlatina, dropsy from, 670
" treatment of, 670
Scarlatina and measles, 391
sequalæ of, 391
Scirrhous, tumor, removal of, 229
Silver, nitrate, use of, 110
Slade, Dr. J. B. obituary of, 548
St. Louis, causes of disease in, 269
Surgery, obstetrics, ether in, 256
Surgeon general, (Mexico,) 267
" his letter to his government, after battle of Cerro-Gordo, 267
Surgery, system of, by Chelius, 491
" " review of, 491
Surgery, elements of, operative, review of, 85

Taylor and Hicks, on a new epidemic, 49
Taylor, medical jurisprudence, 641
" " review of, 641
Tendons, painful crepitation of, 667
Trismus Nascentium, Sims on, 544
Typhoid Fever, on the Urine in, by M. Martin Solon, 787

University of Louisiana, 800, 147, 404
Uterus, diseases of, by Holmes, 549
" practical illustrations of, 549
Uterus, inflammation and ulceration of, 230
" a treatise on, by Bennet, 230
United States Army in Mexico, 90
" " " sickness of, 90

Vaccination, 547
Vade-mecum, 90
" or manual of examinations, 90
" " review of, 90
Vesico-vaginal fistula, 396

Wood's Quarterly Retrospect, 351
" " of practical medicine 351
Wood's Practice of Medicine, 698
" review of, by Boling, 698



UNIVERSITY OF LOUISIANA.

The centre building, which is the Medical Department, presents a front of one hundred feet, by a depth of one hundred and four feet. It contains three Lecture Rooms, large enough for six hundred persons each, with several Dissecting Rooms, a large Museum, and other rooms for Professors, &c. It will be finished in time for the next course of Lectures. (See advertisement.)

PUBLISHER'S NOTICE.

For the satisfaction of our subscribers, we shall continue to publish regularly from the date of this number, a list of amounts received on account of the Journal.

We take occasion once more, to state distinctly that all remittances must be paid to the Publisher, S. Woodall, *No.* 49 *Camp Street, New Orleans*, and that in no case will we be responsible for the non-receipt of any payment, if directed otherwise.

Subscribers are respectfully requested to be punctual in making their remittances, and thereby spare us the trouble and themselves the expense of duns.

The amount of subscription may be sent in any good and current bank note, either of this State, or of that in which the party resides—acknowledgment of the receipt thereof, will duly appear in the succeeding number of the Journal.

S. WOODALL,
Publisher, No. 49 Camp St.,
New Orleans.

N. B.—We have made certain changes in our list of Agents, which subscribers would do well to observe attentively. They will find on the cover of this number, the names of our only authorized Agents from this date.

New Orleans, July 1, 1847.

JOSEPH COHN, Printer, 31 Poydras Street.

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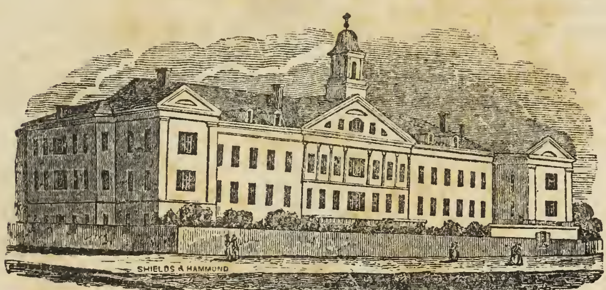

NEW-ORLEANS CHARITY HOSPITAL.

JULY, 1847.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1847.

TO READERS AND CORRESPONDENTS.

We have received a communication from Dr. Gist, of Jackson, Miss., for our next number.

We have received the following books:

1. *Medical Botany: or Descriptions of the Important Plants used in Medicine, with their History, Properties, and Mode of Administration.* By R. Eglesfield Griffith, M. D., &c., &c. With upwards of 300 illustrations.— Philadelphia. Lea & Blanchard. 1847, p.p. 704.

2. *Fascination, or the Philosophy of Charming.* By John B. Newman, M. D. New York. Fowlers & Wells. 1847.

The following pamphlets have been received:

1. *Proceedings of the Annual Convention of the Connecticut Medical Society.* May, 1847. Together with a List of the Members and the Annual Address.

2. *Valedictory Address to the Graduates of the Medical Department of Pennsylvania College*, (Session 1846-7.) By Washington L. Atlee, M. D. Professor of Chemistry.

3. *Proceedings in the Trial of Jeremiah Darby, for the Murder of his Wife.* In the Circuit Court, for Montgomery county, Alabama. May, 1847. (From Dr. Boling.)

The following Foreign Medical Journals have come to our address, from the publishers.

The Dublin Quarterly Journal of Medical Science.

The Dublin Medical Press. May, 1847.

Our customary American exchanges have all been received.

☞ We regret to perceive that the sources of some of our Foreign extracts have not been appended to the articles. The articles on *Cerebro-Spinal Meningitis*, and *Ligature of the Common Carotid* were extracted from Ranking's Half-Yearly Abstract. p. 192, and p. 207.

CONTENTS

OF

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

Vol. IV. No. I. — FOR JULY, 1847.

PART FIRST.

ORIGINAL COMMUNICATIONS.

PAGE

Art. I.—Medical History of Alabama. By P. H. Lewis, M. D., of Mobile. Published by order of the Alabama Medical Society. [A silver cup was unanimously awarded to this essay by the Alabama Medical Society, on the 7th of December, 1846.] - - - - * 3

Art. II.—Review of Opinions Concerning the Cause of the Coagulation of the Blood. By John Harrison, M. D. Professor of Physiology and Pathology in the Medical College of Louisana. - - - - 34

Art. III.—Case of Lythotomy: Extraction of a large Stone from the Bladder of a little Girl. By James Guild, M. D. of Tuscaloosa, Ala. 47

Art. IV.—Accounts of a New and Fatal Epidemic that prevailed recently in Mississippi and Tennessee. By B. J. Hicks, M. D. of Vicksburg, Miss., and B. F. Taylor, M. D. of Whiteville, Hardeman County, Tennessee. - - - - - - - - - 49

Art. V.—An Enquiry whether there is in the Southern States, a Specific Disease that can properly be called Congestive Fever; with Cases and Remarks. By Wm. P. Hort, M. D. of New Orleans. - - 36

PART SECOND.

REVIEWS AND NOTICES OF NEW WORKS.

Art. I.—A Treatise on Diseases of the Air Passages: Comprising an Inquiry into the History, Pathology, Causes and Treatment of those Affections of the Throat called Bronchitis, Chronic Laryngitis, Clergyman's Sore Throat, etc. etc. By Horace Green, A. M., M. D.—Formerly President and Professor of the Theory and Practice of Medicine in the Castleton Medical College: Vice President of the New York Medical and Surgical Society, &c. &c., New York and London. Wiley & Putnam, 1846, pp. 276. - - - - - - - 69

Contents.

PAGE.

Art. II.—Materia Medica and Therapeutics; including the Preparations of the Pharmacœpias, &c., with many new Medicines. By J. Forbes Royle, M. D., F. R. S. &c., Professor of Materia Medica and Therapeutics, King's College, London. Edited by Joseph Carson, M. D. &c. Philadelphia, Lea & Blanchard, 1847, 8vo., 689. - - - - - 78

Art. III.—A Treatise on Diseases of the Eye. By W. Lawrence, F. R. S., Surgeon Extraordinary to the Queen, Surgeon to St. Bartholomew's Hospital, and Lecturer on Surgery at that Hospital, etc. etc.—A new Edition, with numerous Additions, and one hundred and seventy-six illustrations. By Isaac Hays, M. D. Surgeon to Will's Hospital, &c. &c. Philadelphia, Lea & Blanchard, 1847, pp. 858. - 83

Art. IV.—New Elements of Operative Surgery. By Alf. A. L. M. Velpeau, Professor of Surgical Clinique of Medicine, of Paris; Surgeon to La Charité, etc. etc., with a treatise on Minor-Surgery, illustrated by over 200 engravings, incorporated with the text: with a quarto of 22 plates, representing the principle operative processes and surgical instruments. First American, from last Paris edition. Translated by P. S Townsend, M. D. Augmented by the addition of several hundred pages of new matter, comprising all the late improvements and discoveries in Surgery in America and Europe, up to the present time: all under the supervision of, and with notes and observations by Valentine Mott, M. D. Professor, &c. &c. &c. In three volumes, Vol. III. New York: Samuel J. & William Wood, 1847, pp. 1162. - 85

Art. V.—Education: its Elementary Principles, founded on the Nature of Man. By J. G. Spurzheim, M. D., of the Universities of Vienna and Paris, &c. &c. With an Appendix, by S. S. Wells. Sixth American Edition. New York, Fowlers & Wells, 1847. - - 87

Art. VI.—1. Summary of the Transactions of the College of Physicians of Philadelphia. From December, 1846, to April, 1847.
2. Transactions of the Medical Society of the State of New York. Vol. vii., part 1. - - - - - - - - - 88

Art. VII.—The Students' Vade Mecum, or Manual of Examinations upon Anatomy, Physiology, Chemistry, Materia Medica, Surgery, Obstetrics, Practice of Medicine, (including Physical Diagnosis and Diseases of the Skin,) and Poisons. Second edition, revised and greatly enlarged. By George Mendenhal, M. D., Lecturer on Pathology in the Medical Institute of Cincinnati, member of the Philadelphia Medical Society, &c. &c. Philadelphia, Lindsay & Blackiston, 1847. pp. 575. - - - - - - - 90

Art. VIII.—A System of Human Anatomy, general and special. By Erasmus Wilson, M. D., Lecturer on Anatomy, London. Third American from the third London edition. Edited by Paul B. Goddard, A. M., M. D., Professor of Anatomy, &c., in the Franklin Medical College of Philadelphia, with 233 Illustrations, by Gilbert.—Philadelphia: Lea & Blanchard, 1847. - - - - - 91

PART THIRD.

EXCERPTA.

Art. I.—On the Mutual Relation existing between Physiology and Pathology, Chemistry and Physics, and the methods of research pursued in these sciences. By Baron Leibig. (Concluded from our May No.) 92

Art. II.—Generation and Development, - - - - - - 99

Art. III.—On the Relation between the Constituents of the Food and the Systems of Animals. By R. D. Thomas, M. D., &c. &c. - 103

PAGE.

PART FOURTH.

MEDICAL INTELLIGENCE.

FOREIGN.

ART. I.—Epidemic Cerebro-Spinal Meningitis, - - - - - 106
ART. II.—Ligature of the Common Carotid. - - - - - 107
ART. III.—Internal and External Employment of Nitrate of Silver. By Dr. HELLER. - - - - - - - - - - - 110
ART. IV.—Lithotomy in the Female. By W. FERGUSSON, Esq., Surgeon to King's College Hospital, &c. - - - - - - - - - 110
ART. V.—A Lecture on the Utility and Safety of the Inhalation of Ether in Obstetric Practice. By W. TYLER SMITH, M. D., Lecturer on Midwifery - - - - - - - - - - 112

AMERICAN MEDICAL INTELLIGENCE.

ART. I.—On the Poisonous Properties of the Sulphate of Quinine. By WM. D. BALDWIN, M. D., of Montgomery, Ala. - - - - 119
ART. II.—Inhalation of Ether in Instrumental Labor. By W. CHANNING, M. D. - - - - - - - - - - 122
ART. III.—National Medical Association. - - - - - 126

EDITORIAL.

Health of the City - - - - - - - - - 133
Health of the Country - - - - - - - - - 135
Sickness in the U. S. Army in Mexico - - - - - - 138
Hospital Reports - - - - - - - - - - 142
Irish Immigrant Society - - - - - - - - 144
Inhalation of Sulphuric Ether - - - - - - - 145
University of Louisiana - - - - - - - - 147
Mortality in New Orleans - - - - - - - - 148
Meteorological Table. By D. T. LILLIE - - - - - 149

THE NEW ORLEANS

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Part First.

ORIGINAL COMMUNICATIONS.

I.—*Medical History of Alabama.* By P. H. Lewis, M. D., of Mobile. Published by order of the Alabama Medical Society. [A silver-cup was unanimously awarded to this essay by the Alabama Medical Society on the 7th of December, 1846.]

(*Continued.*)

In the foregoing imperfect sketch, it will be observed that no mention is made of the North-west section of the State, or the Tennessee Valley. Our means of information and knowledge of the topography and diseases of that region being too limited to speak with any degree of certainty, we are compelled to leave it to other hands.

Before entering upon a description of the diseases of those sections adverted to, we will take a brief notice of other causes and circumstances influencing disease, than those arising merely from soils.

The vicinity of eddies of rivers, as those of the Warrior and Coosa are extremely unhealthy; it has been noticed that bubbles of air and wreaths of foam are constantly seen on the surface of the quiet waters—the supply of material for this fermentation never ceases. Mill ponds wheresoever found are exceedingly noxious, never failing when spread out beyond the banks of the creek, to effect the entire neighborhood.—The supply of new material for decomposition, furnished by these artificial lakes, is so abundant and constant that miasmatic exhalations continue throughout the entire year.

The waters in common use, as an aliment, are often pernicious:—the surface wells of the prairies resting on a base of limestone, the water is infiltrated through and partakes of the properties peculiar to the subsoil we have already alluded to. In the upland country, the waters are frequently so astringent from the deposits of iron and lime through which they pass, as to produce in many instances a constipated habit of body. We are credibly informed that the artesian wells throughout the prairie region, frequently contain saline properties, the constant use of which tend to change the healthy functions of the system.

The action of all those waters affects the normal condition of the digestive tube, which may not present any marked feature until disease in some violent form ensues, when the attention is directed to an abnormal condition of the bowels that had for some time previously existed. It is, however, not unfrequently the case that those individuals who suffer with chronic disease of the stomach, have been restored to health by the use of the prairie waters, on removal to that region.

We should do injustice to the beneficence of the laws of nature that so strongly marks the character of our State, did we omit to make mention of those numerous perennial fountains, like the Robinson Springs in Autauga county, which yield an abundant supply of the purest waters. And in most of our upland country every hill side throws forth its stream of mineral water, that can vie in medicinal properties with the most celebrated springs of this country or Europe.

The removal of trees or their foliage from around marshes, sloughs, and habitations, is frequently followed by visitation of fevers, peculiar to the locality only ; one instance we will refer to, from many that have been detailed by our numerous correspondents.

"Mr. P. E., had negro quarters situated on the first prairie elevation above the low grounds of a small creek, the fourth of a mile from the houses. This belt of low ground frequently overflowed, causing water to remain in holes over its entire breadth, on the subsidence of the stream, but it was well shaded by a dense foliage, the plantation laying on the prairie in the rear of the cabins. In the winters of 1842 and '43, the trees between the houses and creek, were cleared away, and up to that time, some 8 or 10 years, the negroes living in this quarter had enjoyed uninterrupted health, a case of fever scarcely ever occurring. During the summer of 1843, the first after the forest had been cleared away fever prevailed among the negroes with great violence, continuing until frost. The negro quarters were afterwards removed to the opposite side of the creek, about the same distance from it, but with an intervening growth of timber, and no fever has occurred on the place since." (*Doctor Wooten.*)

It is the generally received opinion that living vegetation protects the human system from the deleterious effects of malaria, and reasoning by analogy, it would appear that experiments made by scientific men have satisfactorily explained the mutual dependence of the animal and vegetable kingdoms on each other for support.

It has been ascertained that ifair rendered pernicious by respiration be confined in a bottle, into which some green plant has been introduced, and exposed to the action of the sun, the carbonic acid will be absorbed, and the air restored to its original condition. The putrefaction of animal matter and the decomposition of vegetable substances would cause a sufficiency of carbonic acid vapor when united with atmospheric air, to destroy every living being, were it not for this wise provision of nature.

This gas, which is poisonous to the human as well as animal species, is a source of nutriment to every variety of plant, and thus, it would appear, exercises a benign influence in protecting man from the deleterious effects from poisonous vapors.

The Easterly winds that occasionally prevail in August, September, and October, are exceedingly unwholesome. These winds being dry, such a rapid evaporation takes place as to change and derange the functions of animal and vegetable life. The skin becomes dry and husky, the head grows dull and heavy, with vertigo, or coma, and a state of general depression ensues. This effect is most perceptible upon the Gulf coast, invariably giving rise to an increase of such disease as may be existing at the time, with symptoms of a more aggravated nature. Whenever an Easterly wind prevails, we frequently find in the course of one night, a marked change for the worse even in those patients that are deemed convalescent, the fever often assuming a grave and unyielding type.

It seems to be the unanimous opinion of physicians in the prairie region, that the summers of 1835, '36, '41, '42, and '43, have been the most humid and the most sickly.

On the other hand, it is conceded that the summers of 1837, '38, '39, '40, '44, and '46, have been exceedingly dry and healthy.

To these general facts are found exceptions, the most prominent of which occurred in the vicinity of Hayneville, Lowndes Co., Alabama, in 1839. The congestive fever which prevailed there during the autumn of this year was very fatal, and its origin was ascribed to the bringing into cultivation a short time previous, a large body of low, humid, prairie soil.

During dry summers, vast tracks of alluvial bottoms of the lower Bigby are exposed to the heat of the sun, and under these circumstances fever is certain to prevail.

In the hilly, granitic, and coal region of the State, as well as on the bluffs of water courses and diluvial elevations of other regions, a different state of things exists.

After a careful study of all the facts collated, we have arrived at the conclusion, that in this description of country the dryest seasons are generally the most unhealthy, and in some portion of the State this has been very conspicuous.

Dr. Kitrell, of Greene Co., says, that during the summer of 1839, and '40, the broken and hilly portions of that country were very unhealthy; every family, even those residing on the most elevated situations were severely afflicted.

Doctor Clarke, of Benton Co., informed us, "in that section of the State, especially such portions as are broken by the hills and ridges putting out from the Alleghany range, they suffered more by disease in 1839 and '40, than before or since. Those Autumns, particularly the former, were the dryest and hottest ever experienced. Springs, hitherto perennial, dried up, the farmers remarking " that the crops were literally made without rain, the dews alone sufficing."

It should be remembered, that in rainy seasons, the waters along the vallies, are fresh and in motion, and the temperature of the atmosphere is not greatly elevated ; both of these circumstances being unfavorable to the elaboration of the elements that constitute a miasmatic influence.

As a general rule, dews are more common in vallies or plains, and near bodies of water.

On the level cultivated prairies of Alabama, they are lighter, and less constant than in the hilly region of country, being confined to slight elevations, the river bluffs and narrow belts of argillaceous mulatto soil.

The absence of dews, on the prairie soil, is probably owing to a variety of causes, one of which is, that the sub-soil absorbs and retains with great tenacity, the diminished humidity of summer.

The surface is so perfectly dry and heated, its immediate atmosphere is but slightly modified or lowered in temperature during the night, and the vapour is not sufficiently condensed for the formation of dew. Another reason is presented in the fact, that the prairie soil is so compact and slightly absorbent, as to possess very little power of attracting aerial moisture. From the fact however, that as the cool nights of Autumn approach, the drenching dews fail not to ensue, the first cause assigned, the want of a low temperature, is the principal one in accounting for the absence of dew in that case.

It is true that those hills and mountains which have an elevation beyond the ascent of vapours raised during the day are free from these phenomena ; but the bluffs, hills and ridges of Alabama, so far from being beyond their reach, are about the proper elevation, not only as regards the vicinity of the vapours, but more effectually to be brought under the influence of refrigeration.

Argillaceous mulatto soils, being but a very little higher in some instances than the prairie plains, are the depositories of dews in summer nights.

This is no doubt occasioned by the character of the soil, it containing sand finely divided, carbonate of lime, clay and organic remains in such proportion as to render it highly absorbed, admitting access of air to a considerable depth ; hence the lowering of the temperature at night, and the great power of attraction in those localities during the day.—Dr. B. R. Hogan, of Dallas Co., in a letter to the writer, says, " should June, July and August be dry months, with a low dew point, we usually have a healthy Autumn.

The state of the dew point has an important influence upon the causes of disease proceeding from miasmatic origin and upon the physical condition of the human frame. It modifies the influence of solar-heat to an inconceivable extent, the chemical affinities of all terrestial exhalations of organic depositions, and the functions of vegetable and animal life. We hazard the conjecture that future investigation will establish the fact that the nearer the dew point approximates the temperature of the summer and autumn, the greater will be the prevalence of every form of miasmatic disease."

What agency the presence of dews may have had in contributing to the unhealthfulness of certain localities, in seasons when there were no rains, or what effect they may have had on the plains in dry and healthy autumns, we will not say, but the coincidence is such as to command attention and invite further investigation on the subject.

The mean state of the thermometer, and hygrometer, in Mobile, for three months, 1846, was as follows:

August,	thermometer	78°—	hygrometer	59°
September,	"	75°	"	55°
October,	"	61°	"	43

From partial observations made in 1845, the same very unusual inequality in the range of these instruments existed. The autumns of those years, 1845 and '46, have been remarkably healthy.

Observation has caused the writer to remark, that during the nights and mornings, dense dews, commonly called fogs, are hovering over and settling down on the hills and bluffs of South Alabama, whilst the atmosphere of the vallies and plains below are comparatively dry. That these elevations are more unhealthy in dry seasons than the vallies, is a fact conceded by all.

It is a popular opinion among physicians, that dew, or an atmosphere saturated with moisture, is most unhealthy; because, in that condition the miasm is suspended, or held in solution in such a manner as to be inhaled or absorbed into the system. Independent of this indirect effect, there can be no doubt that an atmosphere thus charged with water, exerts a direct and pernicious influence upon the functions of the skin and lungs.

Dr. C. A. Lee, of New York, who has been making many experiments in reference to this subject, remarks, relative to the influence of the dew-point in the production of diseases: "This arises chiefly from the circumstance, that a high state of the dew-point interrupts to a greater or less extent, the healthy functions of the skin and lungs; two of the most important organs of the body. I maintain that a perfect decarbonization of the blood, cannot take place in the lungs with a high dew-point; and, consequently, that the vital fluid cannot receive a sufficient quantity of oxygen to fit it for those various offices which it is designed to perform in the animal economy."

We are led, consequently, to infer, from the remarks of this able investigator, as to cause and effect, that as the hygrometer approaches the maximum or minimum point, so, in proportion, the seasons will vary from health to disease.

This principle of reasoning is not inconsonant with the operations of nature, for we find in the vegetable kingdom, that an unusual degree of moisture, tends to depress the healthy property of every variety of plant and forest growth.

Diseases of the Third, or Present Epoch.

Having presented the general outlines of the physical characters of each section of the State, we will now endeavour to give a brief description of the prominent diseases prevailing in each division. It is too much the custom with medical men to view disease in the mass, without directing the attention to its individual character. Diseases that are dissimilar in the manner of invasion, unlike in symptoms, complication, duration and mode of termination, either in recovery or death, are generally looked upon as one and the same affection. This want of analysis in investigating the nature of disease in the abstract, leads to great confusion in medical literature, particularly in the South, where remedies are prescribed through the Medical Journals, in varying doses, for disease under some one head, that may be totally dissimilar in character. It is true, that disease, to a great extent may prevail in some one locality in the same season, presenting precisely the same symptoms as in the various types of intermittent, remittent, congestive and yellow fevers; yet, in other sections of the country the symptoms may be so unlike in many respects as to demand an opposite method of treatment.

Being only in the incipent stage of those investigations which it is our purpose to pursue, and pretending only to a mere sketch in this paper, rather than a full history, our account of the diseases now prevailing, must necessarily be brief and imperfect, with the satisfaction, however, of believing, that so far as we go we are correct.

As the affections of winter, from their increasing severity and fatality, are becoming invested with an interest of peculiar importance, even greater than that which attains to those of summer and autumn, a separate notice will hereafter be taken.

Many settlements and towns, which in the second epoch were noted as the abodes of disease, have of late years become distinguished for great salubrity. The city of Montgomery presents a marked instance of this change. From the settlement of this place, until 1837, a visitation of summer and autumnal fever, was annually experienced; since then, its exemption from this character of disease, has been very remarkable. This fortunate revolution in the health of this flourishing town was coincident with marked physical changes, such as the filling up of slues, ponds and low lands, abounding in organic remains, by the washing down of the ferruginous clay and sand from the surrounding hills.

The exceptions to the many instances of this kind that abound, are to be found upon, and in, the vicinity of the yellow argillaceous soils, which, as has already been mentioned, retain moisture, attract dews, and give rise to disease of much constancy and uniformity of character.

We now propose to describe those maladies which would seem to be the offspring of some emanation from that character of soil, which we have endeavoured to show is peculiar to sections presenting certain geological features, and which have been brought into cultivation since 1830.

Our attention will first be directed to those SUMMER and AUTUMNAL FEVERS, of THE COAL and GRANITIC OF HILLY PORTIONS of THE STATE.

Notwithstanding the extreme vicissitudes of the weather, incident to this region, are sufficient to give rise to an endless variety in the type of fevers, yet the general character of disease is not such as would come under the head of active, or open phlegmasia, but, like those of other sections, they are attended with small frequent pulse, moderate degree of heat on the surface, and in a majority of cases, there is a constant tendency to prostration.

Exposed, as this upland country is, to the effect of northerly winds, it may be assumed, that the warm days proper of summer, are some twenty less in number, than in the middle and Southern sections. For two or three weeks in July and August, the thermometrical range is higher than is exhibited in the pariries, or on the sea board; yet, if we take the mean temperature for any number of months consecutively, it is less in proportion than in the same degree of latitude, either East or West, where the levelled surface of prairie soil presents an unbroken area of perennial verdure. There, the substratum is decomposed lime-stone; the surface, alluvial soil, where caloric and moisture are known to abound.

In the upland country, fevers of an intermittent and remittent type usually make their appearance about the first of July, increasing in number and becoming more violent in the month of August, with occasionally one of a typhoid character; and by the first of September, they have attained their maximum point.

These fevers, usually begin to decline in October; being more unfrequent, and less violent, except in those cases that have proceeded from a relapse, and then the disease is both obstinate and tedious, until repeated frosts have tended to change the character of the atmosphere, allay malaria, and brace up the shattered constitution. For some years past, in low, moist localities, the fevers have assumed a more grave type; what the physicians in those regions term "congestive intermittents." They are mostly quotidian, the chill continuing from two to six hours, and attended with coldness, shivering and the usual phenomena of an ordinary chill.

The cold stage is not attended with the same difficulty of respiration, cold clammy skin, sense of oppression, heat of the internal surface, extreme thirst, together with that marked depression of all the vital forces that characterizes the true congestive fever; the continuance of the cold stage, and moderate febrile reaction, seeming to depend upon the general tendency to a state of adynamia, that so peculiarly marks every form of fever at the present day, rather than any vital or important derangement of the functions. In the course of these, as well as all other acute affections, it is not unfrequently the case, that from some morbid derangement, by the administration of harsh and irritating medicines, vomiting, watery dejections and a state of colapse, may suddenly supervene. Under these circumstances, the medical attendant, unaccustomed to the fevers of the prairies, or not having studied the individual nature of southern fevers, is perfectly assured that he has encountered a case of congestive fever. This character of fever is rare in this region, and is simply referred to here because it is the only description of cases which has been denominated congestive; and the difference in the two diseases will become apparent, when we have described those of the prairies.

The summer and autumnal fevers of this region are principally intermittent and remittent, of a mild nature, attended with little fatality; the only severe, or grave form of febrile affection, being of a continued or typhoid character. Dr. Clark, in his esteemed letter, remarks, "in August, September and October of 1840, in Benton County, typhoid fever prevailed as an epidemic, assuming frequently a malignant, obstinate and unmanageable character. It attacked, indiscriminately, individuals of all ages, without regard to sex or colour. During the prevalence of this fever, we had also every grade and vareity of intermittent and remittent fever, throughout the summer months, but all the *fatal cases*, were of a typhoid character."

Notwithstanding the occurrence of this and other partial epidemics, idiopathic typhoid fever cannot by any means be regarded as the prevailing disease of summer and autumn ; it more properly belongs to the winter and spring months ; yet, as the general tendency of the remittent is to the continued typhoid, taken in connection with those that are essentially so, and that is the only type which is considered grave or dangerous, we cannot err in stating that typhoid fever prevails in this section of the State to some extent at least.

In the summer of 1839 and '40, the writer witnessed many cases of fever of various types in this section of country; some, which *termed* "typhoid congestive," were attended with a coldness of the extremities. profuse perspiration, and a strong accelerated pulse. Contrary to the usual course of fever, the perspiration increases with the arterial excitement, and subsides with it. In other cases, again, there is bilious vomiting, soft feeble pulse, moist yellow tongue, stupor, or coma.

In these forms of disease, the fever generally lasted from ten to fifteen days, and may be comprehended under that variety as described by Chomel, as not being essentially typhoid fever, representing different degrees of severity.

Typhoid fever, whether remittent at the first, or continued from the onset, is attended with extreme debility and great prostration of body, nervousness, irritability, perversion of the senses, stupor, sometimes delirium, and pain in the head and limbs; thirst and heat of skin, are frequently the variable, although prominent symptoms. The pulse is small and frequent, the bowels are slightly tympanitic, and sore on pressure. The tongue is usually round and lengthened, dark in the middle, the edges inclining to bend, and always dry. but when it becomes moist it is generally a sign of convalescence, which may occur in 8 or 10 days, but the usual duration of the fever is from 15 to 25 days.

This fever is not so strongly marked in autumn as in winter, and, separated from it, would not deserve the name, as it does not comprehend the strict definition of typhoid fever, a further notice of it, however, will be taken under the head of diseases pertaining to winter and spring.

In the treatment of the continued fevers of this section of country, whether they are idiopathic, or supervening upon a remittent type, local depletion cautiously practised, diaphoretics, sinapisms, emolient poultices, diffusible stimulants, and mild alteratives, are the remedies usually employed by judicious practitioners. Quinine has been used at different periods, in every variety of dose and form, but always tending to aggravate the disease, and increase that disposition to local inflammation, which usually exist in the cases of that region.

We are, therefore led to infer, that the disease is one of that mixed character, requiring a mild antiphlogistic course, combined with moderate stimuli, to guard against the tendency that invariably exists to prostration.

In the southern portion of the State, pointed out as the Tertiary, the the fevers are less uniform in their type than in any other region, differing in every respect with the varying features of the country, and the peculiar nature of the soil ; for in this section we find every variety of cause that would tend to generate malaria.

Here you meet with the dark lagoon that is overshadowed by the cumbrous foliage of timeworn forest trees; here the brackish marsh abounding in animal and vegetable deposit, the accumulated mass of ages lies exposed to the sun, and here the widespread creek and overflowing river leave their slimy deposit of decomposed matter over a vast extent of adjacent soil to furnish material for the generation of poisonous gases, all combining with their teeming stench to induce disease of grave malignant nature.

As any attempt at a description of all these types is incompatible with the limits of this paper, we will confine ourselves to those cases which occur in the vicinity of the swamp, extending from the junction of the Alabama and Tombigbee rivers, down to the City of Mobile.—The fevers here are strikingly dissimilar to those now prevailing in other parts of the country; the poisonous cause of disease, it will be remembered, arises from swamps and marshes of recent formation, in which vegetable matter greatly preponderates.

The cold and hot stages of the intermittents are characterized by the most violent extremes. The chill lasts from four to six hours, often amounting to a "shaking ague," the fever then ensues and continues with great violence from ten to twenty hours; the sweating stage is scarcely formed before another chill is announced, the paroxysms occuring at irregular intervals, and it must be noted that these intermittents are exceedingly obstinate, usually continuing from 8 to 10 days without any change.

The remittent, or, according to a strict definition, *continued bilious fever*, most usually form, and is commonly known as "swamp fever." It is attended with a full bounding pulse, from 110 to 130 in the minute, a dry and hot skin, pale flattened tongue covered with white fur, which in the course of the disease becomes yellow or dark brown, the pain in the head, back and limbs is very violent, some thirst, nausea and biliary derangement. Once in 24 or 48 hours, there is a tendency to remit, manifested usually by less frequency of pulse, abatement of pain, and moisture about the chest and head. The bowels are usually constipated, all the secretions becoming diminished or suppressed. The blood very soon looses its rich tenacious qualities, becoming thin and pale, and the disease is from 6 to 14 days in running its course.

We have been treating this fever for several years, and seldom succeed in cutting it short. Large doses of quinine change the character of the symptoms, but never cure; convalescence is painfully protracted, or interupted by relapses, until the approach of frost.

During convalescence, or rather that protracted undefined state between the subsidence of the fever and frost, the patient is very weak, presenting a pale waxen appearance, but there is no evidence that any organ, has experienced a severe lesion. In fact, the absence of any tendency to local determination, sinking or collapse of the vital forces or to putresency and decomposition, proves at once its non-malignant character.

In the examination of four bodies made in the Hospitals of Mobile, the liver was atrophied in *one*, being also dry and brittle, like those dying of Yellow fever; and of a pale straw color. In the other three, this organ presented nothing peculiar as a lesion of disease; in fact there was no lesion sufficiently prominent any where to attract attention, except in the serous condition of the blood and soft spongy condition of the cellular and muscular tissues.

FEVERS OF THE PRAIRIE REGION.

To ascertain that malady, which, from its malignancy, constitutes the outlet of human life in this section during summer and autumn—that malady which for gravity is the maximum in the chain of morbid sequences—that malady which most excites the fears of the people, and absorbs the attraction of the medical man, it is only necessary to be brought to the bed-side of one laboring under congestive fever, and the search is at an end. The time of, and circumstances connected with, its appearance in the State has already been detailed. The writer, whilst a student in the Charity Hospital of New Orleans, witnessed the cholera in its most aggravated form; since then he has become familiar with the yellow fever of Orleans and Mobile, and although, to his perceptions, they are so strongly and distinctly marked that no one could fail to distinguish them from all other affections—yet, they are not more so than was the congestive fever which prevailed in this region, during the autumns of 1835 and '36. In those years it maintained the same connection and relation to the intermittents and remittents of the country that yellow fever does to the same diseases, during the epidemics of Orleans and Mobile. But before examining into the connection or unity of these diseases it is necessary to say something of the habit and character of the one which it is now our purpose briefly to consider.

In order that we may reconcile conflicting opinions, and at the same time educe the truth from contradictory statements, it is necessary to enter somewhat into detail.

It is stated by a medical gentleman residing in Marion, that the disease was confined to adult males; the women and children of that village being exempt. After having his attention directed to it, he is confident that those attacked were in the habit of visiting the neighboring creeks and swamps. A medical man who practiced in Dallas county during the years 1835 and 1836 remarks, that the congestive fever was confined to male adults; after some enquiry it was discovered that he lived in the midst of a people inhabiting a sandy ridge, and that the men were in the habit of making daily visits to the plantations situated in the adjoining swamps and along the prairie creeks. Doctor Mabry, of Selma, in his prize essay on this disease, remarks that women and children are equally liable and as often attacked. The people among whom the Doctor practices, live mostly in a low damp prairie region and of course all ages and conditions are exposed alike. A correspondent residing for many years in Montgomery, a town situated amidst diluvial elevations of the prairie region, informs us that he has not yet seen any case of congestive fever, such as has been described by medical men, as prevailing in other sections of the State,—that he has "seen cases of simple fever *collapse*, or become complicated, (under the influence of injudicious treatment) resembling in that condition the congestive state." From abundant testimony, it would appear that this town is exempt from the disease in its true characteristic form;—take, for instance, the able paper of Dr. Boling on the fevers of that vicinity. He describes no case of febrile affection, which physicians of certain localities in Greene, Marengo and Dallas, would recognize as congestive fever. We could give many other instances, where medical gentlemen relying on their own experience, had arrived at opinions conflicting with those of their neighbors, making a confused contradictory mass; but when they are carefully considered in connection with all the attending circumstances, they become reconciled, tending to the developement of facts, which could not have been elicited from the observations of any one individual. In this case the combined testimony before us, points unerringly to the following conclusions: that the poison of the disease is confined mostly to humid prairie, or low swampy lands abounding in organic remains, that this poison does not spread broad cast over extensive tracts of country, or rise to elevated situations like the ordinary marsh poisons—that the more elevated prairie soil, which has in the process of cultivation given out its excess of humidity does not produce the disease—and that, with an equal exposure, all ages and sizes of the white population, are alike susceptible.

In looking back at our sketch of the first and second epochs, the regular gradation of disease will be observed as follows:—intermittents and irregular bowel affections prevail in spring and early summer; as the season advances these are replaced by remittents; late summer and autumn find that the latter have attained their maximum and the fevers are continued, being either of a nervous inflammatory or malignant character. Here is presented a regular gradation of morbid condition, from its mild incipiency to a high toned or malignant action, *without any super-added complication or change of character*, save in degree. In this case no one can reasonably question the identity of these fevers.—We find that the congestive fever is preceded by the intermittents and remittents, precisely in the same manner that the continued fevers were in past times. This apparent connection and dependence afford a strong argument in favor of the identity of the congestive and the other fevers so closely associated with it—but whilst we admit its force, we cannot subscribe to its conclusiveness. He who does not know that the most deadly maladies—maladies wholly and essentially different in form—are often found engrafting themselves upon simple intermittents, has observed to little purpose. This however, as before remarked, is not the place to consider the identity or connection which may exist: allusion is made to the question, for the simple purpose of calling attention to the importance of a rigid scrutiny and investigation, not only of connecting circumstances, but the symptoms pathognomonic of congestive fever.

In 1834, this disease made its appearance in a few localities; in 1835, it prevailed in its most extended and malignant form. The writer began the practice of medicine in Dallas County, in the spring of the latter year, during the summer and autumn of which, together with that of 1836, he was actively engaged in the treatment of this disease.

In 1835, intermittents and remittents prevailed from the first of May until the first of July; they then began to decline, and by the middle of the month the country was exempt from disease of every kind. The congestive fever made its appearance early in August, and continued its ravages until the middle of October. The following case, condensed from notes taken at the time, will best serve the purpose of description.

Mr. A., aged 24, native of the State, of robust constitution, came to my office at 9 o'clock in the morning for advice. Says he has been for the last week engaged in superintending "*some work*" in an adjoining prairie swamp; went to bed last night feeling well, awoke this morning at daylight, since when, he has been weak or languid, not particularly sick, but is unable to shake off a restless, desponding and uneasy feeling. Found his pulse not exceeding 100, but small and deep seated, skin cool and damp, which was attributed to exposure, the atmosphere being warm and very damp. Says he is thirsty but *does not feel chilly;* his difficulty mainly consists, to use his own language, in an "inability to get my breath." In two hours after he returned home, some two miles from my office, he wrote and despached the following note: "Come and do something for me or I shall die." Did not see him until 3 o'clock in the afternoon; his condition then, was nearly as follows—skin cold, of a pale blueish color, muscles soft, unless put upon the stretch by exertion, profuse perspiration over the entire surface, standing in large drops on the chest and forehead, tongue cool, pale and inclined to a livid hue, pulse frequent, small and thready. Action of the heart changed to a *tremulous flutter*, with now and then a violent pulsation, causing the patient to start, urine abundant and colorless, bowels torpid, slight nausea, countenance haggard, expressing the deepest agony or physical trouble, very restless, walks rapidly over the floor for a moment, then sinks exausted, wants an emetic to relieve his breathing, is perfectly sensible. About 10 o'clock at night the perspiration sensibly diminished, pulse improved and the breathing became easier. This partial improvement continued until 6 o'clock in the morning. At 10 o'clock in the morning found him much worse—pulse imperceptible at the wrist, tongue and lips livid, skin icy cold, dependant portions wilted and shrivelled; other symptoms about as yesterday. After a doubtful struggle of 6 or 8 hours, during which time *there* is *no complaint* of *chilliness*, but great heat and burning, the pulse again becomes perceptible, a slight degree of warmth returns to the surface, and hope again enlivens the household. The next morning, I was summoned early. He began to grow worse at 2 o'clock, made several efforts to vomit; had not been chilly; he now speaks in monosyllables, says he "is sensible, but has no breath to talk." Is extremely restless, skin cold, blueish and mottled about the back—each expiration is attended with a harsh distressed murmur, cannot bear any covering—finally, in a paroxysm of wild despair, rises from the bed, rushes to a window, and whilst holding to the facing, is seized with a convulsion. He expired in a few minutes, after being laid upon his couch; having been ill but 52 hours.

In this case, notwithstanding the most active means were used, there was no evacuation of fæces from the bowels. During efforts to vomit, a little water mixed with mucous was occasionally ejected.

By reference to notes and the recollection of conversations had with Drs. Gantt and Herbert, who did a leading practice in the county, the foregoing case represents the general condition and symptoms of those attacked—the difference in the cases, consisting in degree, or the viscera more especially implicated. For instance, in the case discribed, it appears that the first *appreciable* derangement of function, was that of the respiratory apparatus; this grew more and more prominent, until the lungs totally failed in their office. In many cases, there was pain in the head, flushed face, watery injected eye, sensibility to light, and an early supervention of delirium. In this variety, labored and difficult respiration was not so prominent; this, together with the absence of symptomatic gastric complication, caused practitioners to refer to the brain as the seat of mischief. Again, there were cases (and probably a plurality were of this description) where early nausea, sinking at the stomach, vomiting and frequent stools of sero-mucous matter were the most prominent and urgent symptoms of disease—here, the stomach and intestines were pronounced to be the points most especially involved. Although one or the other of these determinations may have existed in one case, or they all may have been manifest in another, producing death in 24 hours; yet, as a general rule, the assemblage of symptoms characterizing the disease were very uniform.

With a majority of those attacked, there was in the course of 24 hours a return of warmth to the surface, and in fact a general improvement, showing a strong effort or disposition on the part of nature to react—but if this reaction amounted only to a gentle glow of warmth on the surface, the pulse continuing quick, and perspiration profuse, the ensuing morn witnessed, in a more aggravated form, the return of all the perilous and depressing symptoms so peculiar to the disease.

The formation of the *true and healthful* reaction could always be distinguished from the false and ephemeral. In the first case, the *perspiration sensibly diminishes;* as the skin becomes dry, the capillaries fill, and a deep general warmth ensues; the pulse, which a short time since may have been extinct at the wrist, is now perceived, and as it becomes raised more to the surface, it lessens in frequency and increases in force. We did not see a case, where the skin became *dry and warm*, that the patient did not recover, and that very rapidly. During the improving stage, evacuations of a bottle green color, and about the consistence of grape jelly, took place; often as much as a gallon passed in 48 hours: this remarkable colluvies was inodorous, devoid of bile, breaking into pieces like the coagula of blood. Occasionally organic or functional disorder followed the patient, but *relapse* or *second* attacks did not occur.

Our attention thus far has been directed to the graver and more malignant cases, about one third of which proved fatal between the 2nd and 3d day. There were those of a much lighter and milder grade, distinguished by the same phenomena—such as cool skin, continued perspiration, thirst, quick thready pulse, interrupted respiration, uneasiness and occasional sighing. These symptoms would deepen and become more urgent in the morning, a partial reaction taking place in the afternoon. In these as well as those previously described, there was no marked remission or intermission; the patient continuing in a pathological condition, until a *dryness* or *permanent warmth* of *skin* announced a restoration of the functions of the body. In no discription or variety of case, could there be detected, even in the *cold stage*, that *shivering* or sensation of *coldness* that causes the patient in intermittents to *seek* the *fire*, the *sun* or *warmth of clothing*:—On the contrary, there was an earnest cry or impatient demand for *cold drinks*, *cold fresh air*, constant fanning and exposure of person. This was the congestive fever which prevailed along the prairie, creeks and low humid swamps of Willcox and Dallas counties in 1835 and '36. The medical men at that time, did not dream of classing it with the intermittents and remittents of the country.

As a general rule, this disease has become modified since 1835; still, however, maintaining in some localities, its original marked violence. As it is the most formidable and interesting disease of summer and autumn, that is known in the bounds of the State, the impressions of medical men practicing in different localities, will not only assist in revealing its true character but are absolutely necessary to the end we have in view, a fair and correct picture, so far as we go, of the prominent disease of the State.

Doctor A. G. Mabry, a resident of Selma, in his prize essay says:—"Of fevers this is the lowest grade of which we have any knowledge—it is an idiopathic disease and may be intermitting, remitting or continued in type. Its invasion is often sudden, coming on after a few hours premonition with a chill which continues for several hours, during which time the patient is harrassed by difficulty of breathing, nausea and repeated efforts at vomiting: the pulse is feeble and frequent, skin cold and clammy, countenance distressed and shrunken; not unfrequently attended with sero-mucous discharges. The duration of this stage varies in different cases, seldom passing off however under three or four hours and often continuing much longer. The hot stage is characterized by a *feeble reaction* of short duration, say one or two hours, after which it gives place to the sweating stage. About the same hour, the next, or perhaps the day after, a second paroxysm takes place, which differs but little from the first, except in severity; the cold stage becoming much longer and the reaction less perfect in every succeeding paroxysm until the third or fourth day, when death closes the scene." The Doctor goes on to state that this is the most usual variety of the disease and that during the *intermission* (if so it can be called) *there is languor, loss of appetite, restlessness* and not unfrequently a painful apprehension of some impending evil.

In other portions of this interesting article it is stated, that the symptoms which denote congestion are the first and most prominent in marking the onset of the disease. Again it may be very slow in forming, the patient complaining of fullness in the chest, loss of appetite, inability to sleep, and is discontended and peevish several days before the supervention of aggravated symptoms.

*Doctor Bates, a resident of the prairies not very far from Selma, in his description of the cold and perilous stage of this disease, uses the following language: "The skin of the whole body is pale and cooler than natural, shrivelled and where pressure is made, as on the back and sides, motley and livid; frequently the whole body is covered with clammy exudation. The nose and ears become icy cold, and the lips perfectly livid. * * * * * There is a great diminution of sensibility, and oftentimes the patient does not manifest any feeling on the application of the most powerful rubefacients. The countenance betrays the greatest anxiety, the eyes are red and suffused. The patient turns frequently in bed, or tosses from side to side, uncovers himself, com-

* This Essay is among the papers belonging to your Society.

plains of great heat, intense thirst, and says, if water is not given him "*he will burn up.*" The respiration is more frequent than ordinary; sometimes it is slower, but very difficult, frequent sighing, with expiration like one out of breath. The pulse is small, and more frequent than natural. In the most violent cases it is imperceptible, except just above the elbow. The heart is felt beating tumultuously as if struggling to free itself of a load. * * * The venous system is greatly engorged. * * * The tongue presents various appearances, sometimes it is slightly furred, white or yellowish, frequently large and flabby. * * * * * * Sometimes there is fullness in the epigastric region, and the patient feels no relief until he has vomited. Oftentimes the dejections are copious, and after one or two evacuations become watery, containing small flocculi of mucous. * * * The intellectual faculties are often retained to the last, * * frequently there is dimness of vision, and obtuseness of hearing."

Dr. Bates' observations have been made within the last three years. It should be borne in mind that the foregoing accurate and vivid description represents the condition of the patient whilst in that stage, which it is contended by some, is synonymous with the cold stage of intermittent and remittent fever. The Doctor says, however, "there is very little difficulty in distinguishing between this and all other diseases."

Doctor H. V. Smith, of Lowndes Co., informs us that 1834 was the first year in which congestive fever made its appearance in that section of the State, and that during the three first years of its prevalence, it was a disease of one paroxysm,—at least, such as has been described by the writer, as prevailing during the same years in Dallas County.—Doctor H. V. Wooten, also of Lowndes Co., has furnished us with notes of cases, taken during the last few years. As Dr. W. resides in the same place with Dr. Smith, and reports none but cases of a distinct intermittent type, his notes will serve, not only to show the modified character of the disease at the present time, but conveys a correct description of this variety. We take the liberty of abbreviating his notes.

"T., a white male, aged 17, was attacked on the 15th September, with a light chill, which was soon followed by febrile reaction; the fever subsided in seven hours with free perspiration. Although he was restless, with a slight feeling of oppression in the epigastric region, still he walked about in the afternoon, and retired at bed-time, without complaining. About 2 o'clock the next morning, he was seized "*with a chill*," and when a short time after, we attended his bed-side, he was found extremely restless, toes, fingers, and nose quite cold, trunk and head hot, complained by gesture of great distress in the region of the stomach, could not speak intelligibly, moans, rolls about the bed, making frequent efforts to get up. The coldness of the extremities increased very rapidly, soon extending to the trunk. About 9 hours after the paroxysm commenced, whilst the extremities were yet cold, and the pulse in that extremely small and frequent state so characteristic of a grave chill, a copious perspiration broke out—this sweating stage ensued *without reaction from* the chill. The grave and depressing symptoms continued until about 10 o'clock at night, when he become easier, spoke sensibly, manifesting a sense of *great fatigue and exhaustion.* An imperfect reaction, the extremities remaining cool, and perspiration continuing, lasted until morning, when there was a return of all the depressing symptoms of the morning previous, attended with profound stupor. He died about 3 o'clock, P. M." It is due to the doctor to state, that he did not have the management of this case.

"A girl 12 years of age, very plethoric, and unaccustomed to the climate, was seized with a chill on the 10th of August. On reaching the house, four hours after the attack, found her sitting up, saying she was getting better. I noticed that her breathing was hurried, complexion, particularly that of the nails and around the eyes was livid, pulse full and frequent, bowels constipated, tongue coated, and complained of fullness about the head and chest. Bled 12 ounces, which relieved the head and chest. As this paroxysm was the first, and came on in the afternoon, did not fear another until the afternoon of the second day. She was so much better in the morning of the second day, that her parents discontinued the quinine and sent her to school. She returned home at noon, complaining of headache, pain in the back. and difficulty of breathing. At 5 o'clock, I was summoned and found her lying with her head thrown back, eyes protruded and fixed, face livid, breathing hurried and laborious, pulse scarcely perceptible at the wrist, and too frequent to be counted; extremities cold, trunk and head very hot; all present were expecting her to die every minute. With the assistance of stimulants and revulsives, she become in three or four hours restless, made efforts to tear off the plasters. At this point, the pulse had become decidedly stronger, but the lividity of the face, and difficulty of breathing, continued the same. Stimulants were suspended for the time, and 18 ounces of blood drawn. After a doubtful struggle of a few hours more, she gradually improved, the respiratory functions being the last to resume a healthy and natural action."

In those persons unaccustomed to the climate, especially if the constitution is robust, the pulse is always fuller, and local plethora much more manifest. We have also noticed, that in unacclimated persons, laboring under an attack of the swamp fever, of which we have previously spoken, there is greater vascularity and more tension of pulse than in the natives or those living long in the country. With this class of patients, in either case, blood-letting can be resorted to with great advantage.

"A., aged 24, of good constitution, complained for the last two mornings of languor and aching in the bones, but would so improve in the afternoon as to attend to his ordinary duties. On the third morning (6th Sept.,) at 5 o'clock, I was summoned in haste, and found him wholly insensible to surrounding objects; would not answer questions, open his mouth, or move the least from his position. With the exception of the chest, the entire surface was cold, that of the extremities intensely so; respiration laborious, pulse small and 140 in the minute; found much difficulty in getting him to swallow. At 7 o'clock in the afternoon, there were signs of improvement, as an increase in the volume of pulse, restlessness, warmth of skin &c. The notes of this case show, that from this point there was a constant increase in the force of the pulse, and warmth of skin, until a high fever was set up. There was no disposition to a return of the cold paroxysm—he recovered in a few days, the lancet having been used very freely."

Other cases reported by Dr. Wooten are in our possesion, but the three we have condensed, will, without being tedious, present a view of that simple variety known as "congestive or intermittent chill." That soreness of the body, sense of exhaustion and fatigue after reaction was established, spoken of by the Doctor, has been frequently noticed by the writer. The patient betrays by his manner of breathing, acting, and speaking, a condition resembling that of one who had passed in safety some struggle which had called forth all his physical and mental powers—the mingled feelings of exhaustion, safety, and exultation, being strongly marked in the countenance.

Dr. Wooten, speaking of the coldness of congestive fever, remarks—"It is very different from that which falls upon the body of one about dying from an inflammatory disease ; in this, there is a kind of deadness about the impression it makes, which is much like that of any inanimate substance ; but in congestive fever, the coldness, though more intense, is of a more active lively kind in its impressions."

Notwithstanding the exhibitions in this disease of *apparent strength*, (regarded by many as real,) we are firmly convinced that great muscular prostration exists. The patient is capable of these surprising efforts only at occasional intervals. In a moment of extreme agony, with his lungs and heart oppressed with dark blood, like the victim of asthma, he exclaims, "I cannot breathe, I am smothering," and by an instinctive struggle his nervous energy is rallied for a moment—he starts up—rushes for the open door or window and falls powerless on the floor. It is said by some that the action of the heart is "loud, strong and tumultuous"--hence, they conclude there cannot be diminished nervous power. True, it is "loud and tumultuous," and often beats violently against the thoracic wall ; but it is that tremulous irregular action (often seen from depressing poisons) where the heart painfully labors to force on the stream of blood which flows in too fast for its exhausted powers. The patient often complains of fullness and oppression, and one ear placed over the heart, conveys to the mind, the idea of a distended organ laboring in vain to free itself. We have here nothing of the bold, distinct measured pulsations which belong to the heart in many of the simple phlegmasiæ.

The cold adynamic condition which so essentially characterizes this malady has been likened to pathological states occurring in other diseases after injuries, loss of blood, &c.; we are unable to trace such analogies. We have under treatment at the present time, two rare cases, caused by subjects falling into a dry heated steamboat boiler, some five days since, in which the condition is not unlike that of congestive fever. The extremities are wilted and shrivelled ; the skin cold and damp, (but not perspiring profusely,) the action of the heart tremulous and oppressed, the pulse in one small and thready, in the other imperceptible, expiration harsh and violent, much internal heat, oppressed breathing and yet muscular strength sufficient to walk 100 feet, but the exertion is followed by sudden and complete prostration.

We beg leave, in this place to present from correspondents, a few of the answers to interrogatories propounded them, touching the identity of intermittent, remitttent and congestive fever. One gentleman remarks, "Remittents and intermittents of a mild character prevail in May and June; I have never seen a case of congestive fever before the 15th of July, after which the remittents disappeared. With a few exceptions the congestive fever supervenes upon simple intermittents. * * * I believe they (intermittent and congestive) are essentially the same disease; congestive being a morbid condition superadded, to the intermittent. Remittent fever may owe its origin, its remote and exciting cause to the same sources, but there is inflammation or exalted irritation of tissue or viscera. I regard intermittent and congestive fevers to result from functional disturbance under a particular state of innervation and probably an altered condition of the elements of the blood, without the necessary existence of high irritation or inflammation." We here take the liberty of asking this correspondent if his own language or narrative of the connection and relative character of these affections do not raise strong doubts as to their identity? Another correspondent answers as follows: "Congestive fever prevails only in the low swampy parts of our country, where a case of remittent fever is seldom seen after the other makes its appearance; which is usually in August. I regard it as pernicious intermittent and identical with the other forms of remittent and intermittent." This gentleman will pardon us for saying, that if the reason assigned, is the only one he has to advance in favor of the unity of these fevers, it strikes us his conclusions should have been of a directly opposite character. Another correspondent, old in the practice, says: "I have not seen a case of continued bilious fever since 1834. In that year true congestive fever first made its appearance, then it was not uncommon to terminate fatally in the first paroxysm, resembling the cholera very closely." Notwithstanding this impression of the disease the same writer goes on to remark, "I have never witnessed a prevalence of congestive fever without an increase of intermittents and remittents; in fact so uniformly is this the case that I have long since viewed them as primarily the same form of fever." Another correspondent says: "In most cases those severely attacked were stricken suddenly down with violent determination to the head, as indicated by stupor, insensibility and speechlessness, this state of things would often recur in paroxysms of 24 and 48 hours internal." Strange to say that this writer, notwithstanding the prominent difference declared by his own description, can see no reason why we should "interfere with the popular nosology of the state which places these diseases under the same head." Were we contending for the distinct individual character of congestive fever, we surely, notwithstanding their conclusions, could not ask for better advocates than these gentlemen.*

Correspondents trace such an analogy between the cold stage of intermittent and congestive fevers, as to be unable to distinguish between them. Let us very briefly define these stages. The cold stage of an intermittent or remittent begins with languor, gaping, stretching, sluggishness in motion and uneasiness in exerting it. At the same time, the face and extremities become pale, the features shrink, and the skin over the whole body appears constricted and dry; the patient becomes

* The gentlemen quoted, are our intimate and esteemed friends, otherwise we should have avoided comment.

chilly, particularly in the back; presently this chilliness or fleeting sensation of cold becomes more positive, *he says he feels very cold,* "and acts and looks as though it was intense;" he goes to the fire, or has piled on him as many blankets as can be procured; he shivers and shakes violently; this frequently occurs too when very little coldness is felt by others except it be the fingers and toes. His pulse is low and feeble, often he complains of pain in his head, back and limbs, the skin remaining dry. After this has lasted some time, fever sets in &c. What now are the phenomena of the cold stage of congestive fever? The patient first complains of being languid and oppressed, he is restless, his skin is cool, damp and sticky, his breathing is interrupted; a few hours elapse and the extremities, as felt by another, become intensely cold, soon extending up to the body; the pulse is here small and frequent, becoming finer and finer until it is imperceptible; he is oppressed, restless, moans, breathes with difficulty, the whole surface is bathed with cold sweat; complains of being hot, *burning up*, cannot bear the fire, all clothing is oppression, he wants the cold fresh air to blow upon him.—These are some of the more usual symptoms distinguishing the *cold stage* of these respective diseases—let us now take comparatively, one of the most simple of the phenomena, perspiration, and trace the *analogy.* In intermittents and remittents it does not come on until *after reaction;* in this case it is one of those fortunate excretions by which nature throws *off* disease; its appearance is the herald of a remission or convalescence. In congestive fever it is a diseased exudation and among the *first symptoms* of complaint, continuing throughout the cold stage, constituting a grave symptom, and in proportion as it diminishes so does the patient improve. Its subsidence here is a remission or the beginning of convalescence. Extend the comparison to all the symptoms, stages, terminations and ultimate results, and it strikes us that *all analogy* is lost. In what particular then do they resemble? It is only in habit or periodicity. After we have noticed the diseases of Mobile City, and detailed a few of the many instances of the blending of the febrile endemical diseases of the State that have come under our observation, we will again recur to this branch of our enquiry.

Occurring in the same localities and at the same time with congestive fever, is an occasional case of violent acute affection, the seat of which would seem to be wholly and entirely in the stomach. Owing to the circumstances under which they appear, together with the sunken haggard appearance of the patient, they are usually classed with congestive fever, and treated accordingly. We have, from time to time, treated four patients laboring under this form of disease, every one of which presented in itself a case of pure idiopathic acute gastritis. Three of the four died, two of them on the fourth and the other on the fifth day after the attack. The following account of one of those cases will fairly represent the whole. We condense from notes taken by Mr. Jenkins, resident student of the Marine Hospital.

Baxley, a large muscular man, arrived in the City yesterday morning, immediately from the prairie region, admitted into the Hospital this morning; says he began feeling unwell on yesterday at an early hour, but did not *give up* until late at night. This morning (10th Sept.) his condition is nearly as follows:—Skin blueish, damp and cool, pulse small, hard and sharp, great anxiety, restlessness and insatiable thirst, countenance haggard and troubled. Complete prostration of muscular power, the patient being unable to raise his hand, wishes his legs flexed upon the body, but is too much exhausted to retain them in that position. Complains of great burning at the stomach ; the vomiting is incessant. As the disease progresses, (the notes go on to say,) the pulse becomes variable and slightly intermittent, continuing small and hard, the tongue dry and very red, as also the whole internal surface of the mouth, the upper lip thinned and drawn up, the eyes injected, red and suffused, epigastrium sore to the least touch or pressure, every thing taken into the stomach is immediately rejected, swimming in an abundant discharge of grass green fluid, this matter is thrown forward on the neck and chest, the patient being too much exhausted to turn to either side. The breathing is apparently easy but very quick, slight enlargement of the abdomen takes place, bowels constipated. Night of the third, speaks incoherently, tongue very dry and red, the only word he utters is water, water. The next morning (4th day) the coldness of the extremities increases, slight convulsive tremors shake the frame, and he died at noon. Examination two hours after death. Surface, dark pale or ash color and exsanguineous. The whole internal surface of the stomach presents a dark red color ; all the coats, with the exception of the serous, vascular, red, and to all appearance thickened, the mucus coat softened, yielding under the least force ; the balance of the digestive canal healthy.—Spleen enlarged, and softened liver engorged, and varying from a healthy to a dark purplish color. Membranes of the brain vascular and congested, the blood vessels about the base much engorged and very dark ; as much as three ounces of serous fluid effused into the cavity of the cranium.

There is a variety of fever termed by physicians "cerebro-congestive," in which the brain is the organ chiefly implicated. It is more common late in autumn, and so far as our observation extends is confined, with now and then an exception, to the blacks. Doctor Silas Ames, of Montgomery, has kindly furnished us with notes of five cases ; these reports are herewith embodied, they not only convey an accurate and vivid picture of the disease, but are so perfect and complete as to be worthy of preservation as models.

"A negro boy about twenty years old, previously well, was taken on the 6th of August 1844, with slight chilliness, followed by a little fever. On the next morning, he was able to walk about a little, and set up a good deal, feeling pretty well, but very weak. (I must remark here, that I have seen persons in the remission after the first paroxysm of this kind of fever, which is generally, but not always mild, and have found the pulse feeble and quick, and the extremities cool.) At 12 o'clock M., he felt cold, complained of head-ache, and laid down. At 2 P. M., he had the following symptoms : dorsal decubitus, countenance natural, respiration slow, quiet, except a little stertor occasionally, and regular, has the appearance of a well man asleep, skin cool and moist on the head and trunk, and cold on the feet and extremities, pulse 140, thready, and exceedingly compressible, pupils dilated, but sensible to light, tongue thinly coated, and ash-colored, and mouth full of saliva, cannot be roused by shaking and loud calling, does not shrink from the cut of a lancet, does not notice the passing of an electro-magnetic current through his chest, although it agitates his body with a violent, and rocking motion. Some medicine being poured into his mouth, he took no notice of it, until his nose was held so as to prevent his breathing, when he moaned, struggled with some strength, and spat it out forcibly. At this moment I spoke to him sharply, on which he opened his eyes, and answered in three or four words distinctly, and correctly, but instantly fell off again into the same profound coma. This remarkable waking up and momentary return to conciousness, I have witnessed several times, under similar circumstances. At seven o'clock the next morning, the remission having begun about midnight, his countenance was sunken and haggard, his skin cool, and pupils a little dilated, his pulse was 110, thready and soft, with thirst, great restlessness, and prostration. He was convalesent on the next day.

"A young gentleman, aged sixteen years, was attacked with a chill on the 9th October 1842, which was quickly succeeded by convulsions; his pulse about 80 beats in the minute was nearly as full as in health, but soft and hollow, generally comatose, but sometimes wakeful, and restless, temperature of the skin natural every where, pupils dilated, refuses to swallow any thing but water, and frequently screams, and struggles violently when disturbed, does not articulate, nor indeed attempt to speak. He continued in this state, about forty-eight hours, without any remission that could be detected, during which he was bled to ten ounces, with a bad effect, had a warm bath repeatedly, with cold applications to his head, and blisters to his neck, thighs, and arms; he also took purgatives, which acted promptly, and well. Quinine was given occasionally, but as he swallowed medicine only when forced to do so, his screams, and struggles, were so distressing to the family, that they gave him anything with great reluctance. After this time however, he took it regularly and freely, with immediate and permanent benefit.

"A stout, and rather corpulant negro man, about 24 years old, was taken with a mild intermittent fever of the tertian type, in July 1844. I learned that during the exacerbations of fever, he was sluggish and stupid, but as he was able to sit up and walk about some during the apyrexia, very little attention was paid to him, and he took no medicine, except some cathartic pills once. On the 7th day of his illness, I found him in the following condition. Dorsal decubitus, countenance natural, except that the eyes were a little sunken, breathing slow, heavy, irregular and stertorous, sweating, skin cold on the feet and hands, and cool elswhere, pupils greatly dilated, and but little sensible to light, pulse varying from 115 to 120, small and yielding to the least pressure, heart beating pretty firmly, but no throbbing of the carotid or temporal arteries, insensible to any common stimulant, but moans and struggles feebly when his nostrils are closed, and when he inspires the fumes of aqua ammoniæ. He died on the next day.

"A negro woman, young, and previously healthy, was attacked on the 13th July 1846, in the afternoon, with a slight fever. On the 14th, in the morning, felt pretty well, but got sick again late in the evening, and was stupid during the night. On the 15th, about 10 o'clock, she presented the following symptoms: face full and of good expression, respiration slow, regular and quiet, looks like a well person asleep; temperature of the skin natural, except below the elbows and knees, where it was cool, pupils dilated, mouth overflowing with saliva, pulse 86, small and very soft; when roused, speaks with great reluctance, and only in monosylables, articulation thick and indistinct, apparently from sluggishness, as there is no paralysis; will not swallow anything without much urging and some violence, being disposed to eject every thing from her mouth, which she would always do if not prevented. On the next day, nearly the same symptoms were present, but much mitigated, and her pulse had risen to 96, being fuller and firmer. She was convalescent on the 17th.

"A negro man, small and spare, about fifty years old, was attacked on the 12th of September 1842, late in the afternoon with pain in the head and great sluggishness in look and motion, and indisposition to speak; nothing further was noticed in regard to him until the next morning, when I was requested to visit him. He was sitting on a pallet spread on the floor, his look timid and watchful and pupils dilated, his skin was cold and dry, pulse 120, thready and yielding to the very slightest pressure, tongue very little coated and natural in colour, breathing slow and regular without sighing, hands tremulous. On being asked how he did he answered "very well sir." I had hardly got through my examination when he began to creep over the floor, peering about as if in search of some small object; afterwards he got up and attempted to go out at the door and appeared surprised that he was prevented. He did not speak except when spoken to, and frequently not then unless addressed in a peremptory tone. When asked to take medicine, he generally assented readily, speaking naturally and respectfully; but immediately after taking it into his mouth he spit it out, without regard to the direction it was made to take, a symptom which exasperated the overseer beyond measure. The same thing happened when water or food was given him. So that he swallowed *nothing* for more than twenty-four hours, during which time he was watched and attended to carefully. He slept none on the day that I visited, nor any in the night following. He got better on the next day, and was convalescent on the 15th.

"The coolness of the surface in these cases is never the coldness of collapse, nor is there even the profuse sweating, vomiting, diarrhœa, oppression of the epigastrium, sighing, jactitation and general restlessness of the obdominal congestive remittent. Neither have I ever observed muttering delirium or picking at the bed clothes. Head-ache is never spontaneously spoken of when the disease is fully developed, although it is a common precusory symptom. The aspect of most of the cases is that of profound sleep, an apoplectic state without the stertor or pulse of apoplexy. Very rarely have I seen cases in which the skin was above the natural temperature with some throbbing of the carotids, but never with any *firmness* of the pulse.

"In the latter cases there is never a profound coma, and there is always I believe some evidence of inflammation in the small or large intestines. Cases *first*, *third* and *fourth*, present the disease in its most common varieties. The refusal to swallow medicines is a characteristic symptom. I have not made notes of any case in which it is not mentioned except *case third*, nor can I remember any other in which it was absent.

"The anatomical characters of the cerebral congestive remittent fever, are little else than fulness of the blood vessels of the brain, with occasionally a coagulum of blood, or a liquid effusion in some part of this organ. I am inclined to think that whatever other lesions may be found after death, these are all that belong properly to the disease.

"I may be generalising too hastily however, for I have had an opportunity of observing them but in four cases; one of them is the third of those I send you. In this case the examination was made about three hours after death. The pia mater was a good deal congested; about the base of the brain the blood vessels were particularly full and dark, the brain showed an unsual large number of large red points when cut; there was also about a drachm of serum in the lateral ventricles, and the vessels on the surface of the corpora striata were very plainly defined. No other morbid appearances could be detected in any other part of the body after a careful examination. In another, besides the cerebral congestion there were only some doubtful appearances of disease in a part of the ilium.

In a third, there was besides the congestion, a small flat coagulum of blood under the pons varolii; there was also a slight circumscribed redness in the smaller curvature of the stomach, and several small patches of the same kind in the greater; the heart was pale, the left ventricle thick and firm, and the right flaccid; some of the mesenteric glands were enlarged. In a fourth, to the usual appearances of the pia mater and brain, was added some redness of a part of the inner coat of the large intestines."

Since 1840, we have examined in the Mobile Hospitals, eleven bodies of those dying in the acute stage of congestive fever; but as our time is limited, and we have already wandered into more paths than was originally designed, the notes of these examinations will be defered for a supplemental article. We will state, however, in relation to the stomach, that, with the exception here and there of a deep red patch very circumscribed, the mucus coat was of a dull pearl color. This coat in 8 of the 11 was certainly much softened, giving way with very little force and not *stripping off* in bands like those dying of yellow fever. In two of the 8 the mucous coat appeared thickened, raised and pulpy, coming off before the finger nail or handle of the scalpel like a thick gelatinous or stringy mucus.

We have already pointed out and sifted that testimony which induces the belief, that with *an equal exposure*, all ages and sexes of the white population are alike susceptible—as for ourselves, we have, notwithstanding much experience, met with no case, the subject of which was under 12 or over 45 years of age; still, as the young and athletic are by habit and occupation more exposed than those in infancy and old age, and being bound to yield what is due to the experience and observation of others, we cannot demur to the conclusion. The negroes, however, who are much more exposed than even the young men of the white population, are seldom attacked. In 1835 and 1836, the writer's circle of practice embraced a population of 1500;—one thousand of whom were negroes and the other whites. During the autumn of these two years he treated 88 cases of grave congestive fever; among the whole number there were but three cases occurring in negroes, and two of these were of that variety described by Dr. Ames and occurred late in October.—Dr. Gale, of Marengo county, who has practiced many years in the prairie region, says, that negroes are rarely attacked, having seen but three or four in his whole course of practice. Of 25 correspondents residing in different sections of the State, two thirds aver, that with the limited exposure to which the whites are subjected, negroes would not have the disease. During warm weather this class of persons enjoy the best of health; but after the approach of cold nights and mornings, such as we have in October and November, disease, such as a low typhoid affection coming on with a chill, the skin continuing cool and damp, is rife among them. In these disorders diarrhœa followed by immoderate collapse frequently ensue, putting a period to the life of the patient in a few days. Disorders of this description are often confounded with congestive fever.

As before remarked, it is only here and there that we now see the disease in that violent form which distinguished it in 1835 and '36. In some localities it is the prevailing disease in September and October; whilst in others it only appears, like the occasional assassin, striking down the vigor and noon of life, to the terror and dismay of friends and neighbors. A few years more and the physical changes going on will have *uprooted* its sources and unveiled to the light its last hiding place.

It will be perceived that our attention has been directed to the more prominent and graver febrile affections, making no special reference to the simple remittents that prevail so generally throughout the State.—Any account of them would not be compatible with the general plan of this paper; and even if it were, we should deem it unnecessary to do more than refer to the very excellent history and definition of this form of febrile affection, by Dr. Boling, of Montgomery.

Before attempting any account or analysis of the prominent febrile affections of Mobile, which will be followed with some remarks touching the febrile poisons of the State, we will turn our attention to the disorders of winter and spring.

DISEASES OF WINTER AND SPRING.

It has been observed by those scientific men who have directed their attention to the subject, that there is but little variation in the average amount of rain from year to year, and that an undue state of high thermometrical range during the summer is usually followed by severity of cold during winter and spring. Corresponding with these ordinances of nature, we find that if the summer and autumn months have been comparatively exempt from disease, there is invariably an excess of winter and spring affections.

As previously remarked, we find that since 1832 the high inflammatory tone of all acute diseases during the second epoch has changed to that of a grave and malignant character, and although this change is very marked in all the affections of winter, yet we find it more strongly exemplified in the diseases of summer and autumn, when contrasted with those of a former epoch.

Individual observation and experience induce the belief, that when the exanthematous affections prevail during winter and spring, there is a peculiar condition of atmosphere, differing essentially from that which would seem to constitute the cause of disease in the pulmonary tissues. For instance, if the ground has been saturated with water to an extent covering its surface, and cold Northerly or Easterly winds prevail, we find pneumonia or pleurisy, either singly or combined, to constitute the most prominent epidemic of the winter or spring months, raging with varying intensity.

When the clouds during those seasons only descend in the form of mist, for any considerable length of time, sufficient however, to moisten the earth and atmosphere, and a Southerly or Westerly wind prevails, we find disease characterized by all those well marked symptoms that constitute the exanthemata.

It is to these two forms of disease, (the thoracic and cutaneous,) that we should properly confine our attention, for those more common to the North, as rheumatism, phthisis &c. are rare among us, although sporadic cases are occasionally met with, but not sufficient to constitute in the regular order, a prominent winter disease. When the months of autumn are closed by copious rains, it is usually noticed that thoracic diseases make their appearance; at first, manifested by catarrh, influenza and incipient bronchitis, advancing onward until the pleura and lungs become seriously invaded. Those gentlemen with whom we have corresponded unite in the opinion, that both pleurisy and pneumonia, either in a distinct or united form, are most generally attended with symptoms of a low grade, strongly resembling a typhoid condition of the system, and that these symptoms, attend the course of disease. Independant of such pathogmonic signs, as cool skin, depressed pulse, and phlegmonous tongue, we have confirmatory evidence of the true character of these diseases in the treatment adopted by physicians; for instead of the lancet, antimony and contra-stimulants, they are forced to resort to those agents that are known to sustain and nourish the depressed system. It is true that in some particular localities there is a tendency to active inflammation; but even here, in a large mass of cases, we find in the early stage, as well as during the oscillating changes that ensue, a tendency to collapse. It has been remarked by a distinguished writer, (S. Forry) on the climate of the United States, that in pneumonia and pleuritis, as well as other winter affections, if the wind be cold and damp, the system is especially liable to all the irregular action of the capillaries generally imputed to those causes. Hence, he remarks, there is reason for the commonly received opinion, that in Georgia and Florida, the dry air of the interior, in conjunction with the aroma of pine forests, is peculiarly congenial to pulmonic affections. If we compare the diseases of the sandy barrens of the lowlands and the hilly pine country of Alabama, with those of the Tennessee Valley, prairies and the river swamps, it matters not as to immediate latitude, we will find the same rule hold good in Alabama, that is applied to Georgia and Florida. But of all the causes tending to designate a particular type of pneumonia or pleuritis, the nature of the soil and attending local circumstances are most prominent, and so marked is this influence, that a knowledge of the locality and grade of autumnal fevers that previously prevailed, is often necessary to a correct diagnosis and treatment of the disease.

Where is the medical man who has not observed during the months when the chilling blasts from a hyperborean region strike on the predisposed constitution, inducing pulmonic disease, the impress of that type which immediately preceded? If there is one who doubts this position, we would direct his attention to the intermittent and remittent pneumonia of the hill top and mill pond, so lately the abodes of fever of the same types. So with the malignant and congestive typhoid pneumonia of the river and prairie swamps, which but a short time previous, were the lurking places of a kindred and equally unfriendly monster. If this be not satisfactory, we would bid him pass to that season, when the vernal breezes of a tropical climate unfold the bounty of nature, and he will still find in the chain of morbid sequences, that local causes are operating with unerring chemical law, to designate the type and character of disease.

In the treatment of those maladies peculiar to this season, it is very important that operating causes should be well considered, and that close observation relative to topography be made; for in some localities, a modified antiphlogistic course and in others, a stimulating plan is required.

With these general remarks, we will proceed to take a cursory review of those winter epidemics that have prevailed since 1832.

We have previously observed, that after a series of influenza and catarrhal fever at the onset of winter, pleuritis or pheumonia is generally presented to our notice as next in the chain of sequences, if favouring circumstances should tend to develop their character. It is unnecessary to take a general range throughout the State, as a few cases drawn from observation and corroborated by the testimony of others, is all sufficient to indicate their true character, varied according to their peculiar locality.

Bilious or typhoid pneumonia as presented to our notice, may, strictly speaking, be separated into a distinct type, for although in both forms of that disease, there may be a chill or rigor to usher in the attack, with all the concomitant symptoms that denote its peculiar character, yet there is throughout its course a very marked difference.

For a graphic and true description of these two forms of pneumonia, we will resort to a paper addressed to your society by Dr. C. A. Woodruff.

"So far as my observation has extended, bilious pneumonia is invariably ushered in by a chill or rigor of indefinite duration, succeeded by an intense heat of the skin extending over the whole surface of the body, and during the febrile excitement the pulse is full, strong and corded, with a slight remission at the interval of eighteen hours. The eye is glassy, the pupil dilated, the conjunctiva heavily injected with red blood, and a wild vacant stare, that on first view would convey the impression of delirium. Nor can the physician console himself with the hope that no cerebral determination exists, for although the patient may readily and correctly answer any question that may be put to him, yet no sooner has he left the room, than a rambling, incoherent conversation is commenced with the attendants at the bed side. The tongue is heavily coated with a thick tenaceous fur, at first white or greyish, and then changing to a dark orange, or deep brown color.

"The alvine discharges from the commencement of the attack, are of a brown ochre or dark green color, and generally continue without material change throughout the course of the disease.

"There is often violent spasm in the abdominal region, sometimes distinctly traced in the colon, and at others confined to the ductus choledochus, attended with the most excruciating misery; the pain in the lumbar region is very intense, extending along the spinal column, and as described by the patient, "as if his back was broken." The pain in the chest is not very acute at the commencement of attack, more usually a general degree of soreness, and a sense of constriction or tightness of the pectoral muscles; the expectoration is of a dark brown viscid mucus, sometimes almost black, in general resembling the outer covering of the muscadine grape, and the blood when drawn from the arm or by cups from the chest, is extremely thick, of a dark colour and quickly coagulates. There is great gastric derangement, constant nausea, the stomach often rejecting the fluid it receives, and there is not unfrequently incessant vomiting of dark green bile.

"Typhoid pneumonia is usually preceded by acute pain over the temporal, frontal and occipital regions of the head, and sharp lancinating pain in the chest; the eyes of a dull leaden hue, watery, with great intolerance of light; the cheek usually suffused with a bright scarlet glow, sometimes extending over the forehead, chin and neck.

"There is no distinct chill, but a slight coldness of the extremities, hands, feet and nose, generally returning every day, without regularity as to time, and not unfrequently, there may be two distinct paroxysms in the twenty-four hours, invariably succeeded by increased arterial action, although the skin is but little above the natural temperature.

"The pulse presents the most marked phenomena from its varied character in this form of disease; at times small, tremulous, and vibrating, while at other times it is soft, intermitting and thready, ranging from 120 to 160 pulsations in the minute, and changing from day to day. The tongue in the commencement of attack, presents a milk or cream colored appearance, with a dark streak running down the centre and red on the edges, but soon it changes to a bright red, dry, cracked and matted.

"There is usually a hacking cough, with scanty expectoration, of an iron rust color, attended with much pain in coughing, great difficulty of breathing, accompanied with stertor in the respiration, a harsh mucous râle, and not unfrequently sighing.

"One peculiarity attendant on this disease, is the intense soreness, complained of over the entire scalp; the abdominal region is extremely sensitive to the touch, and yet, on minute examination, showing no marked symptoms of active inflammation.

"The bowels are generally loose, or easily acted on by the mildest laxatives, and in this respect there is a marked contrast to the bilious type, which usually demands the most active purgatives.

"As the disease progresses, the bowels become more constipated, or when acted on, the dejections are thin, a glairy mucus, or gelatinous bile, and the expectoration freely passes off of a brick-dust color, attended with a copious perspiration over the entire body."

This disease is usually of a remittent or continued form, and may be said to represent the true character of that prevailing in the prairies, along water courses, and in fact, all such portions of country distinguished as unhealthy in the summer and autumnal months.

There is a form of pneumonia, commonly known as "congestive," which may be said to bear the same relation to those described by Dr. Woodruff, that the congestive does to a remittent fever. The patient first complains of being weak, drowsy, but unable to sleep, with a sense of oppression and constriction across the chest, the skin becomes cold, damp, and shrivelled, breathing labored, with all the normal respiratory sounds merged in one loud, continous mucous râle. As the disease advances, a dark red spot appears upon the cheek, the patient expectorates a bloody frothy mucus, every symptom of thoracic congestion increases with fearful rapidity, the function of respiration is at last confined to the bronchia and trachea, the eye becomes glassy and protruded, clearly expressive of deep physical agony, but an unclouded reason; and in this form, the disease proves fatal in from 20 to 50 hours.

In the hilly country, the character of pneumonia is somewhat modified, presenting in many instances more inflammatory action, which perhaps may proceed as well from the peculiarity of location, being remote from those low depressing causes, as from the habits and constitution of the residents.

Well marked and uncomplicated pleuritis is a rare form of disease to be met with during this epoch, especially prevailing to any extent in any one settlement. Sporadic cases, however, are sometimes found, fully developed in every particular symptom, of a true inflammatory type; but when the disease assumes an epidemic form, there is usually marked pneumonic inflammation in conjunction with that of the pleura, constituting pleuro-pneumonia.

The symptoms attendant on this form of disease vary but little from those previously described under the head of typhoid pneumonia, with the exception of more intense pain in the costal region and perhaps less expectoration, although in a majority of cases nice discrimination has to be observed to distinguish between the two diseases.

The cause of these extraordinary phenomena, complication being an exception to the general rule, is not easy to arrive at, but we will quote from our correspondent, Dr. Pearson, of Pickens county, on the subject.

"I do not recollect to have seen a case of old fashioned pleurisy since I came to Alabama, in 1836. Here, the cases are complicated, and in my opinion traceable to miasm, coming on with a chill, &c. I have known it to prevail epidemically, and upon the borders of dirty water courses exclusively."

This opinion seems to be concurred in by most medical men, with whom we have corresponded on the subject, from various portions of the State. The colored population, who are comparatively exempt from severe visitations of fever during the summer and autumn, suffer more severely from these affections than do the whites. Children are often attacked, but the disease is quite manageable, with proper care.

Remittent bilious pneumonia prevailed to a considerable extent among children of all ages and color in Mobile during the past spring, and although many cases were protracted, yet it was not fatal; the paroxysms being characterized by all the symptoms of the disorder in the adult, the remissions more complete, taking place every morning.

Dr. Mabry, of Dallas county, has furnished us with notes of a case of pneumonia, with the post-mortem examination, presenting lesions that would hardly be supposed to exist during life in this disease, and as it may throw some light in pathological investigation, we take pleasure in inserting it in this paper. He remarks: "February 1st, I visited a negro boy, aged 19, plethoric habit, presenting the following symptoms: respiration hurried, pain in the right lower portion of the chest, distressing cough and free expectoration of a prune-colored sputa, feeble and frequent pulse, tongue coated with a very heavy yellow fur, moist, diarrhœa, natural appearance of skin and countenance.

"The treatment observed, was small portions of calomel, Dover's powder, and ipecac, combined, until ptyalism ensued, and bilious evacuations produced; vesication over the seat of pain, nauseating doses of ipecac, and demulcent drinks; but notwithstanding the tongue cleaned and liver became active, the patient grew worse until the 9th, and died.

"*Examination* 10 *hours after death*—found considerable quantity of sero-purulent matter in the right thoracic cavity. The pleuræ of this side adherent nearly the whole extent by means of a thick, greenish, coagulable lymph. The membrane softened on both sides, the whole of the right lung and lower third of the left hepatized. Pericardium, contained a considerable quantity of serum. The substance of the heart healthy, but found a fleshy cord covered with coagulum of dark blood attached to the internal surface of each ventrical, following the course of the aorta and pulmonary artery."

It is a generally received opinion among medical men, in various portions of the State, that typhoid fever, as described by Bartlett and others, has been an annual disease, although not prevailing as an epidemic, except from some local and strongly exciting causes. The first information that is presented to our notice of its appearance in a marked form, was in Dallas Co., during the spring of 1835. In this instance it was the sequence of typhoid pneumonia that had prevailed during the previous winter, assuming at that time an irregular intermittent type.

A planter in that county, for the purpose of procuring manure for some wornout lands, had exposed to the weather, several hundred bushels of cotton seed, which during the latter part of December, became completely saturated with water; the heat retained in so large a mass soon set up very active decomposition. Some fifteen or twenty negro houses were situated in a circuit immediately around the spot where the seed were exposed. About the middle of January, several cases of pneumonia were developed among the negroes, which continued occasionally to attack them, until the month of March. The spring opening warm, the disease immediately assumed a new type, and continued to prevail until every negro above the age of five years had been seized. The attack in these cases was insidious, the disease forming very slowly, there was a slight remission every morning for five or six days, after which, it became continued, with clammy skin, quick compressible pulse, diarrhœa, coma, and sordes of the teeth; the disease ran its course in from 15 to 25 days; average mortality about 20 per cent.

Dr. Pearson mentions a striking instance of a similar character, and as such facts, when clearly ascertained, cannot fail to assist in the most important of medical enquiries, we will here quote, from his esteemed favor.

"The past winter, a friend of mine had his gin house burned down, in which there was a great deal of cotton seed and cotton in the seed. This pile was burning for weeks. As his stock used in this yard, the ground which was prairie, became soft from the treading of the cattle; there being also a good deal of wet weather, it gave rise to a collection of water over the charred seed, which became peculiarly offensive both to the eye and olfactories; with myself it produced a paroxysm of sneezing.

"It was on the South side of his negro quarter, the inmates of which became sick. The first cases were of typhoid, and pleuro-pneumonia, and after the weather became warm, it assumed the type and character of typhoid fever. The members of his family both black and white, remote from the *quarter*, although not more than two hundred yards distant, but in an Easterly direction from the contaminated spot, were exempt from the disease. I feel certain that this disease had its origin as stated, and was strictly local, as the entire neighborhood was healthy. There were as many as 60 cases, and all were benefitted by quinine."

These, and other instances, which we have not space to detail, are sufficient to induce the belief that the decomposition of cotton seed by process of fermentation, produces a gas—probably the ammoniacal—which affects a majority of those who may be subjected to its deleterious agency. These effects of course will vary with the change of season, and in that revolution the constitution of the atmosphere induces varied types.

We have repeated instances presented to notice, where low, dirty buildings become the place of deposit for filth acted on by moisture, or the decaying of the logs, with which a large portion of our country houses are constructed, becoming the medium of disease.

Typhoid fever, as usually presented in this section of country, is variable in its character, in many cases attended with grave and malignant symptoms, owing in a great measure to locality; as for instance, in our prairies and low bottom lands; whilst on the other hand, there is more vascular excitement and inflammatory action in the hilly region of country.

In the former instance, Dr. Hogan, in his letter, says, "The disease is usually ushered in by a chill, not unfrequently a double tertian; the fever being remittent, with partial perspiration, great enteric irritation, terminating in special congestion, with a species of *paralysis agitans*, or, meningitis may supervene. There is more or less pulmonary congestion that is preceded by cerebral disturbance, sometimes in the form of delirium, and at other times it may be attended with stupor, or coma. In brief, typhoid fever may invade by the brain, the lungs, or the bowels, and in the grave cases, all these organs are apt to be involved, and your patient may die from exhaustion, in a physiological condition.

Whenever a patient is found with a dry red tongue, excessive tenderness of the abdomen, small fluttering, or wiry pulse, that is easy to be compressed, the eye dull, leaden and watery, morbid condition of the cerebral organs, and the bowels easily excited to action, we should not hesitate under ordinary circumstances, to pronounce it a case of typhoid fever of the low country.

Typhoid fever prevails in autumn, winter and spring; most usually the latter, and is met with annually in various sections of our country, either sporadic, or prevailing as an epidemic on some particular plantation, attributable to local causes. The symptoms that mark this disease in the upland region are so very dissimilar in many respects from that of the low country, that we do not hesitate to adopt the true pathological signs, as furnished us by Dr. Clarke, a resident of Benton Co., Alabama, who says:

"The fever,—typhoid,—was not ushered in by any distinct chill, but was some days in forming, and crept slowly on the patient. They grew dull, complained of being unwell, "and weak," the skin became dry and harsh, there was anorexia, headache, stupidity, and sleeplessness; there was at first some degree of constipation, subsequently diarrhœa; finally, more or less fever was developed, the pulse becoming accelerated, but always compressible, in some cases from 75 to 90, in others from 100 to 120. The skin usually became hot, especially about the head and across the abdomen; in some there was a tendency to coldness of the feet, which was difficult to remove; in others the feet like the rest of the surface became hot. The tongue by degrees assumed a redness on the edge, and in some bad cases was dry and fissured, there was dryness of the mouth, and constant calls for water.

"One marked symptom usually prevailed in every case, viz: *intense pulsation of the carotids*, the alvine evacuations frequently change as to consistence and color; the abdomen becomes tympanitic, in some cases highly so, and after a few nights insomnolency and delirium.

"The fever generally lasted from 15 to 21 days, and it was from 5 to 6 weeks after the patient complained of being unwell, before restoration to health."

This disease, like the various forms of pneumonia, prevails most among the negroes, especially in Middle and South Alabama. On some plantations two thirds of the residents are attacked with some form of the disease, and in many instances from 15 to 33 per cent. of those attacked have died.

The anatomical character of this disease from the examinations made by Dr. Clarke and the writer, appears to consist of an enlargement and softening of the spleen, dark leaden appearance of a portion of the small intestines, with now and then thickened, dark, ash colored patches in the ilium, which are easily scraped down; very little redness noticed any where in the canal, but on the contrary, a pale darkened appearance. In two cases that died in the Marine Hospital, there was ulceration of the eliptical plates of the ilium, such as the writer had seen in the Massachusetts Hospital; the mesenteric glands were enlarged in every case.

Taking into view the rapid decline of severe autumnal fevers, together with the lessening mortality attending them and the rapid increase of those diseases just pointed out, the winter and spring diseases may now be regarded as the most fearful maladies within the borders of the State. To the negro, whose organization is such as to endure the heat of summer with impunity, these diseases which come on the chilly blast and are nourished by cold and moisture, are peculiarly noxious and alarmingly fatal.

In this section of the Union, scarlatina does not seem to be attended with the same degree of malignancy, as in the older States, neither has it been noticed to observe the same periodic and epidemic character as presented elsewhere. As a general rule it may be said to be sporadic in its nature, some few cases appearing in various sections of the country every year, but generally mild and easily controlled by remedies.—There are however some exceptions in different localities where the disease has assumed a malignant type, but its fatality has usually been confined to a limited space, and but few years have been noticed when this form prevailed.

It would appear that whenever the disease is presented during the spring or mid-summer, its character is usually mild; but if in the autumn or winter it assumes a more malignant type. The winter and spring of 1844, were marked with great fatality consequent on this disease in the middle counties of the State. During that period the disease was complicated in its nature, assuming at times so much the appearance of rubeola with catarrhal affection as to warrant the belief of a distinct disease existing, and then again the two diseases were so intimately blended as to defy the utmost scrutiny of pathology.

The exanthematous affections like those of the thoracic viscera, are rapidly increasing, and if we are to judge by the effects generally produced by physical changes, the day is not far distant when they will become the prominent disorders of the State, and the affections of summer and autumn, gradually yield to an improved state of cultivation.

(*To be continued.*)

II.—*Review of Opinions Concerning the Cause of the Coagulation of the Blood.* By John Harrison, M. D. Professor of Physiology and Pathology in the Medical College of Louisiana.

There is perhaps no phenomenon more calculated to excite the attention of the curious than the coagulation of the blood—the rapid change of form from the liquid to the solid state was enough in itself to attract curiosity, but the interest if the subject was greatly enhanced, when the theories advanced in explanation were found to be so various, so contradictory, and so unsatisfactory. It is my purpose in the present paper to review these theories, they will present some curious specimens of medical logic upon indeed a very perplexing subject.

With the exception of one theory (that of Prévost and Dumas, etc.,) it is now conceded by all that the coagulation of the blood is due to the consolidation of the fibrine held in solution in the serum both together, they constitute the *liquor sanguinis.* The question, then, is limited to the cause of this change in the fibrine. But before taking up this question, it becomes us to give the theory of Prévost and Dumas, and show the grounds of its refutation.

According to Milne Edwards* their theory is as follows: "the attraction," say they, "which keeps the red matter fixed around the white globules having ceased along with the motion of the fluid, these globules

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* Cyclopedia of Anat. and Physiology, Art. Blood.

are left at liberty to obey the force which tends to make them combine and form a net-work in the meshes, or amid the plates of which the colouring matter is included along with a great quantity of particles which have escaped this spontaneous decomposition.".

Müller has the merit of overthrowing this theory,* which was for some time very generally received. His remarks are these†—"Berzelius, observing that lymph contains fibrine in solution, conjectures that the blood must also contain it in that state, because, he says, the lymph is a fluid seperated from the blood. Berzelius therefore suggested that the clot was formed by the fibrin coagulating and enclosing the red particles. This idea of the fibrin being in a state of solution in the blood has been advanced several different times, I have been so fortunate as to discover a definative proof of Berzelius' conjecture. In some frogs' blood which had been received in a watch-glass, I observed that before the whole mass coagulated some colourless transparent clots formed, which I could draw to the edge of the glass with a needle and on pouring off the blood, one or two minutes after it had flowed from the animal, I perceived that there were points or small fragments of similar coagula remaining adherent to the bottom of the glass. To this experiment it might be objected, that in amputating the frog's thigh, which is the readiest mode of obtaining blood from this animal, some lymph had escaped with the blood and had given rise to these coagula, I therefore collected the blood for the future, directly from the great ischiadic artery, which runs among the muscles at the posterior part of the thigh; I laid bare this artery, which is easily found on account of its running close to the great ischiadic or crural nerve, as it is usually called, and collected the blood from the artery only and with such care as to be sure that I had pure blood; I obtained blood in the same way from the heart, which is done with more facility. In this blood, of the purity of which there could be no doubt, the same small transparent coagula were always formed, before the entire mass of blood coagulated; a drop of this pure blood was diluted with serum, and placed under the microscope, the globules then appeared widely separated but in the spaces between them I could discern the formation of a coagulum which connected these bodies together however, with the intervals between them and by placing a needle between any two globules and moving it about, I could set the whole mass in motion; as the red particles of the frog's blood appear very large, when viewed by a high magnifying power, this experiment admits of the greatest accuracy, and is perfectly convincing. There is, however, an-

* So, at least, it is frequently said. I do not know the date of Müller's experiment, but in a work published by Sir Charles Scudamore, in 1826, we find the following experiment and remarks.—"Exp. 38. The Blood, immediately after being received in the basin, was kept briskly stirred with a piece of stick, and this being continued for a quarter of an hour, a mass of fibrine became twisted round it. The coloured portion in the basin remained permanently fluid, and from subsequent digestion of it in water, no more fibrine could be obtained. Hence we see, in a simple and familiar manner, that the solidification of the fibrine is the essential cause of the coagulation of the crassamentum of the blood. This fact of the separation of the fibrine by stirring the blood, is mentioned by Mr. Hewson." Essay on the Blood.

† Elements of Physiology, p. 110, English Edit.

other and much easier, and indeed still more unquestionable method of demonstrating the same fact, knowing that the red particles of frog's blood are four times the size of those bodies in the blood of mammalia, I conjectured that although the red particles of the latter animals pass through filter paper, those of the frog might not. I found this opinion correct; thus as generally happens, the most simple means was the last thought of. I am now able to show at lecture, by an easy experiment, that fibrin is held in solution in the blood, that it passes limpid through the filter and then coagulates. The experiment can be made quite on a small scale, with the blood of a single frog; a small glass funnel and a filter of common white filter paper, or not very thick printing paper, are all the apparatus required. The filter must be of course previously moistened, and it is better to add some water to the blood as soon as the latter is poured into the filter; what then passes through is a perfectly clear serous fluid diluted with water and merely tinged in the slightest degree with the red colouring matter, which, in frog's blood, is not rapidly dissolved, sometimes it is quite colourless. If in the place of water a very dilute syrup containing one part of sugar in two hundred or more parts of water is employed, the red envelopes of the red particles is not at all acted on, and the filtered fluid is perfectly colourless. No globules can be discovered in this fluid, by the aid of the microscope; in a few minutes a coagulum forms, which on account of its transparency would not be remarked, were it not drawn out of the fluid with a needle. This coagulum gradually contracts, becomes whitish and fibrous, and then has exactly the aspect of human lymph. The fibrin of the blood is by this means obtained in a purer state than is possible by any other method. Of course all the fibrin of the blood is not obtained by this process; the greater part of it coagulates before it can pass through the filter. To find the paper best adapted for the filter, some trials must be made with different kinds; if the paper is too thin, some few red particles pass through it with the fluid, and will afterwards be seen here and there in the coagulum; if the paper be of a proper thickness, the coagulum will not contain a single red particle. There is no distinct appearance of granules in the fibrin thus obtained; it is quite hemogeneous; when it has contracted and become white, it acquires a finely granulated aspect. This appearance which it presents when viewed with the compound microscope may however arise merely from unevenness of the surface. There is another mode of proving that fibrine exists dissolved in the blood of the frog, as well as of the mammalia; by adding to the blood of man, or any vertebrate animal, some drops of a very concentrated solution of carbonate of potash, coagulation is retarded so that the red particles have time to subside; in the space of half an hour a soft coagulum forms, of which the inner part containing the red particles is red, while the upper is white."

A change in the fibrine constitutes, therefore, the coagulation. Is this change a chemical one, that is, so far as the composition of fibrine differs in the soluble or insoluble state; or is it one merely of form? All the effects of re-agents on fresh blood and upon coagulated fibrine, as well as the analogy and close resemblance, physically and chemicaly, between fibrine and insoluble albumen, lead us to the latter opinion.

But what causes this change, be it one in composition, or one merely of form? It was natural and proper that enquirers should turn their attention at first to the circumstances in which coagulation takes place, and endeavour to find in them the object of their pursuit. For instance, blood drawn from the veins or arteries of a healthy person and received into a basin, is, when at rest, found to coagulate in from three to ten minutes. In what respects does the blood in the basin differ from that flowing in a continued stream through the blood-vessels? Perhaps all the circumstances cannot be appreciated, but the following are obvious. It is at rest, while that in the body is in motion; the temperature is changed, generally lowered; it is in contact with the atmosphere; it exhales a *halatus* perceptible to the smell; evaporation and *possibly* the exhalation of gases go on from its surface; and it is in contact with substances entirely different from what it meets with in the living body. It was natural that these circumstances should, therefore, first command attention; but how little light has been shed on the subject by the enquiry, will be seen from what follows.

Rest and Motion.—It has been found that motion of the particles of the blood over each other when they have been removed from the body, whether this occur in air or in the vacuum of an air-pump, so far from preventing, accelerates the coagulation. But if blood be put into a vial so that it be completely filled, and then shaken up with beads or gravel, the coagulation is very much retarded but not prevented. So its repose cannot be the cause of the blood's coagulation.

Temperature.—The blood of the human body is variously estimated from 98° to 102 Fahr. If the cause of its coagulation be a loss of heat, it is but reasonable to infer that an increase of temperature would keep it fluid. Such however is not the case. The blood will coagulate at 120° Farh. much sooner than at 98°.

Again, a fall of temperature from 98°, though it retards, does not prevent coagulation, unless the blood be exposed suddenly to a freezing mixture, and then it is frozen. Even then after being allowed to thaw, it will coagulate.

Influence of the air.—It would seem that the air does in some measure accelerate the coagulation of blood. It is shown above that if blood be put into a vial so that it be completely filled, the coagulation is retarded. If, on the other hand, it be shaken up with air, the coagulation is accelerated.

On the other hand, blood exposed in the vacuum of an air-pump, coagulates sooner than when exposed to air. But this may be caused by evaporation or by the loss of carbonic acid gas,—suppositions which will be examined presently.

The fact, however, that blood received into vessels so that it completely filled them and that, too, without the possibillity of contact of air, did not fail to coagulate,* sufficiently shows that air, though it may accelerate, cannot prevent coagulation.

As to the *halatus* or *aura sanguinis* being the cause of coagulation, it is plain such cannot be the case, from the fact last mentioned. The same fact refutes any theory based upon evaporation or evolution of gases from the blood.

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* Mandl. Anat. Gen. p. 227. Hunter, the Blood, etc. p. Scudamore.

Difference of Contact.—The blood, it is contended, is in contact with *living* tissues in the body, but in contact with *dead* matter when drawn from the system; and, therefore, the blood is kept fluid by the influence of those tissues; and when it loses this influence it resumes its natural state—in other words, coagulates. Unfortunately for such an hypothesis, the blood will coagulate in a vein or artery if they be tied so as to confine it. The coagulation is retarded to be sure, but still it coagulates in due time. It is coagulated also in ecchymoses from contusions, from effusions into the celular tissue in wounds, around the walls of aneurisms, and in many other instances.

Therefore, as neither all these circumstances, nor any imaginable combination of them are sufficient to explain why the blood becomes solid when extracted from the body, the intellect of man has undertaken to supply causes, and some of them indeed are very ingenious.

But before going on with this subject it may be well to mention that the coagulation of the blood may be accelerated, retarded, or even prevented by several substances, and by certain conditions of the system.

From a recent work* I here copy a summary of what is known on this subject.

"*The Retardation or Prevention of Coagulation.*†

Fresh blood becomes solid below 32°, without the coagulation of the fibrin, which however occurs after thawing.

The blood of frozen and apparently dead frogs remains fluid, and the same is the case in hybernating animals, in which the temperature of the blood is reduced to that of cold-blooded animals.‡

The coagulation of the blood is retarded by contact with animal membranes; it will remain fluid in tied arteries for the space of three hours. Blood which has been infused into the cellular tissue will remain fluid for weeks. Schultz has observed that blood which has collected in the intestines remains fluid for a long time; moreover, the blood which has been abstracted by leeches does not coagulate, as long as it remains in the body of the animal.§

Gerhard, Hufeland and Kielmeyer, have shown that blood through which an electric current is continuously passed, remains fluid for a long time. Schubeler also showed that positive electricity hinders the coagulation of the blood; moreover, the blood of animals killed by electricity or lightning does not coagulate.

The following salts hinder the coagulation of the fibrin, according to Hewson,‖ Schultz ¶ and Hamburger's ** observations: sulphate of soda, chloride of sodium, nitrate of potash, chloride of potassium, acetate of potash, and borax, if they be added in the proportion of half an ounce to six ounces of blood. If, however, the blood be diluted with double the quantity of water, the fibrin coagulates. (Hewson.) The carbonates and acetates prevent the coagulation of the blood, in all degrees of concentration. With regard to the action of the sulphates, a concentrated solution appears to retard the coagulation; a dilute

[* Simon's Chemistry of Man.]

† [A full account of the various experiments by John Hunter, Davy, Prater, Scudamore and others, on the effects of various agents upon the coagulation of the blood, to the period it was written, may be found in Ancell's seventh lecture "on the Physiology and Pathology of the blood.—(Lancet, 1840.)

‡ Schultz, op. cit. p. 80.

§ L. c., pp. 64 and 81.

‖ Disquisitio experimentalis de sanguinis natura. L. B. 1785.

¶ Op. cit.

** Experimentorum circa sanguinis coagulationem specimen primum dis. inaug. auct. Hamburger. Berolini, 1839.

solution, on the contrary, to accelerate it. (Hamburger.) The same appears to be the case with respect to the tartrates and borates.

The following metallic salts impede the coagulation of the fibrin: sulphate of copper, ommoniaco-sulphate of copper, sulphate of the protoxide of iron, chloride of iron, ferrocyanide of potassium, acetate of lead, and tartrate of antimony and potash.*

Magendie's † observations differ cosiderably from the above. He arranges in a tabular form, ‡ the following salts which tend to impede the coagulation of the blood; the alkaline carbonates, nitrate of potash, and nitrate of lime. All observers agree that the free alkalies completely prevent the coagulation.

The observations of Schultz, Magendie, and Hamburger, show that dilute mineral and vegetable acids prevent the coagulation of blood, which however thickens, and assumes a syrupy or oily appearance. These statements have been confirmed by myself.

The following non-mineral reagents have been observed by Magendie to prevent or impede the coagulation of the fibrin; nitrate of strychnine, nitrate of morphine and nicotine. This statement, as far as regards the nitrate of strychnine, has been denied by Hamburger. §

Hunter observed that the coagulation was retarded by the addition of a solution of opium, a statement however which is not confirmed by Hamburger. The latter observer notices the effect which is produced by the addition of bile, in preventing the coagulation.

Acceleration of the Coagulation.

The coagulation of the fibrin is accelerated, or at any rate not impeded, by a temperature higher than that of the living blood. According to Hewson, it takes place most rapidly at from 114° to 120°. Scudamore and Schroder van der Kolk assert that the coagulation is accelerated by electricity and galvanic currents, which, however, is opposed to the previous observations of Kielmeyer and others. Contact with atmospheric air hastens the coagulation

According to Hamburger, no influence, either in accelerating or impeding the coagulation, is exerted by sulphate of lime, chlorate of potash, or iodide of iron. ||

According to Magendie and Hamburger, the coagulation is accelerated by acetate of morphine. The former observer states that water, a watery solution of sugar, the fluid of dropsy, Seidlitz and Vichy waters, alcohol, ether and mannite; and the latter, that decoctions of digitalis, and tobacco, solution of tannin, iodine, solution of sugar, gum Arabic, starch and fresh urine, have a similar effect.¶

It will easily be seen that none of these facts throw light upon the cause of coagulation.

We now turn to the theories. That of Prévost and Dumas, etc., has already been shown to be untenable. The next to be spoken of is that of Sir C. Scudamore.

Theory of Sir C. Scudamore.—The amount of this theory is, that

* Schultz remarked that hydrochlorate of ammonia, sulphate of potash, and sulphate of magnesia, retain the blood in a state of fluidity, and that even the addition of a large quantity of water does not produce coagulation. After the addition of sulphate of soda, the blood could only be prevented from gelatinizing by constant stirring, a step that was not requisite with the other salts. † Leçons sur le Sang. Bruxelles, 1839.

‡ Op. cit. p. 294. § Ib. p. 45.

|| Magendie observed that the coagulation is hastened by the addition of the chlorides of potassium, sodium, ammonium and barium; of bicarbonate of soda, sulphate of magnesia, borax, nitrate of silver, iodide of potassium, and the cyanides of gold and mercury.

¶ [A summary of Mr. Blake's experiments on the effects of various salts, &c. on the blood, is given in William's Principles of Medicine, page 99.]

coagulation is caused by the extrication of carbonic acid gas from the blood. The subject can perhaps be best put before the reader by transcribing one of the experiments of the author. "Exp. vi.—Blood was taken from a man very slightly indisposed. Four ounces drawn in one minute were received in seperate cups. The temperature of the blood when placed under the air pump, was 84°. In half a minute after the exhaustion, it fell to 80°.

SUBSEQUENT RATE OF COOLING:

Blood in vacuo.		*In the air of the apartment.*	
1 minute after the exhaustion,	79	At the same time,	84°
2 " " " "	78	2 minutes,	83.5
3 " " " "	77	3 "	83°
4 " " " "	76	4 "	83°
5 " " " "	75	5 "	82.5

The blood being now removed from the air pump, was found much more coagulated than the other portion; it was free from air vesicles on the surface, the other abounding with them. It was almost black in colour, the other having the usual appearance. In nine minutes each portion was quite coagulated."

Now, it had been before shown that cold retarded coagulation, yet though cold was produced in the above experiment, coagulation was produced sooner than in the cup left to the air of the apartment. He then goes on to examine whether this effect was produced by the exclusion of the air or from other causes. His experiments led him to the following results:

"Low temperature delays the coagulation of the blood, as shown in the first experiments.

"At a reduced temperature, in vacuo, coagulation is hastened. Atmospherical air being simply excluded in the stopped bottle, the temperature not reduced, coagulation is retarded. All communication with atmospherical air being prevented, the temperature not reduced, coagulation is much retarded."

He then makes these remarks:—"It occurred to me that probably the more or less gradual extrication of the carbonic acid belonging to the blood, was the chief circumstance having an influence on the period of time in which the coagulation takes place."

Then follow a number of experiments which go to prove that carbonic acid gas is evolved from blood. The inferences from them may be thus stated. When blood, contained in a saucer, is placed over lime water, and under a receiver containing atmospheric air, the lime water presents a very dense pellicle in four minutes; the blood just beginning to coagulate:

That lime water alone under the same receiver gives, in the same amount of time a film just discoverable:

That if blood be exposed for some time to the air, so that coagulation may be conceived to be somewhat advanced, and then placed under the receiver with lime water, a much less considerable pellicle is formed. In other words, as coagulation is more advanced before commencing the experiment, the lime water is the less affected.

"The following experiments," says the author, "appeared more clearly demonstrative.

"Exp. xxvii. A portion of blood drawn from a person in health was placed with lime water under the receiver of the air-pump, which was immediately exhausted. In three minutes there appeared a thick and universal pellicle, and the blood was considerably coagulated.

"Exp. xxviii. I made a comparative trial with lime water only, and could not in three minutes discover the slightest pellicle."

There was for a long time a dispute in physiology whether carbonic acid could be evolved from the blood by means of the air-pump. But the recent investigations of Magness, Bischoff and others, have settled the question in the affirmitive.

But it is plain that these experiments of Sir C. Scudamore only prove that the evolution of carbonic acid gas *accelerates* the coagulation, not that it is *essential* to the process. And yet he declares this in the following words. "Its evolution (that is, of carbonic acid gas,) takes place most freely as the blood begins to concrete, and ceases when coagulation is completed. It is evidently *an essential circumstance* in the process of coagulation, as the same causes which retain the carbonic acid in the blood, delay coagulation."*

This theory is completely overthrown by the experiments already mentioned, in which the greatest possible care was taken to prevent the blood coming in contact with the atmosphere, and by others, in which vials were completely filled with blood; and yet coagulation, though retarded, was not prevented. There could have been no possible escape for the carbonic acid gas; the blood ought therefore to have remained fluid. It did not, and the theory therefore falls to the ground.

Theory of Raspail.—This theory is just the reverse of the preceding. So far from the *loss* of carbonic acid being the cause of the blood's coagulating, it is its *presence*, according to Raspail, that produces the effect.

I must premise that Raspail makes no distinction between insoluble albumen and fibrine. According to him, they are one and the same thing, physically and chemically, and it is useless to give two names to the same substance.

He begins the subject by relating the manner in which he deems the blood corpuscles to be produced. According to him they are simply precipitated molecules of albumen. To translate his own words:—"Hyaline globules, soluble in water, ammonia, acetic acid, concentrated hydrochloric acid, coagulable by other acids, by heat, by alcohol, are evidently simple globules of albumen, and not organized molecules.

"Each of these globules may then be considered as so much albumen, at first dissolved in the serum of the blood by the acid of any menstruum whatever, and afterwards precipitated from this menstruum, either by neutralization or by evaporation of the menstruum. Nevertheless the precipitates obtained by means of alcohol are always formless coagula. This is true; but the precipitates of albumen obtained by the spontaneous evaporation of the menstruum which holds them in solution, represent so well all the phenomena of the blood, that by the addition of a

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* Op. cit., p. 103.

red colouring matter, one would think he was looking at true blood.—In fact, let a certain quantity of white of egg be dropt into an excess of concentrated hydrochloric acid; the albumen, at first coagulated white, will very soon dissolve in the acid, taking on a violet colour which will afterwards pass into a blue. If the hydrochloric acid be now decanted and left to spontaneous evaporation, a white powder will be precipitated, which under the microscope will be seen to consist of very small, spherical globules, equal in size, and which the most practised eye might easily mistake for the globules of the blood.

"Now it will be easily granted that the quantity of these globules will vary according to the quantity of the menstruum which will evaporate in a given time, and according to many other accessory circumstances; so that these globules might present themselves in different forms and magnitudes, according to the age, habits, species and sex of the animals submitted to observation.

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"Besides these albuminous globules, the blood still contains a large quantity of liquid albumen; a fact of which the microscope may assure us, either by our letting blood, diluted with water, dry spontaneously, (we then see, in fact, a layer of albumen which cannot be produced by a junction end to end of the globules,) or by coagulating it with alcohol. Keeping the eye at the microscope, globules are seen enveloped by a membranous coagulum which is unexpectedly formed at the expense of the liquid portion.

"Let us now endeavour to discover the nature of the menstruum which renders this albumen soluble, and which, by being neutralized or by its evaporation deposits the globules which swim in the serum, or are carried along the vessels without uniting with each other. The analogy of chemical composition and of circulation between the liquid of the *chara* and the blood, induced me at first to think that the menstruum of the albumen was, in both, acetic acid. Macquer and Homberg had already discovered an acid in blood; Prévost had found acetic acid in it; Berzelius had shown the presence of lactate of soda and potash not only in the blood, but in all the tissues. This lactate we have demonstrated to be but an albuminous acetate of soda and potash. This hypothesis was, it is true, in opposition to the evident alkaline nature of the blood at the moment it leaves the vessels, but this alkaline condition might be consecutive to an acid state, and that might occur which we have already had occasion to verify, with regard to an acid ammoniacal salt becoming blue by contact of the atmosphere. But the constant alkaline nature of the most recently drawn blood, and the coagulation produced by diluted acids, gave no room to doubt but that the menstruum of the albumen was an alkali. This alkali is soda, and above all, ammonia, of which authors take no account, and whose various salts are recognized under the microscope.

"This principle once admitted, the spontaneous coagulation of the blood no longer offers any inexplicable difficulty. For the carbonic acid of the atmosphere, that which is formed in the blood by its avidity for oxygen, or that formed in consequence of the spontaneous fermentation of the elements of the blood itself, saturates the menstruum of the albumen, which is precipitated in the form of clot. The ammonia and water of the blood which springs smoking from the vein, by being evaporated, abandon in time a proportional quantity of dissolved albumen, and the mass coagulated so much the sooner, the less the blood contains of water. I might add that an acid fermentation is susceptible of being manifested in a liquid raised to the temperature of 98° Fahr., and containing at the same time insoluble albumen and sugar, immediately on its exit from the vessels. This acid will render the saturation of the menstruum still more rapid."*

This theory of Raspail is certainly a very ingenious one, but it is as untenable as it is ingenious. In the first place, it is opposed to the facts developed by the experiments of Sir C. Scudamore. It will be remembered that in those experiments coagulation was hastened by the evolution of carbonic acid gas under the receiver of the air pump, and that too, though the temperature was lowered. It is opposed, too, to the fact that arterial, coagulates more rapidly than venous blood. Admitting that there is an increase of fibrin in arterial blood and a less quantity of water, still there is certainly a much less quantity of carbonic acid, for this last has been evolved from the venous blood in the respiratory process. Moreover, Sir H. Davy could distinguish no difference in the time required for coagulation when the blood was subjected to the influence of carbonic acid gas.

Nervous Theory.—Mr. Thachrah in his "Inquiry into the nature and properties of the blood," comes to the conclusion that "the vital or nervous influence is the source of the blood's fluidity, and its loss the cause of coagulation."

Mayer of Bonn, came to a like conclusion from some experiments upon the *nervus vagus.* Dividing those nerves, he found the blood coagulated in the arteries and veins of the lungs, and also in the heart. "But," says Müller, "I have repeated the experiments without obtaining the same result."†

But be the fact so or not, it is impossible to refer the coagulation of blood to the want of nervous influence. The nerves cannot possibly act upon the mass of the blood contained in the larger vessels. They may, to be sure, exert an indirect action upon this fluid in the nutritive process—when molecules of blood come in contact with molecules of the solids—but they exert this influence *indirectly*, and by means of primary changes induced in the solids. A sufficient answer, however, to any such theory, is found in the fact that blood effused into the tissues from contusions, that contained in the extremities of arteries that have been tied, as in amputations, &c., coagulates, becomes organized and a part of the living tissues. Moreover, in certain diseases, such as Asiatic cholera, the blood is found fluid after death, and uncoagulable. It is hardly worth while to mention that in such a disease the influence of the nervous system must be nearly annihilated.

Vital Theory.—This theory bears two phases;—one attributes coagulation to some unknown mode of action of the vital principle; the other to the loss of the vital principle, in other words, to the death of the blood.

The celebrated John Hunter is the coryphœus of the first. As, from many experiments, he knew it to be true that this remarkable change

* Chimie organique, tom. iii. † Elements of Physiology, p. 358.

depended neither upon rest, nor upon the lowering of the normal temperature, nor upon the presence and contact of the atmosphere, nor upon a difference in the nature of the substances in contact with the blood; and as he supposed that he had considered all the circumstances of the case, he naturally inferred that the phenomenon was not due to any of these circumstances, nor to any combination of them, but to something in the blood itself—and this was the vital principle. He uses the following language with regard to this subject. "Coagulation, I conceive to be *an operation of life;* and I imagine it to proceed exactly upon the same principle as the union by the first intention; it is particle uniting with particle, by the attraction of cohesion, which in the blood forms a solid."

One would suppose, *à priori*, that as the blood is taken in a fluid state from the living body and then changes its form, that the cause of the change was a loss of the vital principle; provided the existence of such a principle be admitted. But as coagulation is *an operation of life* it was incumbent upon the theorist to tell us what was the cause of its fluidity in the vessels. It might be pertinently asked if the fluid blood in the living body was dead matter? Coagulation is an operation of life—now what is the cause of its being constantly found fluid in the vessels of a healthy man?

In reply, we have the following strange language. "While the blood is circulating, it is subject to certain laws, to which it is not subject when not circulating. It has the power of preserving its fluidity, or in other words, the living principle in the body has the power of preserving it in this state." Again, "If the blood had not the living principle, it would be, in respect of the body, as an extraneous substance. Blood is not only alive itself, but is the support of life in every other part of the body."

So, coagulation is caused by the living principle, and the very reverse condition (that of fluidity) is caused by the same agent. But it is evident, that until we are informed how this principle operates in the two cases, we are just as wise as we were before.

Puzzled in the extreme by the perplexities of this subject, Hunter, at last, as if in desperation, uses the following extraordinary language.—The passage has been often quoted and needs no comment. "My opinion is, that the blood coagulates from an impression—that is, its fluidity under such circumstances being improper, or no longer necessary, it coagulates to answer now the necessary purpose of solidity. This power seems to be influenced, in a way in some degree similar to muscular action, though probably not entirely of that kind; for I have reason to believe that blood has the power of action within itself, according to the stimulus of necessity, which necessity arises out of its situation."

Let us now turn to the other phase of this theory—to the views of those who contend that fluidity is owing to the vital principle, and coagulation to the death of the blood. It is obvious that such a theory cannot account, for many familiar phenomena, indeed is opposed to them, and theory must always fall when arrayed in opposition to facts. We know that blood, extravasated into the tissues, will coagulate, become organized and part of the living system. It may be converted into various tissues, take on inflammatory action and the various phenomena of disease. The clot left in the extremities of divided vessels after being tied, becomes organized and is not distinguishable from similar tissues. If the coagulation is caused by death of the blood, how can all this happen? Again, in certain kinds of death, as from lightning, malignant diseases, collapse of cholera, etc., the blood remains fluid. Is its *life* the cause of its so remaining?

Dr. Carpenter in his work on Human Physiology, seems to be of the opinion of Hunter, or at least to have adopted a similar one. "That the coagulation of the fibrin is not," says he, "as some have supposed, a proof of the death of the blood, but is rather an act of vitality, appears evident from what has been already stated of the incipient organization which may be detected even in an ordinary clot; and still more from the fact that, if the effusion of fibrin takes place upon a living surface, its coagulation is the first act of its conversion into solid tissues possessing a high degree of vitality. It is absurd to suppose that the blood dies, in order to assume a higher form. When withdrawn from the body, however, the coagulation of the blood is the last act of its life; for, if not within the influence of a living surface, it soon passes into decomposition."

This is singular language from an author who has so ably contended that life is *not* something *superadded* to matter.

But what is the use of so many words to tell us two well known and familiar facts, namely, that fibrin will coagulate in the body and become organized; and, that it will coagulate out of the body and then in due course of time pass into decomposition; for, to so much, and so much only, the passage amounts. But in all, is there one single circumstance mentioned which throws light upon the question, *why* the fibrin in either case passes from the fluid to the solid form?

That the fibrin will be organized in the one case, and pass into decomposition in the other, like all animal matter under similar circumstances, is perfectly plain when the different conditions under which they exist are considered.

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We have seen all the foregoing theories insufficient to account for the transformation of fluid fibrin into the solid state. It is not my purpose to offer another view of this singular phenomenon, but to point out that there are similar phenomena among artificial compounds, and to show that an explanation of the change in the blood can only be hoped for from the progress of chemistry. To that science the problem properly belongs, and by it will doubtlessly some day be solved.

It is well known that fibrine and albumen are closely allied, if indeed as some suppose, they be not identical. The word *fibrine* may therefore, with propriety, be substituted for *albumen* in the following extract from Dr. Turner's chemistry. "Were I to hazard an opinion on this subject, it would be the following: that albumen combines directly with water at the moment of being secreted, at a time when its particles are in a state of minute division; but as its affinity for that liquid is very feeble, the compound is decomposed by slight causes, and the albumen thereby rendered quite insoluble. Silicic acid affords an instance of a similar phenomenon."

Of silicic acid, he says, "In its solid form silicic acid is quite insoluble in water; but Berzelius has shown that if presented to water while in the nascent state, it is dissolved in large quantity. On evaporating the solution gently, a bulky gelatinous hydrate separates, which is partially decomposed by a very moderate temperature, but does not part with all its water except at a red heat."

This, to be sure, does not explain the coagulation of fibrine, since the *slight causes* spoken of are the very things which we are in quest of.—But Dr. Turner's remarks evidently point to the general manner in which the phenomena may occur, as well as to an analogous phenomenon.

But we may point to several other phenomena still more closely resembling the coagulation of the blood, and it will be seen that they too are, in the present state of science, inexplicable. Chemists, when speaking of these phenomena, are forced to use metaphorical language, and therefore employ the expression that the change is *spontaneous*; which, in other words means that they are ignorant of the cause of the effect. It is surely not worth while to argue that there can be no spontaniety of action except in beings possessed of volition.

The first of the phenomena alluded to which we shall mention, is the singular change which takes place in cyanic acid from no known cause. Graham thus describes it: "It is a transparent, very volatile liquid of a pungent odour, highly corrosive, miscible with water. Soon after its preparation this liquid spontaneously undergoes a very extraordinary change: it is converted with the evolution of heat into a white solid matter, *cyamelide*, having the same composition in 100 parts, but insoluble in water and dilute acids, dissolved by caustic alkali, with the formation of ammonia, cyanate and cyanuret of the alkali."

The next substance which we shall mention, subject to *spontaneous* change is chloral—"chloral," says Professor Graham "is a pretty oleaginous liquid, colourless, greasy to the touch, having a penetrating, disagreeable odour which provokes tears; its taste is first oily and then caustic. Its density is 1.502 at 64.4° (18° Cent.) and it boils at 201.2° (94° Cent.) distilling without alteration," etc.

"Like aldehyde, pure chloral cannot be kept long without alteration. It gradually passes into a solid mass, resembling porcelain, without change of weight, and equally whether contained in vessels hermetically sealed or open. This mass is not dissolved by water," etc.

But a still stronger example of these *spontaneous* changes is to be found in the substance termed *aldehyde.* I shall quote from Liebig's Animal Chemistry. "Among those substances which contain no nitrogen, we have aldehyde—a combustible liquid miscible with water, which boils at the temperature of the hand, attracts oxygen from the atmosphere with avidity, and is thereby changed into acetic acid. This compound cannot be preserved, even in close vessels; for after some hours or days, its consistence, its volatility and its power of absorbing oxygen, all are changed. It deposits long, hard, needle-shaped crystals which at 212° are not volatilized, and the supernatant liquid is no longer aldehyde. It now boils at 140°, cannot be mixed with water, and when cooled to a moderate degree, crystallizes in a form like ice; nevertheless, analysis has proved, that these three bodies, so different in their characters, are identical in composition."

Now, here we have changes of form, and (in the last mentioned instance) even more singular than that which occurs in the fibrin of the blood, and equally inexplicable in the present state of science. As well might we invent some *principle* to account for the transformations of aldehyde, as for those of fibrine.

It is now well known, that even elementary bodies are capable of presenting very different forms. Those of sulphur and carbon are well known. If this be the case, the susceptibility to change of form in highly compound bodies cannot, or at least ought not, to be a matter of surprise.

I shall conclude this paper by repeating what has been asserted above; that we must expect from chemistry the solution of these mysteries.

III.—*Case of Lithotomy: Extraction of a large Stone from the Bladder of a little Girl.* By James Guild, M. D., of Tuscaloosa, Ala.

Tuscaloosa, Ala., February, 1847.

Editors of the New Orleans Medical and Surgical Journal:

Gentlemen:—Should you think the following described case worthy of notice in your Journal, you will please publish it.

Mr. J Hewit, of this vicinity, consulted me in relation to his little daughter nine years old, who, he informed me was suffering excessively, and had a continuous dripping of urine, and occasional paroxysms, that seemed almost to take life. Said he had consulted several physicians, and had given a great deal of medicine, without any benefit. From his description of the case, I was confident there existed a calculus in the bladder. I accordingly sounded the bladder on the 20th ult., and found the stone without difficulty. The child was much emaciated, œdema present, and the healthy functions of the system much impaired; in consequence of which, I saught the first favorable day to operate. Accordingly, on the 25th of the same month, I performed the new operation of Mr. Ferguson, and removed the largest stone I had ever seen, or described by any surgeon taken from the living subject. The little patient was placed on the table and tied in the usual way for lithotomy. I introduced a deeply grooved straight director into the bladder, the groove pointing downwards and outwards, and to the left side, and with a probe-pointed bistoury, such as is used in operating for fistula in ano, an incision some half an inch in extent, was made towards the tuberosity of the ischium; the incision being limited to the anterior half of the urethra. I then introduced my finger into the wound, and by gentle pressure could dilate the urethra so as to feel the neck of the bladder, the finger coming in contact with a very rough surface and of immense size. Finding it impossible to get the stone through so small an aperture, I made an incision on the opposite side of the urethra, of the same extent that was first made. I could, then, without difficulty feel the neck of the bladder, and found the whole body filled, and its coats firmly attached to the stone. I then by a very tedious process, broke up the attachment with the scoop and fingers. A large lithotomy forceps was now introduced, when the stone was immediately seized, and though the blades slipped frequently in consequence of its immense size, I succeeded in withdrawing the calculus without crushing. It was about the size of a hen's egg, weighing two ounces, and measuring six inches lengthwise and four inches and five eighths of an inch across its small diameter; it was of an oval form. 27th, visited her, going on well. 29th, much improved, able to retain her water a few hours. 31st, still improving, able to retain her water still longer. Feb. 4th, able to sit up, good appetite, wound almost closed, able to retain her water almost at pleasure. 11th, discharged, able to walk and amuse herself as she thinks proper.

Intimation being given that the child had introduced a pin into the bladder some twelve or fifteen months previous, I sawed open the calculus across its small diameter, and to our great astonishment found a *brass pin* in its center, of an unusually large size, which acted as a nucleus on which the deposit was formed. I am much gratified to find there is no incontinence of urine in the case, and am satisfied there never will be, when the operation is performed in this way.

Sir Astley Cooper states that all his operations with the knife, were attended with incontinence of urine, and recommends a suture upon the edges of the wound. I am well satisfied there is no necessity of cutting the neck of the bladder in the female to extract calculi, for after making the incision as I have described, the parts can be so dilated as to remove any sized stone that may form in the bladder.

My esteemed friend, Professor Brumby, of the university of Ala., has analyzed the stone, and addressed me the following note.

University of Ala., January 30th 1847.

Dear Sir.—The portion of urinary calculus, which you gave me for the purpose, this morning, has been carefully subjected to chemical examination.

It is insoluble in alcohol and very partially soluble in boiling water. In solution of potassa or soda, it is insoluble, but emits a faint odor of ammonia, and deposits, after cooling, small flakes of animal matter. It is readily dissolved by diluted nitric, hydrochloric, sulphuric and acetic acids. From any of these solutions, it is precipitated by alkalies. The solution in nitric acid placed in a small retort, was slowly evaporated to dryness. The saline mass was white. This was then heated to near redness, over a small spirit lamp, when the retort was filled with dense white fumes, evidently of undecomposed nitrate of ammonia, and a white saline residue was left. This was insoluble in water, but dissolved readily when a drop of pure nitric acid was added. This solution rendered neutral by ammonia, was tested and found to contain phosphoric acid, magnesia and a trace of lime.

A portion in fine powder was then heated gradually to redness in a small platinum spoon, over the flame of a spirit lamp. It became black, yielded the odor of ammonia, but did not melt. In the blow pipe flame it was fused with difficulty into a white enamel.

These chemical characters combined with color, form, size, structure, powder and other physical properties, prove this calculus to be of the species denominated ammoniaco-magnesian phosphate, mixed probably with a small quantity of phosphate of lime. Its aspect shows that it is not the fibrinous, a rare species of calculus; it is not the xanthic oxide, for its color is neither reddish nor yellow; it is insoluble in potassa, and its solution in nitric acid, deposits on evaporation, a white, instead of a bright lemon residue. It is neither cystic oxide, nor the uric acid calculus; for it is insoluble in alkalies, and soluble in acetic acid. It is not the fusible calculus, for to convert it into an enamel, requires the highest heat of the blow pipe flame. Finally, it is not the phosphate of lime (bone earth) calculus, since it contains very little lime, is soluble in cold acetate acid, and consists chiefly of ammoina, magnesia and phosphoric acid. Yours truly,

Dr. James Guild. R. T. Brumby.

IV.—*Accounts of a New and Fatal Epidemic that Prevailed Recently in Mississippi and Tennessee.* By B. J. Hicks, M. D., of Vicksburg, Miss., and B. F. Taylor, M. D., of Whiteville, Hardeman County, Tennessee.

(It is a remarkable coincidence that we should have received about the same time the following communications, evidently describing the same curious disease, as it prevailed at localities nearly 300 miles apart. It will be seen that Dr. White gives no name to the strange affection; whilst Dr. Hicks calls it *myelitis petechialis*. We are satisfied it is the *cerebro-spinal meningitis*, which prevailed during the last year in Ireland and some parts of Europe—an interesting account of which may be found among our Foreign extracts in the present number, taken from Ranking's Half-Yearly Abstract, vol. 2, No. 2, p. 192. Upon enquiry, we learn that several cases of a similar nature were seen in this city in the early part of the spring; and on reflection, we are inclined to think it was the same disease that killed so many of the 2nd regiment of Mississippi Volunteers as they passed through this place in January last.—We think it evident that neuralgic and spasmodic affections have increased greatly in the south within a few years past. Eds.)

Editors of the New Orleans Medical and Surgical Journal.

Gentlemen:—In the neighborhood of this village we have had an epidemic that has proven to be one of the most formidable, probably, in the records of medicine. It appears to have been more prevalent near Hatchie river, than in any other section of the country. The disease has been confined principally to children between the age of six and fifteen years. The attack is ushered in with cold chilly sensations, after which moderate heat of surface, pain commencing between the shoulders, extending to the occipital region, rigidity of the posterior cervical muscles, retracting the head considerably backward, as in *tetanus*. Delirium supervenes in an hour or two, contraction of the pupils of the eye, dilitation of the one eye sometimes, with contraction of the other, ptosis of the eyelids, ecchymoses under the eye and on the body, rigidity of the abdominal muscles, spasmodic twitchings of the flexors of the extremities, and a disposition to keep the legs in motion from side to side alternately. A difficulty in expanding the lungs, breathing through the nostrils principally, constipation of the bowels, and sometimes retention of urine. Stertorous breathing comes on, and death soon closes the scene—such are the general symptoms of the disease. It terminates its course in from fifteen to seventy-two hours.—I have known one case to terminate as late as the twelfth day.

Almost every method of treatment has been devised and carried into effect; bleeding, emetics, cathartics, cold douche, cupping, mercurials, blisters; after which, opium, quinine, and stimulants. The system appears to be so excessively shocked that the recuperative powers are not sufficient to sustain the tottering fabric. The necessary chemical change is not carried on in the lungs; the blood is not decarbonized; consequently, from the phenomena manifested, we are led to the conclusion that death is produced principally by *asphyxia.*

I will simply detail one case, the particulars of which will be found highly interesting, illustrating clearly and conclusively the pathology of the disease.

Case.—I was called in consultation with my friend Dr. Durham, an old practitioner, in the night of the 30th March, to see a servant girl, aged 11 years, the property of John H., Esq. She was taken on Sunday evening the 28th, and was seen the following morning by Dr. Durham. She was then delirious, conjunctivæ injected, pupils contracted, retraction of the posterior cervical muscles, twitchings of the flexors, rigidity of the abdominal muscles, hurried respiration, pulse full, but compressible, constipation of the bowels. A mercurial cathartic was ordered, and a blister to the nape of the neck, extending over the occipital region.

29th. Delirium ceased; there is still retraction of the muscles; medicine produced two evacuations; has passed a small quantity of urine; says she feels better. Ordered calomel and pulv. doveri.

30th. This evening I saw her the first time. Intellect clear, complains of pain in the hypochondriac region, retraction of posterior muscles, pupils contracted, the eye looks rather dull, tenderness on percussing the 2nd and 3rd dorsal vertebræ, restlessness, pulse quick and compressible, no fever, tongue of a dull red or purple color around the borders, coated with a thin yellow fur and fissured, respiration quicker than natural, performed principally by the abdominal muscles, dullness on percussion over the right hypochondriac region, auscultation detected slight engorgement of the lungs, no purgative since yesterday. I advised that she should be cupped over the spine pretty extensively, and to have an active mercurial cathartic.

31st. The cupping relived the muscular retraction; skin feels pliant, though there is increased heat of the scalp. In the course of the night it was thought proper by Dr. Durham to administer ℥ i. tinct. opii. camph. to relieve excessive abdominal pain. Bowels not moved; the anodyne had the desired effect. She says that she is better. Ordered ol. ricini. and ol. terebinth., to be repeated in four hours, should it not operate.

When we were in the act of leaving, the mother came out from the cabin and desired us to come back and see her, as she was much worse.

She stated that the patient had asked for a drink of water a few minutes previously, complaining on lying down of pain in the abdomen, and evacuated her bowels in bed. The pupils were contracted, pulse depressed, breathing stertorous. Death closed the scene in a few minutes.

I examined the body in presence of Dr. Durham, and the following were the appearances.

Sectio. Cadaveris, 18 *hours after death.*—The body was not thin, and there was a quarter to half an inch of fat covering the abdominal muscles.

Head.—The posterior integuments were swollen, both pupils were dilated. On removing the calvarium, a considerable amount of blood flowed from the sinuses of the dura mater. The arachnoid membrane adhered with moderate firmness to the surface of the convolutions.

While the brain was being removed, some two or three ounces of serum escaped from the ventricles, being a clear and transparent color. The weight of the brain was not ascertained, but it appeared heavier than usual. The surface of the convolutions was much flattened. The base of the brain bore evident marks of inflammation. The membranes covering the medulla oblongata and the cerebellum, the right lobe more especially, were thickened and opaque, adhering likewise pretty firmly to the fissures of Sylvius. The membranes at the base were unusually vascular, but the substance of the brain itself was not altered very much in color or consistence. The membranes, more particularly, around the third nerve of the right side, were thickened and more vascular than natural. On examining the superior surface of the brain and separating the two hemispheres slightly, they gave way inferiorly. This was ascertained to arise from softening of the lower part of the middle lobe of both hemispheres, as also a considerable portion of the corpus callosum. The corpora striata were very slightly injected and softened, particularly that of the right side; the lining membrane of the ventricles was not altered in color. The brain, taken as a whole, excepting the parts mentioned, was natural.

Spinal Marrow.—On sawing the vertebræ, a considerable quantity of fluid blood gushed out the moment the interior of the canal was reached. It appeared to be perfectly flooded and engorged. The membranes were evidently thickened and highly vascular. The spinal marrow was not altered in appearance, but if any thing, softer than natural. The substance itself was not injected.

Thorax.—On inflating the lungs, the cells were permeate throughout. There was no appearance of hyperemia or inflammation of the substance, or of the lining membrane.

The Heart contained within the ventricles a thick coagulum of blood. The pericardium being cut through, about two ounces of serum escaped. The valves were healthy and the artery was free from deposits.

Abdomen.—The liver was perfectly engorged with blood; an incision being made through its structure, the blood could be squeezed from it as from a sponge. The gall bladder was distended and contained a quantity of thick black bilious matter. The weight of the liver was 5½ pounds.

The Kidnies were congested, but otherwise healthy.

The Spleen was large, but contained little blood in comparison with the liver.

The Intestines contained a quantity of thin greenish matter. There were a few spots of ecchymoses in the lower two fifths of the ilium.—The small intestines contained a few large worms. The alimentary canal was otherwise throughout healthy. *The bladder* bore no mark of disease.

Such are the facts and history of one of the most malignant diseases that it has been my lot to witness.

I forbear making any remarks, as I have, I fear, trespassed upon your columns already too much.

Very respectfully,

Whiteville, Tenn., April, 1847. B. F. WHITE,

(In reply to a note requesting farther particulars, the following were received from Dr. White. Eds.)

The disease made its appearance the 25th day of February, near Hatchie river. The region of country extending along Hatchie, from Bolivar to Estanaula, was the only section that was affected. No case that I am aware, occurred more than six miles out from the river. Some fifty cases that I know of, have occurred. The ratio of mortality in my practice, has been three-fourths out of the whole number. Every case that was taken with the extreme violent symptoms mentioned, died. Towards the last, the disease lost its malignancy to a certain extent, like all epidemics. However, the last two cases that I saw, died. The first of these demonstrated the futility of the antiphlogistic, with the sedative plan. In the second case, no time was afforded for any plan of treatment. The system received such a shock that it could never recuperate. The following were the notes taken at the time.

Miss Nancy E., aged 15 years, taken on Sunday evening, April 4th; complains of pain between the shoulders; spasmodic retraction of the posterior cervical muscles; subsultus tendinum; pain in the head and limbs; delirium, with intervals of consciousness; some fever, face flushed, bowels acted once during the day. April 5th, Monday morning, complains of pain of the stomach, with occasional sickness; slight delirium. Family gave an emetic; acted pretty thoroughly; delirium relieved; bowels evacuated; took a dose of calomel. I saw her first on Teusday morning, 6th April, and found her in the following condition: head elevated on pillows; complains of an intense pain in the forehead; pulse hard and pupils contracted. On percussing the spinous processes, there was found to be tenderness, more particularly over the 2nd and 3d dorsal vertebræ; tenderness on pressure over the right hypochondriac region; tongue covered with a thick yellow fur, fissured and dry; breath offensive. Bled her to ℥ xvi, took ten ounces of blood by cups from the spine. A blister to extend from the neck, over the spine to the sacrum. Proto-chlor. hyd. grs. x, acetate morph. gr. i, for a powder. A cold douche over the head. She has now fallen to sleep. To take sulph. magnesia, 1 oz. in four hours, should she be not evacuated.—Wednesday 7th, I saw her this morning at 2 o'clock; still complains of pain in the head; pulse 75; bowels have acted freely, rational; no subsultus; pupils still contracted. Ordered a cold dash over the head, venesection to ℥ xviii; to take proto-chlor. hydr. grs. x, acet. morph. gr. i, followed in four hours by a dose of sulph. magnesia. In my absence, a professional friend was called at 2 o'clock, P. M. He prescribed a dose of calomel with morphia. I saw her at ten o'clock at night. Head relieved, very little fever, small red spots, not unlike ecchymoses have appeared under the left eyelid, and over the body. To have two drops croton oil, repeated in two hours, should the bowels not act freely. Thursday, 9 o'clock, 8th April, complains of her head, great restlessness, sighing; pulse 100. Bowels were evacuated freely, discharges dark and very offensive. Stertorous breathing came on, and death took place at 12 o'clock.

In reviewing this case, you probably may be led to believe that the opium brought on the stertorous breathing, and that the treatment was sufficient to have produced death. I have seen precisely the same thing occur where no narcotic was given. The fact is, that I had almost lost confidence in any therapeutical agent. In those extreme cases, nothing appeared to be of any service. If any remedy is of any service, it is most unquestionably the preparations of opium.

I may remark, that in some of the cases that survived, the hearing was not restored for some time. The function of taste also, was lost. In one little child I recollect that he could not tell any thing bitter from any thing sweet. Every thing was devoured with the same gusto.

Yours, &c.,

Whiteville, May 18th, 1847. B. F. WHITE.

To the Editors of the New Orleans Medical and Surgical Journal.

Gentlemen:—Conceiving it to be the duty of every medical philanthropist to contribute his mite towards the advancement of a science whose object is the relief of human suffering and prolonging life, I transmit to you for publication if deemed worthy, the following observations upon a disease of most extraordinary character which has lately prevailed in our city, to a moderate extent, but with great fatality. I submit them as taken from the bed-side of the sick, leaving the readers of your Journal to draw their own conclusions. When the disease first made its appearance, with what industry and zeal did the physicians of our city search the medical periodicals for half a century back, in order that they might receive some light as an inheritance from the experience of our predecessors, to aid them in the diagnosis and treatment of this fatal malady, but in vain; hence, I offer no other apology for this communication, hoping it may serve in a slight degree as a beacon, to aid some traveller through the dark maizes of our profession, and to shun the errors which have often prevented success.

The disease of which I speak, I shall call *myelitis petechialis*, as the meaning of the terms will convey an idea of the most prominent symptoms.

The patients were attacked with groaning, muttering delirium, chilly sensations, pallid countenance, extremities cold; which symptoms were soon followed by roving restlessness, flushed countenance, frequent pulse, expression of the eye wild and frantic, surface of the body hot and dry, violent screaming when touched or spoken to, unable to answer questions correctly, to locate the pain or point out the suffering organs; whilst sleeping, alternate pallor and flushings of the countenance were often observed and violent inflammation of the right or left eye was not an unfrequent occurrence. This condition was followed on the second or third day of the disease with symptoms of tetanus, the spinal muscles being very much contracted and rigid; in some instances to such an extent that the patients were enabled to swallow even fluids with much difficulty, accompanied with a loss of power in either the right or left upper and lower extremeties, followed by convulsions of great severity, which would be excited or called into action by touching or raising the inferior extremities as quick as the operations of a galvanic apparatus. During the existence of this disturbed state of the nervous organism there was no evidence of lesion existing in the biliary or digestive organs, but great activity of the urinary secretions, being unnaturally copious and pellucid.

There was no uniform rule for the appearance of the various symptoms in this disease; in some instances the tetanic symptoms were observed as early as the first day of the attack, and in other cases as late as the tenth. In many of the most violent cases, within six hours after the patients were taken ill, *petechiæ* of large size made their appearance upon the arms, over the eyelids and upon the inferior extremities, as if a violent hemorrhagic effort had been made and the blood arrested under the cuticle. These *petechiæ* disappeared on the 4th or 5th day, if the patients were not sooner cut off. In the most violent cases, death ensued on the 5th day of the disease; in other instances they survived from thirty to fifty days; but few cases recovered after the symptoms of tetanus presented themselves.

In consequence of a doubt existing in regard to the pathology of the disease, the treament was pursued with a cautious and timid hand. The stimulating and anodyne course of treatment did not avail, and blood-letting was equally unsuccessful. The course which was of most promise, was of a mixed character. In vigorous constitutions, blood-letting in the onset of the attack, followed by an active mercurial cathartic, was of great service. If the patient should be of a more delicate constitution, cupping the whole length of the spine and early vesication by the use of the newly invented blistering tissue, or the unguent cantharides, would be preferable to the general bleeding; revulsives to the extremities and iced cloths to the head, together with the internal exhibition of the following mixture, greatly aided in advancing the case.

℞ Pulv. gum. camph. ʒ j.
Potassio tartrit. antimonii. grs. ij.
Mucilage gum. acaciæ. ℥ vi. M.

Give one tablespoonful every two hours. An enema composed of ʒ ij. spir. terebinthinæ, ʒ ij. tr. assafœtida and a half pint of thin starch was administered morning and evening, with a view of tranquilizing nervous distress, and keeping the bowels gently open.

After the violence of the disease had abated, the most beneficial tonic for relieving the inertia of the nervous system, which occurred in every instance of recovery, I found to be the following.

℞ Iod. Ferri. ℈j.
Iodine, grs. viij.
Iod. Potassæ, ʒ ij.
Syr. Sassapar. ℥ iv.

Give one teaspoonful every four hours in a little water. This prescription should be continued for some days, unless found to produce gastric distress; then some mild bitter vegetable infusion should be substituted.

The pathology of this disease may be stated in a few words. The brain and spinal marrow being the great centre of the nervous system, any injury done to its substance by mechanical violence or otherwise, must necessarily derange the functions of all of the organs of the human machinery which depend upon their sane condition for the healthful elimination of the vital fluids and the depurative processes that are necessary to keep the organism in a condition suitable for the purposes of maintaining the proper operations of the whole; and when such important functions are so interfered with, the tendency is consequently to a cessation of that nervous power or excitation which is necessary to carry on the operations of vitality.

In the disease in question, there is a positive injury existing within the spinal canal; congestion and inflammation of the spinal marrow and its membranes, which, if not speedily arrested, terminates in softening of its substance, which must be necessarily fatal. This condition of the spinal marrow has been produced by the sudden operations of meteorological changes made upon the nervous expansions of the skin, which impressions are transmitted to the opposite extremity of the nervous apparatus through the medium of the nerves themselves, they being the natural conductors of sensations, at which point morbid action is excited and continued until lesion takes place, if not prevented by timely treatment. The pathology of nervous diseases has been too much neglected; the operations of the nervous system are much modified by the electrical condition of the atmosphere, and the treatment of this particular class of diseases will be unsatisfactory until more attention shall be paid to this particular study. The nervous system is more involved in the diseases of our climate than has been conceived of by medical writers. In intermittent fever we may bleed, blister and give alteratives, and we cannot cure the disease until we administer some remedy that will break up the morbid chain of nervous action, and keep up a proper equilibrium of the vascular system. What is the first organ upon which diseased action locates itself in the fevers of our miasmatic district; is it not the expansions of the nervous system? Do we not see shivering, and manifest derangement of nervous action before congestions of the spleen, liver and other organs take place, and the consequences attendant upon such conditions? Should the energy of the nervous system be of sufficient force, the equilibrium of the circulation would be maintained and no congestions could ensue.

B. J. HICKS, M. D.,
March 31st, 1847. Vicksburg, Miss.

(In reply to a note requesting farther particulars, the following letter was received from Dr. Hicks. EDS.)

VICKSBURG, May 11th, 1847.

GENTLEMEN:—Yours of the 7th instant, has just come to hand.—The disease I gave you a partial account of under the head of *myelitis petechialis*, made its appearance in our city and its vicinity about the 10th of January last, and continued until the close of March; during which time between forty and fifty cases occurred, at least one half of which proved fatal. No case to my knowledge recovered after decided symptoms of tetanus presented themselves.

Only one opportunity occurred in my practice of making a post-mortem examination, which, on account of press of business, was a hurried one, and consequently no organ was examined, except the brain and spinal marrow.

The cerebrum and cerebellum showed no symptoms of inflammation. The medulla oblongata and upper portion of the spinal marrow presented dots of blood when cut into, and the meninges of the same were found to be highly injected. I regret that I did not make a more perfect examination of this case, as no subsequent opportunity presented of investigating the pathology of this interesting disease.

Yours, &c.,

B. J. HICKS.

V.—*An Enquiry whether there is in the Southern States, a Specific Disease that can properly be called Congestive Fever; with Cases and Remarks.* By WM. P. HORT, M. D.

In all the fevers of the South, and South-western States, generally referred to miasmatic origin, there is more or less of congestion, and of what Dr. Carpenter calls "Periodicity or Intermittence." In common intermittent fevers, the chill, ague, or cold stage may be considered the mildest form of congestion occurring in fevers, which nature is competent to relieve by febrile reaction, followed by copious perspiration, and complete temporary suspension of the disease. This remark applies to all the types of intermittent fever,—the quotidian, the tertian, and the quartan, double-tertian, the semi-tertian, the double-quartan, with the exception of a variety of obscure and irregular cases, very properly denominated malignant intermittents, where nature is utterly incompetent to afford relief, and art, however well directed, is not always successful. Bilious remittents are also characterized by "periodicity," without complete temporary suspension of the disease

In the class of malignant intermittents that I have seen, the few paroxysms are well marked; the interval between the paroxysms is seldom more than four hours, and congestion is all the time becoming more apparent; yet I have never heard this form of disease called a congestive fever. It sometimes terminates in death after the first paroxysm; in other cases after the second; but most frequently, unless the disease can be arrested, after the third.

Is there then a type of fever sufficiently distinct in its symptoms from the large class of intermittents, including the regular and irregular, and the bilious remittents, which can fairly be called congestive fever?

My attention has been called to this subject by several articles, which I have read within the last twelve months; but chiefly by a brief article in the New Orleans Medical and Surgical Journal, for January, 1847, page 443, by Dr. Thomas C. Brown, of Woodville, Mississippi.

He commences as follows:

"The term *congestive fever* has become so familiar in the South and West, that the young and experienced are led to look for a distinctly marked and peculiar fever, and are sadly perplexed in their investigations. At one time it is described as "algid intermittent fever," at another as "malignant or pernicious remittent fever." They find it supervening on intermittent, remittent and continued fevers, on gastritis and gastro-enteritis, on typhoid pneumonia, on epidemic dysentery; and in fact, commplicated with most of our summer and autumnal diseases, and sometimes with our winter epidemics." This is all very true; and again, the Dr. observes, "the symptoms of a particular organ, or of several organs, combining with the sympathy of a particular fever or disease upon which it supervenes, must neccessarily give rise to great diversity of symptoms, and will require a variety of treatment as dictated by the existing circumstances. Hence the confusion which results in an attempt to study the symptoms of congestive fever as a distinct species of fever. If we are to have a nosology, in which congestion is to be the cognomen of a particular species, we must adopt a division of fever, somewhat like the arrangement of Armstrong, and dispense with the distinctions of fever into intermittent, remittent, and continued."—These last remarks are entitled to consideration.

Physicians practising in the Southern, and South-western States cannot fail to observe in the course of a few years congestion in every variety as to particular organs, and of every degree of intensity, from a simple chill which lasts but an hour, and is then relieved by nature, to that aggravated and terrible form of disease, called in Louisiana, *perte froid*, and by the Americans cold plague, which may be preceded by a light intermittent, or strike with the force of a tornado, without any premonitory symptom. Within these limits, which include all the various types of miasmatic fever, with an almost endless variety of symptoms, where can a clear line of distinction be drawn that will fairly separate congestive fever as a distinct disease of peculiar type and symptoms, from all the other forms of miasmatic fever?

The Chipola river runs through Jackson County, West Florida. Unlike nearly all our Southern rivers, the lowest land is immediately on the bank of the river, forming wet swamp, beyond which is dry swamp, succeeded by low grounds, the land rising gradually as it recedes from the river, presenting an inclined plane. Beyond this, there is undulating or hilly country, with a growth of oak, hickory, cherry, ash, &c.—And there is another ascent of from one to two hundred feet, where an undulating pine country is found. The swamp is from three to five miles in breadth; and from the river in that part of the country to which I now allude, the distance to the pine woods is also from three to five miles.—Now on this line of five miles, I have seen light intermittent fevers in the pine woods, a more serious fever on the outer edge of the oak ridge, severe in the inner edge, and gradually increasing in intensity down to the edge of the wet swamp, where there was highly malignant and fatal disease, called in that country sometimes congestive fever, and often cold plague. The same miasm or organized matter emanating from the swamp, was the cause of all the foregoing types of fever from the mildest to the most severe; but it operated with very different force according to proximity to, or remoteness from, the swamp. All the various forms of fever were then only different types of one and the same disease, depending on the quantity of poison that had been taken into the system, other things, as constitution, age, temperament, &c., being equal.

In the fall season of the year, a heavy and damp fog with a sickening disagreeable smell rises almost every morning just before the dawn of day, covering all the swamp, low grounds, and the first ridge; and occasionally reaching a depressed point of the pine ridge. When I resided in that country the extension of the fog well defined the limit of the fever region. Frequent observation from an elevated part of the pine ridge, whence I could trace the outline of the fog, rendered me confident of the fact just stated.

Admitting, for the sake of argument, that there may be a class of miasmatic fevers that can properly be denominated congestive, because congestion predominates and is the chief symptom of the disease, yet to attempt to treat it by one fixed plan, would be to consign at least two thirds of the patients to the grave. And unfortunately, this has been too generally the case throughout the South and West. Hundreds of physicians have treated the disease with calomel, relying on it as a specific, of which I may have more to say hereafter, but experience has shown that it is rather a specific in destroying, than in saving life. Another class rely on purgatives, and I have known several who bled on all occasions. My object is not so much to criticise any particnlar plan of treatment, as to condemn the practice of treating any formidable miasmatic disease by a specific invariable plan of treatment, when it should be, and can only be successfully treated by varying the practice according to the symptoms that may be present.

To illustrate this position, I shall introduce some cases, which have occured in the course of my professional career.

Case 1.—Mrs. Spears, aged about 38 years, of a sound constitution, resided about half way between the swamp and the oak ridge. In July 1827, she was attacked with fever, complained of severe pain over the eyes and across the loins; it seemed, she observed, as if her spine was about to break; tongue moist, covered with an ashy colored secretion, no regular paroxysms, pulse small, distinct and rapid, about one hundred and ten. An emetic was administered which operated well; pediluvia, rendered stimulating by salt and mustard, with warm aromatic drinks, brought on prespriation, which however was only partial, and rapidly disappeared; gave thirty grains of calomel, to be followed by a dose of castor oil. I did not see her till about eleven o'clock the next day, when I became alarmed for her situation; her bowels had been opened by the calomel and oil. Her distress was indescribable. She could not remain quiet a moment; the surface of the body and the upper extremities were very warm, the lower extremities cool; pulse weaker than the day before, but more rapid, about one hundred and twenty; great irritability of the stomach; drinks of all kinds instantly rejected; clammy prespriation would occasionally occur on the neck and forehead, and almost immediately dry up. Not knowing at that time what else to do, I took from her arm, in a full stream, thirty ounces of blood. This bleeding acted most favorably. She was quiet before the arm was bandaged. The pulse became fuller and fell to about ninety pulsations a minute ; a blister was then applied over the stomach. In five minutes she was sound asleep, in the course of which sleep, warm prespiration appeared all over the body. After this, there was no difficulty in treating the case. Quinine and castor oil were the only medicines I prescribed. Had I not bled this lady boldly, she would probably have died on the ensuing day. I should, however, have bled her on the first day. A practice since adopted of administering Quinine and morphine in combination, might have proved quite as successful as bleeding in this case.

Case 2.—John Cain, about 40 years of age, Sheriff of Bladen County, North Carolina, a stout man, was attacked with what was called congestive fever, in the month of August 1830. I treated him pretty much as I did the first case, until the third day, when I found him much worse ; an emetic had operated well, but there had been no action of the bowels, although he had taken several doses of calomel and oil. On the third day he was in a stupor, eyes very red, tongue dry, chapped and inclining to a mahogany color, extremities cold, temperature of the body very low, pulse imperceptible at the wrist, head intensely hot, the action of the carotid arteries was strong. I opened the temporal artery on the right side of the head. I did not pretend to regulate the quantity of blood—the bleeding had to produce a certain effect, and until that effect was produced there was no use in stopping the flow of the blood. I judge that he must have lost between forty and fifty ounces of blood, before he became thoroughly aroused, when most powerful action of the bowels immediately ensued, which completely evacuated all the morbid secretions. The symptoms became very much mitigated, and there was no difficulty in the subsequent management of the case. Quinine was the principal remedy. Bleeding from the temporal artery, was in this case indispensable ; the congestion or engorgement of the brain, was so great as to completely paralyze the nervous, and consequently, the muscular system. However, this patient should have been bled at an earlier period of the disease, and more attention should have been paid to the bowels. But it is an evil inseparable from a country practice, that in the sickly season, a physician who has a large practice, can only see his patients once in twenty-four hours. His directions are more general than special ; and too much is left to the discretion of the persons in attendance.

Case 3. Henry Russell, a young man of delicate constitution, and predominating nervous temperament, about twenty-one years of age, was suddenly prostrated by disease, after having been on guard all night in the town of Wilmington, in September, 1831, a time of the year when the night air is extremely pernicious on account of the draining of the rice-fields. I saw him about 10 o'clock, a. m., I could not perceive any symptom of congestion in a particular organ ; his skin was cold and bedewed with a clammy perspiration, his pulse feeble and rapid, tongue slightly furred and of a dark color in the centre, shaded off towards the edges. The eye was not red, but had a very drowsy appearance ; he complained of no pain, but of excessive debility, and frequently sighed. Here was a very different case from the two former cases. Stimulants were required internally and externally; and nothing else could be done, until reaction was brought about. I applied a blister to the whole length of the spinal column, one on the inner side of each arm from the shoulder to the elbow joint, and on the inner side of each leg from the knee to the ankle; I directed stimulating pediluvia, to be followed with sinapisms. Internally, I administered in alternate doses, quinine and carbonate of ammonia. The blisters did not act at first, but being soaked in spirit of turpentine and re-applied, drew well after about twelve hours. Slight reaction took place during the night, and the disease assumed the intermitting type. His bowels were then evacuated by enemata, and the internal stimulating practice continued; at night, morphine, combined with quinine, enabled him to rest comfortably. He was out of danger when reaction was established; but his recovery was tedious.

Case 4. Alfred W., aged about thirty-six, very stout, with a short thick neck; drank freely of brandy and water—he was attacked with malignant intermittent in the month of September, 1834, in Brunswick County North Carolina, twenty miles from the town of Wilmington.—When I saw him, he had just experienced the second paroxysm: he was in a deep stupor, with a low agitated pulse, the skin perfectly dry and warm. As I was not present during a paroxysm, I have to describe the course of the disease as it was related to me. The disease commenced with a severe chill, which lasted for four hours; during the whole of which time, he vomited incessantly; spasmodic cramps in the stomach tormented him beyond description; and towards the conclusion of the vomiting, blood, which from the description given me must have been arterial, was ejected after excessive straining from the stomach. This distressing state of things was followed by fever not well developed, but accompanied with excruciating pain in the head and back in the lumber region. After four hours more of suffering almost too great for human nature to endure, he sank into a stupor, with labored respiration, and eyes but partially closed. After a short time I succeeded in rousing him, when he told me that it would be impossible for him to bear another paroxysm, and that he would rather be shot and put out of his pain at once than endure it. I saw clearly that he could not survive the third paroxysm. I therefore gave him, during the short interval, sixty grains of sulphate of quinine in two doses. When the four hours of intermission or remission had elapsed, and the time for the accession of another paroxysm had arrived, the system was under the full influence of quinine; instead of the dreaded chill, the pulse was full and bounding, and the heat of the surface was much increased, but there was an evident determination of blood to the head, from the red eye, and powerful action of the carotid and temporal arteries, presenting a new feature of danger. In this case however I could not bleed; I knew the habits of my patient; and they who drink freely never bear bleeding well. Besides his strength had been greatly prostrated by the two severe paroxysms which he had experienced; and above all, the excitement was artificial. My object therefore was to equalize the circulation, and knowing that the case was desperate, and that from his habits he could bare an unusually large dose of opium. I gave him eight or ten grains in pills, and watched the result with great anxiety. In ten to fifteen minutes the action of the opium was apparent; the equalization of the circulation was going on rapidly; and in half an hour he was fast asleep, and his body covered with warm natural perspiration. I then left him, after giving general directions, and had no occasion to visit him again. I saw one of the family a few days afterwards who told me that he slept for twenty hours, and awoke much refreshed and with a keen appetite. A dose or two of caster oil was all the medicine that it was found necessary to administer to him, and he then speedily recovered.

CASE 5. Henry Stone, about 26 years of age, of a robust temperament, was apparently in good health when he left a camp meeting ground in Jackson County, West Florida, about 11 o'clock, A. M., in the month of September, 1826. He was accompanying his father-in-law, Major Trippe, to his residence about five miles from the camp ground. Soon after starting he complained of feeling very cold, but it excited no alarm, as it was supposed to be a common chill—the first stage of an ordinary intermittent. On arriving at the house, the disease had made such rapid progress that he was nearly insensible, and had to be taken from his horse and carried to bed. Still the family believed that they could put a stop to the chill, and bring on re-action. After fruitless trials, persevered in for several hours, they became alarmed, and sent for a physician. My friend Dr. Stewart arrived at the house about sun down, but every effort was unavailing; and he died at midnight. Dr. S. informed me that he made use of every external and internal stimulant that he could think of, without for one moment checking the progress of the disease. The Dr.'s remark was expressive, "that he might as well have thrown all the medicine into the yard, and applied the blisters and sinapisms and frictions to the wall." It is to be regretted that a post-mortem examination of this case was not made. The prejudices of the family on this subject were very strong, and they would not tolerate it. In this case, the sensation of a chill increased in intensity without any respite until it terminated in the icy coldness of death. The nervous system was completely overwhelmed and paralyzed, and all the functions of life suspended, except a feeble internal action of the blood vessels, and equally feeble respiration. Now there is nothing in this case to prove that it did not belong to the great class of intermittents or remittents. Had re-action occurred, it is fair to infer that the disease would have become periodical, as such is invariably the case when re-action can be brought about.

CASE 6. In the spring of 1827, in the month of May, Henry Trippe, the brother-in-law of Stone, of nearly the same age, but of a larger frame, and one of the family in whose house Stone had died, was rifle shooting for beef, enjoying, as he thought, perfect health. He was attacked about the same time of day as Stone, and with the same symptoms, but died two or three hours sooner. Every effort to produce re-action was without the least effect. He, like his brother-in-law, was death-struck from the accession of the attack.

CASE 7. In 1836, in the month of August, I was practising on the Bayou Bœuf, and on the prairies of Opelousas, in this State. For several days in succession I experienced a slight paroxysm of intermittent fever, which did not last over three or four hours. I thought I had got rid of it, when I was suddenly prostrated by the aggravated form of disease that supervened. It appeared to me that some person had struck me a violent blow on the back of the head with a heavy hammer; an icy coldness was extending slowly from the extremities towards the trunk, and the power of speech was lost for several minutes. This occurred about 5 P. M., on Thursday. Fortunately, several friends were in the house at the time, to whom I was able to give directions before I became completely insensible. I told them that they had nothing to do but to stimulate me, and bring on re-action. After frictions with warm spirit of turpentine, a blister was applied along the spine, from the commencement of the dorsal vertebræ to the end of the spinal column; blisters were also applied to the inner parts of the arms and legs, as in Russell's case, No. 3, and one large one immediately over the stomach, covering the whole epigastric region. French brandy and water was administered to me freely, and remained in the stomach, until in the course of the night, being very delirious I asked for other drinks, which were immediately rejected; my friends subsequently paid no attention to what I asked for, but gave me brandy. From 5 P. M., on Thursday, I remained insensible until 7 A. M., Saturday, except when occasionally roused by a most distressing singultus. During this time the skin was uniformly dry and cold, the mouth very dry—no urine, no saliva, no bile secreted. In fact, the secretions were completely suspended; and several times I was supposed to be dying. The blisters, however, aided by the internal stimulas, drew well, and on Saturday morning a slight re-action was apparent; the pulse could again be felt at the wrist; urine was secreted and passed, and saliva appeared in the mouth. Finding that my pulse was becoming stronger, and deeming it time to evacuate the bowels, I took a moderate dose of rheubarb and magnesia, in mint water. It was instantly rejected. I then took 20 grains of calomel which remained on the stomach, and an hour or two after several enemata were administered, which speedily excited the bowels. After five or six thin evacuations of an almost black color, I began to sink again rapidly; the purging was stopped by a suppository of opium, and brandy and water taken freely. By day light, Sunday, I could bear further evacuation, when to my great satisfaction, I had conclusive evidence that the secretion of bile was restored. For about a week I experienced a slight chill and fever every day, followed by perspiration, which yielded to quinine in moderate doses. My recovery from this attack was very slow; indeed I was not fully restored to health until after a trip to Florida in April, 1837, and another to Philadelphia, near the first of June. Here the intermitting character of the disease was evident, both before and after the intermediate stage of collapse, which lasted 38 hours. Yet it is almost impossible to conceive of disease more severe than what existed during that period. Soon after my recovery, I lost several cases of as nearly as possible the same character, and which were treated in the same way with the addition of large doses of quinine. Re-action appeared in all of them, but they commenced sinking soon after the bowels were evacuated, and never could be revived again.

It is evident, that if I had not stopped the purging on Saturday night when I was rapidly sinking, so that the room appeared to be filled with smoke and there was a confused ringing sound in my ears, and resorted to stimulus for three or four hours, I must have died in less than twelve hours.

Had I been present in the cases which terminated unfavorably, at the opportune moment to arrest the purging for some hours and stimulate in the meantime, it is probable that some of them would have terminated favorably.

I have repeatedly had occasion to notice that in fevers, such as have been described, whenever the re-action is feeble, purgatives must be administered with the utmost caution. Enemata should be tried first; and they will generally answer the purpose; if, however, a purgative is necessary, the mildest should be selected.

It is lamentable to think of the great number of lives that have been lost from inattention to this important point. Over and again in my practice, and almost every year in the practice of others, before I resided in this city, I have seen, or heard of patients who were thought to be convalescent, re-action having been completely established, but who were purged to death by the repeated doses of purgative medicine that had been previously adminstered, none of which had operated up to the moment of re-action.

Case 8. Dr. W., a gentleman forty years of age, who resided in West Florida, died in the fall of 1836; and his case, as related to me by Col. Pitman in May, 1837, will illustrate the foregoing remarks. The Colonel visited him more or less every day from the commencement of the attack, until it terminated in death. From the description given to me, the disease must have been very similar to one, for which I treated him eleven years before, in 1825. Pain in the head and back, morbid heat of the surface, clammy perspiration appearing on spots of the body, sometimes on the forehead, at another time on the neck, and again at the wrists; pulse not very frequent, but indicating disturbance of the vascular system, great restlessness, frequent sighing, and a dark brown tongue rather dry and much coated, make up the symytoms as described to me.

Efforts were made to purge him, and to substitute a natural general perspiration, instead of one partial and clammy. In this way two or three days passed, the patient growing gradually worse and sinking; dose after dose of calomel, of rheubarb and jalap, of castor oil, and epsom salt, aided by enemata and pediluvia were given, without producing the slightest effect. His medical attendants then became alarmed, and began to stimulate internally and externally, doing at last what should have been done at a much earlier period, immediately after moderate bleeding with cups. But it is evident that the man's fate was already decided. The moment he was roused by the stimuli, so that the bowels could act, he was purged to death by the numerous doses of purgative medicine that had accumulated in the stomach. Every effort to arrest the purging failed as usual.

As the Dr. was a man of sound constitution who usually enjoyed rigorous health, he would probably have been saved by a different course of treatment. It seems to have been a case for cupping, a mild emetic, blister, mixed doses of quinine and morphine, enemata, pediluvia, &c.

Case 9. Bradshaw, an Englishman, of sanguine temperament, and florid complexion, twenty-eight years of age, experienced an attack of bilious remitting fever in Jackson county, W. Florida, during the month of July, 1825. The fever was very intense, heat of the body much elevated, pulse full and bounding, one hundred and twenty per minute, eyes red, tongue moist of a light brown color, red at the edges; he complained of headache, and great thirst, and was very restless. For three days in succession I bled him each day from the arm, to the extent of twenty ounces, and gave a dose of calomel and jalap. The bleeding always afforded temporary relief, the purgatives operated well, carrying off large quantities of bilious matter, the urine at the same time was scanty and high colored. Under this treatment he did not improve.—The fever raged as fiercely the fourth day as it had the first. It was necessary to resort immediately to some other decisive measure. His head was placed over the side of the bed with the face downward over a large tub placed on the floor, and very cool water from a deep well was poured on the back of the head from a large pitcher by a negro standing on a table. He shuddered at first, for the shock was severe, but soon found it very agreeable. In about half an hour, the operation of pouring the water going on all the time, he was cool to the end of the extremities, and experienced complete relief. The fever began to rise again in fifteen or twenty minutes, and in half an hour had increased sufficiently to bear again the stream of water. This practice was continued from 9 o'clock in the morning, until sun down, the intervals of relief becoming each time more prolonged, until at length a copious perspiration appeared on all parts of the body. He soon fell asleep, and slept soundly the whole night, being the first sleep he had enjoyed for four days and nights. The remittent was then converted to an intermittent fever. The fever never returned with the same violence, but diminished in force every day under the cold water treatment. I had occasion to give him a mild emetic; and a decoction of peruvian bark, snake root, and the bark of the swamp poplar, soon restored him to health.

This case was badly treated for two days; after the first day, the bleeding and purging, should have been omitted, and the application of a stream of cold water to the head substituted; the emetic and bitter decoction should then have followed, and he would probably have been spared two days of severe suffering.

I subsequently treated several somewhat similar cases, resorting to the cold water practice and an emetic on the first day of the disease. They all ended in light intermittent fever, which yielded promptly to quinine.

The foregoing cases occurring in Florida, North Carolina and Louisiana, lead to the conclusion: 1st, that the summer and autumnal fevers of the South and South-western States, are all more or less characterized by "periodicity or intermittence." 2nd, That they are all varieties of one and the same disease. And 3d, That the symptoms are so various and different, that no plan of treatment can be adopted. Each case should be treated with reference to the symptoms which may be developed, without thinking of the name of the disease.

If the first proposition be true, that all the summer and autumnal or miasmatic diseases, are characterized by "intermittence" in a greater or less degree, then the term congestive fever cannot be properly applied as signifying a distinct and specific disease.

The difficulty exists in finding a better term. In this Journal for May 1847, page 701, Dr. Lewis, of Mobile, observes when speaking of the third epoch of the Medical History of Alabama, "but, in 1834, we find its approach (i. e. fever) was insidious and unobserved, giving no serious warning of its proximity, until the unconscious victim was secure in its grasp. The patient first complained of depression, *heat and burning*, when to the touch the surface was icy cold. That cold, that first stage, is now the stage of disease and peril; and that re-action which in past days was looked to with fear and trembling, would now be hailed as the messenger of returning health and vigor."

There could be no better description in so few words of what is ordinarily called congestive fever. Art is required to bring on reaction in this case, while nature brings it on in all the other forms of intermittent and remittent fever.

Cases like those of Stone and Trippe, Nos. 5 and 6, have to my knowledge occured in the practice of other physicians, yet they cannot fairly be cited, even as an exception to the general rule of "periodicity." On such occasions, the nervous system is completely overwhelmed; no reaction follows naturally, and all the resources of art, are as unavailing as those of nature; so that we witness but the first stage of a paroxysm, during which the patients die. Whenever, without exception, re-action however feeble, can be established, the true character of the disease is developed, and "periodicity or intermittence" is invariably observed.

The disease called congestive fever, is not confined to the summer and fall. We see deaths announced, as occuring in this city, of congestive fever as early as the month of April, and as late as November.

In all the formidable varieties of miasmatic fever in the Southern States, there is a decided tendency to congestion—but that is no reason why they should be called congestive fevers. "Periodicity" is the characteristic feature. The intermittent or remittent type is apparent before congestion ensues; and in those terrible fevers which destroy life in ten or twelve hours, and which are often cited as proving beyond doubt the existence of congestive fever, as specific in character as the yellow fever, there is probably little, if any congestion. A subtile poison, be it inorganic or organized matter, instantly paralyzes the nervous system, and every function of life is suspended, except feeble pulsations of the heart and large arterial trunks, and an imperfect laboured respriation. It is, in fact, a sudden stoppage of the machinery of life. There is no time for lesions, congestions or any thing of the kind.

The purgative plan of treatment so highly recommended by Dr. Hamilton, may be benificial in the cold regions of the North of Europe, but it is extremely dangerous, and rarely ever successful in the aggravated miasmatic fevers of Southern latitudes. Sad experience at an early period of my professional career, convinced me of this fact.

In the treatment of these fevers, it is necessary to evacuate the bowels, and if enemata do not accomplish the purpose, then a purgative may be tried; but there are cases in which something has been done before we attempt to evacuate the bowels, or before it is possible to accomplish that object. In all those malignant fevers, where the secretions of bile and urine, and saliva are suspended, and the skin is dry as parchment, purgatives will not operate. To persist in giving dose after dose to relieve the constipation as it is called, is in nine cases out of ten, to ensure the death of the individual, even should the secretions subsequently be restored. The practice therefore, is not only useless, but decidedly dangerous. Let the organs of secretion be first attended to, and then very little purgative medicine will be required.

Dr. Cartwright of Natchez, about twelve years ago, published an account of the treatment of a malignant fever, in which congestion predominated. If I mistake not, he gave as much as ten grains of tartar emetic, and thereby completely broke up the disease, and rendered the case quite manageable. I have never ventured to give so large a dose, but have never failed to produce marked benefit, when the circumstances of the case have permitted the use of emetics in ordinary doses. But there are some cases in which they are inadmissible, particularly when there is great irritation of the stomach, which ejects every thing that can be given, and pain is excited by the slightest pressure on the epigastic region.

Again, there are instances where bleeding affords great and immediate relief, and other cases where death would be the inevitable result. It is therefore useless to recommend any particular form of treatment. Let all the circumstances of the case be duly considered, as the season of the year, age, sex, temperament, habits and occupation; also the general character of the prevailing diseases in the neighbourhood, in their inception, course, and especially in their termination; and then prescribe as enlightened judgment may dictate or experience suggest.

In conclusion, I shall offer some remarks on the use of calomel in the fevers that have been spoken of.

There is, perhaps, no one article in the meteria medica more valuable than calomel, when judiciously administered on proper occasions; and it is still more certain, that no other medicine has produced such mischief, and brought such approbrium on the medical profession.

In bilious remittent fevers, when there is an inordinate secretion of bile, can the same remedy be proper which is employed to rouse a torpid liver? In the former case, there is too much action in the organ; and what other effect can murcurial purgatives produce, except it is to increase that action? Day after day have I, in the early part of my professional career, given a mercurial purgative, and on the seventh or eighth, been astonished to see as copious bilious discharges as ever; and no wonder, as I was daily exciting the secretions of bile, instead of endeavouring to restrain it. It also frequently happens, that the young physician is deceived by the appearance of the stools, procured by the administration of mercurial purgatives. They are sometimes quite black; this color arising from the presence of an acid in the stomach or intestines, with which the calomel comes in contact. This is considered as the strongest evidence of the necessity of more purgaitve medicine, and this mistake has cost many a man his life. In every case, however, sooner or later, I had to resort to an emetic, and change the plan of treatment. Rheumatic, and dropsical affections, torpid liver, dyspepsia and a broken constitution, are too often the sequelæ of bilious remittent fever treated with mercural purgatives.

In the more aggravated fevers such as what are called congestive, malignant or pernicious, mercurial purgatives persisted in are equally, if not more inadmissible.

But the greatest objection is to the plan of treatment adopted by so many physicians in country practice at the South, and in the great valley of the Mississippi; I mean when calomel is relied on as a specific, and ptyalism as a rule must be established. In the first place, all symptoms are disregarded, and the disease is treated by name and by rule. In the second place, the mortality is greater than by almost any other plan of treatment. Thirdly, if it effects a cure, it leaves the constitution shattered, and entails diseases almost as bad as the original one.

It would be considered incredible, were I to relate some of the deplorable effects of this mercurial plan of treatment which I have witnessed and heard of. Some have died from the destruction of the jaw bones and other bones of the face, and sloughing of the gums and edges of the tongue. Others have wasted away and perished with mercurial fever, brought on by the attending physician, but which it was out of his power to arrest; while a much larger number have escaped death to wear out a miserable life with a broken down constitution and obscure forms of chronic disease.

Such being too often the lamentable effects of the salivating curative plan in the fevers in question, humanity, as well as common sense, should preclude its use, if there is any milder and safer treatment that can be adopted. And that there is, we have in this city the most abundant proof, while in the medical journals we continually meet with cases of fever of the gravest character treated successfully without the use of mercury.

It may be well to state that salivation can be arrested by washing the mouth several times in the day with a saturated solution of acetate of lead, in two parts of water to one of vinegar, and with the addition of laudanum, say a drachm to four ounces of the solution. The earlier this is used, the better. If the gums are spongy and denuded, the application is very painful for several successive times the solution is used. Of course, care must be taken not to swallow any portion of it, and hence it would be unsafe to use it with young children. I have used it for many years without failing in a single instance. The idea was first suggested to me by Dr. Chapman in the session of 1823–24 at Philadelphia. When lecturing on colica pictonum, the Dr. observed that if calomel is an antidote for the lead in this disease, why should lead not be an antidote for calomel. Some years afterwards I met with the prescription as already given, in one of the New York Medical Journals, taken from a Prussian Journal. It produces a black deposit on the tongue and gums, and even colors the teeth black; this deposit should be scraped off every day with the handle of a silver spoon.

It is probable in this case that the greater part of the mercury is in the salivary glands, and becomes decomposed in the form of a black oxide. A gentleman of veracity assured me that he had scraped off this black deposit, and exposed it to the sun for several days, when globules of quicksilver could be distinctly seen. I have used this remedy in a great number of cases, some of which were recent and slight, and others again very severe and of several days duration, but I cannot recollect a single instance in which the constitution subsequently suffered in consequence of the mercury that had previously been taken. I therefore recommend all to give it a trial, who are at a loss for a remedy to arrest salivation.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*A Treatise on Diseases of the Air Passages: Comprising an Inquiry into the History, Pathology, Causes and Treatment of those Affections of the Throat called Bronchitis, Chronic Laryngitis, Clergyman's Sore Throat, etc., etc.* By HORACE GREEN, A. M., M. D. Formerly President and Professor of the Theory and Practice of Medicine in the Castleton Medical College; Vice President of the New York Medical and Surgical Society, &c., &c. New York and London. Wiley and Putnam. 1846. pp. 276.

We had heard much of this work before we were honoured by its reception, which was but recently, and now that we have examined its interesting contents, we regret that our engagements will not allow us to give as ample a notice of it as we think it merits. As a literary production, its pretensions are quite moderate, but the author's apology, that it was "prepared amid the pressure of constant and constantly accumulating professional engagements," should have due weight; for it must be exceedingly difficult to write carefully and well when harassed with business. This work deserves commendation as an *American production*, for it may not be generally known, that European authors, especially in Paris, command far greater facilities for making books, than the writers of this country. There, if we have been correctly informed, an author, not having time to write himself, has only to *shadow forth* the grand idea and leading features of the desired book, the references and number of pages; and in due season his publisher furnishes it prepared *secundum artem*. He then has only to revise the work and see that he is made to express what he means. Here, it is altogether different; we have to do every thing ourselves, there being neither a sufficient number of authors nor competent assistants to render available the short and easy method of attaining literary distinction. We therefore feel inclined to extend every encouragement to native authors, giving due credit for originality and practical utility, and making proper allowances for imperfections in composition.

When Dr. Green's work first appeared it was severely criticised, and the author accused of appropriating to himself the credit due to MM. Belloc and Trousseau, of France, for discovering the virtues of nitrate of silver in ulcerations of the larynx and pharynx. Dr. Green settles the difficulty by saying, "while I claim no credit for having originated the practice myself, I, on the other hand, give these authors none, for having, so far as I am concerned, suggested it, for I had been in the practice of cauterizing the larynx nearly two years before I had ever heard of Trousseau and Belloc." Their work was published in this country in 1841; whereas, Dr. Green says he took the idea of applying the nitrate of silver *within the larynx*, from a conversation he had with Dr. James Johnson, of London, in 1838, and after returning home he put into successful practice what had been deemed hitherto impracticable. Indeed, its practicability was denied on *theoretical grounds*, even after Dr. Green had made many most remarkable cures. He has now the gratification of appending to his volume, the complimentory testimony of Drs. Charles A. Lee, Alfred C. Post, J. Bryan, S. C. Foster, and Abm. L. Cox, of New York.

His work consists of 10 chapters, under the following heads, viz: 1. *Anatomy of the Larynx, Trachea and Bronchi*; 2. *Physiology of the Mucous Follicles*; 3. *Pathology of the Throat, Larynx and Bronchi*; 4. *Follicular Inflammation of the Throat and Air-Passages*; 5. *Malignant Follicular Disease of the Air-Passages*; 6. *Pathology of Follicular Disease of the Air-Passages*; 7. *Causes of Follicular Disease of the Air-Passages*; 8. *Symptoms of Follicular Disease of the Air-Passages*; 9. *Of the Treatment of Follicular Disease*; 10. *The same.*

Each of these subjects is treated in a lucid and practical style. The treatment, particularly, is illustrated with a variety of interesting cases.

The title on the back of this work would indicate that *bronchitis* was the principle disease treated of; but such is by no means the case. It is devoted almost entirely to the consideration of *follicular inflammation of the fauces and larynx.* Dr. Green says this is apparently a new disease; at least, that it is only within the last twelve or fifteen years that its distinct history has been given. We extract from the work the following:

Description of the Follicular disease of the Air-passages.

"This peculiar malady consists essentially, in its formative stage, of an inflammation of the mucous glandulæ; which is sub-acute in its character; and which may result, as above stated, in hypertrophy, ulceration, or induration of these glandulæ, or in a deposition of tuberculous matter into the substance of the follicles, themselves.

"In its simple and uncomplicated form, the affection commences, invariably, in the mucous follicles of the fauces and pharynx; and is extended thence, by continuity, to the glandulæ of the epiglottis, larynx and trachea; and in some instances to those of the œsophageal membrane.

"So insidious, frequently, is the onset of this disease; and so gradual its progress, that in some instances it will be found to have continued many months, and to have made considerable advance before the presence of any prominent local symptom shall have called the attention of the individual to the existence of the affection. He then, perhaps, becomes aware of an uneasy sensation in the upper part of the throat, accompanied by a frequent inclination to swallow, as if some obstacle in the passage might be removed by the act of deglutition; or, more frequently, there is an attempt made and often repeated to clear the throat by a kind of screatus or hawking, and to relieve it of a sensation of "something sticking at the top of the wind-pipe." About the same time, there is observed an alteration in the quality or *timbre* of the voice, there is experienced in the vocal organs, a loss of power and a hoarseness is present, which at first is hardly perceived in the morning or after a full meal, but which is increased towards evening, and after speaking or reading longer or louder than usual. The mucous secretion, which in a healthy condition of the glands is bland and transparent, becomes viscid, opaque, and adherent, and is increased in quantity. Frequently there is a slight soreness felt about the region of the larynx, but seldom is any cough present at this stage of the disease. In this condition the symptoms may remain for a long period; sometimes for years; nearly disappearing at times, and then again being greatly aggravated by vicissitudes of temperature, increased exercise of the vocal organs, and by various other morbific causes.

"If we inspect the throat and fauces during the progress of the above symptoms we shall find the epithelium, which in the healthy state of the mucous tissue covers its surface, more or less destroyed; its absence being manifested by the slightly raw or granulated appearance which the membrane presents; the mucous follicles will be found hypertrophied, and will appear distinctly visible; especially those studding the upper and posterior part of the pharyngeal membrane. (See plate I.) If the disease has been long continued, a portion of the follicles may be found indurated, or in some instances filled with a yellowish substance having a resemblance to, and presenting the physical characters of tuberculous matter, whilst striæ of opaque adhesive mucus, or of a muco-purulent secretion, may be seen hanging from the veil of the palate or coating the posterior wall of the pharynx. As the disease advances, and the follicles situated at the root of the epiglottis and in front of the arytenoid cartilage, and the still more numerous glandulæ of the laryngeal mucous membrane, become involved in the morbid action all the above symptoms appear greatly aggravated; the hoarseness is much increased, and is constant; speaking or reading aloud is attended with great difficulty; and when continued for any period is followed by pain and increased soreness in the region of the larynx; and by a sensation of extreme languor, not only about the vocal organs but throughout the whole system. In some cases where the disease affects the glands situated in the ventricles of the larynx and near the vocal chords, the voice becomes completely extinguished; or if, by great effort, the patient essays to speak aloud, the vocal resonance is uneven, harsh and discordant.

"In such cases, notwithstanding the situation and extent of the disease, there is seldom present any decided or troublesome cough; and in this respect follicular disease differs essentially from all other equally grave laryngeal affections. Cases have fallen under my observation, repeatedly, where the affection had advanced until the symptoms present indicated extensive disease of the follicles of the larynx and of the membrane covering the vocal ligaments;—until the ulceration of these glands situated at the root of the epiglottis could be felt upon the laryngeal surface, and yet the patient would remain free, or nearly free from a cough, notwithstanding an abundant acrid secretion, poured out by the diseased follicles, would occasion an incessant hawking to clear the upper part of the wind-pipe and the pharynx of this tenacious mucus.

"As illustrative of many points in the above description of uncomplicated follicular disease, I have selected the following cases."

From these cases we select the following as among the most interesting.

"K. H. E. Esq., a lawyer of eminence in this city, aged 38 years, suffered from an attack of acute bronchitis, in April, 1840. Under the most active treatment he recovered from the disease and resumed his professional duties. In 1841–2, he was a member of the Common Council, and in addition to the duties of a full practice, which necessarily involved much public speaking, he was frequently engaged in the exciting debates of the honourable body of which he was a member.

"Early in 1842 he began to be sensible of a slight huskiness of the voice, and of an uneasy sensation in the throat, after public speaking. These symptoms would all subside after a little rest, but only to be renewed at each subsequent public exercise of the vocal organs. It was observed that this hoarseness gradually increased, and that the irritation about the throat impelled the individual to make frequently repeated efforts at hawking, as if to remove some obstruction from the larynx. Being in attendance upon his family during the progress of these symptoms I had frequent opportunities to inspect his throat, and I observed that the follicular glands of the isthmus of the fauces and of the superior portion of the pharyngeal membrane, were slightly hypertrophied, and were pouring out an altered and increased secretion. Believing, on his part, that these morbid symptoms would pass away, no special attention to his case was required or given until the latter part of July, 1842. At this time, a permanent hoarseness was present; the voice was rough and uneven, with a constant irritation and a sensation of soreness in the laryngeal cavity;—symptoms that were all greatly increased by every effort made and continued to speak or read aloud.

"The diseased follicles now presented a very different appearance from that which they had exhibited a few weeks before. The posterior fauces and pharyngeal membrane were studded with elevated tubercles, with inflamed bases or granulations of different sizes, like pustular inflammation—bearing a marked resemblance to the papulæ of varioloid.

"The most pendant portion of the uvula, which was greatly elongated, was also covered by similar diseased follicles. Compelled now, by the severity of the disease, to relinquish in a great measure his professional duties, he applied to me for medical aid.

"August 4th.—Removed the diseased portion of the uvula, and after waiting a few days to allow the truncated part to heal, I applied the nitrate of silver in substance to each enlarged follicle that could be seen, and ordered pills composed of the tenth of a grain of bichloride of mercury with two grains of the extract of conium; one to be taken night and morning.

"24th.—The affected glands are much less in size; the huskiness of the voice and the sensibility of the larynx yet remain. Passed the index finger of my left hand over the back of the tongue and laryngeal face of the epiglottis. The base of this cartilage and the lips of the glottis were slightly œdematous. Cauterized the fauces and pharynx with a solution of the nitrate of silver of the strength of forty grains to the ounce of water; discontinued the pills, and ordered one teaspoonful of the following solution, to be taken in sugared water three times a day:—

℞ Potassii. Iodid. ʒ ij.
Aq. distil. ℥ iv.
M.

"The patient complains of a dull pain in the back of the neck; counter irritation, by means of antimonial ointment, was employed along the cervical portion of the vertebral column.

"September 6th.—The enlarged follicles have nearly disappeared; the mucous membrane of the fauces and pharynx appears smooth and of a healthy colour; but the hoarseness and sensibility in the laryngeal cavity are in no degree relieved. The least excitement, he remarks, affects him injuriously there; and if he attempts to read to his family or to converse in an ordinary tone of voice with a friend, all these symptoms are greatly aggravated.

"Confident, from these symptoms, that the follicles about the vocal ligaments were diseased, I determined to cauterize the interior of the larynx. This was effected by passing the sponge, wet with the solution, over the laryngeal face of the epiglottis, and pressing it between the lips of the glottis into the laryngeal cavity. By the spasmodic action that succeeded, the fluid was expressed from the sponge, and this latter being quickly withdrawn, its removal was followed by a convulsive cough and a free expectoration of adhesive mucus. The unpleasant irritation thus produced in the larynx subsided in a few minutes, and, as is very generally the case in such instances, was followed in the course of the subsequent twenty-four hours by marked relief. As the effects of the cauterization, however, did not entirely pass off for several days, the employment of topical medication was not renewed until the thirtieth, when the pharynx and the interior of the larynx were again freely cauterized.

"This operation was repeated, at first every third or fourth day, for some time; then every week, until the first of November, when the hoarseness and the sensibility about the vocal ligaments had disappeared, and the patient was enabled to resume and again to discharge his professional duties, without any other inconvenience than a debility of the vocal organs, which, however, under the use of local and general tonics, soon passed away.

"In all cases of sub-acute inflammation of the mucous follicles, the tendency of the morbid action is to terminate, ultimately, in ulceration; although, as we have seen, these glands may remain in a state of hypertrophy or induration, in some instances for years, before this form of structural lesion shall occur.—Ulcerations of the follicles of the air-tubes differ essentialy, in their appearances, from those ulcerations of the mucous membrane, which are the frequent consequence of inflammation of that tissue. In the latter, when the result of chronic inflammation, the ulcer commences by destroying the epithelium, and then, extending its circumference and depth, penetrates the mucous tissue and appears in the form of a superficial ulceration, with irregular edges and a rough sloughy base.

"Ulcerations of the glandulæ are preceded by chronic inflammation and hypertrophy of these bodies, and when thus engorged the follicles appear like small points beneath the mucous membrane. If the irritation continues, infiltration of puriform or tubercular matter takes place within the cavities of the glands, by which the parietes are distended, and finally are ruptured; and they are then seen in the form of small reddish elevations, with irregular hardened edges, and having central ulcerations which often extend into the sub-mucous cellular tissue."

Dr. Green reports twenty-three cases illustrating the various complications of follicular disease, but we must refer the reader to the work, for interesting details.

In his 7th chapter Dr. Green treats of the *causes* of follicular disease, under the following heads, viz: *hereditary tendency; climate; debility; sex; influence of age; influenza; eruptive fevers; dyspepsia; exercise of the voice; and tobacco.*

Dr. G. says that "one of the most important among the remote causes of this affection is a constitutional predisposition." In regard to the influence of climate, he says:

"*Climate.*—The influence of climate in the production of a morbid condition of the mucous lining of the larynx, trachea and bronchi, has been noticed by most writers on diseases of these organs. In predisposing the mucous follicles of the investing membrane of the air-passages to take on diseased action, the cold, and especially the cold and moist atmosphere of a northern climate, operates as a powerful agent.

"In its more aggravated form, follicular disease of the pharyngo-laryngeal membrane, first made its appearance, in this country, in New England; and it has occurred most frequently and has proved most severe in the cold and Northerly States of the Union. For several years after the attention of the profession at the North had been called to the frequent occurrence, and the severity of the disease, it was not admitted that the affection had any existence at the South.

"But this exemption from the disease does not obtain at the present day.—Some of the severest cases of follicular disease which have come under my observation during the last two or three years, have been those of individuals coming from some of the most Southern States ; and I have been informed by intelligent patients from New Orleans, that cases of the " Throat Ail" are very rife in that city."

We feel bound to add our testimony to what is here said of the frequency of follicular disease in New Orleans. During the past winter and spring we met with a number of severe cases, all of which were greatly benefitted by the treatment laid down by Dr. Green. We are greatly indebted to the author for valuable hints obtained from reading notices of his work, and therefore more cheerfully perform the task of inviting the special attention of Southern physicians to the subject.

We cannot omit the following remarks on a habit which is indulged to a most injurious excess in the South.

" *Tobacco.*—With regard to the effects which are produced on the human system by the habitual use of tobacco, different opinions are held and have been expressed.

" That a deleterious influence is exerted on the animal œconomy by its use, most pathologists of the present day admit. Of this, after having watched for many years in my practice the effects of this narcotic, I entertain not a doubt ; and I fully accord with the opinion expressed by Dr. Prout, who observes, in his work, " On the Nature and Treatment of Stomach and Urinary Diseases," that it [tobacco] disorders the assimilative functions in general, but particularly, as he believes, the assimilation of the saccharine principle. " I have never, indeed, been able, he adds, to trace the development of oxalic acid to the use of tobacco ; but, that some analogous and equally poisonous principle (probably of an acid nature) is generated in certain individuals by its abuse, is evident from their cachectic looks, and from the dark and often greenish yellow tint of their blood."

" In the " Elements of Materia Medica," Dr. Pereira, in speaking upon the physiological effects of the moderate use of tobacco on the human system, remarks, that he is not acquainted with any well-ascertained ill effects resulting from the habitual practice of smoking tobacco.

" He admits, however, that he is not so competent to speak of its effects when otherwise employed, as in England " the practice of *chewing* tobacco is principally confined to sailors," and is, therefore, less frequently submitted to his observation.*

" As an exciting cause, the use of tobacco, in my experience, has proved a powerful agent in the production of follicular disease of the throat. Acting as a stimulant directly and constantly upon the mucous follicles of the fauces and throat, and greatly increasing, as it does, the secretion of these glands, its employment, as we should conclude à priori, must have a direct tendency to develope the disease, especially if a predisposition to the affection exists ; hence it has occurred to me to notice that of a great number of cases of throat-ail which, during the last year or two, have come under my observation, a large proportion of them have taken place in individuals who had been or who were at the time in the habitual use of tobacco.

" My attention has been called more particularly to this subject from having noticed, several years ago, some observations on the use of tobacco in laryngeal and bronchial affections, by an eminent surgeon of this city. After having alluded to the almost universal use of tobacco in the countries of Northern Europe, he observes :—" In one very fatal and distressing form of disease, to wit, Laryngeal Phthisis and Brónchitis among public speakers, the fact is very clearly established, that the moderate habit of smoking, by the drain it accom-

* Op. Supra Citat. p. 318.

plishes, and its anodyne qualities has been eminently useful, at least as a preventive of that peculiar malady so frequent in the United States, especially among the clergy."*

From this opinion of my distinguished countryman and friend, I am compelled to differ, entirely, by the statistical facts, which I have obtained, on this subject. Not only has the use of tobacco in any and all its forms proved, in my experience, an exciting cause of laryngeal disease; but where its employment has been persisted in, during the treatment of any case, I have found it impossible to restore such to perfect health."

We extract the following from his chapter on *the symptoms of follicular disease.*

"Having already described the morbid appearances which are found in the forming stage of follicular inflammation, and entered quite fully into the pathology of the disease, I shall now only allude briefly to the particular symptoms of the affection.

"It has been stated in a former chapter that the access of follicular laryngitis is in some instances so insidious, and its progress so gradual, that not unfrequently it may continue many months and make considerable advance before the manifestations of disease shall be such as to alarm the individual or to call his attention even to the existence of the affection.

"Ordinarily, however, soon after the mucous glandulæ have taken on a morbid action, there is perceived in the region of the fauces an increased mucous secretion, and an uneasy sensation in the gullet or upper part of the throat is observed, attended by a frequent desire to swallow, as if some object sticking in the passage might be removed by the act of deglutition; or more generally, repeated attempts are made by hawking to clear the throat and allay the irritation, all which difficulties are considerably augmented by every continued effort made to read aloud, to sing, or to speak as in ordinary conversation. If the secretion from the mucous follicles of the throat be examined at this period it will be found to be altered in its character—being adhesive and in some instances of an alkaline quality, and proving to be by its effect on the mucous membrane, of an irritating nature.

"About the same time, if the patient be accustomed to employ the voice in public speaking or in singing, there is apparent to a greater or less extent a loss of power in the vocal organs, uneasiness in the larynx, with sometimes pain on pressure. Hoarseness is also present, which may be light in the morning, or altogether absent, but which is increased towards evening and after speaking longer or louder than usual.

"On inspecting the throat, the fauces and the posterior wall of the pharynx will appear redder than natural, and the mucous membrane covering these parts will be deprived of its epithelium, injected, and studded over with enlarged mucous follicles. (See plate I.) Sometimes, if the disease is recent, these glands will appear quite minute, and will be distinctly apparent only when the pharyngeal cavity is exposed to a full light. In other instances, they will have attained a size sufficient to give a rough or granular appearance to the whole surface of the fauces, while the viscid tenacious mucous which is poured out by these follicles in their morbid state, may be seen coating the membrane, or appearing in patches, or marking its surface with white or yellowish white striæ.

"In some cases, several of the enlarged and morbid cryptæ will become confluent, and uniting form angry looking tubercles of the size of a split pea, which may be seen on the posterior wall of the pharynx.

"In others, again, a deposition of textural matter takes place and the follicle becomes indurated and permanently enlarged, or it may be distended with pus, or with a morbid secretion which will exhibit all the physical properties of tuberculous matter.

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* Travels in Europe and the East. By Valentine Mott, M. D., pp. 83-4.

"If the affection has continued for some time, we shall frequently find some of the diseased follicles in an ulcerated state; these are generally first observed about the palatine arch, the posterior wall of the pharynx, and along the border and on the laryngeal face of the epiglottis. In the first stage, these ulcers are small and superficial, appearing in the form of ash-colored patches, surrounded by an inflamed and slightly elevated base. (See plate II.) Continuing, they at length destroy the mucous follicles, and sometimes involve not only the mucous but the sub-cellular tissues in their progress.

"Accompanying the above symptoms there is often found œdema, and elongation of the uvula, and in many instances hypertrophy of the tonsils.

"In the incipient stage of follicular laryngitis, of the uncomplicated form, there is seldom much cough present. The irritation that is felt in the larynx and which is caused by the increased and vitiated secretion from the diseased follicles is generally relieved for the moment by hawking in this stage of the affection. As the disease advances, however, and the glandulæ of the larynx and trachea become involved in the morbid action, a cough will steal on, which from being slight at first, is at length severe, and in most cases is attended by a free tenacious expectoration.

"In this respect, the cough which arises in follicular disease differs from that which occurs in the early stages of tubercular affection of the lungs. In the latter, the cough will frequently continue for months without any expectoration, or if expectoration should occur, it will consist only of a trifling amount of transparent frothy fluid.

"In another respect these two diseases are essentially different. That peculiar mental condition incident to pulmonary disease—by which the spirits of the patient are buoyed up, and hope often continues bright to the last—is well known. The reverse of this obtains in follicular laryngeal disease. In this latter affection, *mental depression* is to some extent so universally present, particularly where the affection has been protracted, that I have been led almost to consider it a characteristic of the disease.

"If the disease is not arrested until ulceration of the follicles of the larynx and trachea occurs, a manifest influence is exerted by this lesion on the nature of the cough; it becomes greatly aggravated and is more or less paroxysmal. It has, if the structural change is extensive, a peculiar cracked or whistling character, and is attended, moreover, by considerable soreness in the region of the os hyoides. The effects produced upon the intonation of the voice are likewise very apparent, but they differ materially according to the seat and extent of the disease. If the ulcerations are confined to the follicles about the tonsils, the veil of the palate and the pharyngeal membrane, the timbre of the voice is not ordinarily much changed; incomplete disphony sometimes exists; or, in other words, the sounds are merely obscured or imperfectly articulated. But let the ulcerations extend below the epiglottis, and the hoarseness is greatly increased, the voice loses its power, and should the mucous glands within the ventricles and around the vocal chords become involved in the morbid alteration, it is reduced to a state of complete aphonia, and a harsh whisper—which is merely an articulation of the ordinary respiration—alone remains."

Treatment of Follicular Disease.—Dr. Green uses both *general and local* remedies in this disease, but as he says it may nearly always be cured in the early stages by the *local*, and the strong solution of the *chrystals of nitrate of silver* is his grand remedy, we give his directions for using it.

"*Method of applying the solution.*—In the treatment of laryngeal disease, by the direct application of the nitrate of silver to the diseased surface, I have employed ordinarily a solution of this substance of the strength of from two to four scruples of the nitrate to an ounce of distilled water. When, however, there are found extensive ulcerations of the epiglottis, or about the opening of the larynx—ulcerations which it is desirable to arrest at once, I have not hesitated to apply directly to the diseased parts a solution of double the strength of the last named. But one or two applications only of a medicine of this power should be made at one time; ordinarily, however extensive the lesions may be, it will not be necessary to employ a solution of greater strength than one composed of four scruples of the salt to an ounce of water. On the other hand, it has been found that one of less strength than of from forty to fifty grains of the nitrate to an ounce of fluid will have but little effect upon a diseased mucous surface, where ulcerations exist.

"In cases in which it becomes necessary to cauterize the interior of the laryngeal cavity, the aperture of the glottis should not be passed at once; the part should be *educated* by applying the solution daily for several days to the faucial and pharyngeal region, to the epiglottis and about the opening of the glottis.

"Proceeding in this manner, that exquisite sensibility which belongs to the lips of the glottis is in a good degree overcome, and the instrument may then be passed into the larynx without producing half the amount of that irritation which its introduction below the epiglottis would have awakened at first.

"The instrument which I have always employed for making direct medicinal applications into the cavity of the larynx is one composed of whalebone about ten inches in length, (with, or without the handle, as represented in the plate) curved at one end, to which is securely attached a small round piece of fine sponge.

"The extent to which the rod is to be bent must be varied according to circumstances, for the opening of the glottis is situated much deeper in some throats, than in others; but the curve which I have found suited to the greatest number of cases is one which will form the arc of one quarter of a circle, whose diameter is four inches. (See plate VII. fig. I.)

"The instrument being prepared, and the patient's mouth opened wide and his tongue depressed, the sponge is dipped into the solution to be applied, and being carried over the top of the epiglottis and on the laryngeal face of this cartilage, is suddenly pressed downwards and forwards through the aperture of the glottis into the laryngeal cavity.

"This operation is followed by a momentary spasm of the glottis, by which the fluid is discharged from the sponge, and is brought into immediate contact with the diseased surface.

"Every physician who has been present when this operation has been performed, (and a large number have witnessed it from time to time,) has manifested much surprise on observing how little irritation has been produced by the introduction of the sponge.

"If the patient, on opening his mouth, take a full inspiration, and then be directed to breathe gently out at the moment in which the sponge is introduced the irritation caused by the application will be much less than when this caution is not observed. The fact, indeed, has been fully established by repeated experiments, that the introduction into the larynx of a sponge saturated with a solution of the crystals of nitrate of silver, of the strength of forty, fifty or even sixty grains of the salt to the ounce of water, does not produce, ordinarily, as much disturbance as is caused by the accidental imbibition into this cavity, of a few drops of tea, or even of pure water!

"In the topical treatment of the follicular disease it will be found that all larynges cannot be entered with the same facility. Indeed, in some instances where œdema of the epiglottis and of the arytenoid cartilages has existed, I have found it very difficult, in making the first attempt, to pass the sponge of the probang through the aperture of the glottis."

Under the head of *general remedies*, he gives directions for using the following, accompanied by illustrative cases, viz: *nitrate of silver*; *iodine*; *mercury*; *prussic acid*; *muriate of ammonia*, and *change of* *climate.* For his remarks on these we must refer the reader to the work, as our limits are exhausted.

We must here close; but we cannot do so without expressing our decided approbation of both the principles and practice laid down by Dr. Green, and urging our Southern readers to supply themselves with his valuable work. It may be had of S. Woodall, 49 Camp street.

E. D. F.

II.—*Materia Medica and Therapeutics; including the Preparations of the Pharmacopœias, &c., with many new Medicines.* By J. Forbes Royle, M. D., F. R. S., &c., Professor of Materia Medica and Therapeutics, King's College, London. Edited by Joseph Carson, M. D., &c. Philadelphia. Lea & Blanchard. 1847. 8vo., 689.

The author of this book has already won a high reputation as a scientific man, by his "Illustrations of Himalayan Botany;" and by researches into the medical history of Hindoston, the results of which have not yet been published in full, but we must all hope that this work, entitled an "Essay on the antiquity of Hindoo Medicine," will not remain long, as it yet does, in the manuscript form. Many of the interesting things brought to light by these researches are now published in the present volume, and give new interest to many of the articles of the Materia Medica. Indeed, as far as the ancient history of many of these articles, of Asiatic origin is concerned, the Materia Medica of Professor Royle is more full of interest than any work that has come to our notice. He seems to have pursued every mode of research that could possibly throw any light upon his subject, and his accurate personal observation is invaluable in elucidating this previously obscure subject.

This work was prepared at the instance of Mr. Churchill, an eminent publisher, who brings it out as one of his excellent series of medical manuals. The arrangement of the work is good; the medicinal substances are arranged according to the natural system, and each substance considered in reference to its history, its physical and chemical properties, preparation, tests, action, uses and doses. All of these are briefly sketched in a concise and lucid manner, and in a way to show that a master hand was employed in the task.

This work, though apparently designed as a text book for students, is much better adopted to the use of those already familiar with the subject, as in the condensation of his matter, the author has been obliged to omit much that is highly important to the student. The physiological relations and effects of medicines are matters of the first consequence to the student in the acquisition of correct notions respecting their therapeutical properties and applications. In this respect this work is decidedly defective, so much so in fact, as to exclude it from competition, as a text book, with that of Pereira, or Wood and Bache's Dispensatory.—But to those who desire the latest information respecting the history and general relations of medicines, especially those of Asiatic origin, this work will prove highly satisfactory.

As examples of the authors description, &c., we may quote the following:

"ASSAFŒTIDA, L. E. D. (U. S.) Gummi Resina, L. D. Gummy-resinous Exudation (E.) of NARTHEX (*Ferula*, Linn.) ASSAFŒTIDA, *Falconer*. Assafœtida.

"Assafœtida, a product of Persia and Affghanistan, is mentioned in the ancient Sanscrit Amera Cosha. The ancients highly esteemed a gum-resin which the Romans called *laser*, and the Greeks οπος κυρηναϊκὸσ, or the Cyrenaic juice, from being produced in that region. The σιλφιον yielding it was an umbellifer, and is represented on the coins of Cyrene. It has been discovered of late years, and named *Thapsia Silphium*. This laser had become scarce even in the time of Pliny, who as well as Dioscorides describes another kind as obtained from Persia, India, and Armenia, which was probably the same that was known to the Hindoos. Avicenna describes *hulteet* as of two kinds: one, of good odour, from Chiruana (Cyrene?) and the other fœtid, the present *assa-fœtida*.—The term *assa* is no doubt of oriental origin, since it is applied to other gum-resins. Thus Benzoin is called *hussee-looban;* it used to be called *assa dulcis* in old works. Dr. Lindley has received the seeds of a ferula called *hooshee*.—*Anjedan*, the fruits or seeds (φυλλον of the greeks,) is usually translated *laserpitium*. The plant is called *angoozeh* by the Arabs. The root of *silphion* is described by Arrion as affording food to herds of cattle on Paropamisus.

"Assafœtida is produced in the dry Southern provinces of Persia, as in the mountains of Fars and of Beloochistan, but chiefly in Khorassan and Affghanistan; likewise to the north of the Hindoo Khoosh range of mountains, where it was found by Burnes and also by Wood's expedition to the Oxus. (*c*.) Dr. Falconer found it in Astore, introduced the plant into the Saharunpore Botanic Garden, as mentioned in the author's "Product. Resources of India," p. 223, and has obtained from it a small quantity of assafœtida. He also sent home numerous seeds, which were distributed from the India House to several gardens; but the author has not heard whether any plants have been produced from them. But he has no doubt that some of those which the author is informed by his friend Dr. Christison are still in the Edinburgh Botanic Garden, were produced from these seeds, and not from those sent by Sir John M'Neill. The assafœtida is conveyed on camels into India across both the Punjab and Bhawulpore, and is sold in large quantities at the Hurdwar Fair. It is also conveyed down the Indus and by the Persian Gulf to Bombay.

"Two or three kinds of fruit called seeds are met with, which are said to be those of the assafœtida plant; but there is no proof that more than one plant yields assafœtida. Dr. Falconer, an excellent botanist, after examining the original specimens, considers the plant he saw in Astore to be the same as that figured by Kæmpfer; and Dr. G. Grant, who saw the plant at Syghan, says, as stated by Dr. Christison, that its roots, leaves, and flowering stem correspond on the whole with Kæmpfer's description, except that the root is deeply divided, like the outspread hand. The E. P. assign *ferula persica* as probably yielding some assafœtida. There is no doubt that its seed has been sent from the North-west of Persia as those of the assafœtida plant: but there is no proof, nor indeed is it probable, that it yields any of the assafœtida of commerce. The gum-resins of these umbelliferæ are too similar to each other, for any but experienced pharmacologists to determine between *inferior* assafœtida and varieties of sagapenum or other gum-resins.

"As Dr. Falconer, the author's friend and successor as superintendent of the East India Company's Botanic Garden at Saharunpore, has had excellent opportunities for examining the assafœtida plant, both in its native sites and as cultivated by himself, he has favoured the author with the following full account of this important plant, which he conceives belongs to a genus allied to, but distinct from ferula."

* * * * * * * * *

" *Narthex*, both in the characters of the flowers and fruit, and in its " pæony-leaved" habit, differs widely from any known species of ferula, and appears to constitute a distinct and well-marked genus.

" In the Dardoh or Dangree language (the Dardohs being the Daradi of Arrian) the plant is called " sip" or " sup." The young shoots of the stem in spring are prized as an excellent and delicate vegetable.

" The species would appear to occur in the greatest abundance in the provinces of Khorassan and Laar in Persia, and thence to extend on the one hand into the plains of Toorkestan on the Oxus north of the Hindoo Khoosh mountains, where it seems to have been met with by Sir Alex. Burnes,* and on the other to stretch across from Beloochistan, through Candahar and other provinces of Affghanistan to the Eastern side of the valley of the Indus, where it stops in Astore, and does not occur in great abundance. The whole of this region, which constitutes the head-quarters of the gum-bearing umbelliferæ, possesses the common character of an excessively dry climate, indicated in Berghaus's hygrometric map in Johnson's Physical Atlas by a belt of white.

" Besides the gum-risen, the fruit of *narthex assafœtida* is imported into India from Persia and Affghanistan, under the name of " anjoodan," being extensively employed by the native physicians in India : " anjoodan" being the epithet applied to the seed of the " heengseh," or " hulteet." by Avicenna, also quoted by Kæmpfer, and used by the Indo-Persian and Arabic writers generally in describing the assafœtida plant. Another umbelliferous fruit is also imported with it, and sold under the name of " dooqoo" (a word evidently connected with the δαυκος of the Greeks,) being recommended as an excellent substitute for " Anjoodan," which it closely resembles in its general appearance. This I found to be the fruit of a species of true ferula ; it is one of the two assafœtida-like fruits mentioned by Dr. Royle as occurring in the bazaars of nothern India.—The species of ferula yielding this fruit may furnish some one of the obscurely-known gum-resins resembling assafœtida produced in Persia.

" I have examined another kind of umbelliferous fruit in the collection of Dr. Royle, labelled as " the seed of the wild Assafœtida plant collected and brought to England by Sir J. Macneill from Persia," which differs widely from the fruit both of narthex and of ferula, and belongs to another tribe of the order." H.F.

" Assafœtida is obtained by making incisions into or taking successive slices off the top of the root, and then collecting the produce, which is then united in masses, and in this state is usually met with in commerce. It is at first rather soft, but becomes hard, of a yellowish or reddish-brown colour. When broken, an irregular, whitish, somewhat shining surface is displayed, which soon becomes red. The mass is composed of various-shaped pieces, some like tears pressed together, and in some parts agglutinated together by darker-coloured gum-resin. Some parts are cellular. By thus becoming red on exposure to the air, and its intolerable alliaceous odour, assafœtida may be readily distinguished. The taste is garlicky, bitter, and acrid. It is best preserved covered by bladder. It is powdered with difficulty, even when become hard; softens by heat, and burns with a clear flame. Assafœtida is composed of Resin 65 parts, volatile oil 3.6, gum 19.44, bassorin 11.66, salts 0.30, (*Pelletier.*)—Brandes obtained less resin. volatile oil 4.6, and 10.5 of various salts and impurities. The oil is at first colourless, but becomes yellowish-brown, has an exceedingly offensive odour, a bitter and acrid taste, and contains some sulphur. Water will dissolve the gum, and form an emulsion with the other ingredients. Alcohol or rectified spirit is a good solvent, but an emulsion is formed when the solution is added to water. Ether dissolves the oil and all the resin, except about 2 per cent. of a peculiar kind. Ammonia also takes up the active ingredients.

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* Burnes mentions the plant as an annual, probably in consequence of the annual decay of the stems. He states that sheep browse on the young shoots.

Action. Uses. Stimulant, antispasmodic; thought to be emmenagogue and anthelmintic. Much used as a condiment in the East. Useful in spasmodic and convulsive diseases, as hysteria and chorea, also in hooping-cough, flatulent colic, and in chronic-cough."

CANNABIS SATIVA and its variety *C. indica*. The Leaves and Resin of Hemp.

The hemp appears to be a plant of the Persian region, where it is subjected to great cold in winter, and to considerable heat in summer. It has thus been able to travel on one hand into Europe, and on the other into India; so that the varieties produced by climate have by some been thought to be distinct species, the European being called *C. sativa*, and the Indian *C. indica*. The name κανναβις, by which it was known to the Greeks, seems to be derived from the Arabic *kinnub*, the *canape* of the middle ages, Dutch *kinnup* and *hinnup*, German *hanf*, whence the English *hemp*. Herodotus mentions it as Scythian. Bieberstein met with it in Tauria and the Caucasian region. It is well known in Bokhara, Persia, and abundant in the Himalayas. It seems to have been employed as an intoxicating substance in Asia and Egyyt from very early times, and even in medicine in Europe in former times, as we find it noticed in Dale (*Pharmacologia*, i. 133) and Murray (*Apparat. Medicamnium*, iv. p. 608—620,) where it is arranged, as in this work, next to the Humulus. It has of late years been brought into European notice by Dr. O'Shaughnessy.

"The Indian plant has by some been thought to be a species distinct from the European one; but, like Dr. Roxburgh and others, the author was unable when in India to observe any difference between the plant of the plains and that of the hills of India, nor between these and the European plant. The Indian secretes a much larger proportion of resin than is observable in the European plant, but a difference is observed in this point in India between plants grown in the plains, and those of the mountains, and also when grown thickly together. The natives plant them wide apart, to enable them to secrete their full powers. In Europe, the thick sowing, and moister, often dull, climate will prevent the due secretion of the peculiar principles of a plant of the Persian region. But the plants grown in the past season, from the great heat and light, ought to be more resinous than usual. It is not without interest to observe that both the hop and hemp, belonging to the group *Cannabineæ*, owe their properties to glandular resinous secretions. The author, in calling attention to the uses of this plant, in his *Illust. of Himalayan Botany*, stated that "the leaves are sometimes smoked in India, and occasionally added to tobacco, but are chiefly employed for making *bhang* and *subzee*, of which the intoxicating powers are so well known. But a peculiar substance is yielded by the plants on the hills, in the form of a glandular secretion, which is collected by the natives pressing the upper part of the young plant between the palms of their hands, and then scraping off the secretion which adheres. This is well known in India by the name of *cherrus*, and is considered more intoxicating than any other preparation of the plant; which is so highly esteemed by many Asiatics, and serves them both for wine and opium: it has in consequence a variety of names applied to it in Arabic, some of which were translated to me as "grass of faqueers," "leaf of delusion," "increaser of pleasure," "exciter of desire," "cementer of friendship," &c. Linnæus was well acquainted with its "vis narcotica, phantastica, dementens" (anodyna et repellens.) It is as likely as any other to have been the *Nepenthes* of Homer. (*l. c.* p. 334.)*

"Dr. O'Shaughnessy has described in detail the different preparations, as—

1. *Churrus*, the concreted resinous exudation from the leaves, slender stems, and flowers. This is collected in various ways; that of the Himalayas much

* Dr. O'S. states that "no information as to the medicinal effects of hemp exists in the standard writers on Materia Medica to which we have access." It is only in the later writers that it is omitted. Linnæus was acquainted with them, as the author quoted in the above briefly, as being a botanical work.

esteemed, that of Herat and of Yarkund still more so. For a specimen of the last the author is indebted to Dr. Falconer.

2. *Ganjah.* Dr. O'S. describes it to be the dried hemp plant which has flowered, and from which the resin has not been removed. The bundles are about two feet long, and contain twenty-four plants. In N. W. India the name *Ganjah* is applied to the whole growing plant.

3. *Bang*, *Subjee*, or *Sidhee*, is formed of the larger leaves and capsules without the stalks.

"The leaves of common hemp have been analyzed, but the analysis requires to be repeated and carefully compared with that of the Indian plant. The properties seem to depend on a volatile oil, which is as yet but little known, and upon the resin. This is very soluble in alcohol and ether, as well as in the fixed and volatile oils, partially soluble in alkaline, insoluble in acid solutions; when pure, of a blackish-gray colour. (The Yarkund specimen is of a dark blackish-green, another kind is of a dirty olive.) Its odour is fragrant and narcotic; taste slightly warm, bitterish, and acrid. The *Ganjah*, which is sold for smoking chiefly, yields to alcohol 20 per cent. of resinous extract, composed of *churrus* and chlorophylle. Dr. Farre found that already a substitute (*Apocynum cannabinum*, called Indian Hemp in America) is sold for this, though having no resemblance to it, and possessing only emetic and cathartic properties.

"*Action. Uses.* All these preparations are capable of producing intoxication, whether the *churrus* be taken in the form of a pill, or with conserve, or the dried leaf be rubbed up in milk and water with a little sugar and spice, or smoked. As a medicine, it was tried by Dr O'S. in rheumatism, hydrophobia, cholera, and tetanus. In the last such marked benefit and cures were produced, that the hemp was pronounced an anticonvulsive remedy of the greatest value. Its general effects are, alleviation of pain (generally.) remarkable increase of appetite, unequivocal aphrodisia, and great mental cheerfullness. Its more violent effects were, delirium of a peculiar kind, and a cataleptic state. Dr. Pereira was among the first to submit it to experiment, but failed in obtaining any results, probably from changes having taken place in the drug. Dr. Laurie pronounced it uncertain, and not to be trusted to as a narcotic. Mr. Ley, however, found it useful in relaxing spasm, producing sleep, and during its action abatement of pain. Mr. Donovan found its power great in temporarily destroying sensation, and subduing the most intense neuralgic pain. Professor Miller of Edinburg considers its virtue to consist in a power of controlling inordinate muscular spasm. Dr. Clendinning says that in his hands its exhibition has been followed by manifest effects as a soporific or hypnotic in conciliating sleep, as an anodyne in lulling irritation, as an antispasmodic in checking cough and cramp, and as a nervous stimulant in removing languor and anxiety. The hemp may be used in the following preparations and doses, but Dr. O'S., when in England, found that he was obliged to give as much as 10 or 12 grs. and even more; though in India he considered gr. ½ a sufficient, and 1½ gr. of the extract a large dose.

"EXTRACTUM CANNABIS. Resinous Extract of Indian Hemp.

Prep. Boil the rich adhesive tops of the dried *Ganjah* in *Rectified Spirit* until all the Resin is dissolved out. Distil off the Spirit with a gentle heat.

"*D.* This extract is effectual in gr. ss. and gr. j. doses; but 10 and 20 grs. have been given in Hydrophobia and Tetanus.

"TINCTURA CANNABIS. Resinous Tincture of Indian Hemp.

Prep. Dissolve *Extract Cannabis* gr. iij. in *Proof Spirit* f ʒ j. A weaker Tincture may also be made with the dried herb or Ganjah.

"*D. mx.*—f ʒ j. with the dried herb or *Ganjah.* A drachm or so may be given in tetanus every half-hour, until the paroxysms cease, or catalepsy is induced.

"Mr. Donovan states the only preparation to be relied on is the tincture of the resin prepared from properly collected hemp. He advises of the resinous tincture *m*xv. to be added to rectified spirit *m*xlv. and taken as a draught; or, if added to water, it should instantly be swallowed, or the resin would precipitate and adhere to the vessel."

W. M. C.

III.—*A Treatise on the Diseases of the Eye.* By W. Lawrence, F. R. S., Surgeon Extraordinary to the Queen, Surgeon to St. Bartholomew's Hospital, and Lecturer on Surgery at that Hospital, etc. etc. A new edition, edited with numerous additions, and one hundred and seventy-six illustrations. By Isaac Hays, M. D., Surgeon to Will's Hospital, &c. &c. Philadelphia. Lea & Blanchard. 1847. pp. 858.

This work originally embodied the lectures on the anatomy, physiology and diseases of the eye, delivered by Mr. Lawrence, at the London Opthalmic Infirmary. In the various editions through which this book has passed, many and important additions have been added to the text; and now it may be said to represent the present advanced state of opthalmic medicine, both in a pathological and a therapeutical point. For a long time "Lawrence on the Eye" took precedence of almost all other works on the same subject; nor has time detracted from its merits or weakened its high claims as a standard work in the profession. Classic in the arrangement of his subject; neat, clear and engaging in his style, Mr. Lawrence is one of the best authors on opthalmology in the English language.

The first part of this work is devoted to a review of the *anatomy* and *physiology* of the eye, and its appendages; it contains every thing the student and practitioner can desire, and adds greatly to the value of the book. These two subjects being fully developed, Mr. Lawrence enters at some length into the *pathology* of the eye—*inflammation* and the *classification of opthalmic diseases.*

His views on inflammation, are at once sound and orthodox—being founded upon the latest researches made on this long disputed, but now pretty well settled question. These we need not enumerate in this place, as they are already familiar to the most desultory reader.

In his treatment of all the inflammatory affections of the eye, he is an uncompromising advocate for free depletion; in some cases, pushing general depletion even to syncope. He is opposed to the French and German opthalmologists, who advocate small and repeated bleedings, arguing in its justification, that the opthalmia is more speedily and safely cured by this means, than by one or two decisive bleedings. As a general rule, not without, however, numerous exceptions, we are in favour of Mr. Lawrence's views, being governed much by the intensity of the local affection, and the state of the vascular system.

The plates and illustrations in the first part of the book, are superior to any thing of the kind we have seen; and they will enable the reader to understand the subject at a glance.

It is superfluous to attempt to review in detail, a book so well and so long known by the profession. His views and mode of treatment, have long influenced the profession in this country, in the management of the diseases of the eye.

The editor, Dr. Hayes, has, unlike most editors of foreign books, added much valuable matter to the original text; besides introducing and discussing in a very satisfactory manner, some points overlooked or neglected by the author.

Dr. Hays has enjoyed fine opportunities for the study of the eye diseases and from the materials which he has scattered through the pages of this work, we are inclined to think him a close observer and a sound practitioner. Both the author and the editor are opposed to the application of cataplasms and poultices, so common with the Germans, to inflamed eyes; this practice we believe is almost universally condemned by the profession in this country.

Mr. Lawrence, unlike a new oculist, but like a sound physician looks to the constitution of the patient, as influencing the course of the eye disease, and as of the first importance in the treatment of this class of diseases. Many of the affections of the eye partake of the constitutional taint or disease, with which the subject may be afflicted at the time of the opthalmic attack. Thus we have rheumatic, syphilitic and scrofulous opthalmia—and the remedies required to rid the constitution of the disease will likewise relieve the local affection.

Indeed the eye is an epitome of the anatomy of the tissues; it contains the basis of all the different structures of the body. The conjunctiva may be said to be continuous with the mucous structures; the sclerotica with the fibrous, and so on with the other structures of this organ. Hence the necessity of a familiar acquaintance with all the various structures, as well of the entire system as of the eye itself, in order to understand the pathology and treatment of this class of affections.

To give an idea of the contents of this valuable work, we shall sum up the different chapters under which Mr. Lawrence has arranged and classified his subject. After treating *first*, of the pathology of the eye, *second*, he gives an account of the affections of the eyelids; *third*, of injuries of the eye-ball; *fourth*, of the causes of opthalmic inflammation; *fifth*, of the treatment of opthalmic inflammation; *sixth*, of the division and classification of opthalmic inflammation—simple and catarrhal inflammation of the conjunctiva; *seventh*, of purulent opthalmia of new-born infants; *eighth*, of purulent opthalmia in the adult; *ninth*, of gonorhœal opthalmia; *tenth*, of erysipelatous opthalmia, pustular opthalmia, strumous or scrofulous opthalmia; *eleventh*, of variolous, morbillous and scarlatinous opthalmia; *twelfth*, of various affections of the conjunctiva; *thirteenth*, of the diseases of the sclerotica; *fourteenth*, of the diseases of the cornea; *fifteenth*, of the diseases of the aqueous membrane and chambers; *sixteenth*, of the diseases of the iris, under the head, iritis and malformations of the iris; *eighteenth*, formation of artifical pupil; *nineteenth*, affections of the choroid-coat retina, vitreous humor, lens and capsule; *twentieth*, amaurosis and other defects of sight; *twenty-first*, cataract; *twenty-second*, general affections of the globe; *twenty-third*, malignant disease of the eye; *twenty-fourth*, ossifications, calculous concretions, entozoa in the eye; *twenty-fifth*, affections of the orbit; *twenty-sixth*, lastly, affections of lachrymal organs.

Such are the more prominent topics brought to view, and examined in the work before us. From the above, it will be seen that few points of interest, relative to opthalmology, have been neglected in this comprehensive volume.

There is one part of treatment, in nearly all the acute affections of the eye, advised by Mr. Lawrence, to which we most respectfully put in our demurer; we allude to his purgative plan of treatment. In every variety, almost, of opthalmia, whether strumous or otherwise, he is an advocate for free and copious purgatives. Perhaps in beef-eating and beer-drinking England, where the bowels are likely to become loaded and the blood vessels turgid, purgatives may be a *sine qua non*, but in this country, and especially in this latitude, harsh and drastic purgatives, are not only not called for, but they are positively hurtful—first, because they irritate to a high degree, the gastro-enteric mucous membrane, and thus kindle up an irritative fever, which reacts upon, and aggravates the original affection; and second, they debilitate the constitution, without reducing in a corresponding ratio, the primary local inflammation. Mr. Lawrence says, that in *scrofulous opthalmia*, the use of purgatives is generally necessary; and those of an active kind are often required, even in young children. Such a course of practice, would not be likely to be followed by good results in New Orleans, according to our observation. Most generally the mildest aperients, followed by an alterative and tonic course of constitutional treatment, answers the just expectations of the physician, and crowns his efforts with complete success.

In conclusion, we cordially recommend this excellent work, both to the general practitioner and the oculist. A. H.

IV.—*New Elements of Operative Surgery.* By Alf. A. L. M. Velpeau, Professor of Surgical Clinique of Medicine, of Paris; Surgeon to La Charité, etc., etc., with a Treatise on Minor Surgery, illustrated by over 200 engravings, incorporated with the text: with an atlas in quarto of twenty-two plates, representing the principal operative processes and surgical instruments. First American, from last Paris edition. Translated by P. S. Townsend, M. D. Augmented by the addition of several hundred pages of new matter, comprising all the late improvements and discoveries in surgery in America and Europe, up to the present time: all under the supervision of, and with notes and observations by Valentine Mott, M. D., Professor, etc., etc., etc. In three volumes. Vol. iii. New York. Samuel J. and William Wood. 1847. pp. 1162.

The reader may form some idea of the matter contained in this, the third volume of Velpeau's Operative Surgery, after glancing over the title page of the book. It is certainly equal to what it claims to be; being undoubtedly the most voluminous work that ever appeared in the English language on surgery, in this country. Dr. Townsend, the translator, certainly bears off the palmn for industry and perseverance in bringing out this work in a neat English dress. Of the reputation of the author, M. Velpeau, as a surgeon, we need not speak, as all who know anything of the history and improvements of surgery, are quite familiar with his labours, and the fruits of his extraordinary powers. As a book of reference, it is a small library in itself, and invaluable to the practitioner; yet we cannot say that we admire the arrangement of the work as a whole; the subjects are too much crowded together; the mind is confused with the importance and the variety of the matter brought before it; hence, the impressions made, are seldom permanent or distinct. Perhaps, in a work of this size, any other arrangement could not have been adopted, at all events, without repetition or greater labour. Let us, therefore, receive it as it is, without murmur or complaint, for in truth it stands unrivalled, both for the value and amount of matter it contains.

The book is much disfigured, in our opinion, by the constant reference made in the *body of the work*, to the medical periodicals and works of the day. This all serves to balk the reader, to interrupt the sense of the paragraph and to distract the understanding. The references, which are so numerous, should have been placed at the foot of each page, in small type, both as most convenient, and less likely to annoy the student. If these can be considered objections at all, they may be regarded as too trifling to be urged against a work which contains such a large amount of information. In the title page, it is stated that several hundred pages of entirely new matter have been added to the original work, by Drs. Mott and Townsend. We regret that such an amount of valuable *material* should have been scattered through a work, already too large for convenience, destined, we fear, to attract little or no attention, on account of such an amalgamation.

Why did not Drs. Mott and Townsend give us a work on *American* surgery? The material is abundant, the time opportune, and they are every way qualified to produce such a work as would reflect credit upon themselves and their country, The matter incorporated into Velpeau's "Operative Surgery," by the American editors, should have been brought out in a separate form, upon its own merits; this course was due to themsélves, and to American surgery. How long shall American genius and talent thus continue to act as trumpeters of the fame of the writers of the old world, play a secondary, a subordinate part in literature and the sciences? Let us strive to garner up the achievements of our own surgeons, the experience and observations of our own physicians, in works, written by ourselves, and divorced from that forced alliance with foreign productions, which degrades us as a profession in the estimation of the medical world.

The atlas, which accompanies this, the third and last volume of Velpeau's Surgery, is a valuable addition to the work. The designs are good, and well executed. Velpeau, in getting up this work, has done ample justice, to both the ancient and modern writers in this department of our science. He has detailed the operative proceedings of different distinguished surgeons, of all countries, and almost every age. This will make the work highly valuable as a book of reference, and for this reason, we recommend it to the profession throughout the country.

A. H.

V.—*Education: its Elementary Principles, Founded on the Nature of Man.* By J. G. Spurzheim, M. D., of the Universities of Vienna and Paris, &c., &c. With an Appendix, by S. R. Wells. Sixth American Edition. New York. Fowlers & Wells. 1847.

In these days, when the attention of statesmen and philosophers is strongly directed to the mental and physical training, and education of the rising generation, in order to the permanent improvement and amelioration of the human race, such a work as this by Dr. Spurzheim, cannot fail to be read with deep interest by every philanthropist.

A few extracts will show what the work is.

"As to the definition of the work, I think it necessary to state that I intend to introduce in this volume several topics which are not generally considered as falling under education in the common acceptation of the word, merely denoting instruction in literature and accomplishments; I use this term as embracing every means which can be made to act upon the vegetative, affective, and intellectual constitution of man, for the purpose of improving this, his threefold nature."

In speaking of the perfectability of man, he does not contend that they can lose one faculty and acquire another; but that all living beings can be, by judicious training, materially improved, the desirable faculties strengthened, and the pernicious ones weakened. After enumerating many instances, in which desirable qualities in plants and animals may be fostered and strengthened, he says:

"Plants and animals succeed only if treated according to their natural qualities, and the education of man, will not and cannot succeed, without adapting it to his nature."

"Children are not pieces of blank paper, on which you may write whatever you please. Every poet is not a Homer, every musician a Handel, a Mozart, or a Haydn; nor every painter a Raphael. A child that might, by proper education, make a Lacitus, will, under other circumstances, be a Dogberry. He then devotes a chapter to Anthiopology; and proves that organization and faculties are transmissible, according to certain laws, from parents to children. He then treats of vegetative functions, and gives directions for physical training. Of mental cultivation he says:—"It is time to abandon the immense error, that words and precepts are sufficient to call internal feelings and intellectual faculties into active exercise. * * * The sight of a person wounded or in danger, makes a greater impression on the mind, than reading that thousands have been killed in a battle. Natural language, in general, has more effect on the feelings than artificial signs; we are, for instance, more likely to smile or laugh on looking at a gay face, than on hearing the word gaiety pronounced. * * * * From the considerations unfolded in the preceding chapters, I draw the conclusions, that education ought to be founded on the nature of man; that the true principles of education ought not to be confounded with school-learning; that great improvements remain to be made even with respect to instruction in the arts and sciences; and that the education of the feelings, which I consider as the most important, and place far above the understanding, will require to be quite newly modelled."

After considering the details of education, he sums up in the following conclusions:

"The great object of education is, not to create, but to prepare, develop, or impede and to direct the natural dispositions—vegetative, affective, and intellectual. The nature of the fundamental powers, and the conditions on which their manifestations depend, must be known, to enable us to cultivate and direct them. The difference between the feelings and intellectual faculties, is particularly to be attended to. Then if the means of excitement and those of direction be employed, as I have detailed them, arts and sciences will improve, moral evil will diminish, and mankind will become more happy. I do not flatter myself, however, that in the present state of mankind, the most perfect education can abolish all disorders. Hence, institutions of another kind are necessary, which I shall speak of in the following pages."

He then in an appendix, treats of legislation in regard to the punishment of malefactors, and shows conclusively that the present manner of treating criminals, instead of answering the end for which it is intended (the prevention of crime, and the reform of criminals,) actually debases, hardens, and confirms the criminal in his vicious propensities.—The criminal comes from the house of correction, instead of a reformed and useful man in society, an outcast, having the mark of Cain on his brow, from whom every man flees and shuts the door against him; so that, even if he would, he cannot be an honest man. "These things ought not so to be." In this respect his veiws are not new; for men celebrated for their strong intellect, and philanthropic exertions, have long endeavored to reform the present mode of treating criminals. We believe the time is not far distant, when there will be as great an improvement, in the treatment, and punishment of criminals, as has lately been made in the treatment of insanity. Hear Dr. Sperzheim.

"The considerations, examined in the appendix of this work, tend to show that legislation in every branch ought to have only one aim, viz. the general happiness of mankind, and that of each individual, as far as it is compatible with the former, that penal legislation in particular, ought to be corrective; that in prisons, the inhabitants of which are sent back into society, all possible means of correction should be employed: that capital punishment might be abolished, and the crimes for which it is inflicted prevented by proper establishments. As punishment, however, is still the object of the penal code, I have treated of the different degrees of guilt which may be implied in criminal actions, and of some illegal actions that admit of extending motives, such as suicide and infanticide. From this appendix too, it may be inferred, how important and necessary, for legislators and judges, is the study of man."

There is also an appendix to the American edition, by S. R. Wells, containing a description of the temperaments, and a brief analysis of the phrenological faculties. Take the book all in all, it is just what we might expect from the known mental abilities, and enlarged veiws, of its scientific and benevolent author, and the wants of the present state of society. We most cordially recommend it to legislators, philanthropists, teachers, and parents who have children whom they desire to make a blessing to society, and an honor to themselves.

S.

VI.—1. *Summary of the Transactions of the College of Physicians of Philadelphia. From December*, 1846, *to April*, 1847.

2. *Transactions of the Medical Society of the State of New York.* Vol. vii, Part I.

We return thanks for these interesting documents, and request a continuation of the favors. In a brief report on the now celebrated *Letheon*, made by Dr. Parrish to the College of Physicians, we find the following closing remarks, which we think so just, and in which we so fully coincide, that we must make room for them.

"From the above narration, it will be perceived, that a powerful agent for producing insensibility to pain, is now fairly brought to the test of experiment, and if the favorable accounts received, through the high authority which we have quoted, should be confirmed by subsequent experience, and if no serious effects should be found to follow its application, it may justly be considered as an important medical discovery.

"It is not intended of course by these remarks to give the impression that by the evidence furnished of the effects of this article, its true value is, as yet, sufficiently decided, to establish any conclusion in regard to it. This can only be tested by repeated and well directed experiments, made by those who possess a knowledge of the human system, which will enable them to judge of its precise effects, and to these we must look for the decision of its merits.

"Before leaving the subject, we would venture a remark or two, in reference to the manner in which this new agent has been brought before the public.

"Its composition is kept secret, though from its sensible properties, there is little doubt that it is an etherial solution of some narcotic substance. Dr. Bigelow instituted a number of experiments to test the effects of the inhalation of sulphuric ether and oil of wine, neither of which articles possess the peculiar properties of this preparation, though the oil of wine produced effects quite analogous to it; with this, patients were tranquillized and lost all inclination to speak or move, but their consciousness continued.

"That an agent, which is said to possess such useful qualities should be patented, and that the patent should bear the name of an intelligent physician, and a man of science, must be a source of extreme regret to every liberal member of our profession. We understand that both the patentee and the proprietor, hold a diploma from a medical college; and we observe further, that the course of these gentlemen in making merchandize of this discovery, if not openly justified, is excused by the Boston Medical Journal. We trust that these facts are not to be taken as an expression of the sentiments of the medical community of Boston.

"If the agent referred to, really possesses the power claimed for it, if its character as a certain and safe means of assuaging human suffering should become established, how different would be the position of its discoverer from that held by the true promoters of science. Whatever plea may be set up at this late period, for the restrictive policy of securing patent rights for discoveries which tend to relieve the sufferings of mankind, or to extend the boundaries of science, the sordid motive of self-aggrandizement and individual interest in these, as in inventions relating to manufactures and the mere mechanic arts, will undoubtedly be attributed to those who avail themselves of this policy. It is to secure individual rights against the public appropriation of an improvement that the law grants the patent, vesting exclusive ownership in the inventor, with the right to dispose of his invention for his own profit. To us it would appear much more candid and manly for those who avail themselves of this privilege and use it to this end, openly to avow it and not attempt to excuse their course by pretending great solicitude for the public welfare; the fear, if their secret is made known, that unworthy and ignorant people may use it to the detriment of others, when it is well known that many of the most useful and indispensable substances in daily use, are, if properly employed, exceedingly dangerous and destructive.

"How far the eminent physicians who have given this article their sanction, without knowing its contents, are justified in this departure from the rule, which has generally regulated the conduct of the most honorable and conscientious members of our profession in such cases, it is not for us to determine.—In the present instance, they may have deemed the circumstances of such a character as to warrant them in adopting this course; but we sincerely hope that no precedent will be thereby established which shall tend to lower the standard of physicians on this point. There is a principle involved in this question of patents, which is vital to the existence of the medical profession as a liberal, humane and scientific art. If that be given up, and the doctrine be substituted, that the physician has a right to appropriate to his own pecuniary benefit any useful medical discovery which he may make, the barrier that now separates us from the charlatan is broken down, and the high position which our profession has long held as a humane and dignified calling is lost.

"We hope, therefore, that whatever may be the results attained by this new process, that medical men will be especially careful to avoid any compromise of principle in regard to the position in which it stands before the public, and that the whole medical community will, with one voice, enter their protest against keeping secret any process or preparation, a knowledge of which would extend our means for the relief of human suffering."

In the same transactions we find an exceedingly interesting report, by Dr. Moore, on the Meteorology and Epidemics of 1846. Also a valuable report, by Dr. Condie, on the diseases of children; amongst which he gives a full account of the cerebro-spinal affection described by Drs. Hicks and White in our first part, and in our Foreign quotations.

The New York Transactions open with a very able annual address, *on mental manifestation in health and disease*, by Dr. John McCall, President of the Society. The author seems to be deeply impressed with the truths of phrenology, and we confess our entire concurrence with him.

This is followed by an interesting paper *on the Resources of the Medical Profession*, by Dr. Joseph Bates; in which the author castigates the different forms of modern empyricism with commendable severity. It would be well if this paper were widely disseminated amongst the reading community. E. D. F.

VII.—*The Students' Vade Mecum, or Manual of Examinations upon Anatomy, Physiology, Chemistry, Materia Medica, Surgery, Obstetrics, Practice of Medicine, (including Physical Diagnosis and Diseases of the Skin,) and Poisons.* Second edition, revised and greatly enlarged. By George Mendenhall, M. D., Lecturer on Pathology, in the Medical Institute of Cincinnati, Member of the Philadelphia Medical Society, &c., &c. Philadelphia. Lindsay & Blakiston. 1847. pp. 575.

This Vade Mecum seems to be a very neat little work, and quite popular with students and young practitioners, for whom it was written, as the first edition is already exhausted. The second seems much superior to the first edition, since the author tells us he has brought his work up to the present advanced state of the profession.

We again echo the sentiment which we have advanced on several occasions, that we are no advocates for "manuals," "vade mecums," and the like; in justice, however, to Dr. Mendenhall, we think his equal to the best of the kind, and displays commendable enterprise and industry. To those who can content themselves with a smattering of medical knowledge, and who prefer reading by the noon-day sun to studying by the midnight lamp, we can honestly recommend the "Vade Mecum."

VIII.—*A System of Human Anatomy, General and Special.* By ERASMUS WILSON, M. D., Lecturer on Anatomy, London. Third American from the third London edition. Edited by PAUL B. GODDARD, A. M., M. D., Professor of Anatomy, etc., in the Franklin Medical College of Philadelphia, with 233 illustrations, by GILBERT. Philadelphia. Lea & Blanchard. 1847.

The rapid progress of this work through successive editions, shows the high estimate placed on it by the profession. We are assured by the American editor, that the present edition is a careful and exact reprint of the English work, with the addition of such other illustrations, as were deemed necessary to a more complete elucidation of the text, &c.

We need only remark that the work is gotten up in superior style, and must continue to be among the most popular of the day. The publishers will accept our thanks for the copy sent us through the hands of Mr. J. B. Steel.

Part Third.

EXCERPTA.

1.—*On the Mutual Relations existing between Physiology and Pathology, Chemistry and Physics, and the Methods of Research pursued in these sciences.* By Baron Liebig.

(*Concluded from our May No.*)

RELATIONS BETWEEN THE VARIOUS PROPERTIES OF BODIES.—CONNEXION OF VITAL PROPERTIES WITH CHEMICAL.—NECESSITY FOR ANATOMY; ITS INSUFFICIENCY.—CHEMISTRY NECESSARY, BUT INSUFFICIENT.—ILLUSTRATIONS.—VALUE OF CHEMICAL FORMULÆ.—CONCLUSION.

The Law of the Relations of Mutual Dependence existing between two phenomena independently of the causes producing these phenomena.

The examples adduced in the preceding paper clearly demonstrate the existence of a natural law, and show that the properties of a body stand in a definite relation to its composition, and that an alteration in one of the properties of a body is attended with a corresponding alteration in some one of its quantitative relations. What deserves particular notice here is, that the knowledge of this natural law, of the mutual relation between the boiling point of substances and their composition, is altogether independent of the actual cause, or of the conditions, to the joint operation of which the constancy of the respective boiling point of substances is to be ascribed, since the real nature of the boiling point is as much unknown to us as the real nature of life.

The Mutual Dependence of Composition, Specific Gravity, and the Boiling Point.

Every property of bodies stands in some similar relation to their composition, as the boiling point of the foregoing example. A law has been made out for a considerable number of organic bodies, which enables us, from a knowledge of the boiling point, to deduce the weight of a cubic foot of any of these substances; consequently, it appears that the specific gravity of bodies—that is, the pressure exerted by equal volumes on a substratum supporting them, stands in a definite relation to, and varies with variations of, two other properties—namely, their boiling point and their composition.

Relation between Specific Gravity and Atomic Weight.

A similar relation of dependence has been discovered to exist between the respective amounts of heat required to raise the temperature of bodies to the same point, and the relative weights in which they enter into combination, (equivalents.) It is a well known fact, that different bodies at the same temperature contain different quantities of heat. Equal weights of sulphur, iron, and lead, for example, heated to 212°, when placed in contact with ice, melt widely different quantities of the latter. Now, if the amount of heat in these bodies were equal, it is obvious the amount of ice liquefied would in every case be equal. The differences in this respect manifestly indicate differences in the cause of the liquefaction. The sulphur liquefies six and a half times, the iron four times, as much ice as the lead. If we heat equal weights of sulphur, iron, and lead, to the same extent—say from 60° to 400°—with the same spirit-lamp, it is obvious, that if for a given weight of lead half an ounce of spirits, be necessary, then, for the same weight of sulphur, three and a quarter, and for iron, two ounces, would be consumed. These different amounts of heat required to heat to the same temperature various bodies, are on this account termed specific heats. From a knowledge of the unequal quantities of heat which equal weights of different bodies contain, at the same temperature, we may, by rule of three, calculate the weights of sulphur, iron, and lead, which contain an equal amount of specific heat. From this calculation, it appears that 16 parts of sulphur liquefies as much ice as do 28 parts of iron, or 104 parts of lead, at the same temperature. These numbers correspond exactly to the equivalents of sulphur, iron, and lead. So that the equivalents of these and other bodies coincide with like quantities of heat; in other words, require like quantities to raise them to the same temperature. If we consider that the equivalents of bodies represent the relative weights of the atoms, it is evident that the quantity of heat absorbed or yielded up by every single atom, under like conditions, is the same for every atom, and expressed in figures, is inversely proportionate to the weights of the atoms. It is certainly a curious fact, that the quantity of ice which a body melts should have served to determine and correct in many instances, the combining proportion of that body.

Relation between the Specific Heat and Musical Tone of Gases.

It may appear still more wonderful to many persons, that this property (to absorb or to yield heat) stands, in gaseous bodies, in a definite relation to the sound produced by the transmission of the gas through a pipe or flute; indeed, a celebrated philosopher (Dulong) computed, from the difference in the sound thus produced, the respective quantity of heat evolved by gases upon compression, or absorbed in their expansion. In order to understand clearly this remarkable relation between the specific heat of gases and their power of propagating sound, I may refer to an idea occurring to La Place, which is one of his most beautiful and happy thoughts. It is well known that Newton, and many other mathematicians after him, endeavoured in vain to deduce a formula for the velocity of sound that should exactly correspond with the results of actual observation; they succeeded, indeed, to deduce approximate formulæ, but none that corresponded exactly; there remained invariably an inexplicable difference. Now, as the propagation of sound is effected by the vibrations of the elastic molecules of the air—consequently, by the compression and subsequent expansion of the particles; and as heat is liberated upon the compression, and absorbed upon the expansion, of the atmosphere—La Place conjectured that this phenomena of heat ought to exercise a certain influence upon the propagation of sound; and it was in reality found that the formula which the mathematicians had deduced for the velocity of sound corresponds exactly with the results of actual observation, if the specific heat of the air is taken into account.

Now, if we compute the velocity of sound by the Newtonian formula, (i. e., without taking the specific heat of the air into account,) and compare the result with the formula of La Place, we find between the two a difference in the extent of space which a wave of sound is computed to traverse in a second. This difference in the velocity of the propagation of sound arises from the specific heat of the air, or, in other words, from the quantity of heat evolved from the molecules of air compressed during the passage of the sound. Now, it is obvious that this difference in the velocity of the propagation of sound will, in the case of other gases which with equal volume contain and evolve upon pressure more or less heat that the air, be greater or less than it is in the case of the atmosphere, and consequently, that the figures expressive of these different velocities of sound in various gases indicate at the same time the different quantities of heat which these gases respectively contain.

Now, since the acuteness or the gravity of tones depends upon the number of vibrations produced by a wave of sound in a second, and consequently upon the velocity with which the motion imparted is propagated,—and since we know that the velocity of the propagation of a wave of sound is, in all gases, directly proportionate to the number of vibrations of the tones thereby produced by it,—it follows, that from the difference in the height of the tone produced respectively by the transmission of different gases through a pipe, we may determine the respective specific heat of the several gases.

Acoustics owes the rank which it at present occupies amid the sciences to the grand discovery, that musical harmony, that every sound which touches the heart excites us to joy, or inspires us with valour, is the mark and sign of a definite and determinable number of vibrations of the particles of the propagating medium, and thus a sign of everything that may be deduced from this motion according to the laws of undulation. A number of facts concerning tones have been deduced from the theory of undulation; whilst, on the other hand, empirical truths have led to a corresponding knowledge of the properties of vibrating bodies, which properties formerly were entirely unknown.

It is asserted of a celebrated violin-maker of Vienna, that he himself selected the wood for his violins in the forest, making choice of those trees which, under the stroke of a hammer, returned a certain peculiar sound known to himself alone. This, in all probability, is a fable; but there can be no doubt that he knew that the upper and lower boards of a good violin must make a certain number of oscillations in a second, and produce a certain definite sound; and that he ought to be governed by this consideration in the selection of the boards, particularly as to their respective thickness.

Relation between Electricity and Magnetism—Magnetism and Heat—Magnetism and Chemical Force.

If, finally, we take into consideration that the electric current passing through a metallic wire stands in a definite relation to the magnetic properties thereby imparted to the wire—if we recollect that the most minute differences of radiated heat may be measured by the magnetic needle—that the quantity of electricity put in motion is expressible in numbers by means of the same needle—that this quantity may be measured in cubic inches of hydrogen, and weights of metals,—and when we thus see that the causes or forces which govern the properties of bodies, their power to make impressions upon our senses, or to produce effects in general, stand in a definite and determinal relation of dependence to one another,—how can we doubt that the vital properties likewise obey the same laws of mutual dependence, and that the chemical and physical properties of the elements, their form and mode of arrangement, perform a definite and determinable part in the vital phenomena.

Fallacy of looking upon Vital Properties as exceptions to the Laws of Nature.

The false method pursued by many physiologists and pathologists has led them to look upon the vital properties, in some measure, as exceptions to a great law of Nature. How otherwise can we explain their refusal to look upon the number and arrangement of the elements, which constitute the various parts of the organism, as a physiological property affording an indispensable auxillary towards the ultimate attainment of a clear insight into the vital phenomena! How otherwise can we explain that, in the treatment of diseases, they should neglect to take into account the elementary composition of the remedies, and the properties dependent upon that composition, and through which the curative action is exercised? The mere knowledge of the formulæ, of course, does not suffice for this purpose. It is necessary likewise to ascertain the laws of the relations in which the composition and form of the food or of the secretions stand to the process of nutrition, and in which the composition of the remedies stand to the action which they exercise upon the organism.

Anatomy essential to the progress of Physiology.

SCHLEIDEN says, "It is unquestionable that all the advances made in the physiology of plants and animals, from the time of ARISTOTLE up to our own days, have been effected mainly through the progress made in anatomy. The mere sight of the mash-tub, the fire, and the pipe, from which the spirit runs, is not sufficient to afford a clear insight into the process of distillation: this requires absolutely an accurate knowledge of the whole apparatus. Now the organism is a much more complicated apparatus than a still, and before we can attempt to judge of the importance and functions of the several parts composing it, it is absolutely necessary that we should acquire a perfectly accurate knowledge of the structure of every individual part."

But we must not forget here, that from the days of ARISTOTLE to those of LEEUWENHOEK, anatomy has thrown only a partial light upon the laws that govern the vital phenomena; that the mere knowledge of a distillation apparatus does not suffice to inform us of the purpose for which this apparatus is intended, and to instruct us as to its *modus operandi;* and that he who knows the nature of the fire, the laws of the diffusion of heat, the laws of vaporization, the composition of the mash, and that of the product of the distillation, knows infinitely more of distillation, not only than he who simply knows the apparatus in its most minute details, but infinitely more even than he who constructed the apparatus.

Each new discovery in anatomy has added to the precision, accuracy, and extent of the descriptions of the various parts of the organism, and their functions; unwearied investigation and research has penetrated even to the cell; but from this point our researches must proceed upon a new track.

Anatomy alone insufficient.

But if, as many persons seem to think, the further progress of physiology, both for the present and in future, must entirely depend upon the progressive improvements of our knowledge of the anatomical structures of organisms, then, indeed, chemistry can be of no avail to physiology, since it does not contribute to enlarge our anatomical knowledge, having for its object, not the form, but the relations of the form to the elements, and to their mode of arrangement, by which that form is produced. The study of anatomical structures, and of the relations of the various structures constituting the animal organism, serves exclusively to advance the science of anatomy; and the most minute and accurate research into the motory phenomena affords not the slightest clue to the causes and laws which govern these phenomena. It only teaches us the mode and manner in which the motion is directed.

Chemistry the necessary adjunct of Anatomy in the Solution of Physiological Questions.

If, then, anatomy alone will not enable us to solve physiological questions, it is evident that something besides is required for this purpose; and the first thing, in this respect, must surely be the knowledge of the matter of which the form consists, of the forces and properties which it possesses, besides the vital properties, its origin, and the alteration which it undergoes to acquire vital properties. To this knowledge must be added, as equally indispensable, that of the relations in which all the constituent parts of the organism, both fluid and solid, stand to one another, independently of their form. Many physiologists fancy that the discoveries which chemistry has made regarding these highly important questions, have simply served to enrich the domain of chemistry, although, in reality, all these acquisitions occupy, in chemical science, the same subordinate rank as those gained by the analyses of minerals and of mineral waters.

Chemistry alone insufficient to explain the Phenomena of Life.

Another fundamental error entertained by another class of physiologists, is to suppose that the chemical and physical forces are in themselves, or in conjunction with anatomy, sufficient to explain the phenomena of life. One would, indeed, hardly believe, that whilst the chemist, who is most intimately acquainted with the chemical forces, recognizes in the living body the existence of new laws and of new forces unknown to him, the physiologists, who claims but a very defective and superficial knowledge of the nature and action of chemical and physical forces, should attempt to explain the vital phenomena by the laws of inorganic matter alone.

The view which would assign this disproportionate share in the vital phenomena to the chemical and physical forces, may be considered as the extreme result of the reaction against another view that preceded it, and which ascribed all the phenomena of the living organism entirely and exclusively to the vital force. Now, on the contrary extreme, some reject the vital force altogether, supposing that all the vital processes are referable to physical and chemical causes. Forty years ago, it was a favourite notion of physiologists to assume the existence, in the living animal body, of laws different from those which govern inorganic nature, and to assert the most perfect dissimilarity between organic processes and those ocurring in inorganic matter. Many of our modern physiologists, on the other hand, would maintain that there exists the most perfect analogy between these two sets of laws and processes. Both these theories are alike untenable ; they labour alike under one great and fatal defect—viz., the advocates of neither have never endeavoured to establish or determine the differences between the effects of the vital force and those of the inorganic forces, nor their respective similarites or analogies. The deductions and conclusions which were arrived at, were not based upon the knowledge of the points of similarity or dissimilarity which their mutual relations presented, but rather upon a total ignorance of both these points.

True definition of the term " Chemical Forces."

Those physiologists who regard the vital processes as the effects of inorganic forces, forget altogether that the term " chemical forces" implies nothing beyond what is quantitative in the various vital manifestations, and the qualities dependent upon these quantities.

It is entirely owing to the erroneous notions entertained of the influence which chemistry exercises in the vital phenomena, that this influence is, on the one side, greatly underrated, whilst, on the other side, the expectations entertained of, and the demands made upon chemistry, are exaggerated.

Numbers serve simply to express fully established relations of mutual dependence between two or several facts, but not to establish such relations.

When a definite relation has been discovered to exist between two facts, it is by no means the task of chemistry to demonstrate their relation, but simply to express it quantitatively, or in numbers. But these numbers themselves do not constitute or establish any relation between two facts, if such relation does not really exist.

Oil of bitter almonds and benzoic acid are, in origin and properties, two totally different organic compounds. A few years ago the existence of a mutual relation between these two substances was not even thought of. But when it was discovered that oil of bitter almonds becomes solid and crystaline upon exposure to the air, and that the resulting body is identical in properties and composition with benzoic acid, this fact placed the existence of a relation between these two substances at once beyond question. Subsequent observations and examinations showed that oxygen is absorbed from the atmosphere upon the transformation of the bitter almond oil into benzoic acid, and the formulæ deduced from the respective analysis of both substances expressed this transformation in figures, and explained it thus so far as it admitted of explanation.

In a similar manner the study of the alterations which the fousel oil of the potato undergoes by the action of oxygen, led to the discovery of the existence of a definite relation between this compound and valerianic acid; and the numerical formulæ of these two compounds showed that they stand in the same relation to one another as the common spirit of wine stands to acetic acid.

Chemical relation between Urea, Uric Acid, Allantoin, and Oxalic Acid.

The urine of man contains urea, and, in many instances, likewise uric acid. This acid is not found in the urine of certain classes of animals, whilst the urine of other classes, again, contains no urea. The quantity of urea in urine decreases in proportion as that of the uric acid increases. The urine of the fœtus of the cow contains allantoin. Oxalic acid forms almost constantly a constituent of the urine of man. Changes in certain vital processes in the organism are accompanied by corresponding changes in the nature, quantity, and condition of the compounds which are secreted by the kidneys. It is the task of the chemist to express quantitatively, by numerical formulæ, the relation in which these various substances are observed to stand to one another, and to the processes occurring in the organism.

How Chemistry proceeds to express these relations.

Chemistry, in the first place, substitutes for the terms urea, uric acid, allantoin, oxalic acid, &c., numerical formulæ expressive of the respective quantitative composition of these compounds: these formulæ, however, do not yet establish any mutual relation between these several substances; but my investigating the deportment and properties of these compounds, and the alterations which they undergo under the influence of oxygen and of water,—consequently, of those substances which perform a part in their formation or alteration within the organism,—chemistry arrives finally at terms which establish a definite and unmistakeable connexion and mutual relation between these four compounds. Upon addition of oxygen, uric acid separates into three products —viz., allantoin, urea, and oxalic acid. A larger supply of oxygen converts uric acid into urea and carbonic acid; allantoin presents the composition of urate of urea. The comparison of the conditions under which uric acid is found, in chemical experiments, to change into urea with those that attend this process in the organism, leads to the conclusion, either that these conditions (in the case before us, supply of oxygen) are the same in both cases, or that they differ from each other. These differences furnish new startling points for further investigation, which finally leads to the elucidation of the process in the organism.

Urea and uric acid are products of the alterations which the nitrogenous constituents of the blood undergo under the influence of water and of oxygen.—The nitrogenous constituents of the blood are identical in composition with the nitrogenous constituents of the food. The relations between the latter and uric acid,—between urea, and the oxygen of the air, and the elements of water,—the quantitative condition of the formation of urea, &c., chemistry expresses in formulæ, and explains and elucidates them thereby as far as its own province extends.

Functions of the Chemical Formulæ.

It must be evident, even to those not conversant with chemistry, that the difference in the respective properties of two bodies containing the same elements, is dependent either upon a different mode of arrangement of the elements of which they respectively consist, or upon a quantitative difference in the respective compositions of the two bodies. Now, the formulæ of the chemist are intended to express this difference in the mode of molecular arrangements, or the quantitative differences which accompany the qualitative ones. Chemistry at present is unable, notwithstanding the most careful analysis, to establish with positive certainty the composition of an organic body, so long as the quantitative relation remains undetermined, in which this body stands to another substance, of which the formula has been clearly and positively established.

It was in this way alone that chemistry succeeded in establishing the formulæ of fousel oil, and oil of bitter almonds, for instance; and in cases where direct observation fails to discover relations of mutual dependence, the chemist is obliged to create such relations by his art. For this purpose, he endeavours to resolve the body under examination into two or several products: he examines the products, which he thus derives by the action of oxygen, or of chlorine, of alkalies and acids, upon the substance of which he is desirous to fix the formulæ; and it is by these means that he succeeds finally in obtaining one or several products, of which the composition, and consequently the formulæ, are clearly and distinctly known. From the formulæ of these products he finally deduces the formula which he is in quest of, and derives thus his knowledge of the whole from that of one, several, or all the parts constituting that whole.—Thus, for instance, analysis leave us altogether in the dark regarding the number of equivalents of carbon, hydrogen, and oxygen that constitute the sugar atom; the skill of the chemist fails to furnish him with a positive proof, demonstrative of the correctness of his analysis of salicin, or of amygdaline; but sugar combines with oxide of lead, and is, by fermentation, resolved into carbonic acid and alcohol, into two compounds, consequently, of which the formulæ are perfectly and distinctly known; amygdaline is resolved into hydrocyanic acid, oil of bitter almonds, and sugar: salicin into sugar and saligenine.

Use and Import of Chemical Formulæ.

It is clearly evident, that from the known weight of a substance, and the known weights and formulæ of one or two, or all of the products derived from it, the number and the relative proportions of one or two, or all of its elements, in other words, its own formula, may be deduced, and thus the results of the analysis may be verified or corrected.

Reason why the Chemist studies the Products resulting from the Decomposition of Bodies.

The preceding passages show clearly what is the real use and import of chemical formulæ. The correct formula of a substance expresses the quantitative relations in which that substance stands to one, two, or several other bodies. The formula of sugar expresses the sum total of the elements of sugar, which combine with an equivalent of oxide of lead, or the quantity of carbonic acid and of alcohol into which sugar is resolved upon fermentation. It will be readily understood now why the chemist should be so frequently obliged to resolve into numerous products the substance of which he is desirous to fix the composition, and why he should be obliged to study the various compounds which the substance under examination forms with other bodies. Every one of the results arrived at by such investigations and experiments of the chemist serves as a control for the correctness of his analysis. No formula, deserves implicit confidence, unless the body, the composition of which it is intended to express, has been subjected to these operations.

Misapplication of Formulæ to purposes for which they are not intended.

Some modern physiologists, forgetting that the knowledge of the relations existing between two phenomena ought to precede the numerican expressions, have turned the formulæ of the chemist into a sort of unmeaning play of numbers; instead of applying them to their legitimate purpose—viz., to express actually existing relations of mutual dependence between two phenomena, &c.,—these physiologists would endeavour to establish, by these means, relations which do not exist in nature, or, at all events, have never been observed to exist.*

* Thus VALENTIN says, at page 174, vol, i., of his "Manual of Physiology;"—"Microscopical anatomy shows that there exists in the brain and spinal marrow a

Hopes.

But the time will come, although the present generation will hardly live to see it, when numerical terms in chemical formulæ shall have been determined for all the normal functions, processes, and forms of the organism; when the variations and deviations in the functions of every individual part of the organism shall be measured by corresponding variations in the composition of the matter of which these parts respectively consist, or of the products derived from this matter; when the effects produced by morbific causes, or by remedies, should be quantitatively determined; and when a better and more rational method than obtains at present in physiology shall lead to the knowledge of all the conditions of the vital phenomena, and introduce perspicuity and precision into the explanations and elucidations of these phenomena. Posterity will then hardly believe that there was a time when the share that chemistry is intended to take in these acquisitions was contested, when men of science could remain doubtful and uncertain regarding the mode in which chemistry might assist them in the investigation and comprehension of vital phenomena.

2.—*Generation and Development.*

Ovum of Mammalia, and the several changes which it undergoes.—Bischoff has recently published a new work containing the results of his researches on the development of the ovum in the bitch.* The chief conclusions at which he has arrived may, on account of their relation to the development of the mammiferous ovum generally, be briefly stated here. Much of the matter which they contain, however, has been already made known by the author in his previous works, as well as by the researches of others, and may be found fully discussed in the elaborate report on the "Ovum of Man and the Mammifera," by Mr. Wharton Jones.† 1. The unimpregnated ovum of the bitch whilst in the ovary, consists, like that of all mammalia, and indeed of all other animals, of a vitellary membrane, termed zona pellucida,‡ of a vitellus or yelk, of a germinal vesicle, and a germinal spot. The average size of the ovum is 1-15 of a line; that of

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mixture of grey and white substances, and that albumen and oil are associated together in these organs. Instead of acting upon this knowledge of the anatomist, the chemists analyzed this mixture in unknown proportions of albumen and fat, as consisting wholly of fat; this analysis led them to the discovery of a peculiar nitrogenous, fatty acid, upon which they bestowed the name of 'Cerebric acid,' endeavouring, at the same time, to support, upon theoretical grounds, the anomaly of the existence of a nitrogenous fat. But by a chemical deduction, based upon MULDER's formula of protein, it may be demonstrated, that the analyzed substance consists simply of what the results of the anatomical and microscopical researches would lead us to expect—viz., of a mixture of albumen, fat, and phosphorous.

Thus, this apparent anomaly which the cerebral substance would seem to offer, according to the chemical analysis, has, in reality, no existence.

* Entwicklungsgeschichte des Hunde-Eies, 1845.

† Brit. and For. Med. Rev., vol. xvi., 1843.

‡ In regard to the structure of the zona pellucida surrounding the unimpregnated ovum, Bischoff still agrees with Mr. Wharton Jones in considering it to be formed of a homogeneous transparent membrane, without vessels, fibres, or cells, and to correspond in all respects to the vitellary membrane of the bird's egg. The thickness, firmness, and elasticity of this membrane, as well as the fact that the ova of no other class of animals in their primary state are provided with albumen, are all opposed to the truth of the theory held by Krause and others, that this zona pellucida is composed of albumen, either alone or enclosed in a membrane. Neither is there any proof of the existence of any other membrane within this zone which could correspond to the proper vitellary membrane, and the probability is, that as Coste (last Report, p. 296) and others believe, the granules composing the vitellus are held together either simply by their own cohesive force, or by the help of some clear viscid substance.

the germinal vesicle 1-60, and that of the germinal spot 1-240 of a line. Within the ovary, the ovum is imbedded in a layer of cells which, in the form of a membrane (membrana granulosa,) lines the internal surface of the Graafian follicle. At the point of this membrane where the ovum is situated, the cells are accumulated in larger quantity than elsewhere, and adhere to the vitellary membrane of the ovum, constituting the so-called *discus proligerus*. 2. The ova in the bitch (as probably in most other mammalia) arrive at their maturity in the ovary at certain periodic times, corresponding with the periods of heat in the same animal. As circumstances denoting the occurrence of this state of full maturity of an ovum, may be regarded—the swollen state of the Graafian follicle, the larger size and fuller appearance of the ovum itself, the elongation of the cells of the discus proligerus into the state of fine fibres, and lastly, the disappearance of the germinal vesicle. The latter event indeed sometimes does not occur until the ovum has escaped into the Fallopian tube. With regard to the germinal spot at this time, nothing certain can be said. 3. When an ovum is fully matured, it escapes from the ovary, and passes into the Fallopian tube, whether sexual intercourse has taken place or not. If copulation does not now occur, or if access of the seminal fluid to the ovum is entirely prevented, the ovum dies. Inasmuch, however, as the sexual desire is especially strong at this period, copulation and consequent impregnation are usually effected. Even if the ovum has not left the ovary at the time of coition, it may still be impregnated by the seminal fluid passing up the Fallopian tube to the ovary; or it may, at a later period, meet this fluid on its way. There is plenty of time for this to happen, for after leaving the ovary the ovum is usually from six to eight days in passing along the Fallopian tube to the uterus, during any part of which period it may be impregnated on coming into contact with the seminal fluid. When once arrived at the uterus, however, it is no longer capable of being fecundated, and by the time it has advanced to within a few lines of this its destination, the bitch will no longer allow itself to be lined. 4. The number of ova which at a single period of heat escapes from the ovary varies somewhat. They almost all escape at about the same time, there never being an interval of a day between the departure of any two; they are all therefore found accumulated together at the same part of the Fallopian tube, and present nearly the same degree of development. The sèveral Graafian follicles which are distended at the time of heat do not all discharge their ova; some retain their ova, which eventually break up and disappear. 5. Previous to the discharge of the ovum from the fully matured Graafian follicle, there begins to grow from the internal surface of this follicle a peculiar substance, under the form of granulations; and after the follicle has burst, and the ovum escaped, this new growth becomes further developed, so as to form a true corpus luteum. The presence of such a new structure in the ovary must therefore always be regarded as a certain proof of a Graafian follicle having burst, and of its contained ovum or ova (for in some cases a single follicle contains two, or possibly even more ova within it) having escaped; though not as a proof of coition and consequent impregnation having been effected. [This account, as was stated in the first Report,* must be modified in its application to the corpus luteum in the human ovary, in which this new growth in its perfect form probably never occurs, except as a consequence of conception.] 6. In order that fecundation of an ovum may take place, the seminal fluid must come into actual contact with it. After copulation, spermatozoa in a state of active movement may be sometimes found in abundance on the ovary; but whether or not, they are always present in the Fallopian tubes, and consequently around the ova, if these have already arrived there. It has never been clearly shown, neither is it probable, that a sperma-

* Half Yearly Abstract, vol. i., p. 277.

tozoon enters bodily into an ovum.* The action of the semen on the ovum is probably a chemical one; and it appears tolerably certain that (as suggested by Valentin) the fluid part of the seminal secretion is the material by which fecundation is effected, and that the office of the spermatozoa is two-fold: first, by their energetic movements to act as carriers of the seminal fluid to its destination at the ovum; and secondly, by the same active movements, to maintain in its integrity the due mixture and composition of the liquor seminis. The spermatozoa therefore probably acts towards the liquor seminis a part somewhat similar to that performed by the corpuscles of the blood towards the liquor sanguinis. So soon as the spermatozoa lose their movements or are separated from the seminal fluid, the fecundating power of the latter is lost. The liquor seminis, or its fecundating part, most probably enters the ovum by imbibition through the zona pellucida. 7. During the passage of the ovum along the Fallopian tube, the cells of the discus proligerus, which have hitherto surrounded the zona pellucida, gradually and entirely disappear, so that eventually the latter is left quite bare; no deposit of albumen on it takes place, such as is observed to be the case in the rabbit's ovum (as discovered by Mr. Wharton Jones.)—The ovum itself, as it passes along the Fallopian tube, becomes only somewhat larger in size. 8. When arrived towards the lower extremity of the tube, the first certain signs of the development of the ovum take place. These consist in a division of the yelk into smaller and smaller globular masses, which division takes place in geometrical progression with the factor two. These globules into which the entire mass of the yelk is eventually broken up are not cells, but simply agglomerated heaps of the yelk-granules unsurrounded by a membrane, and containing each in its centre a clear unnucleated vesicle, very similar to a fat-vesicle. The cause of this progressive division of the yelk and the source of the clear vesicle in the centre of each ultimate globule are points still in obscurity. It is probable, however, that the central vesicle is the cause of this division and subdivision, and that this vesicle itself is derived from the germinal vesicle or its nucleus.† 9. The forces concerned in propelling the seminal fluid along the genital passages towards the ovum are probably three-fold, and consist, first, of the impetus furnished by ejaculation, which carries the semen quite into the uterus; secondly, of a contractile movement of the uterus and Fallopian tubes, which commences at the vagina, and thence proceeds onwards towards the ovaries;‡ and thirdly, of the energetic movements of the spermatozoa themselves. The ciliated processes of the epithelium lining the uterus and Fallopian tubes can have no share in producing this onward movement of the seminal fluid, since their vibrations take place in a direction contrary to that along which the seminal fluid is advancing. 10. The forces concerned in conveying the ovum from the ovary along the Fallopian tube to the uterus are probably derived partly from the movements of the vibratile cilia situated along the Fallopian tube, and partly from the contraction of the Fallopian tube itself. 11. When first arrived in the uterus the ovum has much the same appearance which it presented when in the Fallopian tube, and the division of the vitellus still continues. Shortly, however, the small globular masses into which the vitellus has divided are developed into cells; a delicate membrane forming around each mass, and the clear vesicle in its centre consti-

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* In the present, as in his previous works, Bischoff states (p. 17,) that although he has repeatedly and carefully searched for spermatozoa in the interior of ova taken from the bitch as well as from the rabbit, he has never succeeded in finding them, and that he has been equally unsuccessful in his attempts to find the opening or fissures in the zona pellucida through which the spermatozoa might be enabled to enter the ovum, as described by Mr. Martin Barry.

† For Coste's account of the Metamorphoses of the Vitellus, which corresponds closely with the above, see last Report, p. 256.

‡ This movement has been hitherto observed only in bitches and rabbits. There is no proof of its existence in the case of the human female.

tuting a nucleus. 12. The cells which are thus gradually formed out of the vitelline granules become joined together, and as their number increases, and they become pressed and flattened against the internal surface of the zona pellucida, they combine to form a delicate membrane, which lines the internal surface of the zona, and contains in its cavity a clear fluid which the ovum has gradually absorbed, and whereby it has become increased in size. The appearance therefore which the interior of the ovum presents in consequence of this new development is that of a vesicle, and it has been named accordingly germ-vesicle, or *vesicula blastodermica* (keimblase.) At one point of the germ-vesicle may be seen a round dark spot, which is called the *area germinativa** (fruchthof;) in this spot the development of the embryo commences. On examining the ovum at this period with high magnifying powers, it may be seen that the yelk-granules are arranged in concentric circles around the vesicle of the cells of the germ-vesicle. In proportion as the cells increase so the yelk-granules diminish in number, and finally disappear altogether. 13. As the ovum thus increases in size by the absorption of fluid into its interior, the zona pellucida surrounding it becomes greatly stretched, so that it loses its double contour, and is converted into a fine structureless membrane. But it still continues to act as the only external membrane of the ovum; for even in the uterus there is no deposit of albumen around the zona. 14. The ovum, therefore, at the commencement of its stay in the uterus, is composed of two pellucid vesicles inclosed one within the other, and closely joined together—the zona pellucida and the germ-vesicle, or vesicula blastodermica; within the latter is situated the area germinativa, or spot where the formation of the embryo commences. At this time the ovum lies free in the uterus, and gradually makes its way to the spot where it will eventually become fixed. The power which determines the part of the uterus at which the ovum shall settle is quite unknown; it does not appear to depend on anything in the organization of the uterus itself.—Sometimes the ovum, when arrived at the uterus, passes across its cavity, and attaches itself at the side opposite to that at which it had entered. 15. When the ovum has attained the size of from one and a half to two Paris lines, a close examination will find that from the internal surface of the vesicula blastodermica, at the point where the area germinativa is situated, a second delicate cellular layer has formed, so that the vesicula germinativa now consists of two membranes, the external of which is called the *animal* layer, the internal the *vegetative*. They are so designated because at the part of the area germinativa which corresponds to the external layer the organs of animal life are formed in the embryo, whilst at the part adjoining the internal layer the organs of vegetative life are formed. Between these two layers there, probably in a short time, is formed a third, in which the blood-vessels are developed, and which may therefore be called the *vascular* layer. It is only at a later period, however, that this third layer can be clearly demonstrated. 16. Until the 20th or 21st day post coitu the area germinativa has consisted merely of a dark accumulation of cells, but it now begins to brighten in its centre, and to be divided into two parts, a bright central portion (the *area pellucida*) and a dark circumferential part (the *area opaca.*) The earliest trace of the embryo commences in the area pellucida, and consists at first of an elliptical, then of a guitar-shaped heap of cells situated in the animal layer, in the long axis of which heap a bright streak may be observed. In the accumulation of formative matter on either side of this bright streak are formed the walls of the body of the embryo. The portions of this formation immediately adjoining the streak are called the *laminæ dorsales*, and the portions external to these, the *laminæ abdominales*, or *visceral plates*. The longitudinal streak itself is called the *primitive groove*. The first trace of the embryo therefore consists of two seperate halves. 17. In the primitive groove are formed the central parts of the nervous system, namely, the brain and the spinal cord.—

* These terms are used by Mr. Wharton Jones in his Report.

Neither of these two arises from the other, but each has its own separate development. 18. After the formation of the central parts of the nervous system, the heart and vascular system are next developed. Here again also the heart is not formed out of the blood-vessels, neither do the arteries and veins originate from the heart, but each is developed separately and independently of the other. Finally, is developed from the central part of the vegetative layer of the vesicula blastodermica the intestinal system, namely, the intestines, lungs, liver, pancreas, &c. 19. The mode in which these systems and the organs belonging to them are formed in the ovum of the bitch, is exactly similar to that which takes place in other mammalia and in birds. The development of these parts proceeds so rapidly when once the first trace of the embryo appears, that after forty-eight hours the three chief systems are laid down.

3.—*On the Relation between the Constituents of the Food and the Systems of Animals.* By R. D. Thomson, M. D., &c. &c.

The first individual who showed that wholesome food should contain matters identical with animal substances was Beccaria, of Bologna, who wrote an excellent paper on the subject in 1742. Dr. Prout has taught and extended this view for more than twenty years, and his opinions are now followed by all physiologists. That the systems of animals are capable of sustentation by a supply of fibrinous matter alone, is obvious from the history of the primitive inhabitants of the prairies of America; but it appears from experiments made on the nutrition of animals with pure fibrin, that an auxiliary in the production of animal heat is either indispensable or advantageous, since animals fed on fibrin alone invariably declined in health. (Magendie.) That the amount of *calorifient*, or heat-producing food, in contradistinction to nutritive food, properly so called, as it has been well defined by Liebig, is out of all proportion greater than that required to supply the waste of solid matter of the body, is obvious from an experiment made by the author on a cow, in a state of rest, in which it was found that $15\frac{1}{4}$ lbs. of food were taken into the circulation in one day. Of this, only $1\frac{1}{2}$ lbs. was nitrogenous or nutritive food; the rest being *calorifient* and saline. From this experiment, frequently repeated with nearly the same results, the author concludes that in such a condition of the system, the natural relation of the nutritive to the calorifient constituents is nearly as one to eight one-third. The author gives formulæ for calculating the amount of nutritive and calorifient food, with a view to determine the laws of dieting.—He also gives tables from his own analyses, of the amount of nutritive matter in about twenty different kinds of vegetables (principally farinaceous food.) By these it is shown that oatmeal consists of 1 nutritive and 5 calorifient matter, and barley 1 and 7—facts which explain the universal employment of these substances. From these tables it is also inferred, that as milk is the natural food of the infant mammalia, the constitution of their food should be formed on the same type, and that of the use of arrow-root or starchy food, where the relation of the nutritive to the calorifient matter is as 1 to 26, instead of being as in milk 1 to 2, is opposed to the principles attempted to be established by the author. He observes that, in nutritive tables, it is usual to give a column of equivalents—representing, for example, 100 parts of beans as equal in nutritive power to 1160 of starch; but, according to the author's views, such a method is not founded on scientific principles. In a correct plan of dieting, a proper equilibrium must be maintained between the wants of the animal organism and the constitution of the food. The importance of this view is supported by the results of an extensive series of experiments, made by the author with different kinds of food upon cows. These results are highly interesting, and were given in a tabular form, but our limits will not allow us to detail them at length.

The author concludes by observing that, when more condensed forms of vegetable are required, the object might be obtained by mixing certain portions of

American flour with different kinds of meal, which could not otherwise be raised by fermentation; for example, by mixing equal parts of flour and oatmeal, flour and peas or barley meal, excellent bread could be formed; and two-thirds of Indian corn with one-third of flour yielded an excellent loaf.

Dr. Golding Bird, while he bore testimony to the industry and patient investigation which characterized the researches of Dr. Thomson, could not agree, in the conclusions at which he had arrived with reference to the dietary of persons in health and disease. It should be recollected, that previous to the labours of Liebig, the proportion of carbon which existed in various articles of food was thought to offer a fair indication of their nutritive power. That distinguished philosopher, however, had shown the fallacy of such a view, and had proved that, with the exception simply of the fatty tissues, every structure in the body was supported, and its waste supplied, by the nitrogenized elements of food.—Dr. Thomson's paper had the merit of satisfactorily showing that animals could not be well nourished on nitrogenized or carbonized food by itself, but that there should be a certain proportion between the two, so that the richly-nitrogized food might make up the waste of tissue, whilst the richly-carbonized would become a source of animal heat. He (Dr. Bird) did not believe that the composition of the food of the infant animal, or milk, gave us any sure indication, in our selection of nutriment for the adult; for milk, nearly identical in composition, afforded nourishment to the infant cat, sheep, and porpoise; whilst, in after-life, how remarkably different was the food of these different animals.

Admitting that the tables of Dr. Thomson were correct, it would seem that the most nutricious food for infants and invalids, next to milk, would be beans and peas. This could not be followed out practically. White bread, according to the tables, was below beans and peas in nutricious power; but, trusting to the same authority, a portion of cheese added to it would, theoretically, raise it high in the scale of nutriments: yet who would carry out this view practically? He admitted that infants might be literally starved from eating arrow-root, in consequence of their supply of nitrogen being cut off; he yet believed that in these inquiries sometimes more than the mere chemical principles must be taken into the account in our determination of the diet we shall select for our patients. The ease with which different kinds of food were digested, and the vital endowments of the stomach, must not be overlooked.

Dr. Babington observed that the last speaker had gone much too far in his criticism, as every one must be aware that animal food and some other kinds of condensed nitrogenous matter might not be suited to children, from their difficulty of digestion. He considered the paper a very valuable one, and deserving of the thanks of the society.

[Although one of the speakers stated that there was not much novelty in the views brought before the society by Dr. Thomson, we confess that to us, and to all with whom we have conversed on the subject, many of the facts and deductions are quite original. The table, which exhibited an increase in the butter of the milk of the cow, in proportion to the augmentation of nitrogen in the food, is perfectly new, and apparently at variance with the theories of Liebig, who derives the butter from the starchy constituents of the food; but which Dr. Thomson reconciled, by considering the food in these cases to be so formed as to restore the proper equilibrium of the system of the animal. This view also leads to the novel suggestion, that by experiment we should determine the amount of matter removed from the system under different circumstances of rest and exercise, and that a true plan of dieting should be founded on such knowledge, and should not be left to mere instinct. We believe with Dr. Thomson, that on this consideration depend the true laws of dieting—a subject of so much the greater interest in an artificial state of society, where the food is too frequently concocted to minister to the palate instead of to the condition of the waste of the system. The relation which Dr. Thomson instituted between the food serving for nutrition and that for the mere production of animal heat was very striking, and we have certainly never seen the subject so treated before. This analysis of arrow-root, tapioca, and sago, was also quite new, and demonstrated the impropriety of employing them as food for children. The table, already alluded to, showed, contrary to the statement of one of the speakers, that arrow-root could not produce fat, and that such views are imaginary. The table containing the amount of albuminous matter in various kinds of vegetable food, was highly important. The higher position occupied by Scottish oats and barley-meal over English flour is a new and startling statement. We concur in the concluding observation in Dr. Thomson's paper, that his remarks tend towards an extensive field of experiment and deduction, of a highly practical nature, and may assist in indicating the direction in which the physician should pursue his inquiries when studying the laws by which the animal system is to be retained in a state of health.—*Reporter of the Gazette.*]

Med. Gaz., May, 29, 1846, *p.* 965.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

(By reference to the papers of Drs. Hicks and White, in our first part, it will be seen that this fatal disease has made its appearance in the valley of the Mississippi.)

1—*Epidemic Cerebro-spinal Meningitis.*—The disease of which an account, as given by Dr. Darby, is mentioned in our last volume, p. 75, has more recently been noticed by Dr. Mayne,* who witnessed it in the Irish workhouse to which he is medical attendant. The pathology of the disease as described by him appears to have been uniformly the same in all the cases examined.

"In the post-mortem examinations which have fallen under Dr. Mayne's notice, the scalp and dura mater exhibited but little undue vascularity; the pia mater covering the hemispheres of the brain was congested, and the large veins, in their way to the several sinuses, appeared remarkably turgid. The free surface of the cranial arachnoid felt dry and clammy, and had lost its transparency in many places, particularly at the base of the brain; but there was no lymph or other inflammatory effusion in the sac of the arachnoid. Lymph of a yellowish or greenish hue appeared on the surface of the encephalon *beneath* the serous tunic; this occurred sparingly on the upper surfaces of the hemispheres, and there only along the sulci, but at the base of the brain it was found in greater quantities, especially in the sub-arachnoid space corresponding to the circle of Willis, where many of the cerebral nerves at their origin were fairly imbedded in it. In the spinal canal a similar exudation filled the sub-arachnoid space; it there existed in sufficient abundance to envelop the cord completely; it also extended down to the lowest extremity of the cauda equina, investing each of the spinal nerves at its source; but in the vertebral canal, just as in the cranium, the cavity of the arachnoid contained none of this morbid secretion. The substance of the brain and spinal marrow appeared remarkably free from lesion; there was no unusual vascularity, induration, or softening apparent, nor did the ventricles betray any evidence of inflammation. A remarkable feature of this malady is the class of persons on whom it has seized. In Ireland, so far as has been ascertained, boys under twelve years of age have been, with few exceptions, its only victims; the seven cases reported by Dr. Darby were all boys, and only one of them had passed his twelfth year; in Belfast ten cases of the disease were noted, all occurring in boys from seven to twelve years of age; and in Dr. Mayne's experience, individuals of the same description have alone

* Dublin Journal, and Med. Times.

been attacked. It is also a curious circumstance that in France the complaint appeared for the most part amongst the conscripts who had lately joined their regiments; and Versailles, Lyons, Mets, Strasbourg, Avignon, Nancy, and Poitiers, were the places in which it proved most destructive to life. Although females have been very rarely attacked, yet they are not exempt from its invasion, for, at the Hardwicke Hospital, one of Dr. M'Dowel's patients was a girl of seventeen, and the other a woman aged thirty-six years.

The symptoms by which this affection commences are in general of a very formidable character, and its accession is usually sudden and quite unexpected; in the majority of cases the patient has been in his ordinary health and spirits up to the very moment of the seizure, and has experienced no premonitory symptoms to warn him of his danger. In four of the cases at the South Dublin Union the boys had eaten a hearty dinner and retired to bed in apparent health, when the disease all at once declared itself. In many instances it commences with severe pain in the abdomen, followed immediately by vomiting, and not unfrequently by purging. In the worst cases these symptoms are accompanied by marked collapse, the extremities are cold and bluish, the pulse is at this time a mere thread, and altogether the disease assumes very much the aspect of cholera. After the lapse of a few hours, reaction, more or less perfect, ensues; the muscular system then presents characters which may be considered almost pathognomonic. The muscles of the extremities, and those of the neck in particular, become remarkably rigid, the head is drawn back upon the vertebral column, and firmly fixed in the unnatural position; no efforts of the patient can bend it forwards, neither can the attendants do so, at least by the employment of any justifiable force. The countenance at this period often assumes very much the tetanic expression; twitchings of the muscles of the face sometimes ensue; the patient loses, in great measure, the power of moving his extremities, so that he is quite unable to assume the erect position, the surface becomes hot, the pulse full and frequent (from 120 to 140;) the stomach often continues irritable, whilst an insatiable thirst torments the sufferer; and the epigastrium evinces marked tenderness upon pressure. Symptoms of a still more distressing nature quickly supervene; the patient may be seized with general convulsions, of frightful severity, requiring personal restraint to protect him from injury; or he may lie in a semi-comatose condition, constantly moaning and grinding his teeth, or even crying incessantly. Towards the close of his sufferings he generally merges into perfect coma, the pulse becomes slow and labored, the powers of speech and deglutition fail, his stools are passed involuntarily, and death finally closes the scene. All this may occur in a surprisingly short space of time; some of the cases ran their course in forty-eight hours, and the greater number terminated about the fourth day, whilst some few were prolonged over a fortnight or three weeks. Examples are on record of death from the disease in so short a period as fifteen hours. The fatality of the complaint in Ireland has been very great, but continental practice has been even still more unsuccessful. Dr. Mayne is of opinion that the active exhibition of mercury will be considered the sheet-anchor in England and Ireland, in the treatment of this as it is in that of other diseases of the serous membranes.

2.—*Ligature of the Common Carotid.*—Our last Report on this subject embraced three cases, the whole of which proved fatal (Vol. II., p. 266.) The following facts are upon the whole more favorable, although it must be confessed, they tend to the conclusion that our knowledge is defective as respects the influence which the operation produces on the structure and functions of the brain. In one case Dr. Blackman tied the common carotid for a *fungoid tumour of the neck.** The fungus had an opening in its centre, which extended

* Amer. Journ. of Med. Sciences, Oct. 1845, p. 333.

to the sheath of the artery, and it discharged about a pint daily of the most offensive bloody matter. This had continued for nine weeks, and the patient's strength was nearly exhausted. The operation was performed without difficulty; in less than an hour afterwards the tumour became cold, the discharge ceased, and by the second day it was obvious that the whole mass would soon slough. The patient, however, became *paralytic on the opposite side*, and ultimately comatose, and died on the eighth day after the operation. Dr. Blackman remarks that the season was most unfavorable to the case, the thermometer ranging about 95° Fahr., and that had the operation been performed earlier, and under more favorable circumstances, he sincerely believes that even if the patient had not been cured, his life would have been considerably prolonged. Again, Mr. Vincent has published two cases of diseases of the brain following the application of a ligature to this artery.* In the first, the operation was performed for the cure of an aneurism; the man became paralytic on the opposite side; he was bled very copiously, and after death, the brain was found quite soft and cream-like, on the side on which the operation was performed.—In the second case, the operation was performed in consequence of an injury to the artery; the patient became paralytic on the opposite side, and even during the operation, he was observed not to move his extremities on that side. After death the veins on the same side, within the skull, were only partially filled; there was effusion of serum beneath the arachnoid, and the convolutions of the cerebrum were flattened and softened. Mr. Vincent makes no comment on these cases.

The following case of aneurism of the carotid artery, recorded by G. White, Esq., surgeon to the General Hospital, Nottingham, is possessed of considerable interest.† It being considered practicable and expedient to deligate the common carotid below the tumour, before the operation, at the suggestion of Mr. Sibson, the patient was desired to exhale completely, and to keep his chest as empty as possible; the upper part of the chest was then closely surrounded with a very long, firm bandage, and he was instructed to respire as much as practicable by the diaphragm and abdominal muscles. By this means the length of the neck was materially increased, and the cervical veins were prevented from being alternately distended and emptied. Mr. White considers the operation was thus very much facilitated. At the commencement of the operation, it was found necessary to place two ligatures upon a large vein, and to divide the vessel between them. Formidable hemorrhage took place immediately after the separation of the ligature of the artery, but was easily arrested. Mr. White considers that it probably came from the portion of the artery connected with the tumour, and he observes, that the case proves the practicability of successfully placing a ligature on a large arterial trunk, *very near its origin.*

Mr. Yates, of Nottingham,‡ on this case observes, that there is no other well-authenticated instance of recovery after deligation of the common carotid, under similar circumstances, although the operation has been performed more than once or twice; that, in such cases, hemorrhage on separation of the ligature is the cause of death; and although recovery took place, it is not unreasonable to expect that hemorrhage will always occur when a similar operation is performed for a similar disease, under the same conditions; because, in nearly every instance of spontaneous aneurism, the vessel is diseased to some extent, both above and below the tumour; and on account of the danger of placing a ligature upon a diseased vessel; and further, that where there is no greater space than an inch or so between the aneurismal sac above, and the clavicle and upper margin of the sternum below, bleeding is almost sure to follow an operation of this kind; and even if the vessel were healthy, it is questionable whether the force of the circulation would not wash away any

* Med. Chirurg. Trans., vol. xxv., p. 37. † Lancet, Feb. 7, 1846, p. 149.
‡ Lancet, March 14, 1846, p. 298.

clot that might be forming within an inch of its fellow trunk, the subclavian artery. These observations are contradicted by the instance of the carotid being tied successfully within the eighth part of an inch of the innominata, by Mr. Porter, and by the fact that Dr. Robinson, of Edinburgh, with equal success, cured an aneurism occupying nearly the entire side of the neck, when it was with the greatest difficulty that he could find space between the disease and the sternum to expose the root of the carotid.*

Mr. Yates further remarks, that whether the bleeding proceeded from the proximal end of the artery, or issued out a tumour by a retrograde movement, is a matter of the greatest moment. One end of the artery could not have been closed at the time of the hemorrhage, and reason would lead us to suppose the end nearest the heart was that one, since it had to bear the greatest impulse of the heart's action, and the force of a heavy column of blood, which must almost necessarily prevent the formation of a coagulum sufficiently large and firm to close up the vessel; that the tumour, in Mr. White's case, became hard, which must have arisen from coagulation, and it is not reasonable to believe that at the same time hemorrhage took place from the end of the artery attached to the tumour. But Mr. Fergusson† states that this opinion—that secondary hemorrhage always occurs from the proximal ends of the vessel—is losing ground, and it is now believed that the occurrence happens as frequently, if not more so, from the distal side of the seat of ligature, and that post-mortem examination favors this view.

M. Triboli‡ tied the common carotid in a man, aged 27, in consequence of a wound near the angle of the jaw. The hemorrhage was arrested in the first instance by syncope, and did not return until towards the end of the tenth day. Two ligatures were employed—one with which the artery was tied, the other a little lower than the first, which was left in its place, to be used in case of emergency. Both ligatures came away the eighth day after the operation, and the case did well.

M. Triboli was aware that in placing the second ligature round the artery he was deviating from the ordinary practice, and he does not appear to have given any very good reason for doing so; two days after the ligatures came away, hemorrhage occurred, and returned repeatedly for two or three days.

Mr. Liston lays the greatest stress upon the impropriety, in this operation, of disturbing the vessel by detaching it from its sheath, or breaking up its fine cellular connexions further than is barely sufficient for the passage of the needle, and upon the disadvantage of applying more than one ligature; but he admits, as an exception, that two ligatures should always be used, when, by any awkwardness, the vessel has been much disturbed from its cellular connexion.§

(We are happy to meet with the facts here set forth, as they may serve to counteract a probably erroneous conclusion about to be drawn from a recent case that occurred at our Charity Hospital, in which the etherial vapour was inhaled. The common carotid artery was tied, as a preliminary step to the removal of a diseased *parotid gland.* Death occurred on the third day after the operation, and the *autopsy* revealed *softening of the brain*, with extensive engorgement. These lesions inclined the operator to attribute much blame to the ether, but it is seen above that like results may follow ligature of the common carotid, where no ether has been used. Let us do the ether justice, although it fail to fulfil all the sanguine expectations created at its introduction into surgical practice.—Edrs.)

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* Fergusson's Practical Surgery, 1846, p. 448. † Lib. cit., p. 449.
‡ Gaz. Méd. de Paris, from Il Raccoglitore Medico. § Prac. Surg. 1846, p. 178.

3.—*Internal and External Employment of Nitrate of Silver.* By Dr. HELLER.

[The diversity of opinion with respect to the therapeutical action of nitrate of silver, has been so great as to induce Dr. Heller to institute inquiries on the subject for himself. He performed experiments to ascertain to what extent it is taken into the secretions and excretions, and also to ascertain what was the effect of its direct addition to the gastric fluid;—the following are the results.]

In several cases of epilepsy nitrate of silver was administered internallly in doses varying from three to ten grains daily; thus, one girl took ten grains daily, and a lad, aged thirteen, twelve grains daily for three months. In the latter case, therefore, about two ounces and a quarter were administered altogether, yet without the disease being in the slighest degree mitigated, or any other effect on the system being produced. In none of the cases did the skin become in the least discoloured. The blood was several times examined in many of the cases; it presented the character commonly found in the blood of epileptics, especially an increase in the quantity of albumen; but in none of the examinations was there a trace of silver found.

The urine, also, contained not the slightest trace of silver; it was clear and without sediment, possessed its ordinary quantity of chloride of sodium, and contained no ammonia. Since the urine, therefore, contained chloride of sodium, no silver could have been present in it, else would the chloride of silver have been precipated, and detected as such, which it was not; and since the urine contained no ammonia, which is a solvent of chloride of silver, so also could none of the chloride of silver have been retained in solution; moreover, after combustion and careful examination of the ash, not a trace of silver could be detected. The fæces, Heller says, contain, in the form of chloride, the whole of the silver which has been administered. To prove this, nothing more is necessary than that the fæces should first be reduced to ashes in a platinum crucible, and then the mineral salts which remain dissolved by a little dilute nitric acid; after this the chloride of silver is left behind, and may be weighed and estimated; it is completely soluble in ammonia.

Heller found that when he mixed nitrate of silver with the gastric fluid, chloride of silver was at once precipitated, for the gastric fluid contains a large quantity of the chlorides of sodium, potassium, and calcium; when ten grains of the nitrate was added to an ounce of the gastric fluid, the whole was precipitated in the form of chloride. From this fact Heller concludes, that so soon as nitrate of silver is taken into the stomach, it is precipitated as chloride, which, being an insoluble substance, passes through the entire length of the intestinal canal, and appears as such in the fæces; the results of examination of the blood and urine also show that none of the silver enters into the blood. This renders easily intelligible the circumstances that such large doses of nitrate of silver may be taken without any observable effect on the system resulting, yet, according to Heller, leaves unexplained the statements of some writers, who describe the skin of patients who have employed the nitrate of silver for some time, as becoming brown, or even black; he seems inclined to doubt the truth of these statements, inasmuch as they are opposed to the results of his experiments, and to the fact of this discoloration never occurring in any of the cases which he noticed.—*Heller's Archiv.*—[*Med. Gaz., July* 24, 1846, *p.* 170.

4.—*Lithotomy in the Female.* By W. FERGUSSON, Esq., Surgeon to King's College Hospital, &c.

[This case was admitted into King's College Hospital, under the care of Mr. Fergusson, October 8th, 1845. The stone was ascertained to be present, and was operated upon on the 11th.]

The patient was placed on the table, and tied in the usual way for lithotomy; a deeply-grooved straight director was introduced into the bladder, the groove pointing downwards, and to the left side; a straight, probe-pointed bistoury was then passed along it, and an incision about half an inch in extent was made towards the tuberosity of the ischium, the wound being limited to the anterior half of the urethra. A slight nick was next made in the right side of the orifice of the urethra. and the director being withdrawn, the point of the left forefinger was introduced into the wound, and then gradually insinuated into the bladder, at the same time dilating the posterior half of the urethra. The finger having touched the stone, was then withdrawn; a small lithotomy forceps was now introduced, when the stone was immediately seized. and, though the blades slipped once, was speedily removed. It was of an oval form, about the size of a pigeon's egg, and composed chiefly of lithic acid. Seven, P. M., patient comfortable, and in good spirits. Water passes freely, and without much pain.

[Everything went on favourably, and she was discharged cured on the 31st.]

Mr. Fergusson called attention to the peculiar operation which had been performed in this instance, differing, as he stated, in some important respects, from the operations usually resorted to in the female. There had been here, as in most other instances, a choice of means of cure. As to lithotrity, he did not think it eligible in this case, chiefly, perhaps, on account of the timidity of the patient, and her consequent restlessness when touched by the surgeons. For the same reasons, dilatation did not appear practicable; and he had therefore to adopt a proceeding whereby he could at once remove the stone while the patient was held steady by main force. The operation which he had performed was of a compound character, the opening necessary for the extraction of the stone having been made partly by incision and partly by dilatation. He had described this operation ever since he had been a teacher, and was disposed to think it the best mode of relieving a female from stone of all with which he was acquainted. In the process of dilatation, he had remarked, both on the living and the dead body, how slowly the external orifice of the urethra dilated, and how much pain it caused; indeed, the tissues in this part seemed incapable of dilating to any extent likely to permit the passage of a large stone, unless the force were very gradually applied. Towards the back part of the passage, however, and at the neck of the bladder, the strictures yielded more readily, and they seemed also to regain their tone with much more facility. It was an error to suppose that dilatation, in whatever way performed, invariably saved the patient from dribbling of urine ever after. Very recently, a case proving the reverse had been under notice in the hospital. A girl nine years of age, had a stone extracted, some years before, by dilatation; but she never since had power over the stream of urine. In lithotomy, as usually performed, there was even a greater risk of this unhappy consequence; and although, in either method, especially by dilatation, many cases were familiar to the profession when no such results had ensued, it was yet felt by most practical men that there was great uncertainty as to what might follow any of the usual proceedings in such cases.

After describing the various operations which had been proposed for lithotomy in the female, Mr. Fergusson stated his conviction, that while there was less positive danger in the proceeding which he had followed in this instance, than in some of the others described in ordinary surgical works, there was, perhaps, greater chance of the parts regaining tone sufficient to give command at will over the stream of urine. The incision in the anterior part of the urethra produced, in his opinion, less injury than dilatation, while, by not applying the knife to the back part of the passage, or to the neck of the bladder, there was positively less risk of severe and dangerous inflammation, while the muscular structures at the neck of the bladder, in the viscus itself, which doubtless gave the power of retaining the water, more speedily regained their natural condition, than after division with the knife. The little notch which he had made on the other side of the urethra might or might not be made, according to circumstances. Here the stone seemed so large in proportion to the parts, that he had thought it advisable; but with a smaller stone, he should extract without it.—It would be unreasonable to conclude, from this single case, that this operation was the best that could be performed; possibly the same happy results might, in this instance, have followed any other proceeding; but it appeared to him that there was so much good in this operation, and apparently so little evil, that it was well worthy of further trial.

Lancet, Sept. 12, 1846, *p.* 289.

5.—*A Lecture on the Utility and Safety of the Inhalation of Ether in Obstetric Practice.* By W. Tyler Smith, M. D., Lecturer on Midwifery.

The only authorities who have published their opinions and experience on the subject I am about to consider, are Baron Dubois and Professor Simpson; but in the accounts of these obstetricians, though valuable facts are recorded, little attempt is made to reconcile experience and the phenomena of etherization, with physiological and pathological actions, the true basis upon which its ultimate adoption or rejection must rest. The task of fixing the relations and true position of a new agency, of such extraordinary powers, is one of great difficulty, but it seems to me that the time is arrived when at least the attempt to do this should be made.

Our opinions respecting the value of etherization in practical midwifery must chiefly depend on our knowledge of the manner in which this state of insensibility can modify the physiological actions of parturition, and also on our obseravation of the results in those obstetric cases in which it has been or may be employed.

If the production of the etherized state merely removed pain, without inducing any other complication, the question, as it relates to midwifery, might be readily decided. The relief of the pangs of travail would indeed be an invaluable boon. But the problem to be solved is by no means so simple or so uncomplicated.—The inhalation of ether takes away from the parturient woman several other elements natural to her condition besides mere physical pain, while it adds thereto other states of the system, which are not observed in strictly natural labors.

The true mode of proceeding is to examine carefully what there is substracted from, what there is added to, the processes of labor, and then to come to a decision as to whether the advantages or the disadvantages of the use of ether in midwifery are likely to preponderate. Etherization is chiefly a new condition of the nervous system, and we must examine the modes in which the different divisions of the nervous system are affected.

And, first, as regards the cerebral division of the nervous system.

Sensation is for a time impaired or abolished. There can be no doubt that, as one of its effects, the pains of labor may be alleviated or abolished by this new application.

Volition may also be suspended; all the voluntary movements are in abeyance when the patient is fully under the influence of ether. In the partially etherized states, violent actions of the voluntary muscles takes place, but they are irregular, like the movements of ordinary intoxication.

Emotion is, in the fully etherized state, withdrawn, or the ordinary emotions are replaced by the disordered emotions of dreaming or delirium.

We must therefore inquire into the uses of *sensation*, *volition*, and *emotion*, to parturition, all of them being intimately associated together in natural labor.

In the expulsory stage of labor, after the dilatation of the os uteri, the efforts of volition are of some assistance to delivery. The efforts of expiration, and especially of the abdominal muscles, are increased, and the patient aids in fixing the thorax and the pelvis, by grasping firmly with the hands, and planting the feet against some convenient place. Sensation and emotion are, in a physiological sense, even of more importance than volition to parturition. In the last pains of labor, when the motor power is prodigious, and the patient is threatened with laceration, the sensation and emotion of pain comes in as a preservative. All the ordinary actions of labor at this period are reflex in their nature, and are, whatever their violence, uncontrollable by the will. But whenever the pain becomes too intense to be borne,—and pain is here a measure of danger,—the patient, chiefly under the influence of emotion, cries out, and her cry, by opening the glottis, takes away all expiratory pressure, and leaves the uterus acting alone. So, in the last stage of labor, upon the mingled agony and exertion of which obstetricians have exhausted their descriptive powers, pain and its attendant emotion play a benign and salutary part. It is now that laceration of the perinæum is most impending, and at this point not only is the glottis opened, but the sphincter vesicæ are suddenly dilated, so as to relieve the perinæum to a very great degree. It cannot but be considered as a singular provision of nature, that at the moment when the outlet of the vagina is threatened with the greatest danger, these two sphincters should suddenly relax before and behind it. We may even deduce from this fact a reason for the anatomical position of the vagina betwixt the rectum and the bladder. Thus, then, volition, and especially the sensation of pain, and its attendant emotion, are of considerable importance in natural parturition.

Some interesting points relating to sensation have been suggested by the facts observed in etherization. It has been again and again noticed that patients may preserve ordinary consciousness, and the uses of the special senses, with a total insensibility to pain. They have in some cases talked rationally, and have seen and heard perfectly while they were unconscious of the performance of the most painful operations. Separate seats have been proposed for ordinary consciousness and the perception of pain. Both the seat of pain, and the exact nerves which are the communicators of pain, are as yet unascertained. It is evident that the nerves of special sense are not the conductors of pain; the optic, olfactory, and auditory, are insensible to pain; and a whitlow affecting the cushion of the finger, upon which the tactile nerves are accumulated, is not more painful than a burn on the back of the hand, or than an inflamed peritonæum. Thus, the nerves of special sense, as such, are not ordinarily conductors of pain. Besides the fact that the intellect may remain clear while sensation is abolished, there are other reasons for believing that the cerebrum is not the seat of the sensation of pain. Longet, for instance, removed the whole of the brain of a living animal, leaving only themedulla oblongata; and on being pinched or punctured, it cried out, and gave the most lively manifestations of pain. These facts would lead to the inference that there must by a special seat of pain, and special nerves for its propagation, from the periphery to this centre of painful sensations. Lastly, it becomes a question whether either affects the seat of pain, or the nerves which conduct to it. Mr. Adams, of the London Hospital, made the ingenious suggestion that the blood was altered in etherization, and that the altered blood paralyzed the extremities of the nerves. Others have maintained that this altered condition of the blood depends on the imperfect inhalation of the ether, and partial asphyxia. We know that a writer of eminence on the nervous system is of opinion, that the central seat of pain, with that of pleasurable sensations, and emotion, and respiration, is in that *nœud vital*, or nodus vitalis, the medulla oblongata, whilst its actual seat is in some part of the ganglionic system. Probably, under the full influence of ether, the whole of the nervous system concerned in the production and perception of pain is simultaneously affected.

But whatever its nature and seat, sensation is removed by the influence of ether; the parturient woman may pass at once from her agony, into a state of oblivion to pain. Pain is taken away!

Labor itself, like a surgical operation, produces what has been termed *shock*. In certain cases, the vital powers fail, the contractions of the uterus and the actions of the heart sink together, and the patient may even die from the pure influence of *shock*. This may take place in labors, difficult it may be, but still from which the majority of women recover in the usual way, so that we have no certain means of estimating beforehand the probable power of the shock of parturition in any given case. Professor Simpson, in his recent paper, asks, "On what division of the nervous system does the nervous shock operate—the cerebral, spinal, or ganglionic? If on the former, it should be kept in abeyance by due etherization." Now, I conceive this important point need hardly be put as an interrogation, or if so, that it should meet with something like a definite answer.

Shock is composed of several elements, and is certainly not confined to the brain; those which are really referrible to the cerebrum, and to the medulla oblongata, which partakes both of the nature of brain and spinal marrow,—for here the cerebral and spinal systems seem fused,—are, pain and emotion, and the effects of these depend on the perfect presence of consciousness and the perception of physical suffering.

We must doubtless look on *pain*—I mean, physical suffering—as distinct from uterine contraction, which in midwifery almost takes the term to itself, not only as a great evil, but as a source of other evils. No human suffering, perhaps, exceeds in intensity the piercing agonies of child-bearing, and no benevolent mind but would consider it an inestimable blessing to be able to relieve women with safety from so great a trial. I do not undertake (to speak ?) the importance of pain or of its alleviation. There are women who after one labor remain their life-long depressed and in dread of the repetition of their sufferings. But in the present subject it is with the immediate effects of pain that we have most to do. Acute physical pain, in the majority of cases, inflicts a shock upon the system, the shock being in proportion to the depressing effects of the pain endured. *Pain*, then, when it depresses the system, is one element of *shock*, whether in parturition or in surgical operations. But it must be said that it is not always depressing; on the contrary, it sometimes acts as a stimulant, and as such is probably salutary rather than prejudicial. Even this great evil is not unmixed with good.

Emotion constitutes another element of shock, as when fear or despair, or some kindred emotion, depresses the vital powers; and this element begins to act, it must be borne in mind, before the commencement of the operation.—Thus, to put an extreme case, the step of the criminal falters, and his heart fails, on the way to execution. But as emotion begins to operate before pain or danger is actually present, it may exert a bad effect on a patient about to be operated on, or delivered, in the etherized state, if there should be any great fear or dread for the result. However, in the majority of cases, *pain*, and *emotion*, as elements of shock, would disappear when insensibility commenced. Many parturient women are destroyed, directly or indirectly, by emotion alone, and so are many surgical patients; these latter, however, in a different mode from the former; there is no escape from labor; but that extreme motion, which in midwifery is so dangerous, prevents surgical patients from undergoing the proper operations at all, and in this manner such patients die. We shall see hereafter whether in midwifery there are not counterbalancing evils more than sufficient to weigh against the partial deliverance from *emotion* and physical *pain* accomplished by etherization.

I now pass on to speak of the *spinal marrow*, by which I mean the true spinal marrow, the seat of the reflex function in its most extended signification; and of the ganglionic system.

There is, speaking generally, in the application of ether, a point at which the cerebral faculties are suspended without any affection whatever of the spinal marrow. But after this has been passed, the respiration becomes stertorous, and spasmodic twitchings, or even general convulsions, many ensue; the spinal marrow is now fully involved as well as the brain. Still later, according to the experiments of Baron Flourens and M. Serres, this organ becomes inexcitable; no reflex movements follow upon the application of an excitant to the surface; and even the spinal marrow itself may be pricked and lacerated without inducing any convulsive action. If etherization be still further continued, the animal dies. In the cases in which ether has been employed in midwifery, it has not affected the uterine contractions—that is, it has not been pushed so far as to materially depress the spinal functions. I have little doubt that accurate observation will show that it may either not influence them at all, or that it may increase or diminish them according to the condition of the patient, and the extent to which it may be employed, though the effects of its successful use would be almost invariably the suspension of voluntary motion, sensation, and emotion, the uterine reflex actions remaining natural, or being but slightly affected. I cannot enter here into the physiology of Baron Dubois, but it is evident that this distinguished accoucheur is quite unaware of the reflex physiology of parturition, and engages in the discussion of points about which there is no doubt.

I have already spoken of the influence of shock upon the brain; still more important is the influence of severe injuries upon the spinal marrow and the ganglionic nerves. An experiment and a fact will prove this. If the whole of the contents of the cranium are removed from a frog, so as not to affect the circulation, and the thigh or the pelvis be now crushed by the sudden blow of a hammer, the heart's action is arrested, and for a time no reflex actions can be excited by external irritation. This experiment shows the part borne by the spinal marrow in the phenomena of shock. You must bear in mind that this physical shock is quite independent of the brain and of sensation. If a man is shot with a bullet through the body, he may drop instantly, though he has not felt himself wounded. He falls, not from the effects of pain, and before the effects of loss of blood are felt, but probably from the simple effects of shock upon the spinal marrow and the ganglionic system.

This fact, that shock in its major part may be still present, though a state of perfect insensibility has been induced, has escaped the attention of all writers on the subject of ether. Cases have been detailed, and this inhalation of ether eulogized, not only as the assuager of pain in surgery, but as the alleviator of the shock of operations. It is most important that it should be understood, that in its true nature shock affects the spinal marrow and the ganglionic system even more imminently than it does the brain.

The stimulant effect of ether upon the brain and spinal marrow at one stage of its operation is of great importance in considering its applicability to midwifery. It has been used by Dr. Ranking in tetanus with the effect of increasing the spasm; it has also been found prejudicial in hysterical and epileptic convulsions; and it has produced convulsions in persons who have inhaled it merely for experiment or for surgical purposes. Baron Dubois vividly describes the symptoms produced in one of his obstetric patients, in which all the most intense premonitory signs of convulsions were induced; the congestion was so great, that he almost expected the eyeballs to syringe forth blood. Now, knowing the tendency to convulsions which belongs to the puerperal and pregnant states, it would at once seem improper to resort, without very grave reason, to the use of an agent so capable of adding to the utero-spinal excitement labor, a new and a direct stimulant of the organ concerned in convulsions.

In some operations there has been a considerable amount of reflex action; in others, the twitchings of the stump after amputation has been considerably diminished. In one of Baron Dubois's cases, the limbs remained motionless; in another, a movement of the thigh occurred on the introduction of one of the blades of the forceps. These and similar facts show that the spinal marrow may be either excited or depressed by this agent. I cannot now enter fully into this subject; but in some cases the loss of sensation and consciousness, and the persistence of reflex action, have been very remarkable. The cerebral and spinal divisions of the nervous system have been as completely separated as they could be by actual experiment. The inhalation of ether has, with almost the certainty of a chemical reagent, decomposed the old cerebro-spinal system into two, and the cessation of its influence has as suddenly reunited them—thus offering an additional proof, if such were necessary, of the independent function of the true spinal marrow. Baron Flourens has also obtained further proofs of his doctrine of the separation of the contents of the cranium into excitor and inexcitor parts, by the phenomena observed in etherization.

One extraordinary circumstance relating to this subject must not be forgotten—namely, the occasional incitement of the sexual passion in patients under the influence of ether. In one of the cases observed by Baron Dubois, the woman drew an attendant towards her to kiss, as she was lapsing into insensibility, and this woman afterwards confessed to dreaming of coitus with her husband while she lay etherized. In ungravid women, rendered insensible for the performance of surgical operations, erotic gesticulations have occasionally been observed, and in one case, in which enlarged nymphæ were removed, the woman went unconsciously through the movements attendant on the sexual organism, in the presence of numerous bystanders. Sexual excitement has also been observed in the male subject. Viewed apart from the moral considerations involved, there is not, in the whole of the wonders related of this extraordinary agent, anything more wonderful than this exchange of the smarting of the knife and the throes of parturition, not for mere oblivion, but for sensations of an opposite kind, pain, in fact being metamorphosed into its antithesis. Still, I may venture to say, that to the women of this country the bare possibility of having feelings of such a kind excited and manifested in outward incontrollable actions, would be more shocking even to anticipate, than the endurance of the last extremity of physical pain. I am only surprised that the distinguished French obstetrician should not have made some observation of this kind. In many of the lower animals, we know that an erotic condition of the ovaria is present during parturition, and that sexual congress and conception may take place immediately upon delivery. It was, however, reserved for the phenomena of etherization to show that, as regards sexual emotion, the human female may possibly exchange the pangs of travail for the sensations of coitus, and so approach to the level of the brute creation. I have no doubt—indeed, I have accumulated evidence to prove, *as a general law*—that at the time of parturition in all ovipara and vivipara, the human female included, the ovaria are in a state fitted for fecundation. May it not be, that in woman the physical pain neutralizes the sexual emotion, which would otherwise probably, be present, but which would tend very much to alter our estimation of the modesty and retiredness proper to the sex, and which are never more prominent or more admirable than on these occasions? If this be so, women would scarcely part with pain, hard as their sufferings may be to bear; chastity of feeling, and, above all, emotional self-control, at a time when women are receiving such assistance as the accoucheur can render, are of far more importance than insensibility to pain. They would scarcely submit to the possibility of a sexual act in which their unborn offspring should take the part of excitor; and as the erotic condition has been chiefly observed in patients undergoing operations on the sexual organs, we must assign as the exciting cause, either the manipulation of the attendant or the passage of the child.—Lucina, fabled as she was to have been born without pain, was a name often given to the severe Juno, and to the chaste and cold Diana, but never to the Paphian queen.

We are now, I think, in a position to argue the questions as they relate to midwifery—What benefits can be conferred, what injuries hazarded by the use of ether? Pain and emotion may be oblitered; all, or nearly all, of shock which belongs to pain and mental emotion may be averted; but, at the same time, volition, and the salutary influence exerted by pain and emotion on the motor actions of labor, are withdrawn. In successful cases—that is, when the brain only was affected—parturition, as a reflex function, would not be interfered with. There remains to the woman, in its full intensity, all that portion of *shock* which depends on the spinal marrow and the ganglionic system.—There is added, in some cases, the increased tendency to puerperal convulsions, and the collapse described in the cases of Messrs. Nunn and Robbs as the effects of the ether itself, and which has also occurred in many unrecorded cases. The dangers incurred by the use of ether in midwifery would be that of convulsion, and of the meeting of the physical shock of parturition with the shock or collapse produced by the ether itself. Whenever this complication occurred, there would be considerable danger. The moral considerations which relate to our decision I have already alluded to. Briefly, I may repeat, there is, on the one hand, the absence of pain and of painful emotions, with their attendant evils; on the other, there is the physical shock unaverted, the tendency to convulsion, and the possibility of dangerous collapse—a possibility which, in some surgical cases, has passed into fatal certainty.

I know it is ungracious to take the part of an alarmist on such a question; but many fatal cases have now occurred after operations in which etherization has been practised. The patient who underwent the Cæsarian operation died; another patient, on whom extirpation of the eyeball was performed sank; a clergyman, whose leg was amputated, never rallied after the operation; two of the women delivered while under the influence of ether, by the Baron Dubois, subsequently died; and fatal collapse occurred in the case of a woman from whose thigh a tumour was removed. Probably other fatal cases have occurred, of which we have no information. It would be difficult to say in how many of these instances the ether contributed to the fatal results: in some, death would no doubt have taken place under any circumstances: but in others, it may be considered well nigh certain that its use was the chief, if not the sole, cause of death.

The morbid phenomena fairly attributable to ether, observed in cases which have recovered, have been—nausea, sickness, stertorous breathing, pulmonary and cerebral congestion, convulsions, and protracted failure of the heart's action. Now, the bare possibility of producing symptoms such as these by a remedial agent, however valuable, renders it imperative that we should be able to distinguish the cases in which the more serious of them are likely to occur, otherwise the agent itself must inevitably fall into disrepute. As is well known, digitalis may produce failure of the heart; opium, congestion of the brain; and hydrocyanic acid, convulsions: and consequently, caution is always observed in the administration of these still valuable remedies; but after etherization, we may have effects which resemble either undue narcotization, an over-dose of hydrocyanic acid, or the cumulative results of digitalis; so that it behooves us to be triply careful respecting its indications and its exhibtion. In two fatal cases, a fluid state of the blood has been found after death.

As yet, very little has been said towards indicating the proper cases for resorting to ether, and those in which it should be avoided. There has been a general rush towards the operating room, such as the world has never before witnessed. Great numbers of cases were successful on its first introduction; and this gave an *èclat* to the subject, and induced a confident state of mind in patients, which has doubtless been an element in the successful results; but now that fatal cases have occurred, all that was mere prestige must fall to the ground; and unless the proper cases for etherization can be distinguished with somewhat approaching to certainty, patients upon whom it may be used will go under the knife influenced by previous dread rather than confidence; and so an item of evil, not properly belonging to etherization itself, will come into play. Such is the constitution of the human mind, that a few fatal cases, even by the side of a great number of successful ones, will be sufficient to transmute hope into fear, confidence into timidity and mistrust. Probably the fatal cases which have become known have already produced this effect, and it is believed that many of our most eminent surgeons are declining the use of ether as much as possible. For the sake of etherization, itself, then, something like a pause is required, otherwise it is to be feared that the old empire of pain will return, and if so, it will seem for the future doubly difficult of endurance, because of the hopeful promise that, in surgical operations at least, it was conquered for ever.

It may be said, that in the dangerous and fatal cases the ether was unduly or improperly inhaled—and from an examination, it does appear that the ether was inhaled for a longer time than usual by some of the patients who died,—but in none does it appear to have been inhaled longer than was necessary to produce insensibility; and in Mr. Robbs' case, if we may believe that reflux movements were mistaken for movements of volition, complete insensibility was not present at any time during the operation. It would appear as though the ether occasionally followed an erratic course, not, as is usual, affecting the brain at all, but still going on to poison the spinal marrow and the ganglionic system, and to alter the condition of the circulating fluid. Indeed, so various have been the symptoms observed by different operators, that ether seems almost as volatile in its effects, as in its physical consitution.

In all I have said I am merely setting forth my individual opinion. I have given you the data upon which it is founded, and which, I believe will stand the test of examination. I should observe, in conclusion, that Professor Simpson is far more sanguine than myself in his views of the application of ether to midwifery. His last paper upon the subject ends in the following terms:

"I have stated that the question which I have been repeatedly asked is this: Will we ever be 'justified' in using the vapour of ether to assuage the pains of natural labor? Now, if experience betimes goes fully to prove to us the safety with which ether may, under proper precautions and management, be employed in the course of parturition, then, looking to the facts of the case, and considering the actual amount of pain usually endured, (as shown in the above descriptions of Merriman, Naegele, and others.) I believe that the question will require to be quite changed in its character. For instead of determining in relation to it whether we shall be 'justified' in using this agent under the circumstances named, it will become on the other hand necessary to determine, whether on any grounds, moral or medical, a professional man could deem himself 'justified' in withholding, and *not* using any such safe means, (as we at present pre-suppose this to be,) provided he had the power by it of assuaging the agonies of the last stage of natural labor, and thus counteracting what Velpeau describes as 'those piercing cries, that agitation so lively, those excessive efforts, those inexpressible agonies, and those pains apparently intolerable,' which accompany the termination of natural parturition in the human mother.

Baron Dubois concluded his address to the Academy of Medicine, as reported in The Lancet, in less glowing terms:—

"My profound conviction is, (he said,) that inhalation of ether in midwifery should be restrained to a very limited number of cases, the nature of which ulterior experience will better allow us to determine."

More recent experience has certainly not been in favor of enlarging the limits of its applicability in practical midwifery. In our own department there is good reason to believe, that after a short time, unless some certain mode of binding or disciplining this Prometheus shall be discovered, it will be rarely, if ever, used in difficult parturition or obstetric operations, and certainly never in natural labor. It will be a disappointment to have to turn from this promised good, but it is better to do so than to follow an ignis fatuus, if so it prove, to the neglect of real and scientific advancement. Let us hope that this glimpse of deliverance from this heavy infliction on humanity, may act as a stimulus to Science to continue her search after some certain and available relief from physical pain—possibly a visionary search,—but still one for which human nature will never cease to yearn.

AMERICAN MEDICAL INTELLIGENCE.

1.—*Observations on the Poisonous Properties of the Sulphate of Quinine.* By Wm. D. Baldwin, M. D., of Montgomery, Ala.

Under this title we find an interesting paper in the American Journal of the Medical Sciences, for April 1847. While the writer gives his testimony to the great value of this important remedy, he cautions the profession against the *abuse* of it, and adduces cases and experiments upon animals to show that it sometimes proves a deadly poison. He is far from being satisfied with the impression entertained by many, of its "*harmlessness* under all circumstances, and when given in any quantity." On the contrary he says, "we have in a few instances been advised of the baneful effects of quinine in producing deafness, amaurosis, hæmaturia, violent gastralgia, sudden prostration, delirium, epilepsy, palsy, &c., and under circumstances so obvious as to leave no doubt of its being the result of the poisonous operation of quinine."

After quoting Christisen and Orfila to prove that *almost all poisons act in exactly the same way on dogs and cats as on man*, he gives the following experiments which he made with quinine on dogs.

Experiments.—"These experiments were performed in the presence of different members of the faculty, and for valuable aid and assistance I am especially indebted to the kindness of Drs. Boling, Sims and McLester, of this place. I will not impose upon the reader a minute detail of each individual experiment, but will endeavour to condense the general results as concisely as possible.

"*Symptoms* which followed the ingestion of large doses of quinine into the stomach of dogs:—*restlessness* generally preceded all other symptoms, as was indicated by the animal changing its position often, and constantly moving from place to place. *Vomiting*, or, in those cases where the œsophagus was tied, efforts to vomit succeeded. *Purging* was noticed occasionally, but in no instance except where the medicine was taken by the stomach. Then came on *muscular agitation*, or *tremulous movements* of the body and extremities, with a *constant motion of the head*, resembling somewhat *paralysis agitans*. In attempting to walk, the dog would totter from side to side and fall, or if he maintained his feet would walk in a direction different from the one which he seemed to desire. When under the full operation of the poison, the power of locomotion, or even the power of standing in the erect position was lost altogether, the extremities apparently *completely paralyzed*. This state was accompanied with more or less *excitement of the vascular system*; the pulse increasing in frequency and rising from 110 to 160, and in one instance even as high as 240 per minute. *Great oppression of the breathing* was present and sometimes *frothing at the mouth*. The *dyspnœa* in all instances was excessive, sometimes panting, at others *slow* and *laboured*, resembling in a most striking manner an acute attack of asthma; countenance expressive of great distress and anxiety. The *pupils of the eyes were invariably dilated*, and generally to an enormous extent, leaving but a small ring of the iris perceptible, and *vision*, as well as could be judged, *was entirely lost*. *Convulsions* were observed in every case (except one,) which was watched to its termination, where the dose given was sufficient to produce death, and in one or two instances where the medicine failed to produce this result. *Furious delirium* was present in one case, as was manifested by the dog barking and biting at everything about him. Sometimes a *profound coma* would ensue, accompanied with slight *muscular agitation*, *slow* and *heavy breathing*, terminating in death in a very few minutes after the poison had been taken, and in a few instances the subject seemed as if stunned by some sudden and powerful blow or violent fit of apoplexy. This latter effect, however, was only observed when it was given to young dogs (half grown and under) through the jugular vein or peritoneum. Its effects upon puppies seemed to be *proportionately* much greater than upon dogs fully grown.

"The time required to produce death varied very greatly with the quantity given and the age of the subject, as well as the mode and manner of its administration, and in some instances it varied considerably when the dose, mode, and all other circumstances of its administration were supposed to be equal; for whilst in some instances fifteen or twenty grains produced the uniform and peculiar train of toxical symptoms, succeeded by death in a very short time; in other instances it required these quantities doubled and repeated until 120 grains had been taken, and a much longer time to produce the same results.—This fact is in accordance with my experience relative to its remedial action upon the human subject, showing that it is governed more, perhaps, in its modus operandi by inherent idiosyncrasies, or created predispositions, than any other remedy. The modes of giving it adopted, were by the stomach, the cavity of the abdomen, and by the jugular vein. When given by the stomach it produced vomiting, and was thrown back generally before a sufficient amount to produce death could be absorbed. By dissolving and largely diluting it with water, a sufficient quantity was absorbed to produce death, in this manner, in one instance. In almost all of the experiments with it by the stomach, however, the œsophagus was ligatured. When dissolved and given by the *stomach* its first effects were observable in about twenty minutes, sometimes shorter or longer, and death resulted in from one to thirty-six hours, usually in four or six. An empty stomach facilitated its operation greatly. When injected into the *peritoneum* in full doses (40 grs.) its effects were appreciable in from four to six minutes, and death occurred in from thirteen to thirty minutes. When injected into the *jugular vein* (in giving it by this mode great care was taken to prevent the admission of air,) its first effects were manifest in a space of time so short as to be almost inappreciable; not more than a few seconds after the nozzle of the syringe was withdrawn, and death occurred in one or two minutes. In all instances, except one, the quinine was dissolved in water by the addition of sulphuric or other acid in quantities barely sufficient for this purpose.*

"When the experiments went far enough to produce amaurosis, short of death, the vision was regained after a time. In one instance the dog remained *totally blind* for two weeks, and afterwards regained his vision slowly. This is also a feature in the second case reported in the commencement of this article. The man regained a very useful degree of vision after a short time. From these, as well as other causes of the kind reported it would seem that amaurosis from this cause is not likely to be permanent.

"Though it operated much more promptly when injected into a vein or the peritoneum, yet I did not observe that it operated with more power or force: that is, I did not discover that a given quantity administered in this way would produce death more certainly than when given on an *empty* stomach. 28 grains injected into the cavity of the abdomen in one instance, and 20 grains injected into the jugular vein in another, failed to produce death, yet these quantities *did* produce death in other instances, as well when given by the *stomach, as by these modes.*

"*The post-mortem appearances* were equally uniform with the symptoms before death. The most prominent and characteristic appearances were the *dark, fluid* and *defibrinated* condition of the *blood*, and the *congested* state of the

* In one instance where it was made into a bolus and enveloped in a slice of bacon and introduced into the stomach, vomiting occurred in twenty minutes, and a large portion of the medicine returned. Except that it did not produce death, its effects did not differ in this from those observed in other instances where it was dissolved and given by the stomach.

parenchyma of the *lungs*, resembling very much *red hepatization.* The vessels of the membranes of the brain were engorged, so also were the liver and kidneys in a few instances. The stomach and bowels were vascular and highly injected in patches The membranes of the spinal cord were more or less vascular and, in one instance, a semi-fluid coagulum of blood was found in the upper half of the theca vertebralis. This was probably owing to the subject being very young, and the convulsions being much more violent and frequent than in any other instance.

"Thus it seems clear that quinine is a *poison*, and one which may be made *directly fatal to life*, and if these experiments upon the dog, *in themselves*, are not conclusive of that fact, which the concurrent testimony of toxicologists would justify us in believing, they at least become so when it is remembered that the symptoms which its exhibition gave rise to, are not only strongly corroborated by, but were almost identically the same with those observed in the human subject, in the few instances where poisoning from this substance is known to have been produced. There is not a symptom noticed in these experiments which has not, at one time or other, been observed in its operation upon the human subject, and the two cases of poisoning in the human subject reported in the commencement of this article, where the same striking and peculiar assemblage of symptoms which followed its administration, were so completely identical with those observed in the dog, most clearly establish the fact that the manifestations of its *poisonous operation*, at least upon the dog, are identical with those observed in the human subject, or at any rate do not differ more than they do in different instances on "man and man."

"Its operation as a poison, as well as a remedy, is certainly peculiar, and it seems difficult to assign it to any particular class of poisons, differing in some respects from all of them. It appears to resemble in its action, more closely than any other, those of the "*second class*" of Orfila, or the class of "narcotic poisons." It does not seem to possess any hypnotic properties; in this it differs from most of the substances included under this head. I do not mean to touch the much agitated question of the mode of its remedial operation, but desire to speak of its poisonous action only; and, on this head will only add, farther, that its operation seems to be principally upon the nervous system, as is clearly demonstrated in the derangement of the senses of vision and hearing, and respiratory functions, as also in the general muscular agitation, convulsions, &c. As it has been detected in the urine there can be no doubt but that it enters and mixes with the circulating masses of the body, and through this means exerts a *direct* influence upon the nervous system, which, as we have seen, is *eminently* excitant when given in quantities calculated to destroy life."

Dr. B. says he knows of "no antidote which will, with any certainty, negative the injurious effects of quinine." He suggests a prompt emetic and such other means as will be calculated "*to augment the natural secretions of the body.*" He is of opinion that "its poisonous effects may generally, if not always be avoided by proper attention to the mode of its administration." He condemns the customary method of *pills* and *mucilaginous mixtures*, and prefers decidedly a solution in acidulated fluid. Although he does not deny the claims set up for it by Dr. Boling, in inflammatory affections, and even admits that on "innumerable occasions" he has seen it manifest "a most decided and unmistakeable *contra stimulant effect*," yet he has sometimes seen it prove a *stimulant.*

Dr. B. rarely gives more than 10 grains at a dose; most generally he gives from four to six grains every 2, 3, or 4 hours until 24 or 36 grains are introduced into the system.

As to the extent to which the article may be given with impunity, Dr. B. remarks:—

"From all that I can gather I am disposed to think from fifty to eighty grains of a pure article of quinine, given in solution at one dose, will produce death nine times out of ten, in healthy adults, and occasionally even smaller quantities. How far its operation may be modified by morbid action is a matter for consideration at the bed side."

Such are some of Dr. Baldwin's views of quinine. We give them for what they are worth; our Southern readers are capable of judging of them.

2.—*A Case of Inhalation of Ether in Instrumental Labor.*—By W. Channing, M. D.

To the Editor of the Boston Medical and Surgical Journal.

Dear Sir,—I beg leave to offer you the following case for publication. It is indeed but a single instance of the use of ether in midwifery practice; still, such is the importance of that discovery which has abolished pain in so many, and in such a variety of cases—and such the state of opinion, and such the popular and professional interest, in everything bearing usefully on the subject, that I venture to present it in an amount of detail which otherwise might seem unnecessary. To my mind, in the present position of this great discovery, this is the most proper method of communicating such facts.

I look back on the occurrences of this trial of ether with entire satisfaction, and with the deepest pleasure. The ether did just what was looked for from its use. It did it at once, and with no circumstances of embarrassment or difficulty. When its influence was no longer needed, its effects passed quietly away, and left a repose—a continued sense of relief, which, in an equal degree, and like kind, I do not remember to have witnessed before. I shall with pleasure communicate through your Journal the results of such farther trials of ether as circumstances may seem to authorize me to make. And no one should venture upon such trials until he is perfectly satisfied that such circumstances exist. A case came under my observation this day, which impressed upon my mind very strongly the importance of this rule of practice. It was one of unusual severity, and the time of suffering was long. Still there were circumstances in the previous history of my patient, and in her actual condition, which detered me from taking ether with me. Such, however, at length, was the urgency, I may say violence of demand for relief, on any terms, and for the use of ether especially! that I sent for it. I felt that the moral conviction, always so powerful in labor, that relief would be obtained from this agent, might revive hope, and give encouragement, where a most depressing despair existed, and that thus the labor might be naturally terminated. Whether my reasoning were correct or not, I can say, that almost immediately (after the messenger was despatched, efficient uterine contractions came on, which speedily, and safely, accomplished delivery. I remain very truly yours,

W. C.

Boston, May 11, 1847.

Mrs. H., aged 23, was taken in labor, for the first time, May 5th, at 12 o'clock at night. I saw her between 9 and 10, of the morning of the 7th, in consultation with her medical attendant, Dr. W. E. Townsend. His pupil, Mr. Jerome Dwelley, was present, and who also from the beginning had faithfully attended to the case. The pains had been frequent and very severe.—Some diminution of suffering had followed the exhibition of an opiate, which had been given before I saw the patient. Patient was well purged with castor oil the day before labor. I found, on examination, the head fairly in the pelvis, where, I was told, it had been many hours. There was no *show*. The vagina was swollen, rough, hot, especially about the urethra, or anterior part of the pelvis. The os uteri was somewhat dilated, but less in its anterior portion than elsewhere, though in no part of its circumference had it cleared the head. It was swollen, smooth, hard, undilatable. It gave just that feel which so strongly intimates that the labor will be protracted, and accompanied by much suffering. The scalp was much swollen, and protruded as a tumor of a conical shape through the firm ring formed by the undilated and undilatable os uteri.

Mrs. H. was comparatively easy, from the opiate apparently. Her pulse was natural. Her strength was not much exhausted. Her stomach bore food well. There was no cerebral trouble, and the bladder had been duly emptied by the catheter. Under these circumstances I suggested delay; and it was agreed to wait to observe the changes which might occur in the present rest, and on the recurrence of pains. I saw her again at noon. Belladonna ointment was recommended, as no important change had occurred in the state of the os uteri. I was called to see her at about 6 P. M., about forty-two hours since labor began. I learned, on reaching the address, that the ointment had been used, and a solution of tartarized antimony exhibited, and that some change had occurred in the os uteri, namely, that it was more dilatable. Her pulse was now 120 in the minute. It was less strong than at noon. She could speak only in a whisper, and with great difficulty even so. She complained of great distress and most earnestly entreated to be relieved of her terrible suffering. On examination I found the os uteri somewhat more dilatable, and it was agreed that the forceps should be used.

Dr. Townsend called on me to make the visit just related. I said to him, in my study, that this seemed a very fair case for the use of ether. He agreed with me in this opinion, and added that he had a quantity of pure ether at home, and a sponge of suitable size for its inhalation, and that he would meet me at his patient's house. We soon met there, and I proceeded to apply the forceps. I selected Davis's solid forceps, because they are narrow, thin and very easily introduced, and seemed less likely to injure the os uteri than a broader and a thicker instrument. The application was perfectly easy, and I made an extracting effort, which was attended with very severe pain. Mrs. H. soon became quiet, and I desired Dr. T. to apply the sponge, saturated with ether, to the mouth and nose. This he did, and in about a minute she was under the full influence of the ether. The first inspiration produced a slight cough, as if the larynx had been irritated. It was like the sound by which an effort to remove some irritating matter from the air-passages is commonly accompanied. The next noticeable effect, and which was quite an early one, was a sudden movement of the body, such as is made sometimes when one is falling asleep, and has consciousness enough to know this, and to rouse the will into sufficient action to prevent it. It was involuntary, still it did not convey the idea of being spasmodic, in any morbid understanding of the term. She was directed to open her eyes, to answer questions, &c., but gave not the least evidence of consciousness of anything said. I now proceeded to extract. The os uteri at once came down again, and much embarrassed the operation, so that I desired Mr. Dwelley to pass his fingers between the shoulders of the forceps and the symphysis pubis, and gently press the protruding os uteri upwards. He did so, and thus removed that part from the chance of injury. The extraction was continued at intervals. Not the smallest complaint was made. The womb was roused to action, and strong expulsatory efforts were made. The head advanced, and everything promised well. But at length the head became again firmly fixed, and this to a degree which prevented its being removed by any such force as I believed it safe to employ. I removed the forceps. The effects of the ether passed off, but as soon as consciousness returned, most earnest demands were made for more. "Put it to my mouth—I shall faint—you must"; in short, all forms of entreaty were made use of to obtain the entire relief that the ether had produced. She had at first refused to employ it. The ether had now been used up, and a short delay took place while a further supply was sent for. I perforated the cranium, fixed the hook, and made some extracting effort. Again was complaint made of the suffering which was immediately produced by the traction. The repose had been entire since consciousness had returned. She thought she was delivered. Said that she had *sense*, knew that she was alive, after the sponge was put to her mouth, but that she had no *feeling* after, and knew not what had happened. She had passed the time in most entire freedom from all pain. She said that there had been light before her eyes, and buzzing in her ears, and that she had been in another world. The aphonia had entirely disappeared, and her voice was natural.—The ether was again applied to the mouth and nose, and when it was ascertained that its full effects were present, extracting effort was made by the hook. Again did the womb act, and the head advanced. Its progress was very slow. Much effort was demanded to bring the head along. The ether was used several times before the labor was over. In fact, she was most of the time inspiring the vapor, largely mixed with atmospheric air, for her pillow and bed-clothes were necessarily kept wet with it, from the mode of using it. There was no accident, or the least untoward circumstance attending the delivery.—There was no pain—no complaint—no resistance of the effort used for delivery. The limbs were perfectly flaccid, and it was necessary that they should be kept separate by an assistant, and the whole weight of the upper one was to be supported. She came to herself soon after the child was born, and again expressed her entire ignorance as to everything that had been done. The placenta was separated, and reached the outlet by the unaided efforts of the womb, and no hemorrhage followed. A swathe was applied to the abdomen, and the patient made comfortable in her bed. I left soon after, having ascertained that her pulse was as good as it had been for some hours, and that everything promised well. It was impossible to determine what injury, if any, so long-continued pressure of the head had produced. The bladder had been carefully attended to, and the least possible amount of examination, I was told, had been made during the whole attendance on the case. The child had been dead some hours.

May 8th, 9, A. M.—I learned that soon after I left. the womb expelled from its cavity a large mass of coagula, with a gush of liquid blood. Cold was immediately applied to the abdomen, and the flow ceased. It was not so great as to affect at all her strength, or her pulse. I learned that she had passed an excellent night, and had slept as tranquilly as if under the kindest influence of opium. Her pulse was 108, of good strength and volume—tongue moist, head clear, and her whole state perfectly comfortable. We were particularly struck with these facts, in the distinct recollection of the long-continued suffering which a short time before had been endured. She had passed no water. The catheter was introduced with great ease, but got clogged with blood in its passage, so as to draw very little, if any urine. Mrs. H. said soon after that she felt a strong inclination to pass water, and in making an effort to do so there was expelled from the vagina a firm coagulum, and immediately after the urine followed voluntarily, and with perfect relief. Directions were given that the greatest quiet should be preserved, and sleep encouraged. Liquid farinaceous diet was ordered.

9th, A. M., 9 o'clock.—Mrs. H. slept most of yesterday, and less well last night. That is, was awake, but comfortable first part of night, slept latter part. Pulse now 104. Skin natural. No pain in abdomen, and no tenderness on pressure. Urine natural. Somewhat thirsty. Tongue slightly dry. No appearance of milk.

10th, 10, A. M.—Patient very comfortable. Pulse 108. Skin warm.—Breasts distended and painful. Abdomen soft. Two dejections from ℥ ij. ol. ric., and as much lemon juice. In all respects doing well.

Remarks.—The ether was applied by a sponge. It was very easily applied. The effect was produced very soon, in about a minute, say after about fourteen respirations, and when consciousness was returning, one or two respirations were enough to procure insensibility. The room, or the atmosphere about the patient, was saturated with ether. Was there not danger of explosion had a candle or lamp been brought into this atmosphere? I have heard of experiments which were designed to prove that this fear is groundless. I have not seen them, and should be unwilling to act in accordance with them. In the knowledge that equal parts of the vapor of ether and atmospheric air, produce a compound as explosive as hydrogen and oxygen, he who uses ether at night should be most cautious to keep a lighted candle or lamp at a distance from the patient. As our midwifery arrangements so frequently occur at night, this may sometimes be an inconvenience. We cannot examine the pulse or the countenance during the use of ether, which it is very desirable to do. But we had better lose such opportunity, than incur the least risk of the explosion of the gas.

Cases are reported of instrumental labor in a Paris hospital under the use of ether, which were fatal by the supervention of puerperal fever. But this result will hardly be ascribed to the ether used, or be made an objection to its use elsewhere, as puerperal fever existed at the time in the hospital, and everybody who knows anything of the disease, must be aware how readily it extends itself from patient to patient, especially in hospitals. It is said that this is especially true of the hospitals in Paris. I have not in memory a case of instrumental labor of so much severity as this above reported, from which recovery was so rapid, or so complete, and in which suffering was so slight. I do not recollect that a complaint was made of any suffering, from the time of the inhalation to the day on which I made my last visit.

Not only in Paris, but in Edinburgh also, has this method been tried in labor. To no one is the profession more indebted than to Dr. Simpson, Professor in the Edinburgh University, on this behalf. I quote from Forbes's Medical Review, the latest No., the leading authority in medical literature in Europe, the following on the subject. I do it for the facts to which it refers, and especially for the caution with which the information is accompanied. From the same Review I make an extract which represents the opinion of Dubois, with an important remark from the reviewer.

"In a communication which we have received from Edinburgh, dated the 22d of March, Dr Simpson states that he had, up to that date, used etherization some forty or fifty times, with the most perfect safety and success. We understand that he has kept it up *for hours*—in one woman four, in another six hours—without the fœtal heart varying above ten or twelve beats during the whole time, the mother in both cases recovering perfectly, and both, of course, astonished at being delivered without being aware of it. We believe that Dr. Simpson, in making these statements, still inculcates caution in the use of the new means; justly regarding all his own trials hitherto, bold as they are, as merely experimental, and as only first fruits which, however delightful and promising, may not be the positive harbingers of an abundant and a wholesome harvest." P. 568.—*The British and Foreign Medical Review, edited by John Forbes, M. D., F. R. S., &c. &c., No.* 46, *April*, 1846.

"M. Dubois's opinion is, on the whole, not in favor of the employment of ether in midwifery, although he admits that he has seen no ill effects that he could, with certainty, attribute to it. He thinks, 'that it should be restrained to a very limited number of cases, the nature of which ulterior experience will better allow us to determine.' He, however, confesses that the result of the cases he has treated in this manner, has lessened the fears with which he originally entered on the trial. We leave the Professor and the Baron—the doughty champions and learned representatives of the obstetrics of Paris and Edinburgh—to fight the battle between them. Time, at least, will ere long determine which of the two is in the right. We are disposed to believe that neither is absolutely so; and that here, as in many other instances of clashing opinions, the truth lies between." *Ib. p.* 569.

The action of the womb in the above case, in the absence of all voluntary agency, was very striking. Not only was there natural expulsatory effort, which was aiding the manual, but the effort was marked occasionally by its usual audible expression, the *bearing down* which is so well known. I was reminded of this effort during insensibility, by a case of most severe puerperal convulsions, which came under my notice the day after the above case. The organic effort, in the entire abolition of voluntary power, was most striking. I have known the child born by this organic agency, without the least apparent consciousness of the event on the part of the mother at the time, or memory of it afterwards. In this fact, established by so many, and so varied observations at home and abroad—in this fact of efficient uterine action, produced by a well-known agent, ether, and the use of which has thus far been so safe, and the application and *modus operandi* of which, a wider observation will do more and more to determine—may we not in these facts look with confidence to the time when labor will be accomplished with an ease, a freedom from suffering, quite as great as has hitherto been the pain which has accompanied it, and which has been regarded as its necessary condition?

P. S.—In a subsequent No. of the Boston Journal, Dr. Channing reports a case of Laborious Labour in which the ether was used with perfect success.

3.—*The National Medical Association.*

It appears that this august sssemblage of the Representatives of the Medical Profession of the United States met in Philadelphia, on the 5th of May last, according to previous adjournment, and went through their proceedings with becoming dignity, harmony and ability. We have not yet had the pleasure of receiving a copy of their report, but as it will probably be too long for insertion in our Journal, we furnish our readers the following interesting summary, taken from a *Supplement to the Medical News and Library*, and the names of the members of the standing committees, taken from the *Medical Examiner*. We are pleased to find the Transactions of the Convention spoken of by the Northern Journals, several of whose editors were present, in the most complimentary manner. They contain some of the reports of the late standing committees, which are certainly very creditable. It must necessarily require some time to develope the full influences of this great Medical Society. Each meeting will doubtless be attended with increasing interest, and we do not despair of seeing the most beneficial results spring from their deliberations. In a future number we may say something about the reports of the late committees.

"The National Medical Convention held its sessions in Philadelphia on the 5th, 6th, and 7th of May, in the magnificent hall of the Academy of Natural Sciences. The number of delegates appointed was three hundred and twenty, of whom two hundred and thirty-three were in attendance, and every State in Union was represented, except Maine, Alabama, Arkansas, Iowa, Florida, Texas and North Carolina.

Dr. Isaac, Hays of Philadelphia, chairman of the committee of arrangements, on the part of the Philadelphia delegation, opened the proceedings with a few remarks, in which he welcomed the delegates to the convention, and expressed the pleasure which his delegation experienced in receiving the members of the convention as their guests. He then proposed for the purposes of a temporary organization, that Dr. J. Knight, of New Haven, who had presided with so much dignity, ability and impartiality at the last convention, should be appointed chairman, which nomination was unanimously confirmed.

Dr. Arnold, of Georgia, and Dr. Stillé, were then appointed Secretaries.

A committee was appointed to examine the credentials of members, which committee have reported—a committee of one from each State was appointed to nominate officers of *the convention*. This last committe reported the names of the following gentlemen, as permanent officers of the Convention; Dr. J. Knight, of Connecticut, President; Alexander H. Stevens, of New York, George B. Wood, of Philadelphia, A. H. Buchanan, of Tennessee, John Harrison, of Louisiana, Vice Presidents; R. D. Arnold, of Georgia, A. Stillé, of Philadelphia, F. C. Stewart, of New York, Secretaries.

The Convention unanimously agreed to the nominations.

The Convention being organized and ready for business, a motion was made that medical gentlemen who might be present from States not represented, be authorized to take seats in the Convention; and that members of the medical staff in the army and navy have the same courtesy extended to them, which was carried by acclamation.

The report of the committee appointed at the last Convention, to report a plan of organization for a National Medical Association, was called up and read. On a subsequent day, the plan, after much discussion, was adopted.—The name of the socioty is "The American Medical Association."

The members to consist of delegates from medical societies, and medical instiutions to be appointed annually, members by invitation and permanent members, the latter consisting of those who have served as delegates.

Each local society has the privilege of sending to the association one delegate for every ten of its regular resident members, and one for every additional fraction of more than half of this number. The faculty of every regularly constituted medical college or chartered school of medicine, has the privilege of sending two delegates. The professional staff of every chartered or municipal hospital containing a hundred inmates or more, has the privilege of sending two delegates; and every other permanently organized medical institution of good standing, has the privilege of sending one delegate.

The Members by invitation consist of practitioners of reputable standing, from sections of the United States not otherwise represented at the meeting, to receive their appointment by invitation of the meeting after an introduction from any of the members present, or from any of the absent permanent members. They are to hold their connection with the association until the close of the annual session at which they are received; and be entitled to participate in all its affairs, as in the case of delegates.

The Permanent Members to consist of all those who have served in the capacity of delegates, and of such other members as may receive the appointment by unanimous vote. They may share in the debates, but cannot vote.

The officers are a Presidsnt, four Vice Presidents, two Secretaries and a Treasurer.

Standing Committees, each composed of seven members, are to be organized at every annual meeting, for preparing, arranging, and expediting business for each next ensuing year, and for carrying into effect the orders of the Association not otherwise assigned—namely, a Committee on Arrangements, a Committee on Medical Sciences, a Committee on Practical Medicine, a Committee on Surgery, a Committee on Obstetrics, a Committee on Medical Education, a Committee on Medical Literature, and a Committee on Publication.

No amendment or alteration can be made in the plan of organization, except at the annual meeting next subsequent to that at which such amendment or alteration may have been proposed; and then only by the voice of three-fourths of all the members in attendance.

Dr. J. H. Griscom, of New York, chairman of the committee appointed at the last meeting, to whom was referred the consideration of the expediency, and if deemed expedient, the mode of recommending and urging upon the several State governments the adoption of measures for a registration of the births, marriages and deaths of their several populations, made a report favorable to the subject, to which was attached a series of resolutions, pointing out the means by which the desired end can be attained, and an address to the State governments. The report was accepted, and the resolutions and memorial adopted. The resolutions were as follows: *Resolved*, 1st. That it is expedient for this Convention to recommend to, and urge upon, the various State governments, the adoption of measures for procuring a Registration of the Births, Marriages and Deaths occurring in their several populations.

2d. That a Standing Committee be appointed by the Convention to take a general charge of the subject, and report annually to the Convention.

3d. That the State Medical Societies be requested to assume the duty of carrying out the objects embraced in the first resolution; and that in those States where no organized societies exist, the delegates therefrom in the present Convention, be charged with the duty for their respective States, and report to the Standing Committee.

4th. That in procuring the Registration, the forms and nomenclature adopted should be, as nearly as possible, similar to those prepared for, and reported to, the Convention.

5th. That the paper hereto annexed, be adopted as the voice of the Convention, be printed, and signed by its officers, and transmitted under their direction to all the State governments of the Union.

Dr. Robert W. Haxall, Chairman of the Committee appointed at the last meeting to report a uniform and elevated standard of requirements for the degree of M. D. for all the Medical Schools in the United States, made a report urging judicious reform, and enforcing the necessity of clinical instruction, to which was attached a series of resolutions which were subsequently amended and adopted as follows:—*Resolved,*

1st. That it be recommended to all the Colleges to extend the period employed in lecturing from four to six months.

2d. That no student shall become a candidate for the degree of M. D. unless he shall have devoted three entire years to the study of medicine, including the time allotted to attendance upon the lectures.

3d. That the candidate shall have attended two full courses of lectures, that he shall be twenty-one years of age, and in all cases shall produce the certificate of his preceptor to prove when he commenced his studies.

4th. That the certificate of no preceptor shall be received who is avowedly and notoriously an irregular practitioner, whether he shall possess the degree of M. D. or not.

5th. That the several branches of medical education named in the report be taught in all the Colleges, and that the number of Professors be increased to seven.

6th. That it be required of candidates that they shall have steadily devoted three months to dissections.

7th. That it is incumbent upon preceptors to avail themselves of every opportunity to impart clinical instruction to their pupils; and that Medical Colleges require candidates for graduation to show that they have attended on Hospital practice for one season, whenever it can be accomplished, for the advancement of the same end.

8th. That it be suggested to the faculties of the various medical institutions to adopt some efficient measures for ascertaining that their students are actually in attendance upon their lectures.

9th. That it is incumbent on all Schools and Colleges granting diplomas, fully to carry out the above requisitions.

10th. That it be considered the duty of preceptors to advise students to attend only such institutions as shall rigidly adhere to the recommendations herein contained.

Dr. Couper, of Delaware, from the committee appointed at the last meeting, to whom was referred the subject of a suitable preliminary education, made a report, concluding with the following resolutions, which were subsequently adopted: *Resolved,*

That this Convention earnestly recommends to members of the medical profession throughout the Uniied States, to satisfy themselves, either by personal enquiry or the written certificate of competent persons, before receiving young men into their offices as students, that they are of good moral character, and that they have acquired a good English education, a knowledge of Natural Philosophy and the Elementary Mathematical Sciences, including Geometry and Algebra, and such an acquaintance, at least, with the Latin and Greek languages, as will enable them to appreciate the technical language of medicine, and read and write prescriptions.

Resolved, That this Convention also recommends to the members of the medical profession of the United States, when they have satisfied themselves that a young man possesses the qualifications specified in the preceding resolution, to give him a written certificate, stating that fact, and recording, also, the date of his admission as a medical student, to be carried with him as a warrant for his reception into the medical college, in which he may intend to pursue his studies.

Resolved, That all the medical colleges in the United States be, and they are hereby recommended and requested to require such a certificate of every student of medicine applying for matriculation; and, when publishing their annual lists of graduates, to accompany the name of the graduate with the name and residence of his preceptor, the name of the latter being clearly and distinctly presented, as certifying to the qualification of preliminary education.

Dr. Bell, of Philadelphia, from the committee appointed at the last meeting, to report a code of Medical Ethics for the government of the profession, stated that the report consisted of two parts, viz :—an introduction, and such a code as the resolutions called for; the first was not ready at present, but that he would take a future opportunity to read it, which was subsequently done.

Dr. Hays, from the same committee, presented the code, which, at a subsequent stage of the proceedings, was adopted.

On motion of Dr. N. S. Davis, of New York, it was—

Resolved, That a committee of one from each State represented in the Convention, be appointed, whose duty it shall be to investigate the *indigenous medical botany* of our country, paying particular attentions to such plants, as are now or may hereafter, during the time of their service, be found to possess valuable medicinal properties, and are not already accurately described in the standard works of our country, and report the same in writing, giving not only the botanical and medical description of each, but also the localities where they may be found, to the next annual meeting of the American Medical Association. Laid on the table.

Dr. McNaughton, of Albany, from the committee, to whom had been referred the resolution offered at the last Convention, which states, "That the union of the business of *Teaching and Licensing* in the same hands, is wrong in principle and liable to great abuse in practice. Instead of conferring the right to license on Medical Colleges, and State and County Medical Societies, it should be restricted to one Board in each State, composed in fair proportion of representatives from its Medical Colleges, and the profession at large, and the pay, for whose services as examiners, should in no degree depend on the number licensed by them," made a report in reference to the subject, mainly sustaining the above resolution. This report, however, states that the committee do not desire to say, that the union referred to is wrong in *principle*—the objectionable conduct, which may have occurred, is, in their opinion, attributable to some other cause. The committee in all other respects express their accordance with the sentiments contained in the resolution. The report and the accompanying resolutions were ordered to be printed.

Dr. Parrish, of Philadelphia, from the same committee, also submitted a majority report, and in opposition to a change in the present order of things in relation to licensing, and recommending that some additional checks be put upon the excercise of the right.

These two last reports were referred to the committee on education, to report at the meeting of the association in May next.

Dr. Griscom, of New York, from the committee to prepare a nomenclature of diseases adapted to the United States, having reference to a general registration of deaths, made a report containing some interesting comparative statistical information in regard to various diseases, and concluded with a deserved tribute of praise to Mr. Lemuel Shattuck, of Boston, who drew up the report. The report was ordered to be printed.

On motion of Dr. Pierce, of Maryland, the following resolution was adopted:

That the members of this convention be requested to ascertain, as far as may be practicable, and report to the next annual meeting, the number of practitioners of medicine in their respective States, designating the number who may have received a diploma from a Medical College, the number who may have been licensed by a Médical Society, and the number who practice medicine without any authority whatever.

On motion of Dr. J. V. C. Smith, of Boston, it was

Resolved, That the thanks of this Convention are due to the officers and directors of the various institutions, who have politely invited the members to visit them at their own convenience—to the committee of reception and arrangements, on behalf of the Philadelphia delegation, for the spacious and elegant accommodations provided—and to the whole medical profession of the city, for the marked kindness, personal attention, and generous hospitality which have characterized their intercourse with this body since the commencement of its deliberations—and to the Academy of Natural Sciences for the use of their room.

It was *Resolved*, on motion of Dr. Garvin, that the thanks of the committee be presented to its officers, for the very efficient manner in which they have discharged the onerous duties imposed upon them.

On motion of Dr. F. C. Stewart, of New York, it was then

Resolved, That all unfinished business be referred to the American Medical Association about to be organized.

Resolved, That this convention, do now resolve itself into the "American Medical Association," and that the officers of the convention continue to act as officers of the association until others are appointed.

A committee was then appointed, consisting of one member from each State, to nominate officers for the American Association; this committee reported the names of the following gentlemen, as officers of the "National Medical Association."

Dr. Nathaniel Chapman, Penn., President.

Dr. J. Knight, of New Haven, A. H. Stevens, of New York, Dr. Moultrie, of South Carolina, Dr. Buchanan, of Tennessee' Vice Presidents.

Drs. Stillé, of Philadelphia, and Dunbar, of Baltimore, Secretaries.

Dr. I. Hays, of Philadelphia, Treasurer.

On ballot these gentlemen were declared unanimously elected to their several offices, as above mentioned.

A committee was then appointed, to wait on the President elect, and inform him of his election.

This committee introduced Dr. Chapman, and escorted him to the Presidential seat. On taking which, he made some feeling remarks. He said he could find no language to express the depth of his gratitude. It had often been his good fortune during his professional life to have been complimented in the same manner, though not in the same degree. This was, he confessed his incompetency to serve the Association as he could desire. He said he loved his profession, and should be ungrateful if he did not: whatever he possessed in his llfe, had been bestowed by its favors; when he forgot it, or deserted it and its disciples, he remarked with great emphasis, may Almighty God forget and desert me. He desired that the association should be persuaded of his ardent wishes for the cause, and that his most strenuous efforts would be unceasingly directed to advance the dignity of the profession, and extend its usefulness.

The association then adjourned, to meet in Baltimore, on the first Tuesday in May, 1848.

The deliberations, some of which were animated, were marked by the most perfect courtesy and good feeling, and the members separated mutually pleased with each other, and prouder than ever of their profession."

"At a meeting of the President and Vice Presidents of the "American Medical Association," held on May 8th, 1847, the following Standing Committees were appointed in pursuance of the order of the Association:

Committee on Arrangements.—Dr. G. C. M. Roberts, Chairman: Dr. A. C. Robinson, Dr. J. H. Briscoe, Dr. J. R. W. Dunbar, Dr. Wm. Power, Dr. W. T. Leonard, Dr. C. Bell Gibson, Baltimore.

Committee on Medical Sciences.—Dr. S. Henry Dickson, Chairman: Dr. J. P. Jervy, S. C.; Dr. Robert Bridges, Phila.; Dr. J. W. Francis, N. Y.; Dr. Wm. T. Wragg, S. C.; Dr. Wm. Power, Balt.; Dr. T. Romeyn Beck, N. Y.

Committee on Practical Medicine.—Dr. Joseph M. Smith, N. Y., Chairman: Dr. René La Roche, Phila.; Dr. John Harrison, La.; Dr. H. M. Bullitt, Mo.; Dr. J. B. Beck, Dr. Isaac Wood, Dr. G. S. Camman, N. Y.

Committee on Surgery.—Dr. George W. Norris, Chairman: Dr. Isaac Parrish, Phila.; Dr. John Watson, N. Y.; Dr. A. L. Peirson, Salem, Mass.; Dr. Jacob Randolph, Phila.; Dr. H. H. M'Guire, Petersburg, Va.; Dr. C. Bell Gibson, Balt.

Committee on Obstetrics.—Dr. Harvey Lindsley, D. C., Chairman: Dr. G. C. M. Roberts, Balt.; Dr. J. Riley, D. C.; Dr. R. W. Haxall, Richmond, Va.; Dr. W. Channing, Boston,; Dr. C. R. Gilman, N. Y.; Dr. S. Annan, Lexington, Ky.

Committee on Medical Literature.—Dr. Oliver Wendell Holmes, Chairman: Dr. E. Hale, Dr. G. C. Shattuck, Jr., Boston,; Dr. D. Drake, Louisville, Ky.; Dr. John Bell, Phila.; Dr. Austin Flint, Buffalo,; Dr. W. Selden, Norfolk, Va.

Committee on Medical Education.—Dr. Alex. H. Stevens, Chairman: Dr. Amos Twitchell, Keene, N. H.; Dr. B. R. Wellford, Fredericksburg, Va.; Dr. Arnold Naudain, Phila.; Dr. R. D. Arnold, Savannah,; Dr. F. Campbell Stewart, N. Y.; Dr. L. P. Bush, Wilmington, Dela.

Committee on Publication.—Dr. Isaac Hays, Chairman: Dr. Alfred Stillé, Phila.; Dr. J. V. C. Smith, Boston,; Dr. J. P. Garvin, Augusta, Ga.; Dr. J. R. W. Dunbar, Balt.; Dr. Gouverneur Emerson, Phila.; Dr. Caspar Morris, Phila.

Committee on Indigenous Botany, under the Resolution of Dr. N. S. Davis. Dr. N. S. Davis, Binghamton, N. Y., Chairman: Dr. S. W. Williams, Mass.; Dr. Eli Ives, Conn.; Dr. Engleman, Mo.; Dr. W. A. Cheetham, Tenn.; Dr. Jos. Carson, Pa.; Dr. Charles Short, Ky.; Dr. E. E. Phelps, Vt.; Dr. A. Twitchell, N. H.; Dr. T. C. Dunn, R. I.; Dr. Lyndon H. Smith, N. J.; Dr. James Couper, Del.; Dr. A. C. Robinson, Md.; Dr. Frederick Marx, Va.; Dr. J. P. Porcher, S. C.; Dr. J. Le Conte, Ga.; Dr. Cartwright, Miss.; Dr. Carpenter, La.; Dr. —— —— Ohio,; Dr. G. Norwood, Ind.; Dr.—— ——Ill.; Dr.—— ——Mich.; Dr. —— ——D. C."

NEW ORLEANS, JULY 1, 1847.

OUR FOURTH VOLUME.

With this number we commence a new volume of our Journal. For more than three years we have labored unremittingly to establish and maintain a Medical Journal in the city of New Orleans, where none existed when we undertook the task; nor indeed was there one in operation throughout the whole Southern States at the time. Since we commenced ours, two able cotemporaries have appeared on the field, and done good service. We hope and trust they have received a more *substantial reward* for their labors than has fallen to our lot. If not, it may be said to consist alone in the *consciousness of trying to do something useful for the medical profession and the good people of the South.* Such is absolutely the fact as to ourselves; we barely receive enough annually to defray the expense of publication, and we make the statement, not for the purpose of lauding our own sacrifices of time and labor, but to remind some of our subscribers that they should not take offence at being called on for the amount of subscription. On reviewing the labors of the past year, we have only to regret that we have not performed the duties assumed, with more ability. We return our grateful acknowledgments to those kind friends who have signified their approbation of our humble efforts, and *especially to those who have aided us with their pens.* We have never set ourselves up, in our editorial capacity, to be *teachers* or *rulers* in the profession; our highest aspiration has been to establish a medium of intercommunication through which the physicians of the South should be kept advised of the progress of medical science, and likewise make known the results of their own observation and experience. As to the importance of the communications which have appeared in this Journal, it is not proper for us to speak; but if we may be permitted to judge by the respectful attention they have attracted abroad, we may at least conclude they have not been *discreditable* to their authors. We have ever believed that the *respectability* of a Medical Journal must depend upon *the amount and importance of its original matter*. Under this impression, we have made it a point to insert all communications that we deemed worthy. The result has been that, owing to our contracted limits, but a brief space has been allowed us for *quotations*. We trust that in this will be found a satisfactory apology for our apparent oversight of the numerous valuable papers which have appeared among our respected American cotemporaries. If we had more space, it would afford us much pleasure to aid them in the diffusion of their light. We renew the invitation to our friends throughout the country, *to write*, because we know it is the most powerful incentive *to study*; for there is pride enough in every American to make him desire to *do well*, whatever he attempts to do *publicly*. It is not presumed that the physicians of the villages and neighborhoods can enlighten the world in regard to the *elements and principles* of medical science. Their sphere is the *application of principles to practice.* To them belongs *the test* of all the aphorisms and dogmas proclaimed *ex cathedra*, and they may do important service by contributing facts and correct observations, which, after all, constitute the basis of the science.

To our city friends, particularly those connected with the Hospitals of New Orleans, we feel that we have a right to look for aid, for they can but be interested in the success of *their own—their only* Medical Journal. To our colaborators generally, and especially our obliging correspondents *on the health of the country*, we return thanks for their generous assistance and beg a continuance of their favors.

One word to the Medical Societies of New Orleans, two of which have been in successful operation for several years—we should be pleased to publish their proceedings, concisely drawn up; and are confident it would be followed by the happiest results. It would doubtless infuse new life and energy into their deliberations. Our Medical College and Societies occupy a commanding position in this great emporium, and have the power to do the community much service and themselves much honor, if they perform their duties. New Orleans must become not only a *prominent seat* of medical learning, but the *focus* of the highest order of medical and surgical practice to a vast and densly populated country around. It combines all the necessary facilities and resources, and nothing is required but talent and energy for their development. We invoke our fellow-laborers who at present occupy the field of action, to stand up to the work with manly determination, lest we live to see ourselves *outstripped and superseded* by the rising generation, impelled by the irresistible spirit of progress and reform.

With these remarks, we close the introduction to the *fourth volume* of our Journal, trusting that we have conducted the work on the principles proclaimed at its starting, and assuring our readers that with their co-operation it shall continue to be sustained.

HEALTH OF THE CITY.

Our city presents a singular spectacle at the present time. Whilst every one of its hospitals is full to overflowing, and several additional houses have been appropriated to hospital purposes, we are sure our citizens never enjoyed a greater degree of health at this season of the year. Indeed, as is customary, there is less sickness among them now than on the 1st of May, when we last went to press. Sickness generally decreases here as the summer advances, unless yellow fever becomes epidemic; and, with the exception of this occasional visitation, the summer, contrary to the prevailing opinion abroad, is always the healthiest season of the year in New Orleans. Whence then the host of invalids now crowding our hospitals? *It consists of Foreign Immigrants (chiefly Irish) and the sick and discharged soldiers returning from the United States Army in Mexico.* The foreign immigrants are the most miserable looking set of poor, half-starved wretches, worn out by the combined horrors of a long sea voyage, ship fever and bowel complaints, ever seen. The poor Volunteers, exhausted by the hardships of a camp life and camp diseases, and many of them mutilated on the bloody battle field, present an aspect equally horrible, and far more calculated to excite our sympathies, for they are *our own brave and patriotic fellow-citizens*, who, at the call "*to arms*," forsook the comforts and endearments of *home*, and voluntarily sacrificed themselves in defence of their country's honor. Poor fellows! how little did they know of the hardships and dangers to be incurred, when they embarked in the enterprise. But it would have been all the same if they had been fully acquainted; for the American heart, excited by patriotism and the thirst for distinction, *quails at no danger, whether present or prospective.* We believe the U. S. Medical Purveyor at this place, Dr. McCormick, is doing every thing in his power for the sick and wounded soldiers who arrive here; but, as we stated before, the Charity Hospital and all the private Infirmaries are now as full as they can hold, and if they continue to arrive *by the hundred.* as they have recently, additional private Infirmaries will have to be erected. The most of the invalids arriving from the seat of war are worn out with typhoid fever and chronic bowel complaints. Many perish on their passage over the Gulf of Mexico, and many barely live to reach their country's shores, where they prefer being buried, to leaving their remains on a hostile and Foreign strand.

The arrival of such large numbers of Foreign Immigrants in a diseased state, is a subject of grave importance for the consideration of our Municipal authorities. We do not think it likely that the diseases with which they are inflicted will be spread amongst our citizens, though such a thing is by no means impossible; nor are we without examples of several inmates of the Charity Hospital having contracted the *ship fever* there. But as matters are going on, this large Institution may be completely monopolized by Foreigners; and in case of an epidemic, our own poor will be totally excluded from its benefits. It occurs to us that they should either at once be sent up to the great West, or there should be established, beyond the precincts of the city, a hospital for their special accommodation. They are *fellow-beings*, and we cannot see them perish in our streets; though, from the present prospects, it is probable the benevolence and generosity of our citizens will be heavily taxed before the summer is over. We understand that a number of our benevolent fellow-citizens have already united under the name of the "*Irish Immigrant Society*," and have established one or more hospitals in the upper part of the city, for the relief of these unfortunate beings. This is very commendable, and we hope to see the example followed in other instances; but we think these infirmaries should be located in retired and open places, lest they become focuses of infection to the surrounding neighborhoods. The miserable objects of poverty and affliction have *other* claims upon the better classes of society than those of *mere benevolence and charity;* they bear along with them *the seeds of destruction*, which, after ripening and devastating *their* ranks, spread abroad through the community and involve all in common ruin. It is, therefore, the *interest of the rich to take care of the poor;* and they must see them well provided for if they would hope or deserve to escape their calamities.

The prevalent diseases among our own citizens since our last date, have been diarrhœa, dysentery, measles, intermittent and remittent fevers, bronchitis, hooping-cough and the like. During the hottest days in June there were several deaths from *sun-stroke.* We have seen more obstinate cases of bronchitis this year than we ever witnessed before. There has been no case of yellow fever. By reference to the *list of interments* furnished by the Secretary of the Board of Health, it will be seen that our mortality is unusually great for the season; yet we have said enough to account for it, and to show that it is not inconsistent with our remark, that there is but little sickness amongst our citizens proper. Our city continues crowded with people, notwithstanding the business season is nearly at a close. However, owing to the peculiar state of the cotton market, the business season will probably be extended much later this year than customary. Among those who are detained beyond their usual time of departure, uneasiness begins to be felt, and we hear many enquiries about yellow fever. Dame Rumour now and then quickens their curiosity by reporting the existence of a case, handed perhaps through some half a dozen mouths; but we do not see the least cause for alarm as yet. We learn that the *Vomito* is raging severely at Vera Cruz; but notwitstanding the frequent intercourse now existing between this place and that, hardly a week passing without one or two arrivals, no cases have been brought to this city. Convalescents from yellow fever are beginning to arrive here. We shall have, this year, a rare opportunity of testing the question of *the transmissibility* of yellow fever; and we do hope that every physician in the city will feel it incumbent upon him to note carefully every fact bearing on the point, that may come within his view. Much is expected from the vigilance of the Board of Health, and it appears the members are duly on the *qui vive.*

The weather since we last wrote has been very variable; during the most of May it was rather cool, but since the commencement of June we have had some very hot days. (See the abstract from Mr. Lillie's Meteorological Journal.)

The River. The extraordinary rise mentioned in our last number continued at this place until about the 20th of May, when the river began to fall and has continued to recede until it is now very low for the season. About the 15th of May, there was a *crevasse* at the town of Algiers, immediately opposite this city. The water poured over in immense torrents, and was only arrested after four or five days, by extraordinary exertions, aided by the falling of the river, which occurred most opportunely. We have thus touched upon all the points which we deem of interest in relation to the health of the city. We hear the usual prophecies and auguries in regard to coming events, but as we do not pretend to any degree of prescience, we content ourselves with noting facts, which may prove useful for future reference.

HEALTH OF THE COUNTRY.

As far as we have been able to learn, the general health of the Southern country is very good. We return thanks to our obliging correspondents for the following letters, and would respectfully request them to extend their observations as far as they conveniently can beyond the bounds of their own immediate practice.

Montgomery, Ala., June 15th, 1847.

Gentlemen:—Below is our list of cases from the 10th of April, to the 9th of June, inclusive, made up from the sources before mentioned.

Abscess 2, Aptha (infant.) 1, Angina (diptheretic) 1, Bronchitis (acute) 19, do. (chronic) 1, Catarrh 2, Cholera Morbus 7, Colic 6, Cystitis (sub-acute) 1, Croup (spasmodic) 1, Carcinoma (of mam.) 1, Dirt-eating 1, Dyspepsia 2, Dysentery (acute) 21, Diarrhœa (acute) 21, do. Chronic 3, Difficult Dentition 10, Dislocation (simp. of clavicle) 1, do. (comp'd. of ankle) 1, Erythema papulatum 2, Enteritis (acute) 3, do. (chronic) 1, Epilepsy 1, Engorgement of cervix uteri 2, do. (with ulceration) 1, Fissure of Anus 1, Furunculus 1, Fistula in Ano 1, Fracture (simple of thigh) 1, do. (comp'd. of leg) 1, Foreign Body in Trachea 1, do. in œsphagus 1, do. in Ear 1, Fever Int. 20, do. Remit. 34, do. Remit. Infantile 12, do. Ephemeral 4, Gonorrhœa 5, Glossitis 1, Gastro-enteritis 1, Gastritis (acute) 2, do. chronic 1, Hernia (Inguinal reducible) 2, Hydrocephalus (acute) 2, Hæmorrhage (uterine) 2, do. (Pulmonary) 2, Injury of Head 2, Inflammation of mamma 1, Leucorrhœa (vaginal) 1, Menorrhagia 1, Masturbation 1, Neuralgia 12, Necrosis 1, Opthalmia (catarrhal) 2, Otitis 1, Orchitis 3, Prolapsus uteri 1, Pneumonia (acute) 5, Psoriasis (palmous) 1, Paronychea 3, Paralasys 3, Peritonitis (acute) 2, Parotitis 5, Rubeola 31, Rheumatism (acute) 1, do. (chronic) 1, Scabies 3, Syphilis (prim.) 3, do. (second.) 3, Suppression of Catamenia 3, Strophulus confertus 1, Stribismus 1, Stricture of urethra 2, Spinitis 1, Synovitis 1, Tumour (fatty) 1, Tonsilitis 5, Ulcer (on leg) 1, Urticaria 3, Varicella 4, Vertigo 1, Worms 4, Wounds Incised 2, do. Punctured 4, do. Lacerated 1, Gun-shot 3.

In all there were 333 cases, and 9 deaths. To wit:—two from Infantile Remittent Fever, two from Acute Hydrocephalus, one from Acute Gastro-enteritis, one from Rubeola, one from Colic, one from Acute Dysentery, and one from a Gun-shot wound.

Very respectfully,

W. M. B.

Memphis, June 10th, 1847.

Gentlemen:—Having been absent from home part of the time, the following report of cases is not so full as it otherwise would have been. It is however furnished as the best exhibit of the character and relative proportion of the different diseases in the past two months.

Cases.—Abscess 4, Asthma 1, Abortion 1, Aracno-spinitis, 1, Bronchitis 5, Burn 2, Convulsions (infantile) 3, (epileptic) 2, Colic 8, Cholera-morbus 3, Cholera-infantura 13, Cynanche Tonsilaris, 3, Cystitis (chronic) 1, Dysentery 19, Dysenteric-diarrhœa 4, Diarrhœa 33, Dysmenorrhœa 1, Dislocation (elbow joint) 1, Dropsy (general) 3, Dyspepsia 4, Endocarditis, 1, Fever (intermittent) 11, (remittent) 31, Gonorrhœa 2, Gastro-enteritis (chronic) 5, (acute) 2, Gastritis (chronic) 2, Hemoptysis 3, Hysteria 3, Hemorrhoids 2. Hydrocephalus 1, Hydrocele 1, Hernia (inguinal) 1, Hepatitis (acute) 1, Jaundice 1, Leucorrhea 3, Mania-à-potu (complicated with convulsions) 1, (with pleuro-pneumonia) 2, Menorrhagia 3, Neuralgia 3, ophthalmia (acute) 2, (chronic) 1, Orchitis 1, Pleurisy 19, Pneumonia 8, Phthisis 3, Porturition (natural) 6, (tedious) 2, (convulsions requiring delivery with forceps) 1, Puerperal peritonitis 1, Purtusis 4, Parotitis 2, Rheumatism 4, Rubeola 30, Rabid Dog Bite 1, Syphilis 1, Spinal irritation 1, Splenitis (acute) 1, Tumor (adipos.) excised 1, Tabes-mesenterica 1, Urticaria 2, Varicella 1, Vicarious menstruation 2, Wounds (gun-shot) 2, (incised) 5, (lacerated) 4.

Making in all 301 cases.

Out of these 12 deaths occurred, viz. 1 of Infantile convulsions, 3 of Dysenteric-diarrhœa, 1 of Diarrhœa, 1 of Gastro-enteritis (acute,) 1 Hydrocephalus, 1 of Mania-à-potu (complicated with pleuro-pneumonia,) 1 of Phthisis, 1 of Puerperal entero-peritonitis—the entero-peritonitis produced by premature delivery, and in twelve days afterwards death,—1 of Tabes-mesenterica, 1 of Pneumonia.

Dysentery has been more common than usual during the spring months.

In its treatment in the last few weeks, after bleeding when the condition of the patient and the stage of the disease would admit of it, I have found an equal mixture in bulk of calc. magnesia, lac. sulphur, super tart. potassæ and pulv. rosin, given in doses of one to two teaspoonfuls every two hours until it operated freely on the bowels, then followed by an opiate, to arrest the mucous and bloody discharges, and by its repetition in the same way when necessary, from a return of similar discharges, to cure the disease more promptly and certainly than any other practice.

L. S.

Jeanerette's, St. Mary, La., June 15th, 1847.

Messrs. Editors :—As usual at this season of the year, this section of country is healthy, with the exception of the prevalence of the measles at numerous points throughout this and some of the adjacent parishes. The cases that have fallen under my observation have been accompanied by a higher grade of febrile symptoms than I had usually met with, but so far as I have learned, little fatality has attended the epidemic.

I think I have seen more trouble among children this Spring, during the process of dentition than heretofore. Infantile diarrhœas have been prevalent in this vicinity.

Respectfully your ob'dt. servant,

J. B. D.

Woodville, (Miss.,) 16th June, 1847.

Gentlemen :—I regret that I have to apologise for my long silence, but my excuse is a paucity of any thing worthy of notice in this section. At this time I have little to state. The people have been exempt from diseases during the winter, and the spring has not produced any, with the exception of a few cases of diarrhœa and dysentery. This has been, generally, of so trivial a character, as to be manageable by the ordinary means, lasting from four to eight days. The number of deaths in the county for the last six months have been few.

A case of *cerebritis* and *hydrocephalus* in a male negro child, five months old, came under my care in April. I was informed the nurse had let it fall some two weeks before, but no symptom arose therefrom at the time to attract attention. It had some fever; moaned in its sleep; was averse to any motion; held its neck rigid; bowels torpid; but good appetite. Cathartics brought away green discharges. The most prominent symptoms, however, were given by the respiratory apparatus; wheezing, cough, strangling and discharges from the nose. The case presented more the appearance of laryngismus stridulus, or cerebral croup, until thirty-six hours before death, when it was seized with convulsions, which unmasked the case, though too late.

Post mortem.—The stomach and bowels pale and containing very little of any thing. Other abdominal viscera quite healthy. Thoracic viscera gave no sign of any lesion, more than a small patch of bloody engorgement at the lower tip of the left lung, (which, however, I attributed to post mortem influences) and a slightly increased quantity of serum in the pericardial sac.

Head.—The sutures were all open from one extremity to the other. On opening the cranium and exposing the dura mater, this membrane was found coated nearly all over with a thick coat of cheesy pus, which was so consistent as to be pared off in large flakes. The convolutions of the brain were nearly obliterated. On attempting to separate the hemispheres, they ruptured suddenly from a slight touch, and about three gills of serum flowed out, when the brain collapsed and left the cranium nearly half unoccupied. The entire brain was so much disorganised and softened that no examination of its compartments was attempted; it was nearly of the consistence of mush or soft butter.

This case is worthy of notice from the indefiniteness of its symptoms. The brain was diseased, but the lungs first gave any sign of disease. The child's holding its neck rigid, made me suppose that some of the cervical vertebræ and muscles had been injured or made tender by the fall it had suffered.

In May, I had a case of *morbus cœruleus* in a well developed primipara male child at full term. I tried the plan suggested by Professor Meigs, of placing it on its right side, inclined at an angle of thirty degrees. This seemed to be of some slight benefit to it and the blueness disappeared for a time, but returned at irregular intervals, accompanied with convulsions and coma. It lived about twenty-eight hours, when it became blue, was seized with a slight convulsion, and died very suddenly. The child never cried out stoutly during the whole time, but merely whined and moaned.

As I have usually given a short report of the seasons and weather heretofore, I will append the following :—The spring has been cool and rather dry. Thermometer ranged, at 10 A. M. from 60° to 79°, and at 3 P. M. from 64° to 83°, during the month of April—wind principally from S. S. W.—rain only on 4 days in the month. May was ushered in by a smart *tornado* on the night of the 1st, which blew down several houses, both great and small, crippled some negroes and prostrated much timber. It ranged from S. W. to N. E., and was attended with lightning, thunder and rain, and hail in some places, succeeded by some cold days. Thermometer at 10 A. M. ranged from 60° to 82°; at 3 P. M. from 66° to 83°—wind from all points—rain on 6 days. June entered with heavy rain. Thermometer at 10 A. M. from 78° to 84°; at 3 P. M. from 80° to 89°—rain on 5 days—wind mostly S.

Yours, &c. A. R. K.

SICKNESS IN THE U. S. ARMY IN MEXICO.

We have made repeated requests of the medical officers of the "Army of Invasion," to keep us informed as to the sickness and surgical operations that come under their observation, but as yet we have not been favored with any detailed accounts for publication. Our cotemporaries at New York and Saint Louis, it seems, have been more fortunate, and have given us very interesting papers from Surgeons Jarvis and Johnson. We have some idea of the arduous duties which devolve upon surgeons engaged with an army in active service, but they owe it to themselves and to their profession to make known the amount of toil, hardship and danger they undergo, that they may come in for a due share of whatever honor and fame may be achieved. Military commanders are in reality the trumpeters of their own fame. Their reports are devoured with eagerness by their admiring countrymen, and their chivalrous deeds are heralded throughout the world. To them belongs the direction of the fierce and bloody conflict! they lead on the armed hosts, fired with the maddening thirst of fame or vengeance, unto the very jaws of death. But when the strife is ended and the battle-field is strown with the mutilated bodies of the wounded and dead; when the strong arm becomes powerless and the manly form is prostrate on the dust; when the vital energies are failing fast and the mind reverts to the beloved scenes of distant kindred and home; to whom in that dark and trying hour does the poor soldier look for succour and relief? It is to the Surgeon. He alone can staunch the bleeding wound and arrest the unfinished work of destruction. By his knowledge and skill much suffering is prevented and many valuable lives are saved. Nor, in the pursuit of his peculiar duties, is the Surgeon secure from the dangers of the battle-field. He may often be seen ministering to the wounded within range of the deadly cannon and amidst showers of musket balls. But the horrors of war are by no means confined to the battle-field: the diseases incident to camp-life, exposure to the inclemencies of the weather, the long and exhausting march, the rude diet and rough nursing, go largely to make up the catalogue of miseries. It has been ascertained that the killed in battle constitute but a small portion of all who perish in active military service. Under all the calamities just enumerated, the Surgeon is constantly called upon. Indeed his duties are *incessant;* and whether the army be at rest, or on the fatiguing march; whether chafed by inglorious inactivity, or stirred by the exciting call "to arms," he knows no rest. His deeds are not emblazoned with the pomp of military fame; yet they are not the less important on that account. The General keeps his eye on the *enemy in front;* but the Surgeon has to combat with the often *far more formidable enemy within the camp.* We beg pardon for having wandered from our theme, but our sympathies are deeply enlisted in behalf of our brethren, the Medical Staff of the Army, whose important services, we believe, are not properly appreciated by Government, and we have therefore taken the liberty of giving expression to these reflections. It is true, their services generally command an honorable though *brief* notice in the army reports; but this is not enough; the world should be better informed as to their privations, their toils, their exposures to danger, and their personal sacrifices.

We resume our subject, the Health of the Army in Mexico. We learn that but little sickness prevails at present among the troops under General Taylor's command. The ill-fated 2d regiment Mississippi Volunteers, which was so heavily afflicted as it passed through this city in January last, continued to suffer until it got high up on the Rio Grande, when, to cap the climax, small pox made its appearance amongst them and committed frightful ravages. Dr. T. N. Love, Surgeon of the regiment, writes us as follows:—"Camp near Monterey, May 10th": "I have had a hard time with small pox—over a hundred cases, including varioloid. We are getting nearly through with it—only about 25 cases of it in hospital at this time, and nearly all of them convalescent. We have had only two new cases within six days. Vaccination has put a stop to it. The general health is improving. We have had two cases similar, in fact, the very same disease that troubled us at New Orleans. They were produced by exposure and imprudence—both proved fatal."

As to General Scott's division, we hear of nothing but the ordinary camp diseases, dysentery, diarrhœa, intermittent and typhoid fevers, beyond Vera Cruz. The main body is doubtless in a very healthy region on the table lands. Yellow fever appears to be raging pretty severely at Vera Cruz. We had the pleasure of conversing with Dr. Tudor, of this city, who has lately been in service at the latter place. He gave us an interesting account of the diseases, but we must content ourselves at present with the following communication, obligingly furnished by Dr. McCormick, who, from his position as medical purveyor of the army, has the best opportunities for obtaining information:

New Orleans, June 22, 1847.

Gentlemen:—At your request I furnish the following information in relation to the sick and wounded of the army:

On the 16th inst. the steam ship Massachusetts arrived from Vera Cruz, having on board one hundred and sixty-three sick from the army.

On the 21st, the steamer James L. Day arrived, having on board one hundred and twenty sick men, also from the army and Vera Cruz.

The military hospital at the barracks being nearly full as many of the men were received there as Surgeon R. C. Wood deemed it proper to take in, with a view to their proper comfort and accommodation. The balance were placed in Dr. Luzenberg's, and Stone, Kenneday & Carpenter's hospital, and after those two hospitals had been filled, about thirty-nine were sent to the Charity Hospital, where they were received and made as comfortable as possible.

In relation to the yellow fever at Vera Cruz, Dr. Laub, of the U. S. Army says: "We have a great many sick; and our list increasing among them some cases of yellow fever, though as yet it cannot be said to have become epidemic. No doubt, however, in a short time we shall have it in all its virulence, at least if the accounts given of it by many here are to be depended on."

Dr. Barton, U. S. A. says: "Vomito increasing—but exactly what we are accustomed to in New Orleans—its type in some instances severe."

Dr. Barnes, who was employed and went from this city to Vera Cruz to assist in the military hospital, says: "The yellow fever prevails to a considerable extent in Dr. Porter's hospital. There are about 350 in it and in Dr. Laub's, of the 1st infantry, and some 82 quartermaster's men. But it has not the malignancy I was led to expect it would present here. The most of the cases I have seen, present more the appearance of remittent than of yellow fever, and were it not for the brilliant and red appearance of the eye, and above all the termination of the cases in black vomit, it would be thought, in the country, *remittent* fever. They either run their course with great rapidity of the symptoms, or improve. I have seen but few of that *typhoid* form which was seen last summer in nearly all of the patients suffering from it in the Charity Hospital, and in which it was almost universally fatal. The fever is very high for the first 24 or 36 hours; a remission then takes place of variable degree and duration, and mounts up again, and again remits, until the 5th or 8th day, when the patient either throws up black vomit, or is left in a state of great exhaustion, free of fever and convalesces slowly. The fatality has not been more than one in twenty. The treatment most successful, you will be pleased to learn, coincides with your ideas as regards fever. The favorite practice with Drs. Porter and Laub, has been quinine and mercurials, and their preference has determined their juniors to adopt the same plan, and I have no doubt has a great deal to do with the favorable termination of the disease. I have treated, myself, in the quarters of the 1st infantry, 8 or 10 cases, whom I did not send to hospital, as at that time I did not prescribe in it. I did this in order to observe the progress of the disease under my own treatment, and the result was that I lost but one case, and he had it supervening on an old dysentery. I used very careful bloodletting locally, with cups, &c., and but once generally, and placed my whole reliance on quinine to subdue the *fever*, which, under its use, was accomplished in 48, often in 24 hours. I have had now, however, a better opportunity of seeing its effects, and can speak more and more favorably of its use.

"I think that the preferable plan of treating it is, to use extreme caution in bloodletting, and to give quinine and blue mass in the first 36 or 48 hours, and after that period the formula I use is generally quinine, grs. 30 or 40, blue mass, grs. x—this give immediately and continue, in the proportions of quinine grs. viij, mass hydrarg. grs. iv. every 5 hours. If a marked effect is not produced in the first 24 hours of this treatment, the doses are repeated for the next 24; but after 48 or 50 hours have elapsed, this treatment may as well be discontinued, particularly if the disease manifests its specific effects on the blood; nature, after that period, had better be trusted than so powerful a remedy as the one mentioned be continued. The only error I can detect (speaking correctingly with regard to my ideas of the pathology and treatment of this disease,) in the practice here, is that bloodletting is pushed too freely and quinine continued to be given, in too large doses, too long. I also think that calomel is contra indicated, and that if a mercurial is wanted, it should be the milder preparations of mercury."

Dr. Dashiell states a case of yellow fever that occurred previous to his departure from Vera Cruz, in a man of good health. who was geting shaved in the barber's shop, and who commenced at once to throw up the *black vomit*, expiring (as a matter of course) shortly afterwards. This comprises the latest medical information I have received from that portion of the army in the direction of Vera Cruz.

Very respectfully,

CHARLES McCORMICK,

Assistant Surgeon, United States Army.

By late advices from the army, we learn that on account of the sickness at Vera Cruz, General Scott has determined to make Tuspan the principal port of entry. He has also had the military stores removed from Jalapa to Puebla. The route will now be direct from Tuspan to Puebla. Our soldiers may thus shun the dangers of yellow fever.

HOSPITAL REPORTS.

As before stated in our remarks on the health of the city, every Hospital is crowded with invalids. Many interesting cases must occur among such a number, and we should be much pleased to receive reports of them.

CHARITY HOSPITAL.

So great has been the number of applicants for admission into this Hospital, that its accommodations have been extended as far as possible. The good Sisters of Charity, (twenty of whom reside in the Hospital,) with their accustomed readiness to sacrifice their personal comfort to the claims of suffering humanity, have given up three of their private apartments to be converted into wards for the sick. Several of the mess-rooms and even the space under the seats of the amphitheatre have also been converted into wards. On the 25th of June we were informed by the Clerk, that there were nearly 800 patients in this Hospital. At the present rate, the annual admissions will probably amount to between *ten and twelve thousand*. Where, on the face of the earth, can such another charity as this be found? As our space is exhausted, our remarks must be brief. The following are the only special reports with which we have been favoured.

Service of Dr. T. M. Logan.—A due proportion of the numerous cases of Ship-Fever, received into the Hospital during the past month, have come under my care—of all ages and of both sexes—and the treatment has been chiefly addressed to the organs invaded, and modified according to their varied complications. Where no special tendency to interrupt the functions, or to destroy the structure of the vital organs was observable, the disease was permitted to run its definite course, and to terminate in health, as it generally does under such circumstances when not thwarted by the *nimia cura medici*. The ratio of deaths among those under my care was 5 per cent.

Into whatever opinion the mind of the medical community may settle down respecting the question of this disease being typhus or typhoid fever, there is one subject connected therewith of deep interest, touching its contagiousness, as the four following cases attest, and which I deem worthy of record.

1. Mrs Bellar, the oldest nurse in the Institution, in which capacity she has been acting ever since the Hospital has been built, and whose good constitution had enabled her to resist all the malignant and epidemic forms of febrile and other diseases to which the inmates are exposed, was one of the earliest victims—the disease going through its regular well-known stages, and terminating in coma and death in about 20 days.

2. Mrs. Elizabeth Gullett, one of the most respectable nurses in the Hospital, where she has been assiduously engaged during the last eight years, was attacked in one of the usual forms—the nervous system manifesting its disorder by direct and great prostration. She is now in the convalescent stage, occasionally retarded by a diarrhœa, which is readily controlled by opium and calomel.

3.——Schmidt, a robust man, whose occupation for a long time past consisted in carrying dirty clothes to the wash-house, also contracted the disease, but ultimately recovered.

4. Abnar Willis, a crippled inmate, also fell a victim to the prevailing fever.

Such facts prove, beyond a doubt, the introduction of the disease from abroad, and, so far as the contagiousness of typhus is as much reputed as the absence of this property in typhoid fever, tend to establish the differential point in the present instance, if they do not identify the two diseases.

Service of Dr. E. D. Fenner. *Wards* 10 *and* 12.—One of these wards (No. 12,) is the only pay ward in the hospital; consequently it presents both medical and surgical cases, and rather a better class of patients than are to be found in the other wards. My engagements have not allowed me time to draw out full reports of particular cases; I shall therefore only offer a few remarks on the general character of those which have occurred.

Surgical Cases. 1. *Amputation of both arms at once.* A young man was brought in at night with both arms horribly mutilated by a cannon which went off whilst he was in the act of loading it, on a late festive occasion in honor of the returning volunteers from Mexico. The case requiring immediate attention, the house surgeon, Dr. Wedderstrandt, promply amputated one arm below the elbow, and the other above. The patient never had a bad symptom, and is now nearly well.

2. *Wound in the abdomen.*—A robust Irishman, engaged in a fight, had a large knife plunged into his abdomen, (left hypochondriac region,) and went to a drug store several squares distant, with the knife sticking in the wound. On withdrawing it, the hemorrhage was very profuse, and the patient seemed like expiring very soon. The abdominal parietes were penetrated, and there was no telling what injury the viscera had received. The wound was closed with adhesive plaster, and the patient taken to the Charity Hospital, ward 12. On arriving there, then night, a dose of laudanum was administed, and he rested pretty well. When I saw him on the following morning, he was suffering from soreness in the wounded region, and distension of the abdomen.—His pulse was good, however, and he retained considerable energy. A purgative enema, with quietude and abstinence were advised. As soon as the bowels were evacuated he was greatly relieved, and from that time recovered so rapidly as to be able to leave the hospital in four or five days.

3. *Double Fracture of the Femur.* 4.—*Compound Fracture of the Tibia.* 5.—*Severe contusion of the thigh.*—After the inflammatory symptoms subsided, these fractured limbs were put up in the starch bandage and are doing well.

Medical. 1. *Ship Fever.*—I have had a number of cases of this disease, but not so many as the other visiting physicians. But few deaths have occurred from it in my wards, and I have discovered nothing new in relation to its pathology. Several cases came into my wards a few days after their attack. I found them with flushed countenances, hot and dry skin, frequent pulse, soreness in the abdomen, pain in the head, pain and soreness of the muscles, especially the gastrocnemii, great thirst, with evening exacerbations. They were much relieved by cups and poultices over the abdomen, mild cathartics, sponging with vinegar and water, &c., to some I gave quinine in 10 grain doses during the remission, with good effect. Some required blisters to the abdomen and nucha, and others were cured by drinking largely of cold water or flaxseed tea. In the latter stages of the disease, the most troublesome symptom I had to combat was an obstinate diarrhœa. This proceeded from ulceration of the large intestine, and was apt to prove fatal. Some of the cases presented very curious symptoms, but I have not space to notice them. I saw several cases among men belonging to the crews of emigrant ships. It would appear that they had contracted the disease from the emigrants.

2. *Diarrhœa and Dysentery.*—Numerous have been the cases with these affections. In the early stages they were easily relieved, but after running on for a long time, it is almost impossible to cure them. Post-mortem examinations have revealed *ulceration throughout the whole extent of the colon and rectum.* As to remedies, I think I have seen more benefit derived from the nitrate of silver and anodyne enemas than any thing else. Before the disease has become settled into the chronic stage, I have witnessed very happy effects from the combination of quinine and morphine.

3. *Colica Pictonum.*—Three cases were relieved by calomel and opium, croton oil, cups over the abdomen, and the warm bath. I like nothing so well as the croton oil as a purgative in this painful affection.

4. *Hæmoptysis.*—A very violent case under treatment.

There have been numerous cases of bronchitis, pneumonia, phthisis, dropsy, jaundice, &c., but I cannot notice them at present.

IRISH IMMIGRANT SOCIETY.

(Since penning our remarks on the *health of the city*, in which we briefly alluded to this benevolent institution, we have been kindly furnished with the following notice of it by one of the Secretaries; which we deem worthy of insertion in our Journal. New Orleans has long been noted for its liberality and benevolence, though there has been so little sickness since 1841, as to offer no occasion for a display of these virtues. We are happy to see by the prompt establishment of this truly benevolent society, that the *good old spirit* is by no means extinct, but still reigns in the hearts of our citizens.)

"This society was organized on the 9th of May, 1847, and went immediately into operation.

The following gentlemen compose its officers:

Daniel Byrne, *President.*		Michal Gernon,	*Secretaries.*
J. S. Bossiere,	*Vice-Presidents.*	Stephen O. Leary,	
L. C. Fallon,		J. P. Kirwan, *Treasurer.*	

"The sole object of its organization is to aid, assist, provide for, and advise all emigrants upon their arrival in this city, no matter what the country whence they came, no enquiry ever being made, or being allowed to be made by our superintendent whose duty is to visit all ships arriving at our city, and attend to the wants of the passengers.

"Its members are composed of all persons who think proper to join it and observe its rules.

"The asylum into which, by *positive* regulation, *no one is admitted* who may be afflicted with *serious sickness*, or any *disease* of a *pestilential* or *contagious* nature, and whose condition may be so declared by one of its visiting physicians, is situated on Duplantier street, and is visited daily by Drs. Harral, Bien, and Hare, who benevolently render their professional services gratuitously.

"Besides, there is a resident physician receiving a salary from the society whose duty it is to prepare the prescriptions left for those inmates who may be laboring under debility, brought on by their sea voyage, or who may be convalescent after leaving the ship.

"A superintendent is also under salary, who remains at the asylum, and in addition to the duties aforementioned, attends to the cleanliness of the house and inmates.

"Since its formation, we have forwarded to the Western country 610 persons, provided out-door relief for 460, found employment for thirty persons.

"We have been enabled to forward so large a number by the kind co-operation of many of our noble hearted Western captains, who have taken numbers free of charge on their boats; the society being required only to provide the emigrants with provisions for the trip.

"The monthly expense is about $300. There are about 150 persons now within the institution."

INHALATION OF SULPHURIC ETHER.

The medical world seems to be more and more agitated by the discovery of this great *composer* of human suffering. It has been called *Letheon* by our classical brethren of the "*American Athens*;" thus deriving a name from the *fabled stream* in whose oblivious waters all remembrance of the past is blotted out forever; yet its announcement has produced quite the contrary effect. It seems to have brightened up the recollections of many, and if their pretensions are to be recognized, they knew all about it a *long time ago*. New claimants to the discovery have appeared in both Europe and America, and it is very probable that when it is made known in Asia and Africa we shall hear of them there. Books have been ransacked, and almost-forgotten observations have been resuscitated to show that it has been *long known that the inhalation of certain vapours and gases will produce a state of insensibility*. In our last number we alluded to a Dr. Wells of Hartford, Conn., who having observed that the inhalation of nitrous oxyde gas, and *sulphuric ether too*, would produce insensibility, claims to be the first person who attempted *to apply the fact to the practice of surgery*; to have convinced himself of its truth by extracting more than 20 teeth; to have preferred the nitrous oxyde because he believed it less dangerous; and to have communicated his discovery to the Medical Faculty of Boston, among whom he signalises Drs. Jackson and Morton. We expressed our surprise that his pretensions, apparently so well founded, should have attracted no attention. Since then, we find that he has forced himself into notice, and there is going on quite an angry controversy between him and the *patentees* of the *Letheon*. It seems also that Jackson and Morton have fallen out and are quarrelling for the honour of the discovery. We have not space to enter upon the merits of these controversies; nor indeed would it be to us a *pleasant task*, such is the disgust we feel that any person having the title of M. D. appended to his name, should, at this day, be so far *insensible* to the philanthropy and magnanimity which have been attached to the profession by a long list of illustrious benefactors, as to attempt to obtain a *patent right* for any thing he may be so fortunate as to add to its means of alleviating human suffering. That the application of ethereal inhalation to the practice of surgery *is a great discovery*, we are free to confess; and we are also gratified at its being an American discovery; yet we can but condemn the illiberal, sordid and selfish motives which prompted the *futile* attempt to monopolise its benefits under the protection of *a patent right*. If such motives were to predominate in the medical profession, it would soon degenerate into *knavish quackery*, which calamity indeed has only been hitherto prevented by the enlarged benevolence and self-devotion of its leading members. It is now made known that Dr. Charles T. Jackson, a respectable graduate in Medicine, was actually concerned with Dr. Morton the Dentist in the application for a patent, and that it was granted to them jointly. Jackson claims to have suggested the use of ether to Morton. We believe Morton does not admit this, and at least claims the merit of developing and illustrating its wonderful powers. Whilst Wells, also a dentist, says he suggested the idea to both of these gentlemen, and that he has been unjustly deprived of the credit of discovery. However the truth may be in this regard, and to whatsoever extent it may prove a blessing to mankind, these gentlemen, (J. and M.) by the course which they have taken, have lost the opportunity to obtain a pure and enviable distinction.—The world will accept the benefit, but cannot entertain a very exalted opinion of those who brought it into notice.

It appears from the Journals that this remedy is being applied to a variety of cases, not the least marvelous of which is *parturition*.—Amongst our extracts may be found some interesting articles on this subject. If experience should prove it to be a *safe and effectual remedy against the pangs of labour*, it will indeed be an inestimable blessing. We have only room to make a brief allusion to some of the surgical operations which have been recently performed under its influence in this city. In our last number we reported a case in which the *parotid gland* had been removed by Dr. Mercier, and in which the ether *failed* to produce its desired effect. We have now to state that the patient died three days after the operation, and that on examination after death, *softening* and great engorgement of the brain were discovered. Some have been disposed to attribute the unfavourable results to the ether, but we think the extract we give in our Foreign Medical Intelligence, proves satisfactorily, that the same result and lesions may follow ligature of the common carotid artery, when ether has not been inhaled.

Dr Alymente

On the 17 octbr ~~I~~ was call in Main street near a colored woman who had ~~been delivered~~ of one child and who shortly after his arrival was delivered of two more. The boy was the first born he was in a separate sack and two girls were held in another.

We have not been favoured with a full report of one of the most extraordinary cases that has occurred at the Charity Hospital, under the care of Dr. Wedderburn. It was that of a man labouring under typhus fever, in whom *spontaneous gangrene* of the leg made its appearance, commencing at the foot. It had progressed nearly up to the knee, and was still marching upwards, when it was proposed as a *dernier resort* to amputate at the thigh, with the aid of sulphuric ether. The patient suffered no pain and the amputation did well. He rallied astonishingly and became convalescent, but unfortunately, relapsed and died about six weeks after the operation.

On the 24th of May, Dr. Stone amputated the leg of a man at the Charity Hospital, under the use of the ether, *but its effects were by no means satisfactory.* It is but just to state, however, that the ordinary ether of the stores was used. The man was heard to say afterwards that nothing could induce him to inhale it again.

On the 22nd of June, Dr. Luzenburg *exterpated the eye* of a man under the influence of ether, with the *most satisfactory results.* We have been furnished the following brief memoranda of its effects.—Patient inhaled the ether 4 minutes, when profound intoxication was produced. Pulse fell from 75 to 40 in the minute—respiration laborious. During the operation, *not the slighest evidence of pain was given.* Toxic phenomena continued several hours. 12 hours afterwards—free from drunkenness and says he knew something of the operation, but is unable to describe his condition—pulse restored to the normal standard. 24 hours—doing well; free from every indication of cerebral disturbance. 36 hours; doing well.

There have been other operations in private practice, but we have not seen them or been furnished details. Mr. Knapp, one of our most respectable dentists, has extracted a great many teeth without pain.—We will conclude by remarking that ethereal inhalation has *signally failed* in some instances, whilst we have read of others in which its effects were fatal. The *real value* of this potent agent has yet to be determined by experience.

UNIVERSITY OF LOUISIANA.

In accordance with the provisions of the New Constitution, adopted by the State Convention in 1845, the Legislature at its last session passed a law for the establishment of a State University, with the above title, to be located in the city of New Orleans. It is to have four Departments—*Law, Medicine, Theology, and General Literature and Science.*—Two of these Departments, viz: Law and Medicine have already been organized, and will proceed with their respective duties next fall. The late Medical College of Louisiana has been merged into the Medical Department of this University, and its Professors will fill its different chairs. The Law Department has been organized by the appointment of the following Professors, viz: Hon. Henry Bullard, late one of the Judges of the Supreme Court of Louisiana, Hon. Theodore McCaleb, of the United States District Court, Randell Hunt, known as one of our most distinguished lawyers, and the Hon. Henry A. Wilde, late of Georgia, but for several years a resident of New Orleans. The late Medical Hall is destined for the Law School, and a much larger building adjoining, is being erected for the Medical School. The Literary Department has not yet been organized. The buildings are all to be on the Capitol Square. See a drawing of them on our advertising sheet. The late class of the Medical College numbered 166; of whom 27 graduated at the close of the term. The prospects of this Institution are certainly very flattering.

MORTALITY IN NEW ORLEANS.

With a List of the Diseases, from April 16, *to June* 26, 1847, *being nine weeks.*
By A. Hester, Secretary to the Board of Health.

Accidental, 2; Accouchment, 2; Affection Chronic, 2; Anasarca, 1; Anemia, 4; Aorta, anuerism of, 2; Apoplexy, 22; Ascites, 1; Bowels, hemorrhage from, 1; do. inflammation of, 8; do. ulceration of. 2; Brain, congestion of, 5; do. concussion of, 2; do. disease of, 2; do. effusion on, 1; do. inflammation of, 2; do. softening of, 1; Bronchitis, 11; do. chronic, 1; Burn, 1; Catarrh, 5; do. pulmonary, 2; do. chronic, 4; Cerebral Congestion, 2; do. effusion, 1; Cerebritis, 6; Cholera Infantum, 7; do. Morbus, 2; Cholerine, 1; Chorea, 1; Colic, (from lead) 1; do. infantile, 1; Colitis Chronic, 1; Congestion, 2; Consumption, 107; Contusion, 3; Convulsions, 24; Coxalgia, 1; Cramp, 2; Croup, 8; Debility, 26; Del'm. Tremens, 13; Dentition, 26; Diarrhœa, 24; do. chronic, 26; Dropsy, 11; Drowned, 27; Dysentery, 45; do. chronic, 35; Encephalitis, 2; Enteritis, 13; do. Acute, 3; do. Chronic, 11; Entero. Colitis, 2; do. do. chronic, 1; do. Encephalitis, 2; Epilepsy, 4; Erysipelas, 1; Fever, 5; do. Bilious, 5; do. Congestive, 9; do. Cerebral, 2; do. Doubtful, 2; do. Intermittent, 3; do. Malignant, 1; do. Nervous, 2; do. Pernicious, 1; do. do. Intermittent, 3; Puerperal, 2; do. Remittent, 4; do. Scarlet, 3; do. Typhoid, 54; do. Typhus, 49; do. Verminose, 1; Gangrene, 2; Gastritis, 5; do. chronic, 5; Gastro-duodenitis, 1; do. encephalitis, 2; do. enteritis, 19; do. do. chronic, 7; do. hepatitis, 1; do. pneumonitis, 1; Head, injury of, 4; Heart, affection of, 1; do. hypertrophy of, 3; Heart, wound of, 1; Hemorrhage Cerebral, 1; do. gastro-intestinal 1; Hepatic Abscess, 1; Hepatitis, 1; do. acute, 2; chronic, 4; Hydrocephalus, 1; do. chronic, 1; Hydro-pericarditis, 2; Hydrothorax, 3; Hypertrophy, 1; Humerus, fracture of, 1; Inflammation, 2; do. chronic, 1; Intemperance 2; Intestinal perforation, 1; Intestines, inflammation of, 1; do. softening of, 2; Jaundice, 1; Laryngitis, 3; do. œdematous, 1; Lungs, congestion of, 2; Marasmus, 8; Measles, 18; Meningitis, 12; do. acute, 1; do. chronic, 1; Metro-peritonitis, 1; Old Age, 6; Peritonitis, 4; do. puerperal, 1; Pertussis, 1; Pleurisy, 1; Pleuro-pneumonia, 4; Pneumonia, 6; do. Typhoid, 4; Poisoned, 2; Premature Birth, 1; Ribs, fracture of, 1; Scald, 2; Scorbutis, 1; Scrofula, 5; Scurvy, 1; Skull, fracture of, 3; Small Pox, 6; Sore Throat, 1; Spasms, 3; Spinal marrow, softening of, 1; Still-Born, 23; Stomach, cramp of, 1; do. disease of, 1; do. inflammation of, 1; do. schirrus of, 1; Sun stroke, 7; Tetanus, 14; do. Idiophatic, 2; do. Traumatic, 2; Throat, gangrene of, 2; Trismus Nascentium, 6; Uncertain, 94; Uterus, ulceration of, 2; Verminose affection, 4; Vertebra, injury of, 1; Wound, gun-shot, 2; do. penetrating, 2. Total, 1,019. Whites, 756; Colored, 263. Under 10 years of age, 334; Over 10 years of age, 685. 1, a native of Africa, aged 112 years; 2 aged 100 years; 1 aged 90 years; 2 aged 80 years.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1847.

By D. T. LILLIE, at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. 1847.		THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. Inches.
		Max.	Min.	Range.	Max.	Min.	Range.				
May	- 1	83.0	63.5	19.5	30.26	29.88	0.38	S.W.	3	0	0.000
"	- 8	81.0	63.5	18.5	30.22	29.79	0.26	S.E.	3	1	1.025
"	- 15	82.0	64.0	18.0	30.06	29.85	0.21	W.	3¼	0	0.000
"	- 22	85.0	65.5	20.5	30.09	29.93	0.16	N.W.	2½	1	0.333
"	- 29	84.5	65.0	19.5	30.17	30.02	0.15	N.E.	3	3	3.835
June	- 5	88.0	72.0	16.0	30.14	30.02	0.12	N.W.	2¾	1	0.225
"	- 12	88.5	75.0	13.5	30.15	30.02	0.13	W.	3¼	0	0.000
"	- 19	90.5	71.0	19.5	30.16	30.05	0.11	S.W.	3	3	5.975

Remarks.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M. and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

LIST OF COUNTRY SUBSCRIBERS

To the New Orleans Medical and Surgical Journal.

S. WOODALL, Publisher,

No. 49 CAMP STREET, NEW ORLEANS.

The following is a correct list of our subscribers, out of this city, at the commencement of the present or Fourth Volume, with the date to which each has paid his subscription as per our books. Should any amounts recently sent, not appear to have been credited, they will, in all probability, in consequence of arriving too late for insertion, appear in our next number.

We have, in many instances, as will appear to those interested, taken no notice of reported payments and remittances which have not been received, in consequence of not having been directed in the manner authorized on the cover of the Journal and for which we will in no case, as we have repeatedly stated, be responsible.

Dr. Arnold, H H	Jan'y, 1848
" Anthony, T A	July, 1846
" Anderson, T P	" 1846
" Alexander, J S	" 1846
" Aldridge, W O	" 1847
" Ames, S	" 1847
" Abney, F M	" 1847
" Andrews, James	" 1847
" Ayres, A O	Jan'y, 1848
" Adams, A L	" 1848
" Austin, W S	" 1848
" Anderson, W D	" 1848
" Banks,	July 1847
" Boswell, J J	" 1846
" Bird, M H	" 1847
" Baker, F C	" 1846
" Barton, E H	" 1847
" Blackburn, D F	" 1847
" Boling, W M	" 1847
" Bates, F A	" 1847
" Benedict, N B	" 1847
" Brunson, A	" 1846
" Bugg, Joel	" 1846
" Bradstreet, J S	" 1846
" Brookes, S M	" 1847
" Brown, Thos C	" 1848
" Binford, Jesse R	" 1846
" Barnett, W D	" 1846
" Barnett, J W	" 1846
" Bronson, Ath	Sept, 1846
" Beaumont, J	" 1846
" Brooke, Henry	Jan'y, 1847
" Barnes, J P	May, 1847
" Battle, J W	Jan'y, 1847
" Beck, T R	July, 1847
" Bragg, John	" 1847
" Baker, R D	" 1848
" Binford, Henry A	" 1847
" Batchelor, J N	" 1847
" Baskin, J H	" 1847
" Boothe, W A	May, 1848
" Benton, R A	Jan'y, 1847
" Bagley, A	July, 1847
" Beverly, L S	" 1846
" Bowman, N	" 1846

Dr. Ball, A	Sept, 1846
" Cobb, B E	July, 1847
" Cutler, A D	" 1846
" Currie, H	" 1846
" Capshaw, Preston	" 1847
" Cabanis & Gist,	" 1846
Dr. Cross, Richard	" 1846
Coleman, F B Esq.	" 1847
Dr. Cook, T A	" 1847
" Calloway, R S	" 1847
" Crump, G W	" 1846
" Carstaphen, J J	" 1846
" Canfield, J R	Sept, 1846
" Chowning, J W	Jan'y, 1847
" Casson, John	July, 1847
" Crawford, James W	Sept, 1846
" Creighton, M M	July, 1848
" Cocke, J M	" 1847
" Cotton, J A	" 1846
" Cuckon, W K	" 1847
" Colmer, George	" 1847
" Crumpton. U J	" 1847
" Cowdin Lorenzo D	" 1847
" Cooper, Geo F	Jan'y, 1847
" Caldwell, J W	July, 1847
" Calderwood, John	" 1848
" Colgin, G S	Jan'y, 1848
" Crawford, D B	" 1847
" Cartwright, S	July, 1847
" Chappolier, F	" 1846
" Chester, Charles	Jan'y, 1848
" Dodds, John C	July, 1847
" Despriz, Wm	" 1847
Dickson, Prof. S H	" 1847
Dancy & Murphy, Drs.	Jan'y, 1848
Dr. Dillard, B F	July, 1847
" Davis, Stephen,	" 1847
" Dupuy, T	" 1847
" Dungan, J B	" 1847
" Draughan, J B	" 1846
" Dewees, O L	" 1846
Dudley, Prof. B W	" 1847
Dr. Davis, F A W	" 1846
" Davenport, P A	" 1847
" Dockery, H	" 1846

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

SEPTEMBER, 1847.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*Medical History of Alabama.* By P. H. Lewis, M. D., of Mobile. Published by order of the Alabama Medical Society. [A silver-cup was unanimously awarded to this essay by the Alabama Medical Society on the 7th of December, 1846.]

(*Continued.*)

SUMMER AND AUTUMNAL FEVERS OF MOBILE.

It is the general opinion of medical men, residing in the interior of our State, that the fevers prevailing in this city, during the autumnal months, are identical with those they encounter in the country, and that it not unfrequently happens a case of yellow fever is there presented, similar to that we are wont to encounter in Mobile. To form a correct knowledge relative to this matter, we deem it very essential, that the marked phenomena attendant on this fever, should be presented in a condensed form.

Having communicated through the January and March numbers of the New Orleans Medical and Surgical Journal, (1845,) a history of the diseases of this city, with all the facts that patient industry could acquire, we deem it unnecessary to travel again over the same ground. From that paper we will extract such facts and conclusions, only, as are requisite for the object in view, without refering to the general history of yellow fever, in all its topographical bearings.

Previous to 1817, the population of Mobile did not exceed 500 souls, consisting principally of French, Spaniards, and free negroes; but after this period the place rapidly improved, and in the year 1819, the accession of Europeans and whites from the adjacent States, had increased the number nearly three-fold in the two years.

The causes which, in all probability, produced the virulent epidemic of that year, are noticed in the history above alluded to, and to it we refer the reader for further information. The deaths that summer, and autumn, were estimated at 400, being nearly one third of the population.

During this calamitous season, it would appear that every character of endemic febrile poison was poured in concentrated abundance upon the inhabitants, each one being active in its peculiar way in doing the work of death. The whites invariably died on the fourth, fifth, or sixth day, of black-vomit, whilst the negroes and quadroons, after protracted suffering from bilious fever, resulting in frequent relapses, cold sweats, and calliquative diarrhœa, shared the same fate. The equal prominence of bilious and yellow fever makes this a remarkable epidemic—the diseases, however, were respectively confined to different classes of persons.

Since the year 1819, bilious fever of a grave and fatal character has disappeared, and the mortality from febrile affections, has been confined to yellow fever.

Such was the consternation produced by the epidemic of 1819, that public attention was then directed to a location on the opposite side of the Bay, and the alarmed inhabitants began rapidly to improve the small village of Blakely, which for a time increased in population, and Mobile was sinking to decay. Amidst the congratulation of the builders of the new city, the same scourge which had borne so heavily on her ancient sister, came with desolating hand, and turned the tide of population and of capital, to its former origin.

The epidemic of 1822 in Blakely, was equally fatal to that of 1819 in Mobile, in proportion to the amount of population; the inhabitants were confounded in the new project, and the former place passed out of existence as a town of commercial importance.

Since 1819, five epidemics of yellow fever, noted in their character, have occurred in Mobile, viz: those of 1825, 1829, 1837, 1839, and 1843; whilst the disease has prevailed in a mild endemic form in the years 1824, 1827, 1842, and 1844.

The yellow fever of 1842 was confined to the Southern portion of the city, whilst that of the year 1843 was confined mostly to the Northern section, and did not invade the district infected the preceding autumn.

From an analysis of the epidemic of 1843, we will endeavor to present such facts and circumstances as will bring to view the several varieties of febrile diseases, and the striking phenomena they present as connected with a Mobile epidemic.

The periodical fevers of the climate in the year 1843, began to be developed early in July, and by the first of September the resident physicians were brought into active employment. Those who were sensible of the increase of disease, and familiar with the incidents of previous epidemics, became confident that they had to encounter the yellow fever in its most violent and terrific form. About the 10th of September, several cases of black-vomit occurred, in the North-west section of the city.

The epidemic may be computed at about 80 days, say from the 20th of August to the 10th of November; the population of the city was at that time about 14,000, and the number of patients treated 1,350, as follows:

Simple intermittent and remittents, ...	500
Intermittent and remittent yellow fever, ...	100
Mild yellow fever of one paroxysm, ...	350
Grave cases of yellow fever, ...	400
Making in the whole number of cases that were known during the season of 1843.	1,350

Of the intermittent and remittent yellow fever, 50 proved fatal; of the grave cases of yellow fever, 190; making in all 240 deaths. This discloses the fact, that during the epidemic, 10 per cent. of the entire population were attacked with disease, and 18 per cent. of those that were attacked, died.

Having set forth the varieties of fever that then constituted the epidemic, we will merely review their peculiar nature, so as to enable medical men in the interior, to perceive the manner in which they often become blended, and to point out the difference existing between yellow fever and the diseases with which they are annually acquainted.

SIMPLE INTERMITTENT AND REMITTENT FEVER.

On the first of September, the scene presented in Mobile differed but little from that which takes place in all inland villages every sickly summer. Intermittent and remittent fevers that began to prevail early in July, had by this time become so prevalent as to attract general attention, but as they were mild, yielding readily to treatment, no serious apprehensions or alarm was created.

About the 10th of September that fatal symptom of yellow fever, the black-vomit, made its appearance. So difficult was it, during the first paroxysm in many cases, to pronounce with any degree of certainty on the character of the disease, that the most prudent were known to avoid committal, by their hesitation to express any positive diagnosis. After the first paroxysm passed off, the physician was still in doubt, in many cases, until the appearance of the second, after which he would not hesitate to pronounce it intermittent or remittent yellow fever, as the case or its specific symptoms might warrant.

In these doubtful cases the judicious practitioner seldom failed to remark to the friends of the patient, that care should be observed or it might run into black-vomit. This difficulty in diagnosis so often occurred as to make it obvious that the types of those intermittent and remittent fevers, are so connected with mild yellow fever, in the first paroxysms, that the distinction cannot be drawn, and should the character of disease be pronounced upon, it is done from circumstances independent of the phenomena that are usually attendant on the several diseases.

Having directed attention to the difficulty that arises under peculiar circumstances, of separating intermittent and remittent from yellow fever, it is unncessary to dwell on their general character. These fevers usually yielded under proper treatment after the second paroxysm, but when the usual anti-periodic remedies were unavailing, there were strong reasons to suspect, notwithstanding the apparent singleness of type, that the morbid condition was one of a complicated nature, and perhaps after a third, or fourth paroxysm ensues, the mask is thrown aside and the patient discovered to have passed into the collapse or black-vomit stage.

INTERMITTENT AND REMITTENT YELLOW FEVER.

The facts in relation to these cases are curious, and when properly considered, cannot fail to throw some light upon the unity of the producing cause of fevers in malarious regions. By reference to the history previously alluded to, it will be observed that the suburbs of Mobile, are contiguous to low marshy soils, swamps or morasses, all abounding in vast quantities of vegetable matter undergoing decomposition through the agency of heat and moisture.

Every summer and autumn intermittent and remittent fevers prevail in these suburbs, whilst those living in the middle part of the city seldom have this character of disease, unless in sickly years, and then the cases are confined to those who are acclimated, while strangers unacclimated suffer in the more violent form of yellow fever.

In the year 1843, very few persons residing on the outskirts of the city escaped periodical fevers, quotidian, being the most usual form.—That portion of the population who were acclimated had but one or two paroxysms, whilst those who were not had chill and fever, complicated with yellow fever. When medical men were treating what they supposed to be simple intermittent, and were not apprised of the real character of the disease until they were startled by the appearance of black-vomit, as was frequently the case, they consoled themselves by stating that a simple case of chill and fever had, *under atmospheric influence, run into black-vomit.*

Now, what are the facts and conclusions to be drawn therefrom?—Although the peculiar characteristics of chill and fever were very prominent, so much as to conceal from the observation of the incautious physician the real nature of the disease, yet there were signs by which the hidden demon could be traced out. During the apyrexia, there were the peculiar pulse, some of the restlessness, and that peculiar appearance of the eye, or a glimmering of that unmistakable physiognomy peculiar to the yellow fever, which once seen can never be forgotten, all of which prominent traits merit the highest consideration; a description of which we will give when treating of yellow fever proper.

Independent of these signs and symptoms there are other facts which force on us the conviction that these cases were complicated, from the initiatory chill, and we ask for them a careful consideration.

We allude to the duration of illness, and the period at which black-vomit made its appearance. Under the appropriate head it will be seen that grave malignant yellow fever, well marked from the onset, terminated between the third and seventh day. Of 28 fatal cases of intermittent and remittent yellow fever, all died within the eighth day after the initial chill; two had black-vomit in the third paroxysm, (quotidian,) and died on the fourth morning. Those that recovered passed through the critical, or collapse stage of the yellow fever, on the fifth or sixth day. Inasmuch, then, as the result of the distinct morbid action, constituting yellow fever, proves that five or six days are requisite to the changes which must precede the advent of this critical symptom, we cannot avoid forming a similar conclusion, that the same morbid process was going on in those intermittent cases which had a corresponding termination.

Every pathological fact yet fully demonstrated and the accumulated experience of time is in support of the position that peculiar changes in the system occur previous to black vomit.

In 1844, remittent yellow fever prevailed to some extent in this city; the cases well marked and of long duration, ranging from 5 to 12 days; they were attended with hemorrhage from the gums and nose in a large proportion of the cases, presenting a low condition of the body, but most generally they recovered.

MILD CASES OF EPIDEMIC YELLOW FEVER.

This description of fever was confined mostly to the natives, to those that were acclimated, or those that were partially so. With those classes it was frequently so ephemeral as to pass of in a few hours, leaving no trace behind save some dull pain in the hips and legs with extreme soreness of the muscles.

As a general rule, however, the patient was confined to his chamber some four or five days. After the chill, which was of very short duration, the pain in the temporal region, in the back and hips was very intense, the flushed face, excited manner, and sparkling eye that so peculiarly characterized the febrile stage could only be compared to the excitement produced by an excess of champaigne.

In those cases, the pulse is more rapid, the skin hotter, and more pain and restlessness, than in those of a grave character. The peculiar physiognomy of the malignant cases cannot be traced in these.— The phenomena of fever proper yield in 10 or 20 hours to a fine equally diffused perspiration, and the patient is enabled to take his nourishment and return to his business in a few days. A moderate mercurial cathartic to excite the liver and act on the bowels gently, the warm bath, and stimulating ptisans are of service, but any excess of medicines is too often injurious and not requisite to a cure. If the patient escapes a severe *drugging*, the disease passes rapidly away, without seriously affecting or materially interrupting any of the functions or their associated relations.

GRAVE, OR MALIGNANT YELLOW FEVER.

As previously stated, persons unaccustomed to yellow fever localities are generally subject to this form of disease, it matters not whether they are from the North or residents of Southern country. Should they arrive in the city during the prevalence of the disease, they are generally attacked between the sixth and fourteenth day after exposure.— Should they have been settled in the city, previous to the appearance of disease, they may escape for several weeks. No precaution or careful attention to bodily health will ensure immunity from attack. In the midst of the best health and vigor, they are often stricken down.— Without the slightest warning, probably while asleep, the patient is seized with a chill or pain in the head, with cold creeping sensations. In a few minutes, fever comes on, the skin is hot, pulse 110 or 120, pain in the head, back, and limbs very severe, the latter being of that character known as the "*broken bone pain.*" In eight or ten hours the fever is modified, the skin becomes moist, pain in the head less, tongue slightly furred, pulse down to 90 or 100, full and bubbling, having lost the hardness or tension a short time previous.

This stage, which is designated as the febrile, lasts from 30 to 50 hours in that class of cases to which attention is now directed. After this fever has disappeared, we find the patient in what is called the *calm* or *passive stage* of the disease. The pulse has sunk to the natural standard, the secretions diminished, evacuations scanty, ash colored, and inodorous, the eye muddy and yellow, the countenance gloomy, dejected or sottish.

In some patients the skin is injected, of a dark red or brown color, while with others it is a deep yellow or light lemon tinge. For the most part, the patient does not complain, is not troubled with inordinate thirst, has no nausea, yet does not desire food. This state continues for two or three days, when another, the "*collapse*" or critical stage, approaches. As this latter period in the disease advances, it will be discovered that the pulse has dropped down to 60 or 70, is full, but gaseous and compressible, the skin continues moist, but not so warm. The patient speaks slowly, drawling out a syllable at a time, he is evidently growing weaker, hangs his head on the side of the bed, hugs the pillow closely. If he does not rally at this point he grows more restless, he sighs, and groans, the secretions are all stopt, the skin becomes cold, his features are sharper, the upper lip thin, and trembling, black-vomit is thrown up, and he soon dies.

Attacks frequently come on, especially about October, with catarrhal symptoms: although, the physician may readily discover the true character of the disease, the patient cannot be made to believe he is suffering with any thing more than ordinary cold. In this class of patients the febrile symptoms are moderate and slow in being developed, they continue on, however, to the collapse or hemorrhagic stage, no calm or passiveness in symptoms intervening, and most usually terminate in moderate hemorrhage from the nose and gums. In those years, when the disease prevails as a moderate epidemic, the commencement of attack is violent and abrupt with a large portion of cases. The subject is restless and agitated, paces the floor with a hurried, impatient manner, and sometimes screams aloud, falling to the floor.

In these cases there is profound coma or stupor for three or four days, and notwithstanding the unpromising aspect of the patient, hemorrhage from some of the orifices on the fourth or fifth day takes place, and is followed with restoration of mind, of all the functions, and recovery is very rapid.

Now and then the physician meets with some patient who continues on his feet up to the black-vomit stage; in fact we have frequently seen them throw up this matter when walking in the street or waiting in the office for medical aid. In this variety there is no chill or pain and but little fever, the skin is usually injected and torpid, presenting a chocolate appearance, the pulse variable, stools thin, and urine copious. The patient, from exhaustion, will lay down, but soon he is on his feet again, often getting up at midnight and going into the streets, where he will walk for hours. Should he be told he is sick and must take medicine, he is certain to object, frowning down with a sullen scowl all entreaties to go to bed or have medical assistance. They are usually very abrupt, use the most violent and satirical language and laugh with a peculiar sardonic grin; these cases are always fatal.

During epidemics, the gloom that surrounds a community so seriously affects some persons as to produce the most fatal results.—Individuals become victims of fear and despair, they are found sighing and weeping, refusing to be consoled, or pacing the floor in a restless and hurried manner. Under these circumstances, the most gloomy pictures of fright and despair arise up in horrid form before the imagination, and if they take medicine they usually remark to the physician, "it is of no use, I am obliged to die."

Here is no symptom for the medical man to meet and combat, the pulse is full, but compressible, the skin moist, imparting a sensation of both heat and cold, the pulse diminishes, getting as low as 45 in the minute. On the third day after this despondent fit seizes them, they begin to *belch*, which they aid by every effort in their power; finally they vomit a little mucus and water, the skin and eye begins to assume the tawny yellow of the disease, and on the fifth or sixth day, black-vomit is thrown up.

The silent, dark and mysterious manner in which these fearful pathological changes take place, leading to black-vomit and death have very frequently caused us to to wonder what must be the nature of that invisible fiend, that comes in so many shapes, the most deadly and unobservable.

With these brief remarks on some of the common varieties, we will dwell for a moment on the most prominent symptoms, distinguishing the great mass of cases. If the attack is ushered in with a chill or cold sensation, the fever becomes very soon characterized by all the usual phenomena of high arterial excitement. We have not seen any cases, however, attacked in this way, where a perspiration did not ensue in 10 or 15 hours, after which the fever began to abate, and in thirty or forty hours from the moment of attack it had almost entirely passed away.

Pain in the head, particularly over the eyes, is most severe during the first or febrile stage, and is always present. As the disease advances, the pain leaves the head and lingers in the hips and lower extremities until the second or third day, when it disappears. Females suffer very little with pain in the head, it being chiefly confined to the pelvic region, and in those cases is very violent. As a general rule, there is not much local pain or soreness at the epigastrium. The muscles of the whole body are generally very sore, and sensitive to the touch. This gives rise to the impression with those in search of gastritis, that the stomach is the principal seat of organic lesion; when, if the examination had been extended, the same pressure on the chest, thighs or arms would have been observed to produce equal degree of flinching.

The passive, or stage of calm, may be dated from the subsidence of fever to the collapse or black-vomit period; and it is very difficult during this stage to detect any symptom of disease. The skin may be natural, countenance perfectly composed, the tongue clean, and the pulse, if altered, is so slight as to require the nicest discrimination to detect it. In a large majority of cases the experienced practitioner can discover and array a sufficient number of symptoms to form a correct diagnosis and prognosis. The restlessness in the febrile stage, is proportionate to the degree of pain and suffering; whilst, in the calm or passive stage, there is a peculiar methodical action; when conversing in a slow, pleasant manner, the patient will remove his pillow to the opposite side of the bed, and in a few minutes he returns to his former position, placing his head in a careful manner, and denying that he feels in the least uncomfortable.

The collapse stage is more marked and regular in some epidemics than in others. Its approach is indicated by increased restlessness and wandering; the patient complains that his ribs are pressing inwardly; desires his limbs to be supported by pillows; and the least exertion is followed by very great prostration. This stage is looked for by the physicians of Mobile with the greatest anxiety, for unless in the first hour of its appearance, the failing powers and energies of the system are rallied and the stomach sustained, black-vomit is inevitable.

The pulse, during the febrile stage, is large and full in robust plethoric subjects, corded and bounding. In the passive stage, it would appear natural to the unpractised observer, but to the experienced observer the sensation it imparts is fully recognized. Its impression under the finger is like an air bubble, rebounding quickly under the least pressure. With the exception of such cases as become *localized*, or are of the ataxique variety, attended with delirium, the pulse becomes slower as the disease advances towards its fatal termination, sinking to 45 or 50 strokes in the minute, the last being very distinct.

The most common appearance of the skin is of a gingerbread red, caused by its yellowness combined with the injected state of the tissues. In the collapse or convalescing stage, the blood partially recedes from the surface, leaving the skin yellow. In some cases, when the powers of life seem to have been overwhelmed at the onset, inducing a torpid or inactive condition of all the functions, the surface is mottled, or presents a bruised appearance, not unlike the swollen limb that has been subjected to bandage and lotions, the skin being purple and yellow.—This yellowness differs widely from that which is presented in jaundice. The urine in jaundice is always exceedingly yellow, but in yellow fever it is not materially so. In some who die, as well as those who recover, the yellowness of skin is wanting.

The physiognomy of the disease is striking and peculiar. With many it throws a gloomy, melancholy shade over the countenance; with others the brow is furrowed, the lip compressed, and he frowns defiance on all around. At other times, it is so blended with a comic lively expression of countenance as to give the patient a peculiar, variable and singular appearance. Once displayed, no effort of the patient can dispel it; he may rise from his bed, laugh and talk with his friends, become exhilerated with wine, or joyous in the anticipation of coming pleasures, but he cannot chase it away. There it sets enthroned upon the face, like the shade of a monster, smiling in contempt upon the efforts of the physician, mocking the assumed gayety and levity of its victim. Nor is the cradle exempt from its visitations. Its gloomy, cheerless mantle is often placed on the infant brow, giving to it a sullen look not suited to its tender face. The symptoms attendant on this form of disease are very peculiar; for, during the first stages, there is less irritability of stomach and vomiting than in any febrile endemic of the State. The matter ejected from the stomach, during the feverish stage, most usually consists of the fluids received in that viscus; but if the irritability continues in the stage of calm, there is vomited a mucus containing particles of flocculi resembling beeswings. If bile is vomited in the second stage, it constitutes good reasons for questioning the character of the disease.

We have known emesis in excess produced by tart. antim. or lobelia, without the least appearance of bile. The small specks or masses noticed in the fluid vomited, enlarge as the disease progresses, becoming in the collapse stage thick and black. The matter ejected in very malignant cases, takes place only towards the close of life; it is preceded by total suppression of all the secretions, great restlessness, and burning in the stomach; there is no nausea, but a coffee-ground fluid is *pumped* up very suddenly, and thrown to a great distance. In a large proportion of cases unchanged blood is vomited. We have seen three brothers vomiting at the same time; one of them, slightly coagulated blood, resembling wine lees; another, fresh blood; and the third, the coffee-ground vomit. These young men were all purging blood at the same time. This exudation of blood is not peculiar to the stomach, it is often found over the whole mucous surface of the canal, bladder, tongue, &c. Black-vomit, in a few instances, made its appearance on the morning of the third day; but the fourth, fifth or sixth was the most usual. Many cases terminated fatally, in which this symptom was wanting, especially with those in which the brain become affected early in the disease. When there was any vomiting in this form, the matter appeared of a thin *tea-like* fluid, containing small, round black particles like grains of powder Recoveries after black-vomit are exceedingly rare; 5 per cent. only of those in which this symptom made its appearance in 1843 survived. So long as this vomit is raised in small quantities, thick and pasty, thrown up with a natural mucus, there is some hope of recovery.

The thin, black fluid, with coffee-ground sediment, is always a fatal symptom. Numerous tests made by Dr. Nott and the writer, have proved black-vomit to be highly acid; no instance to the contrary was discovered in 1843 and 1844. Connected with this fact, that the patient, in the black-vomit stage, complains of burning in the stomach and scalding of the œsophagus, induced Dr. Nott to institute some experiments, with a view to ascertain what effect acids would have upon the blood. Diluted muriatic acid so changed the blood as to resemble black-vomit. In relation to the character of this discharge, it is astonishing there should ever have been a difference of opinion as to decided black-vomit and blood. There is, in shade and appearance, a great difference, but when we see them thrown up together, and running into each other by perceptible degrees, just in proportion to the quantity of blood exhaled, or as it is intermixed with the secretions of the stomach and intestines, the facts are too obvious to question or wonder at for any discriminating mind. It is blood, decomposed or changed in color, mixed with the secretions of the canal just in proportion to the length of time the blood remains in the stomach, or comes in contact with the secretions. If hemorrhage takes place from the nose and gums, previous to the appearance of black-vomit, it is favorable. This discharge usually arrests the restlessness, the inability to take food, and other unfavorable symptoms. It has been mentioned, that the nature of the disease is not altered under any change of symptoms, however favorable; but under the influence of these hemorrhages we have often witnessed great amendment, leaving the countenance bright, soft and relaxed.

In some cases, a strong hemorrhagic tendency showed itself at an early stage, by an oozing from wounds, excoriated surfaces, &c. Hemorrhage from the bowels is a dangerous symptom, but not usually fatal, unless it is excessive at an advanced stage of the disease. In females, hemorrhage from the uterus sets in usually during the passive stage. After it takes place all irritability of the stomach ceases, and the patient rapidly improves; the hemorrhage is seldom so great as to require checking. These hemorrhages are all of the passive kind; the blood is forced through the orifices from blistered surfaces and wounds, by the mere mechanical action of the heart, for the tissues have become so altered that they exert no more power or constriction of the surface than sponge.

The blood does not coagulate easily, and the constant oozing is not easily arrested, except by mechanical means. In those cases, other evidence of the broken down and altered condition of the blood and tissues is found in the small, dark livid spots, called petechiæ, that now and then make their appearance. Large dark purple carbuncles are also observed occasionally in the very low putrid cases. As a general rule, the mind is usually clear, with some hesitation or confusion in expression. In many cases we find the patient apparently rational, but after recovery cannot recollect the incidents of conversation.

In many of the late epidemics there has not been wild or raving delirium in the first stage; where the vis vitæ is immediately overwhelmed, there is profound stupor and unconsciousness, and in those irregular ataxic cases there may appear nervous delirium. Delirium frequently ensues during black-vomit, and in such cases, black-vomit ceases, new strength is gained, and the patient lingers out an existence protracted for several days.

The following particulars concerning 160 of those who died of the disease in one epidemic, will convey a correct knowledge of its duration, and the character of individuals mostly liable.

Died on the night of the 4th day, - - - - -	4
" " " " 5th " - - - - -	10
Died between the 5th and 8th day, mostly 7th, - -	114
" " " 8th " 12th " - - - -	32
	160

Of this number, 4 were infants; 8 between the age of 8 and 15 years; 30 between 16 and 22; 106 between 22 and 40; 26 between 40 and 60 years; and of the whole amount, 160, only 18 were females; no negroes, but two quadroons or light mulattoes.

We here beg leave to call attention to the general febrile character of the disease. It is a malady of but one paroxysm, with phenomena peculiar to each stage, depending upon the degree of change which may have taken place in the blood and solids at the onset of attack. These stages we have thus divided into the febrile, the stage of calm, and of collapse, of which we have already spoken.

In thirty cases of fever where these stages were well defined, the mean duration of each was as follows:—febrile stage, or fever, 25 hours; calm, 117; and collapse, 14 hours. The pulse in those cases was as follows:—two days between 90 and 115 strokes in the minute, two days about natural, and two days and a half below the natural standard. The mean average of the pulse, during the whole course of the disease, being but very little, if any, above the condition of health. This pulse is at no time hard, tense, contracted or wiry, but full, soft, gaseous and wavering. After the first few hours the skin is not pungently hot, but moderately warm, sometimes imparting a feeling of coldness to the hand.

Connecting these leading and general symptoms of the disease with the phenomena of a large class of cases where the three stages are not defined, and where, with a heavy sluggishness, great torpor, and want of vascular and cutaneous action, the disease progresses to hemorrhage and putrefaction even before life is extinct, it seems evident, that while it would be useless to point out the difference existing between this disease and the essential fever on the one hand, and the true phlegmasia on the other, it is equally unnecessary to enquire, where are the marked phenomena that entitle it to the name of fever?

Post-Mortem Appearances.—Doctor J. C. Mott of Mobile, one of the most competent and careful pathologist in the South, has made several autopsies during the late epidemics that have occurred in Mobile. In those examinations, his attention was mainly directed to the stomach and liver. Of 16 dissections, he found the livers in 6 of a pale gingerbread color, dry and friable—2 olive—2 normal, and 6 darker than natural and much engorged. This dry, brittle, shoe-leather or straw-colored liver, we have found in most of the cases where the hemorrhage, either in the shape of black-vomit or unchanged blood had been excessive. We have examined four cases of this description of fever, originating in Havana and dying in the Marine Hospital at Mobile, in all of which the liver presented the above characters. The discharge of blood and black-vomit from the bowels in those cases had been going on for several days before death. This lesion is considered by Louis* as diagnostic of yellow fever. But as it is occasionally found in old drunkards and those who have died of long continued, wasting swamp fevers, and is confined (from our observations) to those cases of yellow fever which have such a termination as we have designated, it cannot, in Mobile, be viewed as a reliable or constant morbid appearance in the disease.†

* If our recollection is not at fault, O'Halloran examined at Barcelona in 1817, 60 bodies of persons dying of violent hemorrhagic putrid yellow fever. In every one of these cases, the liver is described as being of light straw color, atrophied, dry, brittle and gritty. In his work on this disease he points particularly to this lesion, describes it vividly and considers it diagnostic. Yet Louis, who made a few examinations at Gibraltar some 10 years after, seems to have the credit of being the first to point out this lesion—hence it is called "*Louis' Liver.*"

† Within the Tropics where the disease prevails in its genuine uncomplicated form, uninfluenced by climate, or the poisons of bilious fever, as it frequently is on the gulf, this lesion is doubtless far more constant and uniform.

Stomach.—Doctor Nott remarks, "of eight cases in 1843, this organ in four presented no appreciable morbid change; the mucous coats throughout were free from injection or extravasation of blood, smooth and of normal thickness; consistence good, giving strips from four to twelve lines. In the other four cases the mucous coats were more or less red, mammillated and softened. Of eight cases in 1844, the mucous membranes in three were perfectly healthy, and five presented, as the above, various morbid changes; but in no case was the membrane ulcerated or reduced to a pulpy consistence; all the stomachs contained black vomit."

In this organ, like that of the liver, the changes presented will depend upon the character, duration, termination, and treatment pursued in the case. We have examined many bodies and had the pleasure of seeing those referred to by Dr. Nott, and with a few exceptions, have not met with such lesions as would warrant the conclusion that the stomach had been inflamed. In as many as five autopsies during our practice in Mobile we have found the coats of the stomach so thin and decomposed as to give way or tear with the least force. In refering back to the history of these cases, it appears that no black-vomit had been ejected, and that the stomach was distended with this fluid which doubtless had been accumulating there for some time, and by its presence contributed no little to the breaking down of the coats of the stomach. Again, these belonged to that class of cases which are essentially of a low putrid character, the skin often giving away long before death.

The blood is invariably dark and fluid, it coagulates very slowly, the clots are soft, grumous, and easily broken down, showing a great deficiency of fibrin.

In protracted cases where local inflammation supervenes, the appearance of the organs and consistence of the blood differs from that exhibited in those cases which have the usual termination.

Those, however, who expect to find a constant, reliable, diagnostic lesion in any of the putrescent or adynamic diseases that run their course in a few days, will search in vain. The ravages of the storm are not to be traced in well marked lines, but in the general ruin around.

By refering to the history whence we get our data, you will find that the conclusions and statements made in this paper are sustained by a serried array of facts.

Negroes rarely have the disease—in 1819 they suffered severely from bilious fever; the poison of the two diseases being that autumn in active operation. In late epidemics, a few mulattoes only have experienced attacks, and these exceedingly mild, seldom arriving at the black-vomit stage.

What is called sporadic yellow fever, occurring in healthy years, is confined to strangers. During epidemics this class also has to bear the malignancy and force of the disease. Many of the acclimated, among whom are those who have had the disease in former years, experience mild ephemeral attacks. There can be no question that some persons have the disease the second and some the third or fourth time, each succeeding attack being lighter than the one preceding. Relapses or second attacks during the same season are not known; as a general rule, recovery is very rapid, frequently resulting in a condition of health greatly better than that which existed previous to attack. As a general rule, long residence in a locality where yellow fever is the predominant disease will acclimate, but a distinct attack is a greater protection than either nativity or long residence.

So far, yellow fever in Alabama has been limited to Mobile and Blakely, and but the one year (1822,) in the latter place; for since that time this town has been depopulated. In this section of the State there is every description of marsh, from the fresh water to the salt water marshes, newly made by the mingling of vegetable matter brought down by the rivers on the one side, and the marine exuviæ washed up by the gulf's tide on the other—on and around those marshes, people from different parts of the world are constantly settling—they have fevers of every known bilious type, but we have never been able to trace a simple case of yellow fever to any of those marshes or recent formations;* nor have we yet seen a case of "congestive fever," as we have defined it, which had its origin in any of those marshes or in the city of Mobile. Some few persons engaged in watching boats at Stockton, and employed as laborers about old mills, some miles distant from the city, have been seized with yellow fever; but they were in the habit of visiting town.

Experience and observation warrant the belief, that if a city were built up any where between the junction of the Bigby and Alabama rivers and the Gulf, that the excavating and principal changes consequent to such a settlement would be followed by the development of yellow fever, which would ever after assert its dominion over the diseases of the swamps and marshes.

Whilst residing in the interior, a young gentleman *from Mobile*, came into the neighborhood and was seized with congestive fever. A few hours before he died, on the fourth day of illness, he ejected from the stomach coffee-ground black-vomit in large quantities. In 1826, Mobile was healthy, and the town of Montgomery very sickly—in the month of August two gentlemen came from the former place, visited the latter, and after remaining a few days were seized with remittent bilious fever, the disease of the place, and died on the seventh day of illness of black-vomit; no other case of black-vomit occurring. As previously remarked, congestive fever does not originate in or about Mobile; the only perfect type of that disease seen there being confined to such

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* Doctor B. Dowler of New Orleans, a most curious and faithful observer, has arrived at a corresponding conclusion, in relation to the marshes at the mouth of the Mississippi.

We are aware that the most popular writers on malarious fevers, as Williams, Chisolm, Johnson, Bancroft, &c., assert that yellow fever in common with bilious fever has its origin in paludal or marsh poison. After a careful study of the essays of these gentlemen, in connection with those of the resident physicians of the Islands, we discover that this is not the only error into which these travelling authors have fallen. Their observations were confined to the disease as it appeared among the unacclimated troops connected with the different fortifications, and the loose ill considered statements made by incompetent men. Truth, in relation to these matters, can only be arrived at by many years practice and drudgery in some one of the towns where yellow and bilious fever prevail.

persons as have recently come from the interior. In the autumn of 1842, several boatsmen were engaged in raising a sunken boat in a region of country where congestive fever was the prevailing disorder; they came to Mobile 8th of September, and lodged in the infected part of the city. Some six or eight days after their arrival, they were taken ill, and conveyed to the City Hospital, then in our charge. These were the most malignant and formidable cases of disease we ever approached. They were seized with languor, headache, *sinking* at the stomach, followed with labored difficult respiration, cold sweat, lividity of the skin, small thready pulse, in fact, all the prominent characteristic symptoms of congestive fever. The physiognomy was that of yellow fever, as was also the peculiar appearance of the eye. There was a yellowness along the course of the large veins of the thigh and neck, not observable in congestive fever. The thin black fluid, with small flocculi discharged from the bowels, was that of yellow fever. There was retching, but no vomiting. Upon dissection, the stomach and intestines were found distended with black-vomit. In these unequal struggles, the two bold prominent and striking diseases, yellow and congestive fever, were easily recognized, each doing its work of destruction in its own peculiar way. These facts speak for themselves.

We have passed rapidly over the field spread out before us. To enlarge upon the character of soils and formations peculiar to certain diseases, and to point out with some particularity, the differences and resemblances of the various febrile diseases of which we have spoken, as was originally intended, we find incompatible with the limits of this paper.

In the coal, granitic, and hilly portion of the State, subjected to great vicissitudes of weather, we find the fevers to be of the continued or remittent type; usually, when not so from the onset, becoming continued as they become grave and serious. It would appear to be irritative, attended with dry tongue, slight delirium, tendency to serous evacuations, enteritis, and continuing from 10 to 25 days—displaying after death a distinct lesion of the small intestines.

In the prairie or middle part of the State, where the soil is peculiar, abounding in animal matter, we have dwelt at some length upon the character of disease that there meets the eye. It is a malady of short duration, running its course in from two to six days. The general assemblage of symptoms characterizing it, are, a deep-seated thready pulse, cold skin, profuse exudation, pale, livid, mottled, shrivelled appearance of the skin, coldness of tongue, tremulous, oppressed, irregular action of the heart, difficult breathing, violent harsh expiration; feeling of great heat, burning and smothering, anxiety, restlessness, mental depression, and gloomy forebodings of coming evil. Death ensues, and we find the surface and external tissues ensanguineous, whilst the viscera are excessively engorged with dark venous blood.

In the vicinity of the deep, humid vegetable morasses, marshes, and swamps of the Tertiary or Southern portion of the State, we find the history and phenomena of disease describing another type. The fevers there are remittent, intermittent, or continued, lasting from 4 to 15 days. The cold stage, is attended with an intense feeling of coldness, stretching and shaking of the whole frame; fever immediately succeeds, and is characterized by a pungently hot skin, white furred tongue, violent pain in the head and back, pulse increased to 120 or 140, hard and contracted, maddened impatience, and irritability of temper. This fever, high and violent as it is, rages for 10 or 15 days, without producing organic lesion or serious disease of structure. The blood becomes thin and impoverished, but the nervous system has not succumbed, as in congestive fever, nor do we see that tendency to a breaking down of the tissues and putrescency, so characteristic of the yellow fever.—Convalescence is painfully protracted, or interrupted by relapses until winter.

We pass on to the city of Mobile, and then discover, peculiar to her trodden *animalized* streets, yet another character of disease. Its mode of accession is varied; fever rages for a few hours or a day, and then its phenomena begin to lower, a short time elapses, and there is but little outward token of the deep, silent, and destructive changes that are going on. Three or four days pass by, and the prostration of muscular strength, loss of all elasticity and contractility of the tissues and muscular fibre, the passive hemorrhages and rapid tendency to putrefaction, proclaim at once its true and distinctive character.

Here are four varieties of acute disease, peculiar somewhat to certain terrestrial formations. Can any one possessed of candor and ordinary reasoning powers examine into their history, and fail to see the strikening characteristic differences. The enslaved and overtasked mind may reason, that inasmuch as they are all fevers of the same season, appearing, blending, and mingling together, and often running into each other, specific distinctions cannot be made. To this kind of argumentation, we may properly reply, that red, white, black and green, are all colors, they may be so mingled and blended, running into each other by imperceptible degrees, as to produce various shades, and associate in the mind a most intimate and inseparable connection; yet, when they are displayed in their primitive natural character, how boldly and prominently do they contrast.

In that portion of this paper devoted to a consideration of congestive fever, we briefly noticed the difference between that disease and intermittent, remarking that the only point of resemblance consisted in their periodicity, and promising to advert to that hereafter—preliminary to this, we ask attention, as a necessary part of this history, to the connection existing between the yellow and intermittent fever of Mobile.

In 1819, we find the two diseases prevailing in a most aggravated and malignant form—the negroes dying of the one, and the whites of the other. Since that time, yellow fever has been invariably preceded by a great increase of intermittent and remittent fever. In 1843, we find that in the first stage there is great difficulty in distinguishing the diseases; and in the suburbs of the city these diseases were often blended, constituting intermittent and remittent yellow fever. By passing into the dwellings you would find children, born of the same parents, living under the same roof, and nourished at the same table, attacked near about the same time with a complaint which for some hours is characterized by the same phenomena. In the sequel, one portion of them prove to be simple bilious, and the other grave malignant yellow fever. Again, examine into the character of the mild endemic remittent yellow fever of 1844, and say if the connection which existed between the congestive and intermittent fever of 1835 and 1836, which has been candidly portrayed, was any closer than that which binds together intermittent and yellow fever.

After a careful study of the foregoing history of the yellow fever of Mobile, brief and imperfect as it is, no member of your society will doubt the distinct individuality of its character, and that the poison or agent of its production is not in common with other fevers, but also distinct and peculiar. The old argument, founded in ignorance of a correct knowledge of all the facts, that yellow fever is but a high grade of bilious fever, differing only in degree, is scarcely worthy of notice—for bilious fever often prevails with a malignity unto death, without displaying any of the symptoms pathognomonic of the other. Again, well marked cases of yellow fever are often lighter and more ephemeral than even the slightest case of bilious fever. On the other hand, history discloses the fact, that congestive fever with all its perilous depressing phenomena, unlike that of any other, often displays in a mild and harmless form the same marked distinctive symptoms—all going to prove, considering it in connection with bilious fever, that they are not different grades of violence merely.

In further elucidation of the question of the unity of this family of malarious diseases, it becomes necessaay to refer to intermittent fever in its pure uncomplicated form, and in doing so, unhappily for the general salubrity of our climate, we need not borrow from the observations and reflections of those living in other countries.

The obstinate and violent grade of fever incident to the swamps and marshes of the Tertiary already described, we believe to be identical with intermittent fever, only differing in degree. Aside from this and the other disorders which have been the burthen of our narrative, we now wish to direct attention to the diseases of the mill-ponds, simple marshes, creeks, and rivers of the poor unproductive portions of our country. In the vicinity of these we find a disease, which throughout all time has occupied a conspicuous place in the catalogue of endemics, and is easily distinguished from all others. It is intermittent fever, a definition of which is here unnecessary. In some portions of Alabama, in many parts of Florida, and throughout Southern Georgia it is the only endemic disease. In many of those sections of country, especially along Flint river, the natives do not experience violent well marked attacks; but the prominent ears, flat nose, tumid belly, large spleen, slender limbs and waxen skin tell, too plainly to be mistaken, of the powerful though indirect effect of this febrile agent on the constitution. It is only under circumstances of this kind, where the only supposed cause is the decomposition of vegetable matter, that the disease is seen in its *simple genuine character*; in that form, it is the most inveterate, obstinate, well marked endemic of which we have any knowledge. In many instances it may be interrupted or changed in type, but cannot be cured; its victim may fly to Canada or London, but it will still be with him, ready at an unguarded moment to re-inflict its sting.

The product then of simple vegetable matter decomposing in poor sandy soils, would appear to be simple intermittent fever; and although it is usually modified, complicated, or changed in its effects by the emanations from different formations, yet with the exception of large cities and the frozen regions, few indeed are the spots of earth where this peculiar morbific agent does not exist. From the fact too, that we see persons affected on the highest hills, or living miles distant from the foci of this malaria, proves that it is lighter and more ærial than other morbific agents—another cause, doubtless, for its wide spread and general influence.

This chain of facts having brought us to the conclusion, that this active busy "imp of the marshes" is almost co-extrusive with the earth's verdure, it only remains, in order that we may prove its pliancy, suppleness, and perfect adaptation to the system, it matters not what may be its pathological condition, to point you to most of the diseases around us. For instance, idiopathic, and even traumatic tetanus, pneumonia, rheumatism, neuralgia, puerperal peritonitis, and other disorders, to say nothing of the immediate family of diseases to which it probably belongs.

In view then of all the relations and circumstances detailed in the foregoing pages, it is not speculative when we assert that the same meteorological influences necessary to the production of one of these poisons, seldom fail to call forth the others—consequently the effect of the one is seldom experienced without that of the other being manifest—under some circumstances, producing a disease of a blended or complicated character; whilst under others, the effect of one of these agents may so completely predominate, as to impress its separate and specific effect on the constitution. Hence it is that the development of congestive and yellow fever is preceded or attended by a great increase of intermittents, and in many instances, the former, which are properly diseases of one paroxysm, are so materially influenced by the other, as to become periodical in their course.

The exanthematous fevers, as small-pox, chicken-pox, measles, and scarlet fever, occupy amongst the groups of diseases what is called the type family.* In their mode of access, phenomena, changes, periods or stages, duration, impossibility of interfering by art with their course and perfect immunity after attack, constitute a perfect resemblance; yet a characteristic eruption stamps them as distinct diseases, caused by a specific poison or contagion.

The family of autumnal fevers that have occupied our attention, do not, in their mode of access, phenomena, changes, periods or stages, duration, impossibility of intefering with their course by art, &c., &c., discover the same resemblance one with the other, as do the exanthematous fevers—still, their origin, history, and great affinity, entitle them to be classed as one of the families of disease that belong to our State. The typhoid fevers, and probably the bilious and remittent pneumonias, should be placed on the outer skirt, whilst periodical or bilious fever proper, should occupy the centre of this circle of disease.

THERAPEUTICS OF THE THIRD EPOCH.

It will be seen that the active inflammatory nature of fevers during the preceding epoch, required the free use of contra-stimulants; anti-

* Bartlett's Phylosophy of Medical Science.

mony, venesection, and calomel usually combined with jalap were used indiscriminately to reduce the high phlogistic condition of all febrile affections, and generally with marked beneficial results. About the year 1830, the active progressive medical mind of the South and West, received new light in relation to the therapeutical action of calomel.— This newly found virtue which excited surprise that it had not been discovered before, consisted in a certain stimulant or sedative property depending on the nature of the case, and the amount given to produce the effect which the practitioner desired. The modus operandi of this venerable remedial agent became the subject of discussion by members of the profession, who at that time were dvided into three parties, each maintaining the respective doctrines inculcated at the alma mater from whence they derived the first principles of medicine, and based on experience in the treatment of disease in their native States.

The graduates of Philadelphia, adhering to the precepts of their venerable instructors, resorted freely to venesection and the use of antimony; those coming from the rice fields of the palmetto State, generally gave salts and tartar emetic; and the former resident practitioner of the time-worn lands of Virginia or Georgia, based their system of treating disease almost entirely on the use of calomel. Thus we find that men in all professions may carry with them early prejudices, without reference to climate or circumstances.

The advocates of these respective doctrines remained equally divided for a short time only, for soon they found the diseases of Alabama presented new features from those they were accustomed to witness in the older States, and want of success in practice compelled them to abandon original theories, and to direct their thoughts in a new channel of speculation.

In the far West, a new theorist in the science had sprung into existence, and with pompous display promulgated his researches in pathological anatomy to the world; and his disciples, burning with ardent zeal, infused the doctrines of their master into every hamlet throughout the populous valley of the Mississippi.

Of all the doctrines of former or latter days that have elicited the attention of mankind, none ever found the same favor with popular opinion as did that which was peculiar to this sect. The theory and practice of Dr. Rush had attained a wide spread reputation, yet there was too much obscurity in his principles of practice for the common mind; but this doctrine fresh from the Western temple of Esculapius, was within the comprehension of the most ordinary intellect, and the pathological theory of Dr. Cooke became at once the medical doctrine of Alabama. The ploughman, the mechanic, and the politician of the village were ardent votaries at the shrine of this modern Proteus.— In that confidence with which ignorance is too often sustained, when it is supposed to have grasped and matured a new principle in science, they went forth in the high-ways and by-ways inculcating it to others, and woe be to modest, doubting physicians who viewed medicine as a science, founded on rational principles extending beyond this recent discovered secret in pathology.

The constant habit of political discussion gives this description of persons a facility of strong and rapid expression of ideas, which being attended with no small degree of impudence and presumption, render them no ordinary antagonists, when the calm observer of facts met them in argument, especially as they seldom failed to carry along with them the sympathies of the people. The uneducated disciple of Cooke did not hesitate to treat the most difficult cases of disease according to this new-fangled system with perfect impunity, it mattered not as to the result. The planter would not send for a medical man to treat his negroes, and seldom the white members of his family. In this way the introduction of this popular doctrine was often injurious to the interest of those routine doctors who had promulgated it.

The practice deduced from the doctrine of vena cavaism, as advocated by Dr. Cooke and his disciples, differed but little from that promulgated by the celebrated Dr. Hamilton, of Edinbourg, and consisted principally of active purgation, by means of a well known drastic pill, bearing the name of its compounder. The principle object to be obtained, according to their view of the matter, was to stimulate the torpid and congested viscus, to eradicate the black bile from the system, and arouse the general secretions to action. Armed with a box of those pills, the uninstructed practitioner went forth to conquer or to kill, and if perchance victory over the grim enemy of man was the fortunate result of the champion, great was the laudation and many the converts; but if, as was not unfrequently the case, a much more aggravated form of disease than the original supervened, it was purely viewed as the will of fate.

In 1835, a new impetus was given to this absurd doctrine in medicine that had gradually begun to decline in popular favor, from want of success attendant on the practice, as is usually the result of all visionary schemes after having run their allotted time with the susceptible masses. This improvement in pathology and practice had its origin through the influence of a certain Doctor E. who was well known in South Alabama, where he made his appearance in the winter of 1834–35, a man of commanding figure, extremely tall, prominently marked features, and although his long hair was whitened by seventy winters, still he possessed all the energy and activity that belongs to the prime of life.—Added to his imposing appearance, there was a boldness and fluency of speech that swayed the multitude, and having at one time been the student of Dr. Rush, and subsequently graduating at the far famed University of Edinbourg, he failed not to impress his theory and practice on most of those who came in contact with him, and the few who had the rashness to oppose his opinions soon withered beneath the popular blast of an ignorant multitude.

Doctor E., in his pathology, located the primary accumulation of blood in the large venous plexus of the mesentery, instead of the vena cava or liver, as assumed by his predecessor in pathological notoriety, Dr. Cooke; but his therapeutics did not differ materially from the latter gentleman. It was only a change in appearance to sustain a sinking cause, to usher into existence a new enthusiast, and to give another local habitation and name to disease in the abstract as embracing unity.

Under such influences, at least one half of the medical men and most of the heads of families, imbibed to the fullest extent, this fallacious and erratic doctrine, which, from its apparent consistency, and coming from so venerable a fountain, seemed to the uninitiated to simplify the nature of disease. With this confidence in their knowledge of pathology, the medical practitioner of that period in the treatment of disease, discarded all relevant symptoms that so prominently mark the character of each type of fever, and directed their sole attention to the liver, as the balance wheel of power by which the sluggish and morbid current of life was to be again revivified.

The uninstructed mind, incapable of dispassionate reasoning, in fancy's fearful apprehension, was wont to behold at every sick bedside no less an adversary than the champion of death, presiding in the form of hepatic inflammation or congestion, in which all their attention was absorbed, and with which they entered into a rash encounter with formidable weapons of potent agency, to master with a deadly blow, the grim monster, forgetting that disease is tractable only as we ascertain the powers of nature in its morbid condition and assist in relieving it. The unfortunate liver was here made the citadel of attack, against which enormous doses of *calomel* were discharged in rapid succession, until the undermined frame gave way to repeated bombardment. The voice of suffering humanity appealed in vain to the doughty chieftain who carried on the warfare, to cease for a time his operation, to raise the siege, and permit nature for a space to recover its balance of power, but without the remotest chance of pacification. The calomel vial was again and again appealed to, to stimulate this dormant function, until bile on bile, black as the demon of darkness, was thrown forth, which in their ignorance of chemical action, was mistaken for the presence of the enemy, and the exhibition of forced excretions, instead of being viewed as a sign of subjection to the power of medicine, was heard as the voice of menace and the warfare was continued with greater activity.

This contest between the *doctors* and the *liver* was generally maintained until a serious change ensued from derangement of structure or organic lesion, and the patient sank into the arms of death. It is unnecessary to enter into detail of the principle involved in the therapeutics of those misguided men who had been turned aside from the true path of medical knowledge, and wandered in the wilderness of error, who had listened to the false teaching of a visionary enthusiast, and embibed such extraordinary doctrines; they give rise to reflections of too serious a character to permit the language of ridicule, and yet we are hardly disposed to treat them with any degree of gravity.

The following comments, from page 237 of Bartlett's phylosophy of medical science, are applicable to the practice pursued in Alabama, during the years 1834–35 and 36.

"The follies of German uroscopy are outdone by those of practical vena cavaism. Its votaries seem to have forgotten that there is any organ in the body except the liver, and in the management of disease, the only important points to be determined are, what is the colour, what the consistence, the odor, and the quantity of the stools.

"Diagnosis is wholly discarded as a matter merely of idle curiosity, and of no practical importance; and prognosis is founded almost exclusively upon the character of the alvine evacuations. If these are *bilious*, as it is termed, if they are consistent, and dark colored, every thing is going on well, and the prognosis is favorable.

"The Cookite would be utterly at a loss, in regard to the state of his patient, if he should be deprived of the aids which are furnished him by a daily and nightly inspection, ocular and nasal, of the stools. They constitute his guiding star, his rudder, and his compass; they shed a clear light on all his pathway, which but for them would be darkness and uncertainty itself. The language of his sect, as usually happens in similar cases, has passed into the popular tongue, and we hear from all invalids daily and hourly complaints that the liver does not *act*, that the liver is *torpid*, that the liver is *locked up* and so on. Almost every ailment to which the body is subject, functional or organic, trifling or grave, chronic or acute, is immediately referred to this ubiquitous and autocratic organ.

"All and each of these ailments can be removed only in one way, by the restoration of the biliary secretion, by inducing the liver "to act"; and this can be accomplished with certainty only by one remedy, calomel."

About the year 1830, the Thompsonian, or steam system of practice was under certain restrictions licensed by the Legislature of this State. A leading principle of this doctrine was, that "Life is heat and death is cold," and when the vital spark was in danger of being extinguished, it was to be supported by the free use of stimulants externally and internally.

However false the theory, the practice was much more appropriate to the type of disease of that period than any other in vogue. We should not, therefore, be surprised that this practice, though confined to the hands of that illiterate class, which is ever ready to seize upon any and every thing new or monstrous in politics, religion, or medicine, should have achieved signal triumphs over the equally false theories and but too fatal practice of the unreflecting *routinist* of the regular profession.

We have already stated that about the years 1834 and '35, typhoid fever made its appearance on many of the plantations, especially in Dallas and Perry counties. The character of this disease has already been noticed. Treated as it was by the "regular Doctors," with salts and tartar, active cathartics, mercurials to act on the liver, &c., the disease was fatal to one-half of those attacked. The want of success amongst the "regulars" was so marked that they were in many instances discharged, and the *red pepper* gentlemen summoned in their stead.—Those of the latter class who used the capsicum freely, and laid aside to a great extent emetics, were in comparison rewarded with marked success, and a great impetus was in consequence given to the popularity of the Thompsonian system. In many of those localities where its triumphs were achieved, it still holds its place in popular favor—other remedies of a kindred nature have been since added to their materia medica, as quinine, camphor, opium, &c.

Though the mass of the "regulars" rushed blindly on, like Sangrado, slaying every thing before them with the exhausting remedies which the schools had prescribed, the errors were too palpable to escape the notice of all; the thinking part of the profession, appalled by the mortality of disease, began to study its nature and look out for more rational indications of treatment. To minds of this class are we indebted for the advance of our profession; and to such perfection has our experience and observations arrived, based as they are upon the modern lights of pathology and the *modus operandi* of remedies, that the acute febrile diseases of the South are now met with the full confidence of success. An incidental notice, as it were, of the general plan of practice agreed upon by most of our medical men in the treatment of some of the maladies described in the foregoing pages, will close this paper.

The successful treatment of congestive fever demands the most prompt and energetic application of positive remedies—*la medicine expectante* is here out of the question, and the indications stand forth clearly and boldly; the heavily oppressed nervous and vascular systems, the cold and shrunken surface, the engorgement of internal organs, the general prostration of the vital powers, all appeal strongly to you for prompt relief.

To equalize the circulation and call back the vital heat to the surface, is the first indication, and with this view revulsives are applied extensively over the external surface, strong cataplasms of mustard are applied to the abdomen and extremities, and the more extensively the better; in addition to such applications, with which too many practitioners are satisfied, we would beg léave to impress upon the reader the importance of dry cups along the whole course of the spine, to be followed immediately by one of the above stimulating cataplasms. The reaction following this plan has been so prompt and decisive in many instances, as to force upon us the conviction that in the spinal column is to be found the pathological centre of all the mischief. In plethoric subjects, determinations take place to the abdominal or thoracic viscera, demanding also the free use of cups.

In the Summers of 1835 and '36, when it was not unusual for the patients to remain several days in a cold adynamic condition, (the disease constituting but a single paroxysm,) we were driven to the use of long continued frictions, a copious kettle of decoction of capsicum and mustard in brandy, (or rather tincture,) was prepared, and a servant stationed at each extremity was directed to keep up rapid frictions with the liquid heated almost to the boiling point. In this way a partial reaction was produced and kept up until the internal remedies had time to come to the rescue and stave off the fatal *collapse*.

Calomel, opium, quinine, and piperine are the favorite remedies for internal administration; they are used singly or variously combined, according to the condition of the patient and peculiar notions of practitioners. If vomiting and purging, either separately or together exist, calomel and opium (5 grains to 1) may be given every hour until quiet is restored; enemata of starch and laudanum are also used with happy effects. Many practitioners, who are ordinarily opposed to complication of remedies, prescribe in urgent cases something like the following formula: Calomel grs. v, opium gr. $\frac{1}{2}$, piperine gr. i, and quinine grs. iii, every hour or two—some one of these ingredients may be left out where from its specific effect it is thought inapplicable. After a few doses the calomel and opium are withdrawn, and the quinine increased in quantity and continued for a longer or shorter time, according to the duration and circumstances of the case.

Quinine is not given in congestive fever as a *stimulant* to promote reaction. On the contrary, it is a popular and well-founded impression that when given in adynamic diseases in large doses, its effect is decidedly sedative and consequently prejudicial in this stage of the disease. It is therefore given in combination with stimulants, as opium, capsicum, brandy, ammonia, &c., in order that it may have time to exert its peculiar anti-periodic effect. During the state of extreme depression, many gentlemen give opium and brandy to the exclusion of every other remedial agent. Some give as much as 100 drops of laudanum every hour until a change in the symptoms is produced. We have never given it in this way, but are assured by gentlemen of experience and judgment that it soon arrests the pernicious tendency of the disorder, and places the system in a condition to be easily and happily influenced by external and internal stimulants, and thus enable them to guard against another paroxysm.

We have already commented on the great abuse of calomel in former years. The prejudice excited against this article is so great that many medical gentlemen are reluctant to acknowledge its utility in the treatment of this or any other disease. To satisfy the caprices of patients, we have sometimes treated cases of congestive fever without calomel, but have met with great difficulty in establishing full reaction and in restoring the secretions to their normal condition. We are convinced that nine-tenths of our practitioners, (amongst whom are many of its loudest denunciators,) use mercury in some form in all acute diseases—the mass of testimony in its favor is too strong to be rejected.

Although quinine, as before remarked, would, if given alone, be hurtful in some stages of this fever, yet it is administered by all, especially in cases of intermittent or remittent type; its omission even in decided doses would be severely censured by the great mass of medical men in this State.

In but few cases do even the most "bold and bloody" use that "double edged sword," the lancet—that *ignis fatuus* theory has often led the way to the chamber, but the murdered victim has as often risen up in judgment. It may occasionally happen under peculiar combination of symptoms, as in the second case of Dr. Wooten, that depletion may be resorted to, but such cases are exceptions to the broad general rule.

Dr. Ames, in his notes of cases, remarks of the cerebro-congestive-remittents, that "quinine is better borne in this than in any other form of congestive fever. It seems to be tolerated as tartar emetic is tolerated in inflammatory pneumonia, and bleeding in encephalic inflammation. I gave recently to a negro boy, under twelve years of age, about fifty grains within twelve hours, without producing any deafness or ringing in the ears. Its good effects, however, were none the less evident.*—Bleeding, so far as I have seen, even in the smallest quantity, does mischief, and the bad effect is in proportion to the quantity and the rapidity with which blood is drawn. Blisters and sinapisms are valuable adjuncts, particularly the former, and so is the hot foot-bath. When the pulse becomes more feeble under the use of quinine, and the sweating profuse, and debility greater, I know of no remedy equal to carbonate of ammonia to counteract these effects. When they are observed I always sus-

* Dr. Lewis takes this method of informing his friend Dr. Boling, that he, (Dr. Lewis,) would be as far from classing these disorders with *the phlegmasiæ*, as Dr. B. would "*the unknown poison of yellow fever*" with remedies.

pend the quinine, and these observations apply as well to one form of malignant fever as another."

In the treatment of the yellow fever in Mobile, no physician of any experience expects to *cut it short;* hence, all due excess or the least forcing is carefully avoided. Emetics, blood-letting, and in fact all contra-stimulants are viewed as highly pernicious. We have already mentioned that the physicians of that place watched the approach of the collapse stage with great anxiety, for it is at that particular juncture that his art is most available. When it is discovered by the wavering variable pulse, increased restlessness and the other signs premonitory of the advent of this critical stage, a stimulating treatment is immediately begun. There is probably no disease or condition to which the system may be reduced that requires more than this the administration of positive stimulants. We are convinced beyond persuation to the contrary, that by the use of brandy alone, given at the proper time, hundreds, even whilst trembling on the verge of the grave, have been rallied, sustained, and restored to life. But we cannot stop now to give even an outline of the treatment pursued in this disease; to such gentlemen as feel an interest in it, we respectfully refer them to the March number of the New Orleans Medical and Surgical Journal for 1845.

Although quinine in large doses, was not in general use until 1835 and '36, yet there were a few who gave it in 5, 8 and 10 grain doses as far back as 1830. Dr. Parker, of Florida, during a short sojourn in Alabaman in 1831, advised its use to many in 5 and even 10 grain doses. This gentleman considered it tonic and anti-periodic. Drs. Perrine and Cartwright, of Mississippi, and Dr. Fearn, of Huntsville, were probably the first to give it in large doses during the exacerbation of fever. We believe that Drs. Allison, of Dallas, Vincent, of Autauga, and Ames, of Montgomery, gave this salt in 5 and 8 grain doses in every form of endemic fever as early as 1834. They administered it during remissions or the declining stage of fever.

On the subject of its early use in what are supposed to be inflammatory affections, Doctor Ames addresses the writer as follows: (his letter was not intended for publication.) "In 1835 I gave it first in an inflammatory affection—the case was remittent pleuro-bronchitis, which the quinine removed as by a charm. At this time, this form of disease was rare, and I do not recollect that I used it again until 1839. You were certainly the first to use it extensively and generally in pulmonic diseases in 1837. In the fall of 1842, and winter following, several physicians in this place began to employ it in the inflammatory affections which then prevailed, but I do not think its purely contra-stimulant nature was fully recognized at that time. The circumstances would have led to its use, on account of its acknowledged febrifuge qualities alone. The ordinary endemic remittent fever became, on a change of weather in October, suddenly complicated with pulmonic inflammation, *without any* other change in its type; the treatment consequently was not changed, and quinine was found to be quite as beneficial afterwards as before the change—allowance being made for the increased violence and obstinacy of the disease induced by local inflammation. Since then it has been employed in our inflammatory powers as one of the principal remedies; and the occasional observation of its effects in cases in which the causes of our endemic fevers were not in action, and in which there were no symptoms, not common to the inflammatory affections of higher latitudes, has satisfied some of our medical men of the true nature of the remedy; whilst others, with the same facts before them, still look to its well known febrifuge and anti-periodic properties for the explanation of its beneficial effects in these cases. My own opinion as you are aware, was formed several years before, partly from observations of this kind, but principally from facts of another class, without which, it seems to me, the evidence is very imperfect. These relate to cases of direct debility; either diseases of this kind, or exhaustion from hemorrhage, or protracted diseases in which the depressing effects of bleeding or cathartics have been aided by long abstinence.—In these cases quinine is always injurious, quickening the pulse and augmenting the general debility. In many instances, during the last 16 years, have I seen its use in this description of cases followed by the most disastrous consequences—apparently depressing the powers of life with astonishing directness and rapidity.*

Doctor Ames mentions that we were "the first to use quinine generally in pulmonic diseases in 1837;" the disease alluded to was intermittent pneumonia, which prevailed throughout the months of November and December of that year, confined however to those localities where simple intermittent was the predominant disease. The paroxysms in those cases were very violent, attended with difficult breathing, prostration, small, compressible pulse and difficult expectoration, that which was raised being more or less cold, and intermixed with blood. The remissions [for notwithstanding each paroxysm was usually ushered in with a chill, it cannot be properly regarded as intermittent,] were, considering the violence of the paroxysms, very complete. After the two first cases, which proved fatal, we began what we then viewed as an experiment—the use of quinine, in decided doses. We lost but few cases after this. We did not, after some reflection and experience, consider these inflammatory affections; nor did we give quinine, believing it would act as a contra-stimulant; for in that case it would not have been advisable; but we gave it solely in reference to its anti-periodic properties, and to quiet nervous irritability.

During the two past seasons of winter and spring, we have treated in the Marine Hospital, 40 cases of pneumonia—the subjects were mostly active robust young men belonging to the volunteer service. We studied the character and tendency and effect of remedies on this disease, with no little cure and interest. Twelve were of a remittent form, 5 pleuro-pneumonia, 10 bilious, and 13 typhoid pneumonia. In all those of the remittent, and a few of the bilious type, quinine was used at different times; but was beneficial, so far as we could discover, in only 3 of the remittent, and very partial in these. In three of the bilious variety the pulse was corded and tense; general blood-letting was practiced in those. Cups were used in every case, but it was seldom

* What would Dr. Ames, with his well-earned knowledge of its effects, say of a *Southern professor* who detailed with much self-satisfaction, to a large assemblage of students and medical men, a case of obstinate, profuse, uterine hemorrhage, in which other remedies being slow to act, he administered 60 grains of quinine?

that the strength of the patient would permit the abstraction of blood even in this way. Tartar emetic was not tolerated in doses exceeding the eighth of a grain. Alterative, such as calomel and Dover's, and blue pills were occasionally given, expectorants were also used, and purgatives were necessary in most of the cases. The reliable remedies, however, were mustard cataplasms and poultices after the manner advised by Dr. Baldwin of Montgomery, and the carb. ammonia. These were used more or less in every case, and throughout the whole course of the disease. In 6 cases of the typhoid or continued variety, we gave brandy in combination with ammonia, and with decided advantage. In many cases it is necessary to sustain and strengthen the patient in his exhausting efforts at expectoration—otherwise, the immense accumulation of pasty, dark-colored matter within the cavity of the chest would have suffocated him. Stimulants, when judiciously given, instead of checking, materially aid expectoration, upon the ease and profuseness of which depends the life of the patient. This disease runs its course in from eight to 14 days, and it is dangerous to attempt to *cut it short*; 3 of 40 only proved fatal.

We are convinced, beyond the shadow of a doubt, that had contra-stimulants been freely used in the treatment of these cases, *not one would have survived*; had quinine then been given with the best results, it should not on that account be classed among contra-stimulants—if so, brandy, opium, and ammonia are entitled to the same distinction.

Gentlemen of the present day are of opinion that had quinine been freely used in the treatment of the "bilious inflammatory fevers" of the second epoch, the mortality and suffering would have been materially lessened. In the vicinity of the river swamp there occasionally occurs, in plethoric healthy emigrants, cases of continued bilious fever, attended with "violent pain in the head and back, injected eye, hot skin, and *corded tense pulse*." We have administered quinine in "sedative doses" in many of these cases, and it never failed to augment all the inflammatory febrile symptoms. We could, time permitting, go on stating innumerable instances, all going to show that quinine is hurtful in all the truly phlegmonic, whilst it is beneficial to a proverb in all the adynamic periodical affections of the State.

Dr. W. M. Boling has maintained, in able and elaborate articles on the use of quinine, that it is contra-stimulant and highly serviceable in the treatment of *inflammatory affections*. Inflammatory diseases, strictly so, are now rare among us; they belong, as we believe the preceding pages will show, to a period long since gone by. We have examined somewhat the notes of cases upon which Dr. Boling predicates his opinions. Complicated as they are, these cases cannot be properly classed with the phlegmasiæ. With a gentleman, who like Dr. Boling, has so materially contributed to the medical literature of the South, and reflected so much honor on his adopted State, it is painful to differ; and nothing but the obligations, resting upon an impartial chronicler, could force us into such a position. We may be wrong and Dr. Boling right, be this, however, as it may, he has too much liberality not to agree with us in the sentiment of the poet,

"One man's word is *no man's* word,
Truth demands that all be heard."

We cannot conclude without stating that in 1834 Dr. B. R. Hogan began, in the treatment of *all* our acute endemic diseases, the use of the most active stimulants, and that it is the impression of this and many other gentlemen, that they can be used in *some stage* of every variety of Southern fever, with signal advantage ; but like quinine and morphine, experience and discrimination are required in their use.

In submitting this rough and hasty sketch, permit the writer to say, that he has experienced no little trouble in collecting the material which is embodied in the "first and second epochs"—if, however, he has succeeded in gleaning from the neglected *past*, any thing of value or interest to the profession of the State, then he is repaid for his labors. In his history of the diseases and therapeutics of the third epoch, he has aimed at nothing more than to designate the particular portions of the extensive pathological field spread out before you, which most requires cultivation. The description of the predominant leading character of disease somewhat peculiar to certain geological formations and the distinctions drawn between these diseases, are peculiarly his own. In your large association, each one of those sections is represented ; the members can judge whether the writer has drawn from diseases and the circumstances that surround them, or the time-worn dogmas and false deductions in relation to Southern fevers, which have shackled and deluded the profession for so many generations. Whether the effort meets the approbation of the society or not, he at least hopes it will start enquiries and investigations which may lead to valuable results.

December 2d, 1846.

II.—*Remarks on Retention of the Placenta.* [Read before the Medico-Chirurgical Society of Louisiana, on the third of September, 1845. By E. D. FENNER, M. D., Corresponding Secretary,]

It will be recollected that at our last meeting, I apologised for not being prepared to read my promised paper on the subject of *Retention of the Placenta*, and asked permission to offer in its stead the report of a *case of Hepatic Abscess*. My apology was accepted, and *my case too*, but I was still held to my obligation to read the proposed paper at the ensuing meeting. From this proceeding, I am induced to infer that considerable interest is felt in regard to the subject I had chosen, and I now take pleasure in bringing forward what I have to say, chiefly under the hope of eliciting the opinions of the different members of the society.

The subject is surely replete with interest and importance, for it often presents one of the most embarassing cases in medical practice, and I am induced to believe that very vague ideas prevail in regard to its management.

At the very threshold, I must declare the belief, that most writers on obstetrics authorise and the generality of practitioners exercise too much manual interference in the management of the *retained placenta.* I have no doubt that death has thus been caused in thousands of cases, which, if left alone to the efforts of nature, would have terminated most happily.

As I speak to practical and experienced men, I need not descend to minutiæ, but will confine myself as much as possible, to what I conceive to be important points and a brief narration of such facts as have fallen under my own observation.

Upon looking into the authorities, ancient and modern, we find that different rules of practice have prevailed at different periods, in regard to the management of the *placenta.*

Hippocrates did not recommend any extraordinary means to bring it away. The introduction of the hand into the uterus for this purpose, does not seem to be advised, or to have come into consideration in his time. The case was left pretty much to nature.

Celsus appears to be on the opposite extreme; he put no reliance in the efforts of nature, and directs that if the afterbirth is not expelled very soon after the child, the hand is to be introduced for the purpose of taking it away.

Paré, who wrote in the fifteenth century, seems to have generally followed this practice, but recommends in *strong and repeated terms*, the exercise of *extreme caution;* not to use violence, lest we should invert or otherwise injure the uterus.

Ruysch, who paid great attention to this subject, and who, by his anatomical researches, threw much light upon the physiology of the fœtus, forbade the placenta to be extracted hastily—choosing clearly to run the risk of the evils that might follow the imperfections of nature, rather than of those which would be incurred by the harsh and violent methods then in use.

The practice of Celsus, however, continued in vogue till the time of William Hunter. This celebrated physician, having witnessed many fatal results from the officious interference of accoucheurs in removing the placenta, resolved again to rely upon the efforts of nature alone.—After mature deliberation, and not without considerable apprehension as to the result of the innovation, it appears that he and Dr. Sandys, who with him had charge of the Middlesex Hospital, determined to put the resolution in practice. We learn that in the very first case, the placenta was retained twenty-four hours, but no ill consequence followed. The practice soon became general; but several untoward accidents having occurred, it was again abandoned and that of Celsus revived, which, modified and improved, has continued in vogue down to the present day.

Nearly all the recent writers on obstetrics direct, that if the placenta is not expelled within a short time, (say *one hour* or *two hours,*) after the child, the hand must be introduced to ascertain the cause of detention, and generally to remove it; but I fully concur with our distinguished countryman, Dewees, in setting "my face against *Time*" in the management of such cases. Circumstances alter cases, and no *rule* can be universally adopted.

Denman has laid down the very best practice in the management of the placenta, founded, as he justly remarks, "on common sense and observation;" it is, that the placenta ought to be, and is generally expelled by the action of the uterus, in the same manner as the child, feeling ourselves at liberty, and called upon to assist, *only* when this action is not equal to the purpose, or when a hemorrhage or other dangerous circumstance demand our assistance.

The opinion prevails almost universally, that if the placenta be not brought away within a few hours after the birth of the child, the woman is placed in great danger, independent of uterine hemorrhage; that it is a dead and extraneous mass which becomes pernicious every moment it remains, and that if not brought away immediately, the os uteri would close upon it so that it could not be extracted. Hence there is generally felt upon such occasions a great degree of apprehension and alarm. But there is a great deal of error in all this.

It is certainly desirable that the placenta should come away soon after the birth of the child, and all prudent and judicious means should be used to accomplish this object, for then the delivery is complete and much anxiety is removed; but the placenta may be retained for hours, days, or even weeks, without proving fatal.

Dr. Ramsbotham says, that "cases are on record in which the placenta *never passed*, it having been supposed that the whole, or the greater part of it, had been absorbed by the action of the uterine vessels."

Knowing the astonishing resources of nature, Dr. R. says he should not have the temerity to deny the possibility of such an occurrence, yet he would consider it unwise to leave the placenta in the uterus under the hope of getting clear of it in this way.

He relates a remarkable case that fell partially under his own observation. In 1829, he was called to assist a medical friend in the case of a young woman, first pregnancy—*sixth day* after delivery of a dead child, the placenta still retained, the funis broken away, no hemorrhage, os uteri almost closed, could not feel the placenta, slight fever, but no urgent symptoms. He saw her again two days afterwards—nothing could be done. His friend, (in whom he says he had every confidence,) watched the case closely *more than a month;* during which time a portion of placenta the size of a walnut was expelled. She recovered her health perfectly in six weeks; nothing more of a solid nature ever passed; he does not know whether the woman ever menstruated again.

Dr. Denman once saw an instance of a whole placenta retained till the fifteenth day after the birth of the child, and then expelled with little signs of putrefaction, except upon the membranes; the whole surface which had adhered, exhibited marks of a fresh separation. The recovery of this woman was very fortunate.

You are all aware that retention of the placenta may take place from three causes:

1st. Uterine inertia;

2d. Adhesion of the placenta to the uterus;

3d. Irregular contraction of the uterus.

In regard to retention by inertia, I shall be very brief. A slight inertia may generally be expected after the tremendous effort required to expel the fœtus; at least, it most frequently happens that some time elapses before the uterus resumes its contractions with sufficient force to expel the placenta. At this stage, the accoucheur should excite the action of the uterus by means of friction, compression, and cold wet cloths over the hypogastric region, gentle stimulants, ergot, &c.—When the womb is felt firmly contracted, and the pains recur, traction at the umbilical cord may be used—retching also tends powerfully to excite the uterus to action. Blundell relates an instance where the physician being fatigued with exertion to excite the action of the uterus, and also embarrassed, left the chamber for a short time—while he was absent, an old woman stuck a candle down the patient's throat, which excited violent retching, and the placenta was at once expelled. The feelings of the doctor upon his return, are not to be envied. I have upon several occasions, where the placenta remained several hours from ordinary inertia, known it to be brought away by the action of a dose of castor oil upon the bowels.

There is one principle not to be forgotten at this stage, that is—*not to take away the placenta unless the uterus be felt firmly contracted.*

Retention by adhesion presents a much more difficult case, and here I am inclined to believe that the most of writers authorize too much manual interference. They caution us seriously and emphatically against the dangers and injuries to be apprehended, but still direct *the adhesion to be broken up by the hand, and the placenta to be forcibly taken away.*

Now if there be but partial adhesion, there is liable to be considerable and even dangerous hemorrhage; and if the uterus is disposed to contract, it would certainly be proper to take away the placenta. But if the adhesion be *extensive* and *strong*, with no pressing emergency from hemorrhage, convulsions, or any thing of the kind, I should certainly deem it more safe to trust the case to time and the efforts of nature, than to run the risk of inversion, or of exciting fatal inflammation of the uterus, by *tearing away the placenta*, as many direct. I have no doubt that thousands of women have perished by this proceeding, although I suppose it must be considered *secundum artem.* I confess my own experience has been quite limited in the management of retention by adhesion, occurring at the full period, and I now call attention to it chiefly with the view of eliciting the observations of other members of the society in regard to it. I will lay before you, however, the following extraordinary cases of abortion, in one of which the placenta was retained 3½ *days*, and in the other 35 *days*.

Case.—On the 21st December, 1837, I was called to see Mrs. W., a rather small and delicate lady, the wife of a Mississippi planter, aged about 25 years. Her health had been quite delicate for some time from diarrhœa and abdominal pains. She had had before numerous abortions and premature births. From the customary irregularity of her catamenia she was now uncertain whether she was pregnant or not.

I found her suffering uterine pains—she was shivering and chilly—pulse quick—great thirst—and a slight hemorrhage. Upon placing my hand upon the hypogastrium I could distinctly define the dimensions of the uterus, and was convinced that she was not only pregnant, but on the eve of an abortion.

I gave her a pill of the acetate of lead and opium, and had hot bricks applied to the feet.

Within an hour afterwards, a fœtus of about three months was expelled. She expressed herself as feeling much better; the uterus contracted pretty well, and the hemorrhage was trifling. The tenderness of the abdomen was so great that she could not bear friction. A cold wet towel was now applied over the hypogastrium, and gentle traction made upon the umbilical cord. The cord, however, was so weak that it was easily broken off.

The ergot was now administered, which slightly increased the uterine pains, but not sufficient to expel the placenta. I then attempted to introduce the hand; but the relaxation necessary for the expulsion of so small a fœtus was so slight, and the pain and soreness were so great, that I was compelled to desist. There being no dangerous symptoms, I left her under the influence of the opiate pill, and directed a purgative enema to be administered a few hours afterwards.

Four o'clock, P. M.—I found my patient easy and comfortable—she had slept—pulse natural—no thirst. The enema and also a dose of oil had been given, but without moving the bowels—the placenta still remained—no hemorrhage.

I determined to await the operation of the purgative, and left her with directions to repeat the opium and lead pills, and to re-apply the cold towel if hemorrhage should appear.

22nd. Patient had spent a good night, and was cool and comfortable—bowels had operated three or four times, but the placenta was retained—soreness diminished.

I determined to make another effort to extract the placenta. I introduced my hand gently into the vagina, but found it impossible to make it enter the *os uteri*. The placenta could be felt with the finger, and a portion of membrane was extracted that admitted of considerable traction; still the mass could not be brought away.

Finding it impossible to get the hand into the uterus, I determined to irritate the parts no more.

Ordered the ergot to be tried again, and a warm fomentation to the abdomen.

Evening.—Found patient cool, easy, and comfortable, with the exception of slight nausea produced by the ergot—about the customary amount of lochial discharge—the placenta still retained. Advised a glass of warm water, and the finger to be thrust into the throat to encourage the ejection of the ergot from the stomach—other directions continued p. r. n.

23rd. Rested well through the night, but now has slight uterine pains—skin cool—no thirst—pulse rather frequent—moderate discharge from the uterus. Ordered low diet and perfect rest.

24th. Had considerable uneasiness in the bowels, but no fever.—Ordered to be purged with ol. ricini.

Evening.—During the action of the cathartic, the *placenta was expelled*, and the patient is now relieved. The placenta was very large, and quite fœtid. Not an unpleasant symptom followed, and the lady soon recovered.

Here we have an instance of the placenta retained *three days and a half*, without any unfavorable result. I thought at the time this placenta was retained by *adhesion*, but as the attachment could not be reached with the hand, there must remain some doubt. It is possible it was retained by spasmodic contraction of the os uteri.

Case.—The following remarkable case occurred in a servant of my own, a negro woman aged about 35 years; healthy, and of robust statue, but very narrow in the pelvis; so much so, that her previous labors, of which she had three at full time, were *extremely difficult*, proving fatal to the children in every instance *save one*.

On the night preceding the 1st of March, 1846, she aborted with a fœtus at about the 4th month. She had but little hemorrhage, but the placenta did not come away. I arrived at home about 11 o'clock on the morning of the 1st of March, and found her pretty quiet and comfortable—a dose of castor oil had been administered, and her bowels were freely purged, but the placenta was still retained. There was no flooding, nor any distressing symptom. I made an examination *per vaginam*, and found that the placenta, still within the uterus, could barely be touched by the finger.

I administered ergot freely, and when there were slight uterine pains, I drew gently at the umbilical cord; but this was so small and weak that it immediately broke away. The fœtus was very small, and the vagina and os uteri were so little dilated, that I found it impossible to introduce my hand. The uterus was firmly contracted, and there was no hemorrhage. I let her remain quiet.

March 2nd.—She had rested well, and was quite easy and free from fever. Gave her a diuretic of spts. nitr. dulc. and infusion of parsley roots, and directed her to sit over a vessel of warm water, until she felt an urgent inclination to urinate.

A copious flow of urine was soon produced, but the placenta was still retained. She was kept on low diet.

3rd. Rested well—in the evening had slight excitement, and slight discharge from vagina. Gave cathartic pills at bed time.

4th. Felt better—bowels freely moved—no fever—no thirst—some appetite—not so much uterine discharge.

5th. Did not rest well—had pain in back and abdomen—slight hemorrhage—to-day feels better—uterine discharge considerable, and slightly fœtid. Evening.—pulse a little excited—disagreeable throbbing in the head. Gave a purg. pill, of rhub. al. and cal.

7th and 8th. Symptoms all better—uterine discharge small, and very slightly offensive.

10th. Chloe says she feels almost well—walks about, and has a good appetite—she says her abdomen and womb do not feel as if any thing were retained—but it is evident the placenta has never been discharged; and I, myself, removed the umbilical cord without it. The uterine discharge is but slight, and *not now offensive*.

15th. Patient did very well since last date, until yesterday, when she had headache and pains in the limbs. She took a cathartic pill last night, and feels much better this morning. She has an appetite, and walks about the house. The discharge is light colored, moderate, and has a very slight odour. Placenta still retained.

25th. Patient has done well since last report, until last night, when she had a return of the pains in her body and limbs. This morning feels quite unwell. Has slight fever—tongue white and furred—she has a sense of weight and bearing down in the pelvis. Gave her a cathartic.

26th. Entirely relieved—she went to work of her own accord—says there is still some whitish discharge—placenta still retained—and her abdomen appears somewhat enlarged.

From this date she kept about her business, and appeared as cheerful as usual, until the 3rd of April, when Mrs. C. who was living with me, was taken in labor; and while waiting upon her, Chloe was suddenly seized with flooding.

She went to bed immediately—had cold wet cloths applied to her abdomen and vulva—and took a pill of opium, acetate of lead, and ipecac.

The hemorrhage was promptly checked, but she had considerable pain in the head and back—her extremities were cold, and she rested badly during the night.

April 4th.—Hemorrhage ceased, but feels very badly—is costive.—Gave her a dose of castor oil, and ordered her to keep her bed. About 4 o'clock in the evening, (the time she was attacked the day before,) hemorrhage returned with great violence. Oil had not operated—she was very much prostrated—extremities cold—pulse feeble and quick—pain in the back and hypogastric region. Repeated the opium and lead pill and cold applications, and gave brandy and water.

The further minute details of this case, would be too tedious to relate upon this occasion. Suffice to say, it proved to be one of the most desperate attacks of uterine hemorrhage that any woman ever survived. In spite of the most powerful remedies—opium, both in substance and tincture, in extraordinary doses—acetate of lead—French brandy—camphor julep—the tampon, &c., &c., the hemorrhage continued for two or three days, and she was reduced to the very verge of the grave. She had dyspnœa, dimness of vision and syncope—she was almost pulseless, and the extremities as cold as clay—in short, nothing but incessant attention, both by day and night, saved her from death.

But to return to the object of our special attention, *the placenta.*—This was expelled on the night of the 4th of April, *thirty-five days after the birth of the fœtus.* It was reduced to the size of a walnut—it appeared to be firm and fleshy—and *not at all putrid or offensive.* A professional friend, (Dr. Wydown, of Clinton, Mississippi,) examined it carefully with me, and neither of us had any doubt of its being the *long retained placenta.*

The convalescence of the woman was exceedingly tedious. Being pretty black, she became of an *ashy hue,* from the state of anæmia to which she was reduced. She recovered her health perfectly, however; has been pregnant again; had a dead child at full time, and is still living, a robust and hearty woman.

I presume that in this case the retention was caused by *adhesion;* for the placenta could have only been prevented so long a time from *complete putrefaction* by the continuance of the circulation between it and the uterus.

It is particularly to *retention by irregular contraction of the uterus* that I wish to call the attention of the society, for I am convinced both by experience and reflection, that very grave error prevails generally in regard to it. You are aware that this irregular contraction may take place either at the mouth of the womb, or in the central circular fibers, or in the circular fibers of one of the cornua.

When the contraction occurs in the central fibres, the womb is divided into two distinct compartments or chambers. In this case, the placenta may be partly in both. This is properly the *hour-glass contraction* spoken of by authors.

It is believed that more or less adhesion of the placenta remains in all cases of irregular or spasmodic contraction, and that its cause may be traced to this, assisted by improper traction on the umbilical cord and other improprieties.

Ramsbotham and Dewees think it is a very rare case; the former says he never met with but three or four well marked cases; and the latter that he never met with a single instance, where he had had the entire management of the case.

On the other hand Burns says it is *exceedingly common.*

In regard to this question, I am disposed to believe it depends pretty much upon *one's fortune;* and although I have no doubt the occurrence is frequently brought about by officious and improper management, yet that independent of this, one physician may meet with as many cases in the lapse of two or three years, as another will in forty.

For the most part there is not much hemorrhage attendant upon *hour-glass contraction*, yet it is sometimes quite severe where there is but *partial adhesion* of the placenta, accompanied by irregular and spasmodic contraction in one of the cornua of the uterus.

To come at once to the management of retention by irregular contraction, I must declare my belief that this contraction is always of *a spasmodic nature*, and that it is far better and more safe to rely upon *anti-spasmodic remedies* for its removal, than the direct application of *manual force.*

Obstetrical writers mention a variety of remedies and expedients to be resorted to for the purpose of detaching and bringing away the placenta, but the most of them, although with well guarded precautions, place the chief reliance *upon the hand.*

This is the point to which I invite your particular attention, for my conviction is that *too much force* is generally resorted to for the purpose of overcoming this difficulty.

It is surprising to me that any person who had ever measured the power of his hand with that of the gravid uterus in violent spasmodic action, should ever expect to gain any thing by the contest, or direct others to attempt a similar feat. Authors tell us that when hour-glass or other irregular contraction is discovered, the hand is to be carried up to the point, and first one finger is to be introduced into the stricture, and then another, and so on until a sufficient number is admitted to exert an expanding power, and that then by a persevering effort of dilation the uterine spasm may be overcome, the whole hand passed through the stricture, and the placenta taken away. Now this may succeed in some cases where the contraction is slight and confined to the mouth or one of the cornua of the uterus; but I speak from experience when I say *it will always fail in the real hour-glass contraction of the central fibres, and that a continued and persevering effort will be attended with great danger to the patient.*

The following interesting cases, have fallen under my own observation.

Case 1.—The very first case of midwifery I was called to attend after commencing the practice of my profession, and during the first year, proved to be one in which the placenta was retained about *sixteen hours, by a perfectly well marked hour-glass contraction.* It was a first labour, and occurred in the person of one of the most respectable ladies of the village in which I then resided. It was in the year 1830—the lady was of good size and well formed—the labour lasted about six hours, and a fine healthy child was born. Friction over the abdomen produced firm contraction of the uterus, but not sufficient to expel the placenta. In the course of an hour or two, ergot was given, which produced slight pains, and traction was made upon the umbilical cord, but with no good effect. The placenta could not be felt with the finger. There was no hemorrhage, and as both the patient and myself were fatigued and it was late at night, it was concluded to let her rest until the next morning. I remained near her all night, and on the following day found the placenta still retained. My patient was quite easy, with the exception of mental anxiety, in which I participated with her to the fullest extent. A consultation was called, but I was sorry to find that my medical friend, although more experienced in the profession than myself, had seen but little more of obstetrical practice. The ergot was again given, and every other means resorted to which we thought advisable; but without effect.

It was now resolved that I should introduce my hand into the uterus to bring away the placenta. I did so, and found a firm stricture around the centre of the uterus, dividing it into two distinct compartments, with a portion of the placenta above, and another below. Of course it was recognized as the *hour-glass contraction*, and I proceeded at once according to the directions of the books and teachers, to endeavour to overcome it by gradual dilatation. I introduced one finger into the stricture, and with considerable difficulty, a second, and after awhile, the end of the thumb; but the action of the womb now became so violent as to give me considerable pain, and to render my hand *perfectly powerless.*

I let my hand remain quiescent during the contraction, and when the uterine pain passed off, would renew my efforts to dilate the stricture; but this was immediately followed by a return of the spasm, and after a tedious and persevering effort, becoming convinced of the *utter folly* of what I was attempting to do, notwithstanding I had the sanction of high authority, I withdrew my hand.

We were now at the end of our expedients, and entertained the most gloomy forebodings as to the fate of our patient. We dwelt with painful anticipation upon the dangers of secondary hemorrhage, putrefaction of the placenta, and inflammation of the uterus. After a while our patient began to complain of strangury—she could not evacuate the bladder.

We gave her an infusion of parsley roots with sweet spts. nitre and laudanum, and applied a fomentation to the abdomen. She soon dropped to sleep, and after an hour or so awoke with an urgent desire to urinate. She got upon the vessel, discharged urine, and to our astonishment and great delight, the *long sought placenta also.* It was evident that an anodyne diuretic had brought away what the whole power of my hand could not move ; but we were not prepared to appreciate the full value of such a lesson of experience. Our patient had afterwards considerable inflammation of the uterus, but finally recovered. She has since borne a number of children, but has had one or two attacks of puerperal mania.

Case 2.—The following year I was called in consultation in a case of retained placenta. The subject was a stout and healthy country woman, belonging to the lower rank of society in Tennessee. She had borne two children previously without any difficulty. I arrived at the place late in the evening, and found that a healthy child had been born ten or twelve hours previously, but the placenta was still retained, in spite of the best efforts to bring it away, of a very respectable and experienced village practitioner. There was no hemorrhage, and the womb could be distinctly felt like a globe in the hypogastric region. I carefully introduced my hand into the uterus and discovered a well marked *hour-glass contraction*, almost precisely like the one just described.

It was determined that I should endeavour to dilate the stricture by the gradual and persevering application of manual force, according to the direction of authors. The result was precisely as in the case just related. I soon felt convinced that the application of *five times* the power I possessed in my right hand would have been inadequate to overcome the stricture. Indeed, my hand soon became paralyzed by the spasmodic action of the uterus. My friend Dr. B. now opened a vein and attempted to bleed the patient *ad deliquium animi*, but this could not be accomplished; nor did I perceive the least relaxation of the uterine stricture. I now withdrew my hand, and we determined to try the effects of some relaxing remedies. Tartar emetic was given in nauseating doses, and a tobacco fomentation was applied over the lower part of the abdomen. The nausea soon became distressing, and she vomited. In this state of prostration the hand was again introduced under the sanguine hope that no difficulty would now be met. But such was not the case, the stricture was found as unyielding as ever, and no effort of the hand could make any impression upon it. It was now past midnight, and all of us, patient, physicians and attendants, being nearly exhausted with fatigue, it was determined to administer a decisive anodyne and let her rest until the morning. I assented to this the more willingly from having witnessed its good effects in a previous case—my medical companion had never met with a similar case before. Forty or fifty drops of laudanum were given, and we all laid down to sleep.

On the following morning we found that our patient had slept several hours, and now only complained of some uneasiness in her bladder and bowels. An infusion of water-mellon seed with sweet spts. nitre was given and she soon rose and emptied the bladder, but the placenta still remained. A purgative enema was now administered and during the action of the bowels the *placenta was discharged*, after having been detained *about* 27 *hours.*

Eperientia docet—and here was the repetition of a lesson that has been indelibly impressed upon my mind. Would to God I had learned it at lighter cost than *the loss of my patient!* Within 24 hours the woman was seized with violent inflammation of the uterus and peritoneum, which terminated fatally on the 3d day, in spite of all our efforts to arrest it.

These two cases served to convince me that the nature of *hour-glass contraction* was not well understood; and that the directions laid down by authors for overcoming it by *manual force* are not only *unwise*, but fraught with the greatest danger to the patient.

I am satisfied that the affection is purely *spasmodic*, and I would as soon expect to overcome the rigidity of Tetanus, by manual force, as a true and well defined hour-glass contraction of the uterus. The *grand remedy* is a decisive *anodyne*, either by the mouth or rectum; this will overcome the stricture, and then the regular action of the uterus may be induced, either by gently moving the bowels or bladder, or if it be still retained by a partial adhésion, and hemorrhage appear, it may be taken away by the hand; but let me enter my solemn protest against the exercise of much force by the hand within the uterus, either with the view to overcome irregular contraction, or to break up extensive adhesion of the placenta.

On the examination of a number of authorities upon the management of this case, I find that nearly all of them speak of opium among the remedies to be used, but they do not allow it the importance that I think it merits—*mechanical dilatation* is their *grand remedy.* As I fear I am trespassing too far upon the patience of the society, and being desirous to have the observations of members upon the subject under consideration, I will not fatigue you with a reference to authorities with which I dare say you are sufficiently familiar.

Before quitting the subject of hour-glass contraction, I beg leave to lay before the Society the following case communicated to me in a letter from a very intelligent professional friend in Mississippi, (Dr. G. G. Banks, of Clinton,) dated January 29th, 1845. Alluding to a notice of Chailly's Midwifery in the New Orleans Medical Journal, he remarks:

"I was particularly struck with your comment on the treatment of hour-glass contraction of the uterus, in which you object to the forcible introduction of the hand, (as advised by the Annotator Dr. Bedford, and also by other high authority, Dewees among them,) and for the plain reason that I fully agree with you. The following case has just occurred in my practice. Mrs. C., during her recent confinement (being the 9th or 10th) had *hour-glass contraction*, which my partner, Dr. W., who was in attendance, found it impossible to over come without dangerous force and most excruciating pain. I was sent for, and reached there about three hours after the birth of the child. The stricture was then unrelieved, and of course, the placenta still retained. There were no symptoms calling for immediate delivery—the hemorrhage, which was at first profuse, had ceased—and she objected to the manual delivery on account of the pain, which she said was *agonising* as soon as the hand touched the contracted fibres of the uterus. On reflection, it struck me that the remedy most to be relied on in rigid os tincæ would be the proper one in spasm of the circular fibres of the uterus. I so informed Dr. W., and we determined to give a full *anodyne* by *enema*, and to wait for some time the result. Eighty drops of the tinct. opii. were given, and in one hour the placenta was removed without pain and with the greatest facility."

You perceive gentlemen that this case is in direct accordance with the principles I am endeavoring to sustain; which are,

1st. That all irregular contractions of the uterus are of a spasmodic nature, and that we should rely chiefly upon anti-spasmodic remedies for their relief.

2nd. That the utmost caution and prudence should be exercised in forcibly extracting the placenta from the uterus: in short, that it should not be attempted except under *pressing and dangerous emergencies.*

I fear I have already greatly wearied your patience, but I must beg of you to bear with me whilst I recall to your memories the following striking remarks of the able and judicious Denman, to which I heartily subscribe.

"In the writings, and in conversations on this subject, the introduction of the hand, for the purpose of bringing away a retained placenta, is often mentioned as a *slight thing;* but I am persuaded that every person who attends to the *consequences of practice*, will think it of importance, that, if possible, it ought always to be avoided." And further,

"It must be acknowledged, that it is always a very desirable thing to bring away the placenta wholly and perfectly, not only for the satisfaction of friends, but for the real good and interest of the patient. Even the membranes should be managed with caution, for though a portion or the whole of these might be left without danger, they occasion a *fœtor* in the discharges, and often so much pain, as to create a suspicion of disease. But without meaning to give authority to negligence or misconduct, to rashness or violence, we may suppose a situation in which we must submit to some evil, and in which all that is in our power is to choose the least. There can then be no doubt, but that it is a less evil to leave a portion of the placenta behind, than to do any positive injury to the uterus, in striving to bring it away."

P. S.—I am happy in being able to add the following recent case of retained placenta, illustrating the principles attempted to be laid down in the foregoing paper.

On the 23d of June last, I was requested by a professional friend, in this city, to visit an obstetrical case of his, to which he was unable to attend. He informed me that the subject was a valuable negro woman belonging to a gentleman on Red river; that she had been sent to the city to be confined, so as to obtain the best medical aid, as there had been great difficulty in removing the placenta at her two last confinements and that she had come nigh being lost; that the child had now been born about three hours, and that the placenta was still retained. At 11 o'clock, A. M., I visited the patient, and found her a very intelligent woman, about 37 years of age, and the mother of eleven children. She was having very slight uterine pains, accompanied by but little hemorrhage. The uterine globe was distinctly recognised through the abdominal walls. On examination I thought I could recognise the insertion of the funis into the placenta. A little camphor was given, and friction was made over the abdomen. The pains seeming to increase, I made considerable traction on the cord, but without moving the placenta. I resolved at once to introduce my hand and endeavor to find out the cause of the detention. There was no difficulty in introducing the hand so soon after the passage of the child. I found the placenta just within the mouth, but on passing my fingers around it soon discovered an irregular contraction of the circular fibres. This was not so strong, however, as to exclude my fingers, and I carried them far enough to discover a pretty extensive and strong adhesion to the fundus of the uterus. I now made gentle efforts to detach the placenta with my fingers; but it caused considerable pain, and I desisted. Being able to grasp so much of the placenta with my hand, I made cautious traction upon the whole mass, pressing at the same time with my left hand externally upon the fundus; but finding it not to yield, I withdrew my hand, and resolved to test again the views I entertained in regard to the management of such cases. I gave her 50 drops of strong laudanum which was at hand, and directed her to be kept perfectly quiet until my return.

At 6, P. M., I found her quiet and easy. She had slept an hour or two; no fever; no hemorrhage; no pains. I ordered a dose of castor oil, and if the placenta should not come away during its operation, to take afterwards 50 drops of laudanum again.

On the following morning I called to see my patient, and had the gratification to learn that during the action of the oil the placenta had passed, about 11 o'clock the night previous. She did not take the second dose of laudanum; had rested well, and was now in a most satisfactory condition. The patient informed me that in a similar condition at her last accouchement, the efforts to bring away the placenta had been almost incessant for two or three days, and that she came nigh dying of the consequences.

III.—*Criticisms and Controversies relating to the Nervous and Muscular Systems.* By Bennet Dowler, M. D.

It is impossible to separate the controversial, from the scientific portion of this paper. Whatever the reader may think of the former, he cannot but regard the latter as relating to a subject of great interest, especially at the present moment. If great errors have been assented to as great truths—if deceptive experiments have been relied on to establish principles, which can have no scientific, or even possible connection as cause and effect—if dangerous analogism has replaced exact knowledge and pertinent observation—if hypothetical and "mysterious" assumptions are henceforth to constitute the only foundations of the nervous system or neurology, including nearly the entire circle of the medical sciences, surely it is a most reasonable thing to scrutinize without delay the fundamental principles of medical belief.

The warlike aspect recently assumed by the goddess of neurology, is without parallel. Opposition to the reflex-hypothesis has been repeatedly denounced within a few months, by some of the foreign medical journals, as "calumny, cowardice, consummate meanness, damning crime,"—nay, more, Providence itself has been invoked—prophecies have been uttered;—the Lancet prophesieth, concerning the rejecters of Dr. Hall's reflex doctrine on this wise: "We venture to prophesy, that posterity will take ample revenge upon the present race of laggards and obstructors in the way of truth;" it affirms also, that among the Royal Society there is *but one* man who *understands* this doctrine, namely, Dr. Hall himself;—"there is not *another Fellow* within the College who thoroughly comprehends the advance which has been made in the physiology of the nervous system."*

War is a necessary evil, dangerous it may be to the combatants, but often beneficial to the public. The passions die,—truth lives. If any real conquests be made, posterity will reap the benefit.

I regret for the reader's sake, as well as for my own, that any personal matters should have a place in this paper, but as this is unavoidable, it is hoped that he will be more than compensated for this evil, by scientific references, facts, and illustrations from different points of the medical compass, shedding a degree of light on some of the dim paths in the tangled forest of physiology.

Content with the approval, the encouragement, the generous efforts to disseminate my researches, hitherto extended to me in Boston, New York, Philadelphia, Louisville, Lexington, Charleston, New Orleans, and elsewhere, I had determined not to reply to foreigners—not even to Dr. Hall, whose communication in the New York Journal of Medicine for January, of the current year, though restricted to eight lines, is marvellously exuberant with animosity, nay, it is positively belligerent. It is intended in another part of this paper, to take a slight observation on his parallax in the neurological heavens. There is, indeed, no necessity that I should return railing for railing, were I so inclined.—The retribution of his own countrymen is coming apace."†

About midsummer, a medical gentleman of New Orleans, called my attention to the Medico-Chirurgical Review for April of the current year, in which I found an attack upon myself, implicating my American friends,—an attack in which that journal has greatly departed from its accustomed rules of sound criticism. My determination was now changed. It seemed due to truth—due to the distinguished American critics, who with scarcely a dissenting voice, had pronounced in favor

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* Dec., 1846, Amer. Ed.

† "Dr. Hall's conduct to other physiologists, his predecessors and cotemporaries, abundantly manifests that no reliance whatever ought to be placed on his assertions where his vanity is concerned, and how seldom is it not!

"In sober earnestness, indeed, we think it impossible to contemplate Dr. Hall's actual position in the estimation and regard of his cotemporaries and fellow-laborers in science, without compassionate sympathy which is at once melancholy and distressing; he lives the very Pariah of the physiological caste, the Ishmael of a desert created and sought out by himself, with his hand against every man, and ever man's hand against him."—(*The British and Foreign Medical Review.* London, January, 1847.)

of the originality of my experiments on muscular contractility—due to myself, to attempt a reply. Two methods presented themselves;—first, to publish the residue of my experiments, with a generalized view of the whole, leaving my trans-Atlantic critics to their own consciences, and to the soothing influences of time. But, then, if fifty cases will not convince unbelievers, neither will one hundred. I therefore determined to adopt the only remaining method, that is, to meet my opponents in their own way, to abandon things for words, experiments for logical abstractions, entities for theories, with, however, an occasional *fact*, the ponderosity of which might serve to keep the controversy from flying beyond the influence of gravitation and materialism; for, it will be seen, that the neurologists have entered their protest against "material studies."—The Lancet maintains that, while Dr. Hall's discovery of the true spinal marrow is destined to revolutionize to a great extent physiology and pathology, and to alter our ideas of the action of remedies,* a "reason of its tardy reception," "is the material studies of medical men," as, "humoralism, morbid anatomy, and organic chemistry," "all material in their objects, rendering men's minds inept to the study of phenomena and laws of action of a vital force or *imponderable, such as the excito-motor-power.*"† In plain English, the experiments, scalpels, "*true* spinal marrows," frogs, tortoises, salamanders, ARE IMMATERIALITIES, invisible, incompressible, undilatable, inelastic, formless, unextended,—and all the changes in morbid anatomy, as induration, softening, brittleness, color, form, size, weight, vascularity, tuberculization, ulceration, gangrene, and soforth, ARE IMMATERIAL. The new school builds its immaterial, imponderable system, upon the grossest materialism—not on Pantheistic Materialism, but on frogs and the like; a few obscure *material phenomena* from *these*, on being transferred to *man*, become *immaterial*, and in their action, according to Dr. Hall, "as definite as the ordinary ray."

Unfortunately for neurological neophytes, no *immaterial*, *imponderable standards*, weights, measures, and tests have as yet been revealed. In this critical state of things, and as a prelude to some remarks in the sequel, a passage from Goethe's Faust, is deemed worthy of consideration:

DOGMATIST.—I will not be put out of my opinion not by either critics or doubts. The devil though must be something; for how else could there be devils?

IDEALIST.—Phantasy, this once is really too masterful in my mind.

REALIST.—Entity is a regular plague to me, and cannot but vex me much. I stand here for the first time, not firm upon my feet.

SUPERNATURALIST.—I am greatly pleased at being here, I am delighted with these; for, from *devils* [*frogs*,] "I can certainly *draw conclusions as to good spirits*" [*men.*]

The peculiar advantage in this kind of warfare is, that it can be prolonged *ad infinitum* without the danger of whipping or being whipped, and in many cases without the remotest probability of coming to any satisfactory conclusion whatever. A whipping *de facto*, that is by

* Lond. Lanc., Nov., 1846. † Ib. Dec., 1846.

experiments, is a materializing process, a regular entity, a species of realism, unfriendly to the soarings of the Ideal, in logical speculations. No fact in the history of the human mind is more extraordinary, and at the same time more instructive, than that *bias*, shown by even great men, to attack the very foundations of knowledge. Berkeley's arguments in disproof of the existence of matter, have not been yet fairly answered, it is supposed, by even Dr. Reid. The Ideal is a regular and most worthy entity with many.

Schiller, in his Æsthetic letters, says, "chained to the Material, man is all his time only serving his own designs, before he allows to show a special personality in the art of the Ideal. He requires for the last a total revolution in his whole mode of perception, without which he would never find himself *on the way* to the Ideal."

Far be it from me to say a word in depreciation of the transcendent value of neurological knowledge. The brain, its spinal prolongation, the individual nerves, constitute a central sun, which illuminates and controls several minor systems, which would otherwise roll on in cheerless obscurity. The senses themselves, those inlets of knowledge, psychology, the doctrine of the mind, many morbid changes and healthful actions, vital functions and anatomical relations, have been not only elucidated, but to a great extent refered to their appropriate laws,—not indeed, by theories in themselves as inconceivably obscure as are the phenomena whence they are illegitimately derived, under the denaturalizing vivisections of the inferior animals, not by meaningless tremors, vibrations and convulsions in the muscles from irritating the spinal roots, and thence generalizing almost without limit,--not by localizing and isolating the functional phenomenona (with a precision unknown even to phrenologists,) to a mathematical point or figure, in some root or ganglion in the anterior peduncles of the cerebellum, or the thalami nervorum opticorum, as the special residence of the great All or the Me of neurology. Anatomy itself, to which Bell finally gave in his adhesion, is not the only route to be pursued; for strictly, there is not one neurological doctrine, which *à priori* is determinable from mere structure, from any aggregation or configuration of nervous atoms;—here even analogy is as voiceless as the dead. Neither the sense of taste, nor the sense of smell can be infered from any nerve-structure alone. Indeed, the whole material world, is up in arms with analogies *against* this view as countenancing the fundamental doctrine of Bell, Hall, and many others, who ascribe motor *force* to the nerves only. It is possible to conceive that the muscles and bones, even the skin and the membranes, from their mechanism, might be endowed with force, motion, and the like, but as for the nervous matter, it has not from its very organization, a much greater pretention in that direction than the blood itself. Analogy (not a very convincing kind of proof, it must be confessed,) is against this assumed, exclusive moving power as inherent in the nerve. Do men make ropes of sand, or levers of water? The method adopted by the reflex neurologists to show that the muscular force, is merely a derivative one from the nerves, is inconclusive, nay absolutely erroneous as I have fully proved by adopting their own point of departure. The destruction of the spinal cord, the division of every discernible nerve and all the muscles not concerned in the special functional act intended to be performed, do not in the human subject even diminish the intensity or duration of muscular contractions,—do not prevent regular, and I might say intelligent flexions and extensions to which the spinal methods can make no pretensions.

The reflex neurologists who are not partisans, feel that the ground is constantly sliding from beneath their feet. Clear as " Euclid, definite as the ordinary ray," are expressions which they never use. They can scarcely regard their theory as rising beyond a probability, or provisional admission.

The editor of Documents on the modern Discoveries in the Nervous System, sums up, albeit doubtingly, thus,—" under these circumstances of *indicision* and *doubt* as to *past experiments*—of parts *different* in nature manifesting *similar* results, is this fundamental question *again thrown open to discussion.* Nerves directly opposed in their nature as the spinal roots are supposed to be, on irritation, yield results which are more or less similar, *instead* of being *precisely opposite.* The character, too, of the motions occasioned by such experiments, appears in no way to resemble that of the *calm* and *deliberate acts* induced by impressions on the organ of touch, but that of involuntary and irresistible *spasm.*—It seems not improbable, therefore, that the immediate subject of these experiments has *not hitherto been understood*"—" so as to derive from them *any satisfactory information.*"*

The editor of the Medico-Chirurgical Review, convinced that some explanation of my experiments must be given, otherwise Bell and Hall's discoveries as founded on spinal experiments upon the inferior animals must disappear forever, (the first discovery, Harvey's being perfectly safe from any foreign flag,) offers one so essentially absurd as to defy competition. To see by means of the finger, toe, or stomach, at every possible angle, and through the opaque earth, to maintain that a dose of quinine or arsenic grows stronger the more it is divided—to maintain with the Reviewer that the physiology of a reptile is essentially that of man—are mere truisms in comparison with the following: "The *cause* of the contractions above described, [by Dr. Dowler,] and similar instances which occurred in this country during the prevalence of the cholera, is doubtless the *rigor mortis*, and is, therefore, independent of the nervous centres. Such is the conclusion of the author." Now this conclusion is not only as absurd as any conclusion ever was, but is directly opposed to all my published statements. The *rigor mortis* the *cause* of muscular *contraction!*

I might here ask, by what principle of plain-dealing physiology or of common sense, can Bell and Hall's *denaturalizing* experiments, with galvanic and instrumental irritations upon the spinal roots, of dying and dead frogs, &c., *be good* for the explanation of *human* physiology, pathology, practice, medicinal agents, obstetrics, clear as "Euclid," "definite as the ordinary ray," a mere incarnation of exact science,—how comes it that unmeaning, "convulsed, tremulous" actions upon such animals *constitute* "*the most important discovery,*" while, from the same point of departure, regular, definite, prolonged, functional flexions and

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* 3. Lond., 1839.

extensions produced on *man, without galvanism, without the spinal marrow, without any discernible* nerve left undivided, must go for *nothing!* prove *nothing*, but the *rigor mortis!* As a mad-doctor might never hear anything equal to this in the mad-house, it may deserve a little attention like any other monstrosity. If this be physiology, "the American professional mind," is, as the editor fears, ignorant enough.

To be obliged to define the simplest words, and to prove self-evident truths, is as lamentable as it is difficult—difficult, because no terms remain so clear as the very matter to be explained. There is, however, no alternative. It is my misfortune to have a critic who requires this sacrifice. I ask the commisseration of all charitable Æsculapians, and the more so, as "definitions which throw light on some things, cause darkness in others."

῾ΡΙΓΟΣ, Rigor, *stiff, unbending, inflexible.* "Rigor Mortis, *stiffness of death.* The *rigidity of the limbs that occurs in dissolution.*"*

"Cadaverous *stiffness* is a constant phenomenon, and is characterized by the *firmness* of the soft parts and the *resistance* and *immobility* of the articulations."†

"The *rigor mortis* is due to a particular state of the muscles, ensues at a certain period after death,—never later than ten hours, and after a time ceases."‡

"The action of *real* death is that which takes place in the *stiffening* of the body, and until then it is not dead."§

"When muscular *irritability ceases, cadaveric rigidity sets in;* it is the first certain evidence of death. *The body may be lifted like a plank.*"||

M. Louis in a letter to myself, says that the *rigor mortis* is one of the most certain signs of death—"*la roideur cadavérique une des signes le plus certains de la mort.*"

Finally my own authority, is, at least in this particular, good for something, as it is the result of many prolonged observations. I will quote from the essay which the Reviewer had before him when he penned the "conclusion" that "the cause of the mascular contractions described [by Dr. Dowler] is doubtless the rigor mortis." In pages 32, and 20, it is said,

"Every dissector sees in the relaxation or flexibility which *follows cadaverous stiffness*, the *first* step towards putrefaction." "*Sometimes, post-mortem rigidity sets in* during the *paroxysm of contraction*, producing a very singular phenomenon,—a hard mass, which continuing for hours, feels like bone. I have known this to be mistaken for a fracture badly set, or for a bony tumor." Again, "*rigidity prevented flexion.*" In a few cases the *rigor mortis* appeared to be *wholly wanting*, at least for many hours, that is as long as observed; in some it was very transient, but in *none* was *contractility observed during the flexibility which follows rigidity;*—in *no case* could any appropriate *contraction*, as flexion of the forearm be effected *during the action of the* rigor mortis; but, in those very rare cases wherein *no rigidity* perhaps ever occurred, *contractility*

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* Dunglison's Med. Dict., 1846.
† Beclard, Gen. Anat. 108. ‡ Müller, Phys. 656. § Hunter's Lect. c. ii.
|| Guy and Lee, Med. Juris. 381. In a few particulars, not material to the matter under consideration, I dissent from these authorities.

existed. I recollect one case, (though I have not now time to search for it in XVII. vols. MS.,) where in the woman's body was every where flexible,—even the muscles of the neck, where rigidity usually begins were as limber as in complete syncope, while, for hours, the flexors of the arm contracted vigorously, when struck with the edge of my hand, lifting a hatchet weighing about three pounds from the floor to the breast. But, for the most part *strong contractility* is the fore-runner of *strong rigidity.* Contractility is dynamic, rigidity static. Antagonists are they. Nay, they differ more than motion and rest, being in fact both physical and physiological *contraries*, but never related as *cause* and *effect.*—Mr. Hume defines "a cause to be an object followed by another, where all the objects similar to the first, are followed by objects similar to the second; or in other words, where, if the first object had not been, the second had never existed." According to the Reviewer, muscular contraction can never take place, until after the *rigor mortis* sets in, as the effect cannot precede its cause or antecedent. The body must be perfectly stiff, to be perfectly limber. The greater the rigidity, (the cause) the greater the effect (contraction), that is, a cadaver so inflexible that a limb cannot be bent without severing the muscles, is at the same time capable of performing repeated, prolonged, regular functional motions, as extensions, flexions, &c. To use the language of the Reviewer, somewhat differently applied, "if the publication of these views be the result of any peculiar importance attached to them on the other side of the Atlantic, we fear that modern physiology has not penetrated very deeply into the [European] professional mind." Is the Reviewer altogether competent for the responsible office of deciding upon the scientific claims and rights of cotemporaries? Years of toil, the long watchings of the mouldering cadavera, facts gathered by actual observation, experiments repeated from time to time along the frontiers of death, because they do not favor the theories of the Reviewer, are at the small expense of a little ink, blotted out, or misrepresented without an attempt to test their truth, or study their import. A physician of New Orleans, observes several thousand physical phenomena, and publishes about fifty individual histories;* an editor in London, closeted among the classics, seeks not the *thing* but the *word*—Rigor mortis. This is literally following the advice of the devil (Mephistopheles) to the medical student, in Faust, and is directly in point:

"Meph. A fine *word* will stand you instead. Attend but one master and swear by his *words.* Generally speaking *stick to words*; you will then pass through the safe gate into the temple of certainty.

Student. But there must be some *meaning* connected with the *word.*

M. Right! only we must not be too anxious about *that*; for it is precisely where the *meaning fails* that a *word* comes in most *opportunely.* Disputes may be admirably carried on with *words*; *words* form a capital subject for *belief*; a *word* admits not an *iota* being taken from it. * * * The spirit of medicine is easy to be caught; you study—and let things go on in the end—as it pleases God." But if there be any devil in London, he never would have advised the term, *rigor mortis* as explanatory of the *cause of muscular contraction*, inasmuch, as this

* Some of these were published in the West. Jour. Medicine, April, 1843.

would but compromise his understanding without any necessity, seeing that the excito-motory-reflexians have already, a vocabulary not to be surpassed in obscurity, puzzling to the devil, and rivaling the Choctaw. *Rigor mortis* is a plain word, with but one meaning,—represents but one idea. "*Falsehood*, says Locke, is the joining of *names otherwise* than their *ideas* agree."

Logic is as dangerous as gunpowder in careless hands. At one moment the Reviewer determines to foreclose the question of originality; and with that view, he exclaims, all Europe knew this before!—At the next moment, wishing to use the argument of authority, he says, all Europe is arrayed against you! Can any one resist the *vis inertia* of "the Europian professional mind?" Now the argument of authority, is only good where opinion is to be weighed against opinion, but, good for nothing when opinion is to be opposed to fact.

The Reviewer has, however, made an attack upon one fact, that I had incautiously admitted, but not as an observation of my own; for to tell the truth, I was misled, if at all, by "the European professional mind," and this I regret the more, as it is, however true, wholly immaterial to my argument, and must have been intended as a quotation. I hold the fact in abeyance, nay, I abdicate it without mental reservation, as *non est inventus;* I give it up to the experimentalizing wizzards, that they may throw it into their cauldron once more,

"With adder's fork, and blind worm's sting,
Lizzard's leg, and owlet's wing."

It is, however, proper to show that I have the highest European authority for the fact. I will put the editor of the Medico-Chirurgical Review in one scale, and the Professor of Natural History, of the Royal College of Henry IV, Paris, in the other,—authority against authority.

REVIEWER:

"In further illustration, the author, [Dr. Dowler,] invokes the supposed fact that 'an earth-worm may be cut into several pieces, and that each portion becomes a perfect animal.' No one acquainted with the structure of this annelide and with the laws of development, could *imagine* such a departure from the principles of formation; but for the information of Dr. Dowler, we may state that, by numerous experiments made some years ago, we ascertained that no portion of the earth-worm severed from the head, however large, survived beyond a limited period, dependent upon the length of the segment: the part so detached dies ring by ring."

H. MILNE EDWARDS:

The lubricus terrestris or earthworm.

"If we examine the disposition of these different sets of apparatus, which concur, each in a different manner to the support of life, we shall find that they extend uniformly from one extremity of the body to the other, and that each transverse segment of the animal differs but little, or not at all, from the others; it is a constant repetition, includes all the organs necessary to vital movement. If an earth-worm be cut transversely into two, three, ten, or twenty pieces, *each of its fragments may continue to live as a whole*, and to constitute *a new individual.*" (*Anat. and Phys.* 15. Translated by Dr. Lane. Boston, 1841.)

The Reviewer quotes the following passage, (in brackets.) from my essay: ["The reflex school maintains, not only that the integrity of the spinal cord is indispensable to transmission, but that the division of the anterior roots is a complete barrier to muscular motion. This doctrine is not based on the healthy living body. It is not, with a few obscure and unimportant exceptions deduced from morbid conditions, but from the last agony, and more than all, from the recently dead state of the inferior animals—a kind of proof by no means satisfactory. It should never be forgotten that experiments on the inferior animals, as frogs and turtles, are inconclusive in establishing the complicated physiology of man:"]—whereupon the reviewer remarks: "It is difficult to conceive, with the evidence possessed upon the points here refered to, how this passage could have been penned. What, it may be asked, are the phenomena displayed in the anencephalus infant that survives its birth? It breathes, it cries, it sucks, it discharges the excreta of the body. How, we would ask of the author, are these complex, associated movements performed? Do they involve any nervous agency?—if so, what is the part implicated? Brain there is none; and we may presume that even Dr. Dowler would not attribute either to the nerves or the great sympathetic, the power of originating and combining in functional action, muscles so numerous and remote as those engaged in the functions named. What other conclusion remains, but that the spinal cord is the necessary and active centre."*

The fairness with which I have laid down the reflex doctrine of transmission, the supposed essentialism of the spinal cord as the agent of muscular action, no candid man who really knows anything of the matter, anything of the writings of Dr. Hall, and the entire school of Philo-Hallians, will deny. The present paper demonstrates that;—but, if a doubt remain, the numerous volumes of the Medico-Chirurgical Review, for a quarter of a century, will show that I understand, and quote Bell and Hall truly. Let the present editor read that valuable work upon that point, together with Æsop's Fable of the Belly and the Members, in which he will learn that coalitions for or against any one organ exclusively, even the *true* spinal marrow, is bad physiology. The brain is good. The heart is good. The muscular system, as well as the spinal cord is good. The controversy against "the Belly was kept up as

* This unfortunate, reported by M. Lallemand, lived only three days. Its movements were not energetic. Dr. Hall, and the *ci-devant* Reviewer, many years ago, had worked this monstrosity into spinal capital. The interpretation in 1834, is reiterated in 1847, namely: "It must be obvious that the muscular motions in this instance *must* have been the result of impressions *transmitted* from the nerves to the medulla, and of actions impressed by the medulla on the muscles"—a very *reflexive* explanation. For, if the *sucking impression began* in the mouth, it had little to do to travel down into the spinal marrow, and back, for *nothing*, seeing that in such an emergency the *inherent* sucking power was already in the muscles of the mouth. A back-woodsman on being asked for an explanation of the word *disembogue*, defined it satisfactorily to himself and interrogator, thus: "You disembogue me, and then I will disembogue *you*." The mouth and spinal marrow mutually mystify each other, and if there be any thing in the decalogue, requiring us to explain *every thing*, I propose to give the mouth the preference, and hush the matter up.

long as any thing of that kind can be kept, which was until each of the rebel members pined away to the skin and bone,—the hands wouldn't work, the teeth refused to chew, &c. Then they found there was no doing without the Belly, and that he contributed as much to the maintainance and welfare of all the other parts as they did to his." Robert Whytt knew something of this. But he called it sympathy,—a remarkable consent of parts, and so on. I have no doubt that the Reviewer's *brainless* babies missed their brains very much; had their "true spinal cords been withdrawn gently," (as Dr. Hall would say,) their breathings, cryings, and excretings would have been damaged, and still more, had their hearts been "gently extracted." What then? What other conclusion remains says the critic, but that the spinal cord is the necessary and active centre? forgetting the "Belly," and even the heart! forgetting the mutual dependence and modifications of associated organs.

If logic chopping were allowable, I would adopt the Reviewer's method, and ask, "what other conclusion remains but that the muscles are the necessary organs in sucking, crying, and defecating? Allow the anencephalus infant, a brain, a spinal marrow, a perfect nervous system, and every other organ in the most perfect state, and allow at the same time that, by some freak of nature, the muscles are wanting in their attachments, either at their origins or insertions, then of breathing, crying, and sucking there will be none, of complex associated movements there will be none, and of the Reviewer's exclusive spinal arguments there will be none.* The argument is altogether in my favor, and more than all the experimental proof is mine, beyond question. I have proved that in New Orleans, dead men and women from the icy circle to the torrid zone,—after cutting off the connections with the spinal marrow,—after amputating the shoulder so as not to injure the muscles of the arm, after dividing all the discernible vessels, and nerves, and tissues, except the individual muscles to be called into action, can perform during many hours, definite functional motions, not the tremulous, convulsed, and therefore, unmeaning motions, which from Haller to Bell, have been vaguely refered to irritability, but the elementary or simple motions from which are compounded all the varied actions of the living man! The two muscles which bend the forearm for anatomical simplicity, and still more for the enormous mechanical leverage which they overcome,† when a weight is placed in the palm, are the most convenient for experimental purposes.

I have, as it were, insulated the muscular FORCE, and have noted, very imperfectly I admit, its isolated phenomena, shown its periodicity, its

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* "That motions peculiar to life can exist without the accompaniment of *brain and nerves*, is proved by the existence of those animals which are destitute of them." (Blane's Med. Logic, 121, Amer. Ed.)

Even Bell, admits "that animals, without possessing nervous cords, are susceptible of the impressions and of the reactions necessary to their existence." (Anat. and Phys. 11, 51. Am. Ed.)

† Bonelli states that the whole force expended by the muscles of the arm, when stretched horizontally, is 209 times greater than that of any weight suspended at its extremity, and that the force of the biceps, compared with that of brachialis, is as 3 to 2.60, or as 15 to 13, and their absolute forces 300 to 260 lbs. (*Lancet*, May, 1846.)

fits of action, duration, direction, exhaustion, extinction, its times, velocities, its decreasing ratios from increasing times, from repeated efforts, and from augmenting weights; nay, more, that it may, in a certain sense, be weighed, that is, exactly counterbalanced by the gravitating force of pounds and ounces. I will not say that I have proved positively, THE EXISTENCE OF AN IMMATERIAL ENTITY, OF FORCE IN THE MUSCLE, but, I may say, that I have offered some presumptive proof to that effect. Omitting that immaterial, psychological entity, the soul, where is there so much proof, so many tests, going to show in any other tissue of the body, a force, possibly immaterial, and isolated, possessing so many properties, and which can be approached by so many methods, avenues, and tests? Inaccessible until after death, this force, whatever may have been its complications and dependencies during life, shows an utter independence of the spinal marrow, as well as of the entire nervous system. The brain, the spinal marrow, the nerves, under similar circumstances, appear as so much inert matter, without force, without function, without any apparent vital phenomena whatever, actually dead, and in the strongest contrast to their high functions in life, as the instruments of the mind, of sensation and of some of the principal vital phenomena. But there is a constant bias to assumption, and to be satisfied with the minimum of evidence in neurological doctrines, and the more so, as the subject is obscure; the mind yields to a flattering illusion rather than confess ignorance.

How brainless infants contrive to cry, suck, and excrete, I will not tell the Reviewer, because I do not know. But this I know, that if Reflexians fix as the point of departure, the morphological type, unmeaning experiments upon the spinal marrow of the inferior animals, under denaturalizing processes, and thence proceed by analogizing speculations, to appropriate to their theory the encyclopædia of medical science, not excepting "all the emotions, appetites, and passions," surely, experiments upon the human subject, must be far more conclusive and comprehensive, and were they applied in the same latitudinarian manner, nothing would be left for future discovery; muscular motion would be to the organic, what gravitation is to the inorganic world, and the physiological mechanism would be like the celestial, but a matter of calculation, an estimate of simple and compound forces and velocities. Until then a mystery will hang over the physiology of the "anencephalus infant," which in the mean time, will deserve all the compassion expressed by an old poet,—

> "Ill fated youth! what stars malignant shed
> Their baleful influence o'er thy *brainless head*."

Upon the whole, it is evident that my experimental researches are not acceptable to the Reviewer, because they disprove that which the Medico-Chirurgical Review had so often proclaimed as a discovery, as the fundamental principles of his distinguished countryman, and of medical science, as if Bell and truth were synonymes. The *naïveté* with which this is set forth, is admirable: "we have no wish, however, to affirm that these observations are devoid of interest; nor that, *if published as illustrative of a somewhat obscure class of phenomena* connected with muscular action, they would have been uninstructive; *but* considered as the lever by which the magnificent superstructure of *modern neurology* is to be *overturned*, we hold them to be most vain and futile." Now by "modern neurology" the Reviewer means Bellism and Hallism, as taught by experiments on frogs and the like,—animals which he has pronounced "essentially like man!" My experiments and arguments, do not in the least conflict with, much less pretend to "overthrow the magnificent superstructure of neurology," or any portion thereof, except the erroneous. "But *if* they had been published" to confirm Bellism and the exaggerated estimates of the same, in the Medico-Chirurgical Review, they would probably have been unqualifiedly "interesting and instructive!" Is the Reviewer a better neurologist than I am, because he believes more errors? Is he aware that Bell is nearly as great a skeptic in his *own* discovery, as founded on experiment, as I am? Bell, the critic loves well, but not wisely, as is easily proved. He swears by Bell's words, and by Bell shall he be judged, for the blunder has been growing many years! Here, the Review is authority.

It will be borne in mind, that the muscular motions in the inferior animals which Bell describes, upon which he reasoned most doubtingly, but upon which, according to the oft-repeated declarations of the Medico-Chirurgical Review, "*all our knowledge must ever repose*," are wholly unlike those I have produced, described, and published, that is, Bell's are not the *appropriate motions* intended by nature, such as are performed during life. In Bell's Nervous System of the Human Body,—his great work, it is said, that "on irritating the anterior roots, an *evident* motion was produced on the muscles, not only *perceptible* to the eye, &c. The motion was not the slight tremulous motion arising from the natural irritability still remaining in them, but it was *convulsive* and *spasmodic*.*" Bell's animals were, so far as he indicates their condition, not half dead. He proceeds thus: "cut across the nerve which had the power of exciting the muscles, and stimulate the one which is undivided, the animal will give indications of *pain*; but although the nerve be injured so as to cause *universal agitation*, the muscle with which it is directly connected does not move." "To expose these nerves requires the operator to *cut deep*, to *break up the bones*, and to *divide the blood vessels*. All *such* experiments are much *better omitted*; they *never can lead to satisfactory conclusions*."† True, but what then becomes of the discovery? Indeed, this eminent man lost faith in his own discovery, at least, in its experimental proof. When M. Müller published experiments confirmatory of Bell's theory, the latter repelled them, declaring that "he preferred to build on *Anatomy* and the *vital powers*, not on the *galvanic* conducting powers of the nerves."‡ Was there ever such a case before? The discoverer has misgivings, doubts,—repudiates his own experiments without which, his discovery can have no existence, except as a *conjecture*, without even the aids of analogy; the more he doubts, the more strongly others believe for him; the more *obscure* are his experiments to himself, the more concentrated is "the burst of light" to every one beside! When he had much faith, others had none; when he had little faith others had much. Who has the truest respect

* Amer. Ed. Preface, 10. † ib. 128. ‡ Med. Chir. Rev. 1834.

for the memory of the illustrious Bell, Dr. Dowler, who believes in his honesty, or his London friends, who do not?

My Reviewer speaks of "Bell's splendid discovery," as "having brought conviction to the professional mind of Europe," as being "a great truth henceforth to be ranked as one of the fundamental principles," while the closing paragraphs in Bell's Nervous System, are devoted to the discrediting of the very experiments without which, I repeat, this discovery is nothing but an opinion: "I feel a hesitation," says he, "when I reason on *any other* ground than on the *facts of anatomy.*—*Experiments* are more apt to be *misinterpreted;* and the very circumstance of a motor and sensitive nerve being generally combined together, affords a *pregnant source of error.* It is natural to suppose that the galvanic influence might be brought to bear on this subject; but I may be permitted to suggest to any one who pursues it in this way, that it will be necessary to *distinguish the effects produced by the nerve as a mere conductor,* and when performing *its living functions.* The nerves *dead* or *alive* may convey the *galvanic power like a wet cord;* but if the nerve be in possession of its living property, a great deal will depend on the *direction* in which the galvanic fluid is transmitted.*

"Does the nervous fluid (to use a hypothetical term) pass ever in the same direction, outwards from the brain in one nerve, and towards it in another, can it be propagated by the same tube or fibre backwards and forwards in two opposite directions, at the same instant of time? I apprehend that it cannot. What then is the difference of those two nerves? Is it in the direction in which they convey their impression, [or fluid] since it is proved that they are both connected with the sensorium, and both connected with the muscles? I am *inclined* to say that it is so.—It *may* then be, &c. I am *quite at a loss,* &c. I am *inclined* to say there is a circle in the nervous system; that one nerve conveys its influence towards the muscle, &c. At all events a mistake has prevailed†," &c. How cautious! nay, skeptical! Long since I pointed out the error into which writers have fallen in attaching to Bell's experiments a value, a meaning, an extent, and a certainty, disavowed by *himself.* His almost dying declarations against the conclusiveness of his own experiments, though left on record in his most authoritative work, are rejected lest the great Islanders should lose the glory of "the second great discovery!"—a new feature in literary annals! In the Lancet the experiments are praised,—the experimenter condemned!—It "deplores the weakness of this eminent man in *dispraising,* in the latter part of his life, physiological experiment, which had been the means of establishing the product of his own genius."‡

What then is this "second great discovery" which Sir Charles Bell is said to have made, and of which, albeit, *he* speaks so doubtingly?—The uninitiated would scarcely suppose that an actual discovery, such as some persist in calling this, could be still involved in the utmost obscurity;—that experiments, experimenters, and opinions, are still about equally divided;—that the anatomical, physiological, and pathological phenomena, involved, afford no positive knowledge, whether the

* Nervous System, 180. † Ib. 218–19, On the *Nervous Circle,* App.
‡ December, 1846.

brain and spinal cord perform actions separate, independent, opposite, conjoined or associated. Their *rôle* in the hypothetical Nervous Circle; the precise functions of the spinal nerves in receiving, carrying, and returning the hypothetical fluids, impressions, vibrations, particles, and "the mysterious messengers," first to the anterior, then to posterior roots, or the contrary, no man knoweth. M. Magendie concludes from his experiments, "that sensation does *not* belong *exclusively—n'est pas exclusivement*—to the posterior roots, any more than motion to the anterior:" "I obtained contractions from each—*avec les deux sortes.**—Hence, "Magendie allows both *sensation* and *motion* to *anterior*, as well as *posterior roots*. Müller's experiments on frogs would, indeed, indicate a distinct appropriation, but the anomalous organization of these animals discovered by Volkmann, will probably appear, to all who are not partizans as calculated to leave the great question *still subjudice.*"†

In the Physiological Anatomy and Physiology of Man, the first volume of which I have lately seen, and which was first published in London in 1845, the following statements are found: "The irritation of a motor nerve in an animal recently dead, causes contraction of the muscles to which it is distributed. The simplest way of applying a stimulus for experimental purposes is by passing a galvanic current from a small battery."‡ "The function of the anterior roots of the spinal nerves was discovered by Bell, *but from the violence of the operation and the pain produced in performing it, it was impossible to determine* what degree of sensibility remained in parts supplied from the divided roots."§ These authors say, that when the anterior roots are divided at their emergence from the cord, "no motion can be excited by stimulation of the surface, nor by stimulation of the cord itself"—"the stimulus acts through the afferent nerve upon the centre, by which the motor nerve is excited."‖ They witnessed Matteucci's electrical experiments on frogs while their work was passing through the press, and conclude with him, "that the muscular current is quite *independent of the nervous system.*"¶ "*Contractility* is a property of the *living* muscular substance as *such.*"** "We enter our protest against the doctrine which assigns the spinal cord as the source of muscular irritability. This doctrine, indeed, has but slender support in reason or experience. It is contrary to all analogy to assign to *one* tissue, the power of confering vital properties on *another*. If bone, tendon, and cartilage have their distinctive properties, they possess them in virtue of *some peculiarity inherent* in their mode of nutrition, and do not derive them from *any other* texture. And surely, it is too much to suppose that a tissue, like *muscle*, so complex in its chemical constitution, and so exquisitely organized for the development of its proper force, should be *dependent on the nervous system*, or *a portion of it, for its contractile power*. Our own experience is *quite opposed* to the statement of *Dr. Hall*, that in cases of palsy dependent on cerebral lesion, the muscles of the affected limbs acquire an *increased*

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* Jour. Phys. Expér., Oct. 1822,—cited Doc. 97, 98.

† Doc. Nerv. Syst. 111, Lond. 1839.

‡ 303–4. § 306. ‖ 312. ¶ 378. ** 190. These doctrines are very sound—such as I had previously advocated, published, and *proved experimentally*.

irritability from the cord, which he supposes to be the *source of irritability remaining intact,* while the influence of the exhauster of irritability (the brain is removed.) In all our experiments, which have been numerous, we have found the palsied muscles less excitable by the galvanic stimulus than those of the sound side."* "It has been supposed that the *tone* of the muscular system is maintained by the *spinal cord.* We can only remark, that the phenomena which characterize that state are just as obvious in muscles taken from animals, recently deprived of the spinal cord as in others; and that the analogous state, the *rigor mortis,* comes on as distinctly when the cord and brain have been removed, as if they were untouched."† "The removal of the spinal cord produces flaccidity of the muscles, owing to the immediate cessation of the slight degree of active contraction necessary to *maintain a certain posture.*" "We use the term spinal cord in its ordinary sense —we *reject the hypothesis* of a *true spinal cord,* anatomically distinct from that which has to do with mental nervous action."‡ Whether these able writers are always consistent, the preceding, and especially the following quotations will show: in summing up they "conclude 1. That the spinal cord *in* UNION *with the brain,* is the instrument of sensation and voluntary motion to the trunk and extremities. 2. That the spinal cord may be the medium for the excitation of movements, independently of volition or sensation, either by direct irritation of its substance, or by the influence of a stimulus conveyed to it from some surface of the trunk or extremities by its nerves distributed upon that surface. This latter office of the cord, *although recognized* by WHYTT, PROCHASKA, BLANE, and FLOURENS had not attracted all the notice which its great importance merits, until the researches of Dr. Marshall Hall and Professor Müller *drew attention to them.*"§

The Reviewer biased in favor of his "distinguished countryman,"‖ as well as English frogs and turtles, and remembering that the Review stood deeply committed to Bellism, naturally enough shows an eagerness to maintain his ground, and to annihilate oppositionists. But no one could have anticipated his attack on the human subject, nor his pertinacious preference for frogs, as subjects for the study of *human* physiology! But here he is not singular, for on the Continent, physiologists of the highest standing manipulate this interesting animal, with electricity and with scratchings, from year to year, and in some instances by the authority of the State, as in the case of Professor Matteucci, of he University of Pisa. Happy Matteucci! he lectures on frogs by appointment from the Tuscan government,—the treasury pays him. He experiments on frogs, and the Royal Society honors him with the great Copely Prize. To ply frogs with electrical batteries, so as to cause unnatural muscular motion, is the exclusive road to medical honor, as well as to medical knowledge, and opens the true gate of discovery!— Molière mentions a doctor whose pathological doctrines all centered in the lungs. His patient said he had "pains in the head." "Exactly,"

* 341. † 340. ‡ 340, 339. § 312.

‖ The Medico-Chirurgical Review, (January, 1847,) in the name of Great Britain, thanks Harvey, Bell, and Hall "for the undisputed glory of the two mightiest discoveries in physiological science!"

said the doctor, "poumons."—"Dimness of sight."—"Poumons."—"Weakness in all the limbs."—"Poumons."—"Colic."—"Poumons." "An appetite."—"Poumons."—"A love of wine."—"Poumons."—"Sleepy."—"Poumons." So in physiology, *Grenouille!* frog!

The Reviewer sets out with "a decapitated turtle or frog,"—tells the old story about "irritating the posterior roots," "the muscular actions, which are totally prevented by the *destruction of the cord*," and concludes, "can any one, we ask, entertain a doubt that the consequences would also be the same in man, with a spinal centre constructed upon essentially similar principles?" It is necessary to dwell a moment on this assumed analogism, nay, essentialism, to speak like the Reviewer, who stakes his all upon the naked, spinal experiment, without the aid of sneezing.

Is it true that a frog is the *essential analogue* of man, in its anatomy, physiology, pathology, parturition, natural history, and so forth? Of the frog, Cuvier says, "it buries itself during the winter under ground, or in the mud below the surface of the water, where it continues to live without *food or respiration*."* Réaumur knew frogs to live in hot springs at 111°, and Spallanzani, at 138° Fahr.† Many facts have been reported, showing that they have lived for ages without food and air in rocks and trees, wherein they had been completely and narrowly enclosed, &c.! Mr. Paget's Report on the progress of Anatomy and Physiology in the British and Foreign Medical Review, for April, 1845, contains the following statement, which is here somewhat abridged:—"To prove the functional independence of the sympathetic nervous system, Volkmann and Bidder, (Müller's Archiv. 1844,) have published an extensive series of experiments on the effects of removing from frogs, the brain, or spinal cord, or both, leaving the medulla oblongata,—the muscles were rendered at once incapable of contracting upon either voluntary or reflex stimulus, the circulation continued unimpaired two weeks after crushing the cord, fourteen days after destruction of the brain, five days after destroying both at the same time; the pulsations were as frequent and vigorous as in healthy frogs, the processes of exudation, absorption, urinary secretion, defecation, digestion, continued as usual; on the whole, no organic function was materially disturbed by the destruction of the brain and spinal cord."

The following quotation is taken from documents on the modern discoveries in the nervous system:‡ "The circumstances in the structure of frogs pointed out by Volkmann, (an anomalous distribution of ganglia and ganglionic fibrils upon the precise parts which are the subjects of experiment,) must in all experiments on these animals, have been attended by demonstrations only of correspondingly *anomalous*, instead of *regular function*."—Once more: "Redi removed the brain from a land tortoise. It appeared for several months to *enjoy life*, and *exercise* its *functions nearly as before the loss of the brain*."§

Professor Matteucci, in his work on Living Beings, (1847,) maintains, from his celebrated experiments upon frogs, that contractility is the vital

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* An. King. 286, Lond. 1840.
† Ency. Brit. Ed. 1842, XIX, 153.
‡ Intro. 3.
§ Edin. Ency. X. Herpetology.

property of the muscle *alone*,* and cannot be explained by electricity or any other known cause. The same number of the Review, which declares in opposition to myself, that the doctrine of Bell and Hall "is a great truth henceforth to be ranked as fundamental," offers to the rising sun of Italy, superlative laudations: "Matteucci's work is alike valuable and interesting to the general reader and the professional man, to the natural historian and natural philosopher, to the physiologist and the practical physician, etc." At the same time it is said, "our readers will not fail to perceive that the conclusions drawn by Matteucci from *his experiments* are opposed to the views of Dr. Marshall Hall. The Italian professor regards the irritability of the muscular fibre as *inherent*, whereas Dr. Hall considers it as *derived from what he calls the true spinal system*." Now, if "the professional mind of Europe" can adopt these contradictory views of the Reviewer, or prove that man can live in good health for months without the brain and spinal marrow, "that little or nothing can be learnt in physiology without these cruel reptilian experiments, (heretofore denounced by the Medico-Chirurgical Review,†) and that premises laid among these, can legitimately be appealed to, in order to bring out conclusions in the medical sciences, so as to harmonize human physiology, pathology, therapeutics, and obstetrics, then, it must be admitted, that "the European mind" has made great advances, indeed. But it may be doubted, whether any act of the "professional mind," can make frogs essentially like men, not to say anything of endowing them with a physiological superiority! Sidney Smith maintains that frost cannot be put off by act of Parliament, nor can spring be accelerated by any majority of both houses. In analogy, of an obscure kind, the same difficulty is not experienced. Hamlet saw in the entire world, the analogue of an extensive prison, having "many

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* "Inattention to the structure of nerves has led to *a mistake, that they have a power of contraction*." (Bell's Anat. Phys. II, 53.) The very nerves which give all motion, have none whatever! Though I am no friend to the *exclusive* motor function of the nerves, I never could go so far as this, much as it is in my favor. Although the entire limits of this paper, would scarcely suffice to warn the student against the illusions of the microscope, as an *exclusive pathological instrument*, yet, the following statement by M. Mandl, (author of a treatise on the microscope,) before the Academy of Sciences, in a recent sitting, may be worth translating: (See L'Illust. Feb. 20, 1847.) M. Mandl, said that he had observed motions in the nervous system of leeches, magnified from 50 to 60 times. He separated a portion of the living leech, from the ganglionic chain, placing it in a drop of water, after having torn its black, cellular envelope, isolating the ganglion and the nerve, he noticed vital contractions as in the muscular fibres. M. Flourens remarked upon the occasion, that he had seen in his experiments upon the functions of the nervous system, a real and active movement on bringing together the two ends of a divided nerve. M. Serres, he continued, had long ago, published experiments upon the contractility of the ciliary nerves.

Professor Liebig, adopts the bold assumption, that the nerve is the exclusive source of muscular motion, and this too, in the most literal, nay, mechanical sense, so that the *rôle* of the muscle, is that of a mere subordinate, passive, powerless instrument. (See An. Chem. 66.)

† When this, the Fiftieth volume of the Review, recommending as the very basis of physiology, cruel and fallacious experiments, shall be placed side by side with its fellows, will not the great *ci-devant* spirit of that work frown upon the new comer?

confines, wards, and dungeons; Denmark being one of the worst."— Polonius found in a cloud, the analogism of many things at once; "it was like a camel, like a weasel, and very like a whale."

Next to the Duke of Otrànto, (Fouché) the Reviewer is becoming the most profoundly cunning and calculatingly equivocal of mortal men. In his review of Dr. Hall's works, (January, 1847,) biting irony, bitter denunciation, and extravagant laudation, are showered upon that author in equal proportions, while his *morale* and the fallacious import of his experiments, "his manglings and his mutilations" are painted the most revolting colors; whereupon, the Reviewer (who could have anticipated what follows?) takes a fit of patriotic glory, in behalf of the happy land which is "honored as the birth-place of Hall's mighty discovery:" "Ten years ago, (says the Reviewer,) we spoke of his labors in these words:—'He has evolved a *simple fact* (*that of involuntary contractions following the irritation of the corresponding sensory nerves as long as the part retains its connection with the spinal cord*) INTO AN EXTENSIVE AND INGENIOUS THEORY.'" The Reviewer divides the world into two classes, ("ocean into tempest wrought to waft a feather or to drown a fly!") both being against Hall; "one party attempts to rob him of his fame, the other to quench the torch of discovery! No such reproaches can be made against us!" Fortunate critic! May you live a thousand years, and have for your epitaph—"No such reproaches, &c." You utterly repudiate Hall's experiments as worse than useless. Yet, they have "evolved the simple fact," "the great discovery," "the ingenious and extensive theory!" Reader! I pray you, to remember that in the year 1847, this renowned Review based the second great discovery, wholly on Dr. Hall's irritation of the spinal sensory root, and the consequent muscular movement. This is really the true state of the question, the real issue, and woe to him who shall change it! The malediction as read by Dr. Slop, is not too severe for such an one. The American Journal of the Medical Sciences, in an elaborate review of Dr. Hall's Nervous System, (February, 1839,) says: "The system is simple, and *rests upon experiments* which may easily be repeated." If the issue be not changed, Bellism and Hallism will soon be expunged from, instead of forming the material of the medical cyclopædia.

An English traveller has admitted the superiority of American thunder. I regret that I cannot in return, concede as much for English Logic, *if* the Reviewer's be taken as an average sample? First, the Reviewer lays down the broad deduction, immovable as the rock of Gibraltar, ponderous as *inertia itself*, "Dr. Dowler brings forward as a *novelty* what *is familiary known to all careful observers on this side of the Atlantic.*" But after a few broadsides in this behalf, (the smoke being dense, and fearing that I might not only survive, but prove a *bona fide* discoverer,) the Reviewer terminates the engagement by a *coup de main*. Being influenced by a scientific *amor patriæ*, and knowing a very learned and worthy countryman of his, straightway he hoists the flag of discovery over his domicil: for if after a declaration, that all Europe knew all about this matter, it should turn out that nothing was known, then it would be very well to fall back on the question of priority.

"Mr. Bowman was the *first writer* who distinctly showed by microscopic observation, that the individual muscular fibres *contracted* *independently* of the presence of the *nerves*; and we have *here the clue to the more extensive but essentially the same phenomena related by Dr. Dowler.*" If Mr. Bowman ever made any observations similar to mine, I am wholly unacquainted with them. I am convinced, from the concurrent opinions and statements of writers,* that none such exist. I ask for documents and dates. It is believed that no work of his, having any bearing on this subject, had been received in this country, or even published in London, until years after my experiments upon post-mortem contractility began, and a number of cases had been published in the Western Journal of Medicine. Mr. Bowman's Physiological Anatomy, sound as it is in general, is quite *too recent.* It contains nothing so far as published, at least so far as I have seen, touching my method or results. I have quoted, in another part of this paper, all that I have seen having a bearing on certain views of mine. The insinuations of the Reviewer about "microscopic observation," and his "fears that physiology has not penetrated very deeply into the American professional mind" are gratuitous, and, I herewith give the opinions of the American mind" as published in various journals, on the originality and nature of these researches, in opposition to those of the Reviewer, without any fears that the European and American minds, will differ materially on this or any other medical subject. As to the CLUE, that is, "the thread that is used to guide a person in a labyrinth—any thing that guides in an intricate case,"—why should I take the *clue* in this case, from Mr. Bowman, seeing every body, on at least one "side of the Atlantic," had long known the whole matter? The Reviewer says, "Mr. Bowman was the *first*, &c.,"—"Dr. Dowler took the *clue* from him," to discover "what is familiarly known to all careful observers!" As a medical bull, this is not bad; as logic, it is contradictory; as a criticism, it is the morphological type, which each sentence of the entire article tends to develope, until a monstrosity is at length brought forth, in which St. Hilaire himself, were he alive, could scarcely detect any unity of organization.

I will presently proceed to show what is the opinion of "the American professional mind," concerning these researches, upon which the Reviewer "places a very low value," while at the same time, he denounces in effect, all who think differently, as ignorant of physiology. The writers of the following extracts are personally unknown to me, except as being among the most eminent authors, and professors, in America, not one of whom, it is confidently believed, could be benefited by the praise, or injured by the censure of the Reviewer. Were I so immeasurably beyond the reach of the editor, I would regard his criticisms as being as harmless, as they are uncandid and sophistical.

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* In the new edition of the Encyclopædia Brittanica, Dr. Roget, in his elaborate treatise on physiology, makes the following important statement, showing the actual doctrines, as late as 1842,—a period later than that of my experiments: "Mr. Mayo ascertained that after any nerve which supplies a voluntary muscle is cut through, either in a living animal, or immediately after death, mechanical irritation of the part of the nerve disconnected with the brain, as for instance the pinching it with the forceps, causes a single sudden action of the muscle or muscles it supplies. On the other hand, a like effect cannot be produced by irritating mechanically the nerves distributed to those muscles over which the will has indisputably no influence." (XVII. 675.)

The Reviewer, assuming an unwarrantable jurisdiction over the new Continent, as if unwilling to permit the republication of any pamphlet but his own Quarterly, fulminates the charge of culpable ignorance, (at least by an indirection,) against the American profession, if it shall "attach any importance to my researches," a provisional amnesty, but not all available, inasmuch, as the dreaded contingency has really happened. Sentence must be pronounced: "If," says the Reviewer, "the republication of these views, [Dr. D's.] be the result of any peculiar importance attached to them on the other side of the Atlantic, we fear that modern physiology has not penetrated very deeply into the American professional mind:" but, as if the very demon of contradiction had possessed his *critique*, he, in another place, (forgetful one!) acknowledges these views to be both "*interesting* and *instructive, if* they had been published to illustrate a class of obscure phenomena," and *not in opposition* to his favorite theory, "upon which *all* our knowledge must ever repose." His sneer at "the American professional mind," is undeserved, seeing that by his *own estimate*, my researches are "*not devoid of interest, are not uninstructive*,"—a praise, which that journal expressly denies to, perhaps, nine-tenths of the publications in Great Britain, as its pages will testify. But *Samuel* Johnson set the example: he abused Goldsmith freely, but would allow no other person that luxury: so with the Reviewer. But to spit a criticism, or rather an animadversion over the Atlantic, for "attaching importance" to what the Reviewer himself calls an *interesting*, and an instructive republication, is a thing without example.

Of all the medical journals in the country, that of Boston has been the first and most decided in speaking upon this, and upon several subjects, in which I have been an humble actor, but in terms so flattering to the experimenter, that I must forego the advantage of its disinterested and weighty testimony. I have, in making the quotations which follow, avoided complimentary expressions as much as is possible consistent with my aim, which is, to show the *importance* and *originality* of these researches, which the Review and others, seek to undervalue by an array of great, opposing names, without any direct proof whatever, excepting stale experiments, wholly unlike mine, and foreign to the subject under consideration, and therefore, inadmissible in the study of human physiology, much less for the establishment of an *entire new system of philosophy*—another Novum Organon.

With the exception of a single journal, which dissents to one, perhaps more, of my leading opinions, the numerous medical journals "on this side of the Atlantic," which have noticed the "researches," have conceded the question of originality, or "have attached importance" to the same, though not having copies of all these, the following may suffice.*

"The experiments made by Dr. Dowler on Post-mortem Contractility, *are highly interesting* and *important* to the physiologist. * * * *These experiments are new*, and reflect great credit on Dr. Dowler as an accurate observer." (Western Lancet. Lexington, Kentucky.)

"Dr. Dowler is favorably known to the medical world as the author of *several original views in physiology*. His *interesting* trains of re-

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* I have sometimes *Italicized* the text.

searches on Febrile Caloricity, and on the Post-mortem Contractility of the Muscles, have not failed to elicit the attention of the profession."—(SOUTHERN MEDICAL AND SURGICAL JOURNAL. Augusta, Georgia.)

"We are free to confess the fact that contractility, in the cadaver at least, *is inherent in the muscle, independently of all its nervous connections, is abundantly proven*, admitting the experiments to be valid, which we have no reason to doubt. The muscular contractions must have been produced *exclusively* by the act of purcussion upon the muscles, *not involving the nerves* leading to them; for contractions were in all instances *limited to the muscle* or muscles struck, and hence, by percussing appropriate muscles, each or all the muscles of the forearm, for example, could be produced at pleasure. * * * The observations of Dr. Dowler *demonstrate* that muscular contiactility exists in the *tissue, per se.* * * It is well known that the *subjects* of the experiments upon which the doctrines of the reflex school *are based*, were taken chiefly from the Batracian class of animals. * * * The coarse, cruel, and *fallacious experiments* practiced, are *justly* repudiated. * * * We come now to the method of Dr. Dowler, which certainly has the merit of simplicity; but is not less worthy of attention and confidence on that account. * * * In closing this review we should do injustice to our appreciation of the labors of Dr. D., if we omitted to express the great gratification with which we have read his valuable paper. His observations on muscular contractility, and the temperature of the body after death, are of a striking character, and cannot fail to excite the lively interest of physiologists. We are sorry to learn that not only have attempts been made to deprive him of his claims to originality, but that the exactitude of his observations has been flippantly discredited. The experiments can easily be verified, which does not appear to have been done before calling his facts in question. We trust he will be encouraged to continue and extend his experiments, relying as he certainly may do with confidence, that justice will be done both to himself, and to the facts which he may develop." (BUFFALO MEDICAL JOURNAL, New York.)

"Our pages have in a former number contained some account of Dr. Dowler's experiments in reference to *post-mortem caloricity*; since then, the profession has been made acquainted through other publications with his observations on *post-mortem contractility*, which are little if at all less curious and suggestive than those on the former subject."

(MEDICAL EXAMINER, Phila.)

"The essay" [on contractility] "is *replete* with matter of *very deep interest*, may be the means of introducing more *correct views* of the functions of the nervous system, and a *modification* of at least some of the *pathological theories* of the day. The *results* of Dr. Dowler's experiments are we must confess, as *unexpected* as they are *important*. That the muscles were capable of being excited to contract for sometime after death was known to physiologists, but it was admitted that the period this capacity to contract existed was very short, and that it could be excited into action only by some *powerful agent, as electricity* or *galvanism, but that genuine muscular contractions could be produced for many hours after death by a simple blow of the hand, was a fact* NO ONE SUSPECTED *until the appearance of Dr. D's.* ORIGINAL *paper.*" (THE AMERICAN JOURNAL OF THE MEDICAL SCIENCES. Phila.)

I copy a small portion of my scientific correspondence, omitting names, places, and dates, as I have no means of knowing, at this moment, whether the writers would, or would not allow me to publish their names. I will say, however, that as professors and authors, they stand high "on *this* side of the Atlantic." They are "well informed observers," though they do not charge me, as does the Reviewer "with bringing forward as a *novelty*, what was *well known* to *all* careful observers."

"Your article on the reflex, &c., makes a great deal of talk here.—Prof. —— is warm in its praise; they *all think* you have gone *far to upset Marshall Hall.*"

"Nor can your *facts* run any risk from his * * * *speculations.*—Go on fearlessly. Truth is mighty and will prevail."

"He [Dr. Hall] will give you a sharp review. It will be a battle worth looking at "when Greek meets Greek," &c. I can't say how bets would go at present, though I have heard several good judges will stake two to one, &c."

"I must say without any intention to flatter you at all, that you have been *perfectly successful* in the accomplishment of your object. But is it not very unkind in you, to knock this eminently beautiful reflex theory on the head? It was so pretty, so very consistent with many phenomena! such a beautiful assumption! I do not know what some of our *confrères* will do, now thrown so completely at sea again. Those experiments must have fallen like a bomb-shell among the *nervous physiologists.* Your *discoveries are exceedingly important.* Time will apply them to practical purposes. Anticipate that time. Go to the utilitarian* work of deduction. To be sure you have blown to the winds the fine cob-web theories of your opponents, together with all their deductions. That is something;—for next to the discovery of a new truth, is the destruction of an error. [I] should like to be near Hall, &c., when he reads your paper."

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* Should my correspondent ever see this paper, let him read what Professor Whewell has said upon this subject: "The nature of knowledge must be studied in itself and for its *own sake*, before we attempt to learn what external rewards it will bring us. (*Philos. Induc. Sci.* i, 14.) "*No scientific discovery* can, with any justice, be considered *due to accident.* In whatever manner facts may be presented to the notice of a discoverer, they can never become the materials of exact knowledge, except they find his mind already provided with precise and suitable conceptions by which they may be analyzed and connected." (Ib. ii, 189.)

Schiller says of Wisdom,—

> To some she is a goddess great;
> To some the milk-cow of the field—
> Their worship is to calculate
> The *butter* she will yield.

The London correspondent of the New Orleans Bulletin, in his letter (of the 3rd of July, 1847,) giving an account of the Scientific Association of Great Britain, which had just closed its labors, and which includes many of the learned throughout Europe, adds, "the only objection to the Association is, the *applicability of their discoveries is not in any way made the subject of attention.—To discover, not to apply, is their motto.*"

Ask, O! student of medicine! ask your unprejudiced judgment, were there ever errors so general, and yet so stupendous, as those now called modern discoveries in the nervous, especially the spinal system. Are they not wholly based on certain experiments, which have no known connection with the doctrines sought to be deduced from them? Have these phenomena any fit scientific application, except to the individual animals or class experimented on? Have these phenomena any probable value even in this identical class excepting the special conditions induced by vivisections, and so forth? Can denaturalizing processes, illustrate, nay constitute complete discoveries in, and for conditions essentially different? Were these phenomena derived directly from man, or from an animal precisely similar, would they be admissible proof, by which, to establish the one hundredth part, of what is now supposed to be established by them? Are not these phenomena for general purposes obscure, blind, meaningless, and therefore, valueless? or at least not yet matured into exact science? These experiments are praiseworthy, and constitute a portion of knowledge, highly interesting, in comparative physiology, but very limited in their import; but they neither prove the nature nor *modus* of sneezing, nor do they constitute scientific midwifery; they do not prove that the passions and paralysis, hæmorrhage and convulsions, are exclusively due to "the true spinal marrow." These matters are thus, not without reason, alluded to frequently; and, as I have satisfied myself, if not others, that muscular contraction is not dependent on the spinal marrow or nerves, and, that therefore the fundamental principle of the new system is absolutely erroneous, I deem it no crime to publish what I believe, though the times be troublous, though neurological terrorism now reigns, and opposition, like spitting against the wind, is spitting in one's own face, France, so celebrated for medical science, heretofore, generally opposed to the most important improvements and doctrines of English medicine and surgery, (witness Hunter's doctrine, of healing wounds by the first intention,) has bowed to Bellism, and of course, Reflexism will follow. But history is, in such cases very instructive.

Broussaisism lived and died within a few short years. Before his doctrine prevailed, France exported leeches to a considerable extent; in 1833, the imports exceeded the exports, about forty-one millions, being an increased consumption to that amount. Three or four years ago blood-letting to fainting often repeated in the same case, was essential! Now in some of the largest fever institutions in the world, not a vein is opened during the entire year! Humoralism replaces solidism; the nerves, gastritis; stimulants, leeches; quinine, calomel! What is it, that authority cannot be brought to support? If great names can be quoted in favor of Reflexism, the same is true of mesmerism, hydropathy, and homœopathy; but facts, nature, science, do not change, or if they should even change, they cannot be brought to sustain propositions and theories which contradict each other.

The following just views, by a correspondent of the Lancet,* deserve much attention: "How strange and inconsistent in modern pathology is this exclusive reference to the nerve, in all questions relating to the nature and treatment of muscular disorders! In the operations of disease, every organ, excepting the muscle, is supposed to originate its

* October, 1846.

own symptoms, and to maintain its own process of damage or cure.—Heart, liver, lungs, kidney, are thus made responsible, by name and in their complete structure, for the disorders affecting their several functions. The muscle alone, of all organs, in truth, the most independent is never suffered in the lists of nosology, under its proper designation, but finds a place, by right of spasm and palsy, in the loose catalogue of the neuroses, as a mere part and offsett of the so-called nervous system. By most practitioners, when a consultation on disorders of the contractile function, muscular and nervous are used as convertible terms, for the expression of their views in the treatment of the case. In the physic of 1846, there is no greater, no more mischievous error, than this substitution, in the complete organ, of a part for the whole, this degradation, in the nosology of spasm and palsy, of the blood and the fibre, by distinction, undue and exclusive of the nerve. No palsy, it is well known, is more sudden and complete than that which follows on interruption of the blood-current in the affected muscular structure, though its nervous material be everywhere sound and complete. Be assured that it is not a mere speculative question, how far in palsy of the muscles, the prevailing nervous theory should be suffered without rebuke." The writer argues that patients are put to great and useless torture by this limitation of the treatment to the nerve, "by cupping, blistering, moxa-burnings, caustic issues; from the drain and irritation consequent on these severe local applications, there is serious, it may be fatal, irritation of the original symptoms."

Without admitting that the recent neurological discoveries, so called, are well established fundamental truths, I propose to give a critical sketch of the historical progress of the same, which, however, imperfect, must be regarded as a desideratum to the medical student, who will with every aid, find himself perplexed in this branch of science, even at the present era, after all the illusory announcements put forth, as if the whole temple of medicine were completely irradiated with some sudden gush of light. The discoveries attributed to Bell and Hall, are viewed by many as due to those who preceded them, and even to ages very remote.

In a review* of Mr. Adam's Translation of the works of Paulus Ægineta, an author who wrote according to Springel, A. D. 634, it is asserted by the Translator, that all the merit of the discovery of the anterior and posterior roots of the spinal marrow, belongs not to Sir Charles Bell, but to the ancients, Erasistratus,† Aretæus,‡ and Galen; especially the latter, who maintains that the "nerves have three uses, namely, to communicate to the organs of sense their respective sentient faculties; to excite motion; and to enable their organs of the body in general to discern what might be injurious to them," (lib. v. c. 9.); a theory which Aretæus had previously published, namely, that there are "*sensatory* and *motor* nerves" altogether *distinct.*§

Passing by all the neurological observations of more than thirty centuries, which afford little more than prelusory suggestions, unaccompanied by demonstrations, we come down to the moderns, particularly to Robert Whytt, of Scotland, who died in 1766, after having published several

* Med. Quart. Rev. April, 1834, cited, Doc. 7, et seq.
† B. C. 304, Sprengel. ‡ A. D. 81, ib. § Documents, &c., 8, 9.

ingenious works on the physiology of nervous system. In his book on the Nerves,* he dwells chiefly on "that sympathy which obtains between the various parts of the body," and which he calls "a remarkable consent." But his essay on the Vital and Involuntary Motions of Animals, published in 1751, falls more directly within the range of this inquiry. Like that most acute, but often visionary metaphysician, Bishop Berkeley,† he ascribed muscular motion to the soul. Whytt goes so far as "to conclude that the motions of the *separated* parts of animals are owing to the *soul* or sentient principle still continuing to act in them,"‡ "though not attended with reflex consciousness, a power which the soul only exercises in the brain."§ "The various sympathetic motions of animals produced by irritation, whether in a sound or morbid state, are owing not to any union or connection of their nerves, but to particular sensations excited in certain organs, and thence communicated to the brain or *spinal marrow*."|| "Dr. Hales informed me (he adds,) that having many years since tied a ligature about the neck of a frog to prevent any effusion of blood, he cut off the head, and thirty hours after, the frog moved its body when stimulated: but that on thrusting a needle down the *spinal marrow*, the animal was strongly *convulsed*, and immediately after become *motionless*."¶ These quotations are not reproduced for their truthful expositions of physiology, nor for their similarity to the reflex doctrine, but for their language and suggestive character; as "*reflex sensation, sympathy transmitted to the spinal-marrow, a loss of motion on disintegrating the latter*:" for example, the statement that sympathy is transmitted *to* or *from* the spinal marrow without nervous connections, very naturally suggests the converse question, whether the nerves may not be the identical agents of transmission.

The same course of reasoning will apply to many doctrines affirmed by Haller: "The *same nerves*," he remarks, "most evidently are subservient both to *sense* and *motion*; so that we are not allowed to adopt *two distinct systems of nerves*, one *motory*, the other *sensative*." (*Phys.* CCCLXXXIV.) Here the doctrine of Bell is mentioned, not assented to. If writers four thousand years ago had denied the possibility of setting up the printer's types, and the present useful applications of steam-power—had they asserted that lightning rods, electric telegraphs, and rail-roads would prove useless—vaccination, inefficacious—mercury, a cure for salivation—the bark and its salts or quinine, fatal in agues—or had they argued, that the blood was circulated *towards* the heart by the *arteries* and *from it* by the *veins*, and that etherization increased the pain of a surgical operations, it is almost certain that these false views, would have awakened inquiry, or rather would have revealed the truth, in almost every instance, and as a consequence: This mode of reasoning is not, however, very satisfactory, though worth something. Thus reasoned Polonius:—

"By *indirections* find *directions* out."

The reflex function or action of the nerves is the favorite doctrine of

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* Third Edit. Edin. 1768. † Berkeley's Works, ii, 90. Lond. 1843.
‡ Documents, &c., 113. § Ib. 116. || Ib. 121. ¶ Ib. 121.

Professor Prochaska* in his Latin works, from 1784, to the early part of the present century..

I give a little abridged, and corrected the translation of the work cited,† which fairly expresses the original, so far as I am capable of judging on comparison of both; Prochaska, says: "External impressions, which are made upon the sensorial nerves, are propagated rapidly through their whole length to their *origin*, whence they are *reflected*, according to a *certain law*, passing into certain corresponding nerves, through which, being again rapidly propagated *to the muscles*, they excite certain determinate motions. This place in which as in a *centre*, the nerves appropriated to *sense* as well as *motion*, *meet* and communicate, and in which the impressions of the sensorial nerves are *reflected* upon the *motor nerves*, is called the sensorium commune—a term already received by physiologists," [*including the spinal marrow.*] The original differs from the modern style only in its greater clearness; in quo impressiones nervorum sensoriorum reflectuntur in nervos motorios, etc.

Prochaska regards the *sensorium commune* as the great *reflector;* but unlike more recent writers, *he includes under that term, the entire spinal cord—totam medullam spinalem*, and although he does not *always limit*, he clearly recognizes the *separate, distinct*, and *independent reflex action*, now referred to "the *true* spinal marrow," and rendered as obscure as style can make it. Here, there is no room to doubt, because he first lays down the doctrine, and then gives examples: "That the sensorium commune extends to the spinal marrow, we learn from the *motions* remaining in decapitated animals, which could *not* take place without the consent and co-operation of the *nerves arising from the spinal marrow;* for if a decapitated frog be pricked, not only does it retract the punctured part, but it crawls and leaps, which could not be without the consent of the sensorial and motor nerves, of which the *common seat* must be in the *spinal marrow*—cujus consensus sedes in medulla spinali—*the part* of the sensorium commune *remaining*." "This *reflection* takes place whether the mind be *conscious of it or not*." Again and again, he distinguishes the *reflex function of the spinal marrow* from that of the *brain*, physiologically, and pathologically. "To these we must add all those motions which for sometime remain in the body of a decapitated man,‡ or other animal, and are excited by *pinching* the body, but especially the *spinal marrow*, and are governed by the residual part of the sensorium commune, which *is in the spinal marrow*"—per

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* Commentatio de Functionibus Systematis Nervosi, Opera Minora, etc.—The verb *reflecto* and its cognates, together with terms characterizing the laws of light, as *angulus*, *incido*, *incidentis*, as well as *motor*, and the like were favorite words with this author, in his neurological speculations, nearly half a century before their adoption by Dr. Hall.

† Documents and dates of Modern Discoveries in the Nervous System.—London, 1839.

‡ Galvanic muscular contraction, was forbidden by the King of Prussia in 1805, to be practised on decapitated criminals, because it increased the pain beyond the prescription of the law; as such persons were supposed to retain sense and consciousness! (*Med. Rep.* ix.) An electrical battery, sufficiently strong, would instantaneously kill, and probably, without any pain!

residiuam sensorii communis partem, quæ in medulla spinali est, reguntur. "The *reflection* does not obey mere physical laws, wherein the *angle of reflection* is *equal to the angle of incidence*,* but *peculiar laws.* Many examples prove *this general law of the reflections.* Irritation of the internal membrane of the nostrils excites sneezing:—Vomiting, tremor, chorea, paroxysms of intermittent fever, &c."

"A general law, according to which the sensorium commune," [this term, be it remembered, includes the spinal marrow,] "*reflects sensorial* into *motor impressions*, is our preservation:† so that certain *motory* impressions follow *external impressions* hurtful to the body, producing motions tending to ward off and remove the source of injury; and, on external or sensorial impressions beneficial to us, producing motions calculated to perpetuate that benefit. The principal function of the sensorium consists in the reflexion of sensorial into motor impressions—præcipua functio sensorii communis consistat in reflexione impressionum sensoriarum in motorias. No muscular motion can be excited, unless a stimulus applied to the sensorial nerves passes by a certain reflection to the motor nerves, and excites muscular contraction; it is certain that the reflection of impressions—reflexionem impressionum—for inducing these motions, takes place without consciousness—sine animæ conscientia."

Dr. Hall who quotes Prochaska largely,‡ and must, therefore, know his doctrines, maintains, nevertheless, "that there is nothing in that author possessing *the most remote similarity to his own*," whereas, it is evident, that Prochaska has expressed the *reflex doctrine as clear as is possible*, and free from the ambiguities and unwarrantable extensions, which Dr. Hall has given to it,—at best, a *mere hypothesis.* Let all who doubt this, examine with care, Dr. Forbes' paper on this subject,§ which I have just seen, and from which I do not quote, except the following passages as illustrative of Dr. Hall's "efforts to sustain the *status* of the profession" in Europe, upon which the reader will find some further remarks in another part of this paper; a crisis has been reached—the nervous system is excited, and a little raving, with subsultus, and an occasional convulsion, are to be expected. Hence, the incoherent words of Dr. Hall, as "cowardice! calumny! falsehoods! courts medical!" etc. Dr. Forbes translates whole pages from Prochaska, from which he shows conclusively, that Dr. Hall's pretensions to *originality* are unfounded,¶ adding, that the latter, "has taken every opportunity to depreciate the merits of Whytt and Prochaska, as if he were impelled by the conviction that his struggles for fame would be valueless, unless he scornfully trampled their claims under foot.

"Dr. Hall's eternal trumpeting of his own great deeds, especially in regard to the reflex function, in every work he has published in almost every

* Ubi angulus reflexionis æqualis est angulo incidentiæ.

† Generalis tamen lex, qua commune sensorium impressiones sensorias in motorias reflectit, est nostri conservatio.

‡ Nervous System, 27, 45. Phila. 1836.

§ British Foreign and Medical Review, for January, 1847.

¶ "All the *fundamental* and *acknowledged* views which are claimed by Dr. Hall, as exclusively his own, are to be found in Prochaska's writings most succinctly and most clearly set forth." *Ib.*

scrap he has printed," &c.—"He has over and over again, and loudly proclaimed his own unequalled merits in this department, and scoffed at the pretensions of other excellent men, his predecessors or cotemporaries. He has either studiously passed over in silence or openly ridiculed and maligned Prochaska's doctrine, and poured on the heads of those who did no more than assert their resemblance to his own, all the venom which his bitter nature could engender. And up to the very hour at which we write, he continues to boast as loudly as ever, of his originality, &c. We can add no comment in words that can in any way emulate, in damning potency, the eloquence of this simple statement.—It is grievous to be forced to write it down; it is melancholy to contemplate its full import."

I feel justified in making these quotations to show the manner in which Dr. Hall treats those who do not, any more than myself, receive him in his mission as a discoverer, as well as to show that, if I do not always speak of Dr. Hall, (a man of ability, I admit,) with the respect which might seem requisite in matters purely scientific, there are the strongest reasons for my justification.* But leaving this subject, I proceed to conclude the historical sketch of modern neurological discovery, omitting for want of space, many illustrious names.

Mr. Alexander Walker preceded Bell, Magendie, Müller, Hall, and indeed, all others in discovering the leading features of what is now called Bell's discovery; for although the Bellites differ with him in regard to his ascription of the *sensiferous* property to the *anterior*, and the *motiferous* to the *posterior* roots, these are rather details than an absolutely new conception. His first publications were dated in 1808; those of the following year, speak thus: "wherever a part having both *sensation* and *motion*, is supplied from one nervous trunk, that trunk envelopes *both a nerve of sensation* and *one of volition*. The only apparent difference is that their motions takes place in different directions—the latter resemble the arteries, the former the veins." The "nervous circle" is dwelt on. The action or function of the nerves "passes to the spinal marrow, by the anterior fasciculi of the spinal nerves, which are, therefore, nerves of sensation, and the connections of which with the spinal marrow or brain must be termed their spinal or cerebral *terminations*,"† [even better than *Reflectors.*]

"In a comparatively recent work, (by Mr. Walker,) to which is prefixed some account of the author's earliest discoveries, of which the more recent doctrine of Bell, Magendie, etc., is shown to be at once a plagiarism, an inversion, and a blunder, associated with useless experiments, they have neither understood nor explained."‡

More than twenty years after Mr. Walker had published this view, setting forth in the clearest manner the *afferent* and *efferent* function of

* The Medico-Chirurgical Review, (January, 1847,) admitting as it does, Dr. Hall's claims as a discoverer, declares nevertheless, that "he is one of those characters that *will not correct his errors*; that *pertinacity* is the *very woof* of his character; that he deems it a point of *honor* never to change or concede aught, except to *himself*; that he is *jealous to paltriness*; that he continually repeats, *usque ad nauseam*, the *same* facts, dogmas, that he has not only erred, but has suffered much from this *arrogancy* of character, &c."

† Doc. 15, et seq.

‡ Doc. 17.

the nerves, after stating that this double function is performed by *separate nerves*, "a nerve of *sensation* and one of *volition*," we find Mr. Bell making exactly the same statement: "The nerves are sometimes separate; sometimes bound together; but they do not, in any case, interfere with or partake of each other's influence—one filament for sensation, another for muscular motion.*" In the same work, he quotes from his lectures to the College of Surgeons, a summary view of his system, of each portion of the nervous circle, of innervation, of experiments, and of muscular contraction; of each he speaks with becoming doubt, until at the end of the enumeration, getting out of the deep waters of uncertainty, he exclaims,—"At all events, you observe that *a mistake has hitherto universally prevailed* in supposing that *one* nerve could perform *two* functions of opposite tendencies,"† and all this, the only thing he felt certain about, Alexander Walker had published long before.‡

M. Müller undertook to experiment upon rabbits, with the view of ascertaining the functions of the spinal roots. "But he found that the previous operation of opening the vertebral canal was so difficult, and attended with such excessive pain to the animals, as frequently to *induce involuntary twitches of all the muscles even when the nerves were not directly irritated*, so that he was precluded from deducing *any satisfactory conclusions*."§ He, therefore, fell back upon frogs, and upon galvanism! by which, through the anterior roots, he caused "*convulsive movements*."

The Medico-Chirurgical Review calls this substitution of frogs for rabbits, "a happy thought!" and the reasons are these: "the vertebral canal of the frog may be opened with very *little trouble*, and with comparatively *trifling pain*; the animal is so *tenacious of life*, that it remains quite *lively* after the operation."‖ If all this be true, it is one of the worst analogues that could be chosen for human, or even rabbit physiology. Who knows that frogs suffer but *little* from the dissection of the spinal cord? Is not this very *tenacity of life*, so unlike man, a good reason, not for their *selection* but their *rejection*?

I proceed next to the discovery claimed by Dr. Hall.—What that doctrine really *is*, seems to be at least, sometimes misunderstood. In a *critique* on my essay on muscular contractility, my quotations and references, were regarded as *too general*. It is remarkable, however, that the parties most interested have not complained. The short sketch now called for, will obviate all difficulties in this respect, and at the same time be acceptable, as I hope it will, to the student, showing what *is* the reflex system of Dr. Hall, and all Philo-Hallians, *not* the reflex systems of *others*. There is one fundamental error in the logic of this school, which must be corrected, otherwise nothing can be determined, namely, the introduction of collateral, irrelevant matter, with assumed

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* Nerv. Syst. 20. † Ib. 218, 219.

‡ It would be tedious, and indeed, ungrateful, to enter upon the charges against Mr. Bell, in relation to certain dates prefixed to his papers, some of which it seems, were antedated, either by himself, or by others. (See Documents, 37.) Even his paper, "A New Anatomy of the Brain," which was printed, but *never* published, was later by several years, than Mr. Walker's publications.

§ Med. Reg. cited from Med. Chir. Rev. 1834. ‖ Idem.

analogies, quite overlooking the positive, direct, experimental proof, based upon the spinal cord, its roots, and the terminations of its nerves, which constitute *the* discovery, and *not* sterility, hæmorrhage, tic; the passions, parturition, &c.,—concerning these, no direct experiment has been offered.

By confounding the reflex doctrines, and by assuming for the living body, sundry reflex actions beyond the pale of my inquiries on contractility, I have been misapprehended by several critics. To prevent latitudinarian criticism, "stale, flat, and unprofitable" analogisms, I wish it to be distinctly understood, that, by nervous circle, the functions of the spinal roots, the reflex action, &c., I mean the doctrine of Bell and Hall, *founded on certain experiments*, as explained by themselves and their most ardent friends. If the *issue* is to be changed again and again, if new elements are to be introduced into this doctrine, then the war of logic will be eternal.

I proceed, therefore, to make the *amende honorable*, as my former quotations have not been sufficiently definite, I begin with Dr. Hall, in an order somewhat chronological, quoting in part from his first papers, as recently republished in London,* as well as from his later works.—"All movement ceased on withdrawing the spinal marrow" [of a turtle.]† "The *presence* of the spinal marrow is *essential*,"‡ [to muscular contraction;] it *ceases* on *removing* the spinal marrow"§—attaches itself to any part of an animal, the *corresponding portion* of the brain and spinal marrow of which is *entire*."|| "The reflex function consists of impressions carried to and from the medulla"¶—"first pursuing an *arrière* course to the spinal marrow, being afterwards reflected upon the muscles,"** "incident into the medulla, reflected from the medulla."†† A horse was knocked down—the cornea pricked—the orbicularis and abducens contracted; whereupon Dr. Hall philosophises thus: "There can be *no doubt* that a filament, &c., conveyed the impression to the medulla oblongata. All this is wonderful, and I believe, hitherto quite unknown to physiologists;"‡‡ [the reflex function is] "some mysterious influence"—[how then, a *perfect discovery?*]; "another fact,—the whole *tone* of the muscular system is the result of an excito-motory function.—The limbs of an animal, or of a part of an animal separated from the influence of the cerebrum become *relaxed*, on destroying the *spinal marrow*."§§ "I gently withdrew the medulla and brain" [of a turtle.] "All the phenomena," [i. e. muscular contractions,] "ceased—no movement followed;"|||| "the limbs were no longer obedient to stimuli, and became *perfectly flaccid*, having lost *all their resiliency*. The sphincter lost its *circular form*, becoming *lax*, *flaccid*, and *shapeless*.—The tail was *flaccid* and unmoved on the application of stimuli;"—"proves that the *presence* of the medulla is *necessary* to the contractile function,—the reflex character of this property," [and,] "that of the tone of the limbs, &c., depend upon the medulla spinalis,—effects not hitherto suspected by physiologists."¶¶ The author asserts "that the phenomena

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* Documents, &c., 1839. † First Com. Zoolo. Soc., Nov. 27, 1832, p. 136. ‡ Doc. 137. § Ib. 138. || Id. ¶ Ib. 142. Second Com. Zool. Soc., Aug. 12, 1834. ** Ib. 143. †† Ib. 144. ‡‡ Lect. Nervous System, 1836, Phila., 33, 34. The fact here given does not justify the conclusion. §§ Ib. 40. |||| Ib. 41. ¶¶ Ib. 42.

detailed, *subsist in distinct portions* of the medulla"—"distinct and separate portions,"—which being destroyed, the *corresponding* portions *of the muscular system deriving nerves* therefrom, will be no longer contractile, etc.* Dr. Hall's next work, Memoirs on the Nervous System,† is but an iteration of those previously cited.‡ The necessity of the *integrity* of the spinal marrow in order to muscular action, with the incidental question of originality of the discovery, are the leading topics of this work.

The following passages, cited from Dr. Hall's work on the Reflex Function, published in 1833, will be found in the Medico-Chirurgical Review, for July 1834: "There are four modes of muscular action,—1, voluntary; 2, that of respiration; 3, involuntary; a *fourth, excited* by the application of stimuli, which are not, however, applied *immediately* to the muscular or nervo-muscular fibre, but to *certain membranous parts*, whence the *impression is carried to the medulla, reflected*, and *re-conducted to the part impressed*, or conducted to a part *remote from* it, in which muscular contraction is effected"—"in a curved, reflex course—requiring the *connection* with the medulla to be *preserved entire*." The third kind of muscular motion, the involuntary, he calls *irritability*, of which he gives this definition,—"The movements of irritability are the result of the *immediate* application of a stimulus to the nervo-muscular fibre *itself*." "The reflex function is different; its seat is in the medulla—*ceases* when *it is removed*."

In the last edition of Dr. Carpenter's esteemed work on Physiology, published in both England and America, during 1846, it is said in relation to the reflex doctrine, that, after the brain of the frog is cut off, muscular contractility remains, whereupon, the following explanation is given: (the Italics are chiefly mine.) "We are *not* to suppose that the stimulus *acts at once upon the muscles*, without the nervous system being concerned at all; throwing them into contractions by *direct influence*. For it is quite *certain* that unless the nervous trunks remain *continuous with the spinal cord*, and unless the part of the spinal cord with which they are connected remains *sound*, *no action* will be the result. If the trunks be *divided*, or *either* of the roots by which they are *connected* with the *spinal cord be severed*, or the lower portion of the spinal cord itself be *injured*, *no* stimulation will cause the muscular movements;§ "if the anterior roots be touched, contractions are immediate— *if divided*, *no* such a result follows, whatever amount of irritation be applied—if the posterior roots be touched, no vigorous muscular contractions, the movements are evidently of a reflex character, being called forth by the anterior or *efferent roots*."||

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* Passim. † Lond. 1837.

‡ "This practice of constantly repeating himself, is a striking illustration as well of the irresistible tendency to self-glorification, so characteristic of Dr. Hall, as of the comparative barreness or limited range of his intellect. Perhaps there never was an author who persisted so perseveringly and systematically in thrusting in the same things, over and over again, &c." (*Brit. and For. Med. Rev.*, Jan. 1847.)

§ Phys. 236. || Ib. 502.

The reflex-neurologists, fail utterly, in showing any positive or even probable connection, as cause and effect, between their experiments and theory. The vast assemblage of physiological and pathological phenomena, which they claim as having been explained in the clearest manner by these experiments, have not been traced link by link, either in the ascending or descending series; their order, uniformity, succession, antecedence, sequence, have not been ascertained and made known, with the concurrent, but unessential concomitants and co-incidents; their pathological anatomy, whether *material* or *immaterial*, has not been plainly traced to these experiments. Indeed, no material or immaterial morbid anatomy of the spinal cord, the seat of so many diseases has ever been given, or even described, and, yet there must be in this cord, a change for every malady. What are the spinal anatomical characters of hæmorrhage, or hydrophobia,—sterility, or strangury,—paralysis, or passion,—asthma, or abortion,—tic, or tetanus,—all spinal in their location? Now, I lay it down as an axiom in morbid anatomy, that no great and important tissue or organ of the whole body, presents on an average, so *few well marked structural alterations*, as the spinal marrow, provided it be examined in its *material form*, from one to six or even twenty-four hours after death, leaving out immaterialities, incorporealities, and spiritualisms. In fact the *spinalists* have been an unsuccessful sect. Le Gallois, (the date of whose publications on the vital functions of the cord, is not precisely recollected,) early in the present century, claimed the medulla spinalis as the source of life to the entire trunk, as well as the exclusive seat of sensation and motion, all being independent of the brain! The cord ought to be the focus of morbid alteration—if anatomy have any thing material in it, as some people have supposed in their simplicity and ignorance. Its physiology, I repeat it, is still more obscure than its anatomy. No one will pretend that there is anything in the physical structure of the anterior roots, by which motion can be recognised or inferred as a nerve-property. The physico-analogical argument is against the supposition, and still the more so when the roots are supposed to communicate to the muscles a power, which they themselves do not possess in virtue of any special physical, or anatomical adaptation. I do not positively deny that the anterior roots excercise an influence upon muscular motion during life, but, I contend, that their influence as the exclusive motory force or agent, is not proved, is not even probable, while the muscles do possess adaptations in size, strength, direction, origin, insertion, and mechanical contrivance, every way adapted to act in the most independent manner as motors, as far as any one tissue can lay any claim to independence.*

John Hunter's massiveness of intellect, enabled him in a great degree, to resist the momentum of mere theory. He spoke but the language of

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* Englishmen, ought by this time, to know something of muscular motion, as Dr. Croone, who died, in 1684, bequeathed not only money, but the profits of a house, for annual lectures on that subject. The Croonian Lectures on Muscular Motion have continued 163 years! Many rich prizes, in both Continental and Insular Europe, are awarded annually, for similar purposes. It is easy to swim when held up by the chin!

common sense, when he declared in his lectures on surgery, that "much more has been given to the brain and nerves than they deserve. They have been thought to be the cause of every property in the animal body; that independent of them the whole body was a dead machine, and that it was only put in action by them. But although their actions are absolutely necessary in the machine, they are not so universally so as has been imagined. They are not the cause of growth, nor do they even preserve a part from death, although the whole as a whole cannot live without them."

The reflex theory, including Bellism, unlike every other theory, has not a single, clear application in practice. Humoralism appeals to chemistry, and solidism shows disorganizations, but the reflex doctrine, nothing. In dysentery, consumption, croup, pneumonia, the alterations are palpable, and the treatment can be directed upon intelligible principles; but the reflex pathology and therapeutics are wholly unknown;—perhaps, this is the proper method of managing immaterialities, that is, to know nothing about them; and yet, we are told that "the material studies of medical men, as humoralism, pathology, anatomy, and chemistry, render the mind inept" for the sublime study of the *true* spinal cord! The consequence is, that "these material studies," should be banished from our medical schools, as "they render the mind inept" "to the second great discovery." Which college will set the example?

It is owing to these visionary theories which promise so much and disappoint the student's expectation, that so many turn out of the right way into the paths of quackery. Faust studied theory profoundly until he lost confidence in practice, and while the people were praising him and his father, for their great success in curing fever, he exclaimed—"thus did we with our hellish electuaries, rage in these vales and mountains far worse than the pestilence. I myself have given the poison to thousands; they pined away, and I must survive to hear the reckless murderers praised;"—he therefore, soon renounced both the theory and the practice: "I no longer fancy I know anything worth knowing.—Then I have neither land nor money, nor honor, nor rank in the world. No dog would like to live so any longer. I have therefore devoted myself to *magic*, * * * and drive no longer a paltry trafic in *words*."

The reflex doctrine "is the second great discovery, destined to revolutionize the science of medicine," or it is a great error destined to consume, without any compensating advantage, much of the student's valuable time,—to lead him into a fallacious method of experiment and of reasoning, and to mislead in both the theory and practice of his profession. Taking this latter view of the matter, and believing that every cultivator of science is bound to do all the good he can in the discovery and diffusion of the truth, as well as in the correction of erroneous principles, and believing, moreover, that the reflex neurologists, cannot be understood, simply, because they do not understand *themselves*, I will indulge the hope that the reader will not attribute to me unworthy motives, whatever errors may be committed in this discussion.

Is it modest in reflex-neurologists to affirm that they are fifty years in advance of their age—fifty years before their ignorant, ungrateful cotemporaries—fifty years before our unworthy planet was ready for their advent? And what have they done? Reflected a reflection:—Reflected an opinion—the hypothesis of another. "Opinions formed from opinions—what are they but clouds sailing under clouds, which impress shadows upon shadows." These theorists would, by their illusions, convert the fair field of science into a sterile waste. THE REFLEX-MIRAGE, like that in the deserts of Africa, presents flowery medows, pure fountains, and hospitable dwellings where none actually exist.

The ink with which the last sentence was written, was scarcely dry, before the last number of the Lancet was received, in which it is proposed to examiners, to *require all candidates to be examined in the reflex theory!** Innocent youths, never yet guilty of a homicide, *secundum artum*—candidates for M. D.—who have learnt in the Lancet, that not one of the Royal Society, (the most learned in Europe,) understands the reflex discovery, (its inventer excepted)—these poor lads are required to comprehend that which the gray beards themselves cannot.

* * * * * The sun had gone down upon the earth. The moon was mounting above the plains of Louisiana, while many reflex-moons were dancing upon the turbid waves of the Mississippi, which rolled noislessly beneath my window. The pendent gray moss, a parasite of the cypress forest which overlooks the city, now blackened by night, waved silently in the breeze. I fell asleep, as soundly as John Bunyan while writing the Pilgrim's Progress. I dreampt I was a young man, walking thoughtfully upon a shore, but whether it was the shore of the Mississippi, the Chesapeak, the Hudson, the Thames, or the Seine, I could not tell. Hardby arose a great temple, whose spire pierced the clouds. It was the medical college, wherein I was going to be examined for the degree of M. D. I feared the ordeal. Every artery of my head throbbed. I hastened to my room, to review my studies. The sciences, one after another, passed before my mind. Surgery with his catlings, scalpels, and saws—chemistry with its crucibles—obstetrics with her screaming infants! To the dead body I was perfectly at home. I marched up to a skeleton, in my room, and struck it with defiance! Dry bones, I know you all! The skeleton grinned! A voice came from its hollow skull. REFLEX! REFLEX! REFLEX! I was once a student, but the nerves distracted me—turned my brain. "I sought the bright day, and with an ardent longing after truth, went miserably astray in the twilight."—Beware of the reflex function, for even the Royal Society cannot, the Reflexians do not, and the students should not, understand it.

[Being informed by the printer, that there is not space for more than half of my article in the present number of this Journal, I ask the indulgence of the reader. As the article is, so far, nearly through the press, it cannot be re-organized, and will be, therefore, more desultory and incomplete than I anticipated.]

(*To be continued.*)

* June number.

IV.—*Obstetrical Cases.* By R. H. DAY, M. D.

CASE 1.—On the night of the 17th of February 1840, I was called to see the wife of Mr. W. C.,—aged about 17 years.

After riding about ten miles, I reached the place just before day, and found her in labour with her first child. She had been in labour six days, and had with her an old lady who acted as midwife for the neighborhood. Upon examination, I discovered, presenting at the superior strait, a large tumid oval something, I could not tell what; whether it was the breach, head, or shoulder, I was unable to decide in my own mind, as the distinctive marks of either *presentation were wanting.*—Upon further enquiry of the old midwife, I learned that during the first day of her labour, the waters broke, and that the arm presented and the hand protruded from the vulva, and that she had replaced it; from which period the presentation had been right.

Getting this clue, I readily distinguished this as a shoulder presentation, and found the hand and elbow doubled up by the side of the shoulder, returned by the thin edge of the os tince, with the humerus fractured, and all very much swollen. The child was dead and partly putrescent. The mother was quite exhausted and in a state of morbid excitement. She had been roughly and frequently handled, and was now so irritable and tender, that the slightest touch caused great pain.

Concluding in my own mind that an instrumental delivery, and that by mutilating the infant, was the only chance of success for the mother, and that but a slender one, I made known my views to her husband and friends, and, requested the counsel and assistance of my friend Dr. D. R. Allison, (now of Saltsburgh, Pa.) He was accordingly sent for, and in a few hours arrived. But in the interim I had employed myself diligently in composing my patient by the use of warm anodyne fomentations to the vulva, not without some benefit.

Dr. Allison, after a proper examination came to the same conclusion I had, and recommended the same course. It might be well to state here that in connection with the great exhaustion of the general system there was a cessation of regular uterine contraction. The uterus closely embraced the fœtus, the os uteri fully dilated and all so unyielding, that it was impossible to introduce the hand and bring down the feet.

We accordingly commenced the operation, first by removing the arm with the scapula attached to it. This was easily done, mostly with the fingers, the muscles and tissues readily giving way to gentle force. The húmerus was found fractured in two places, through the rudeness of the old midwife, and the arm, &c., considerably sphacelated. The abdomen was next opened and the fœtus eviscerated; and then by the aid of a hook, made at the time by the ingenuity of Dr. Allison from the bail or handle of a common water bucket, and friction over the abdomen, she was soon delivered. The placenta was removed without force or difficulty, and she seemed as smart a few minutes after the operation as could be possibly expected.

It is but justice to say that Dr. Allison, performed the major part of this operation.

Prescribed for her to take at night, submur. hydrarg. Dov. pulv. aa grs. 10, to be followed next morning with ol. ricini. On the 19th I visited her again; she was doing well—lochia abundant, but rather more offensive than common. Bowels soft, no pain nor tenderness. Enjoined rest, light diet, &c. &c., with instructions that if at any time within a few days, she had pain in the bowels with tenderness on pressure, to send me word immediately—and that unless I had a hearing from her I would not return as I lived some distance off.

On the 22d at night I was called, soon started, rode fast, and soon reached her residence. She was in considerable pain, at times very severe; pulse small, feeble and frequent; skin moist; belly large, but not tympanitic; uterus somewhat swollen and a little tender, lochia suppressed.

From the symptoms present, I could not believe it was active inflammation, but rather an extreme irritability and morbid sensibility of the uterine nerves and a passive congestion of its venous system. I determined to use full doses of opium and warm fomentations to the bowels. Accordingly I gave her acet. morphia and ipecac, and had flannels, wrung out of hot vinegar assiduously applied over the abdomen.

I remained all night. After the second dose of morphia, she became easy, slept comfortably, and in the morning was quite easy and altogether improved. Lochia had returned—leaving such directions and medicines as I thought necessary, I again took my departure with the understanding that I should be notified if she grew worse, or did not continue to improve. On the night of the 24th, they again started a messenger for me, but in consequence of high water they could not reach me. They sent to Dr. Allison, who being from home was not obtained, and from his house the messenger came to mine; I accompanied him, but in consequence of the circuitous route we had to travel, did not reach there till 9 o'clock of the 25th. She expired about two hours before my arrival. What terminated her existence? I believe a low or sub-inflammation of the uterine veins, terminating in gangrene. Probably the inflammation was of the erisypelatous character.

Had I a case of the same kind to treat again, in addition to the means used, I would cup over the sacrum, or apply leeches if they could be had; give internally small doses of calomel, act. morph. et camphor, and moderate stimulation.

Case 2.—February 28th, 1841. Early Sunday morning a messenger came in haste for me, to visit the wife of Mr. J. H., of Knox county, Ia., distant about five miles. I immediately started and soon reached the place. Upon enquiry I learned that Mrs. H. had been in labour some five days; pains the first few days very regular and violent, for the last day very weak and frequent, amounting to nothing more than slight irritable spasms. She was aged about 35 or 36 years; was pregnant with her sixth child; her previous labours, all natural and easy; she has been hearty and of robust constitution.

Condition at present.—Excessive exhaustion, with much nervous irritability and stupor; slight spasmodic contractions of the extremities and uterus. Upon examination per vaginam, found the os uteri fully dilated, with the head and one hand partially in the excavation of the pelvis, but completely locked, or bound in the superior strait. The hand had descended with the forehead and was firmly pressed and wedged against the arch of the pubis. The waters had escaped on the second day of her labour; and from what I could learn from the attentive old lady, the head of the fœtus had occupied about the same position as at present, soon after the escape of the liquor amnii.

I tried to move the head and liberate the hand, but could not; nor was the slightest impression produced by the feeble contractions of the uterus which frequently took place; the hand of course was much tumefied below the point of stricture and prevented its return into the cavity of the uterus.

I endeavoured to use the forceps, but was foiled in this, the pelvis being so completely filled as to prevent the introduction of a single blade, without doing violence to the soft parts of the mother.

My patient was so exhausted and feeble from the long continuance of her violent and ineffectual throes, that I dared not use the lancet; and also believing that it would not enable me to disengage the head or hand even should the uterus be relaxed by it. It was rather presumable that the fœtus was dead; the mother had not felt it move for the last thirty-six or forty-eight hours, I reflected upon her situation, and came to the conclusion that the only plan which could be persued, with any hope of success to the mother, was the operation of craniotomy.

This operation being attended with some risk to the mother, and as I thought from her present worn out and irritable condition rather hazardous, I took the husband aside, and made known to him my views of his wife's case and her danger, as honestly and as candidly as I could. I also requested medical assistance, fearing she might sink during the operation.

The case being one of emergency, he declined calling in any other help, desiring I should do what I could for her as speedily as possible.

I had a pretty strong infusion of secale cornutum made; braced up and encouraged the mind of my patient as well as I could. The ergot being ready, I administered to her a dose of it; and having no obstetrical instruments with me, except a pair of forceps, I took a pair of strong sharp pointed scissors from my pocket case, introduced the fore and middle fingers of the left hand, placing them upon the vertex, and with the right hand carefully guarding the points of the scissors, with the forefinger, pushed them up and lodged their points in the cranium at the anterior edge of the posterior fontanelle. By perforating the skull in several different places, and opening the handle of the scissors, and turning them in various directions, I very soon succeeded in breaking up the skull, and completely evacuating the brain. The ergot by this time began to act slightly. I gave her another dose, which in a few minutes produced very good pains. The fœtus now began to descend a little; lodging my middle finger of the right hand in the base of the skull, I was enabled to exert considerable traction, which, together with the noble effects of the ergot, speedily terminated fortunately this alarming case. The placenta was expelled in due time, without difficulty—and by light cooling medicine, and careful conduct on her part and her friends, she rapidly recovered, without any bad consequences at all.

In reporting the two foregoing cases, and the few remarks I shall make thereon, I am actuated by a sense of professional duty, and trust they will not be found without interest or benefit. It was my intention to have reported them long since, but owing to circumstances over which I had no control, have been prevented. The first thought which suggests itself is, could any other treatment have been adopted in either case, more rational and with better success? And secondly, could any means have been used at the commencement of labor, to have prevented a resort to instrumental delivery?

In reference to the first question, after a careful consultation of authorities, and a deliberate consideration of the cases and attendant complications, I am still firmly of the opinion, that in each case the most judicious and successful practice was resorted to.

In the first case it will be recollected, that the lady had been in labor six days; that on the first or second day, the membranes yielded and the waters escaped, and that then the arm presented and protruded; that it was forcibly attempted to be returned, and in the attempt fractured in two places, by which the child died—the shoulder enormously swollen, and perhaps the spontaneous evolution of the fœtus prevented. The waters having escaped, the uterus contracted upon the fœtus, and embraced it closely upon every point. The woman became gradually exhausted and excessively nervous and restless; and when I first saw her, presented the most haggard appearance, being affected with general irritability and constant spasms of the uterus, or at least frequent, short and weak contractions. The child was beyond doubt dead. Under such circumstances, the safety of the mother only could be consulted; and I know of no course more likely to have been successful, than mutilation of the fœtus.

In the second case, there was also great exhaustion, but not so much irritability as in the first. The only difficulty seems to have been the large volume of the advancing body. This might not have been a serious obstacle if the hand had not descended with the head. But as it was, the most violent, long continued and energetic throes, had failed to overcome it, but had thrust the head and hand sufficiently low to prevent the return of either. The os uteri already sufficiently dilated, and offering no resistance to the passage of the fœtus, and the patient quite worn out, the idea of blood-letting or relaxants was out of the question. And as the long continued and most powerful contractions of the uterus had failed to expel the fœtus, it was deemed not only doubtful, but impossible, for any amount of ergot to succeed, unless the volume of the escaping body was reduced. This rational measure was adopted the more readily as it was almost certain that the child was now dead; and the result proves the correctness of the plan. But could these fearful and dernier resorts have been avoided, or their necessity prevented had timely obstetrical skill been invoked? I must answer, I believe it could.

I have some four or five times, had the hand to present with the head in my practice, and on two or three occasions, the arm was entirely protruded; and yet by a little timely assistance, the natural efforts of the uterus were quite competent to the task; and in every case the labor was fortunately terminated without the use of instruments.

In the first case, if violence had not been used, the natural powers might have produced a spontaneous evolution, as I have twice seen, and the child delivered with safety—or if not, the feet might have been brought down, and perhaps terminated without much difficulty or serious consequences.

In the second case, if a physician of experience had been present at the escape of the amniotic liquor, I should judge with moderate skill he might have succeeded in keeping the hand from engaging with the head, and by this means the child would have been spared, and the mother subjected to no hazardous operation.

How great then is the attendant risk, and perilous the custom so rife in many parts of the country, particularly in the South, of committing parturient cases to the management and direction of ignorant and unskilful women! How fraught with danger the practice of not securing the services of experienced accoucheurs until the eleventh hour! How many useful lives are either destroyed or rendered miserable through false notions of modesty! And how many husbands and fathers are made to wring their hands in bitter anguish at the deep distraction inflicted upon their family circle by the reprehensible custom of employing old women as midwives! Let physicians think and write upon this subject! Let it be honestly and judiciously presented to the views and understandings of the fathers and mothers in our land! And if such cases must be put in charge of women—to which I yield my hearty assent—let public opinion and the laws of our State require of all such a proper study and due qualification for their important and responsible avocation. Let not *old age* be their only qualification, as at present; for however much we may venerate age and its multiplied experience in the ordinary occurrences of life, it alone can never fit one to practise a profession or science so intricate in its nature and so crowded with responsibilities.

Pattersonville, la., May 25th, 1847.

V.—*A Case of Schirrous Tumour removed from the neck of a lady, whilst in the Mesmeric state.* Communicated by W. R. GIST, M. D., of Jackson, Mississippi.

(Having long been acquainted with both the reporter of this case, and the consulting physician, and knowing them to be men of high respectability and unquestionable veracity, we cheerfully give it a place in our Journal. We have recently conversed with Dr. Langley, and he confirms every thing stated by Dr. Gist. F.—EDR.)

Subject.—Mrs. Matthews, aged 40 years, slender, rather delicate, but of ordinary health, nervo-phlegmatic temperament, and mother of eight children.

I had been consulted frequently during the last four years about a small tumour which she had, and which was located over the lower half of the parotid gland, on the left side and just behind the angle of the inferior maxilary bone. It was, when I first saw it, about the size of a hazelnut, but had increased very much within the last six months, and was, at the time of the operation, about the size of a nutmeg, but with a base much broader and very firm in its attachments. Dr. W. S. Langley and myself had long since pronounced it cancerous. It was now very painful, and in every respect seemed about to assume its malignant form. The ordinary remedies had been tried without any benefit; nor had we any hope that we could effect a cure by the use of medicines. We therefore advised exsection as the only possible remedy that could avail any thing; we also informed her husband, Col. Matthews, that even this was a doubtful remedy.

Mrs. M. consented to undergo the operation, but she had all the horror of the knife that a delicate female could have, and requested me to mesmerize her, informing me at the same time that she had been mesmerized some years ago by her brother. The following is the result.

June 25th, 1846, I visited Mrs. M., caused her to be seated in the parlour, and succeeded for the first time in putting her fully under the magnetic influence in about ten minutes. I let her remain so for about a half hour, and then *demagnetized* her. She now said she felt pleasantly, only a little drowsy and fatigued.

This was repeated on the 27th and 29th, and with the same success, every time being more easily put under its influence.

July 2nd. I met Dr. Langley by previous agreement; my patient now was much agitated, as she suspected that we were intending to operate, yet she was afraid to ask. I, however, soon succeeded in getting her to take a seat, and in five minutes had her *completely insensible.* Col. Matthews now asked in Gen. Clark and lady, Chancellor Cock, and Mr. G. Boddie, who were close by the house of Gen. Clark. In the meantime my student, Mr. Dismukes, brought in my instruments, and all was ready for the operation.

Dr. L. now examined the pulse and found it 80, respiration 15, skin soft, countenance placid and serene. I proceeded to the operation by grasping the tumour in my left hand, raising it, skin and all, as free from the muscles as I well could, and plunged a sharp pointed bistoury under the centre of the tumour, carrying the knife out below, cutting away the entire skin and cellular substance, together with a small portion of the muscular fibres. Dr. L. then seized the integument with a tenaculum, and I completed the operation by carrying the knife and removing the same attachments of the upper half. It bled freely; we consequently waited some time before we could satisfy ourselves that we had removed all of the diseased fibres. This being done, I closed it by the interrupted sature; making three stitches, and completed the operation by applying the ordinary dressing, occupying in all about fifteen minutes.

Dr. Langley now counted the pulse again, and found that it had not changed one beat in the minute; neither had the respiration changed, nor was there, during the whole operation, *the least sign of pain; not even the contraction of a muscle or change of countenance; all was placid, calm and serene.* Having removed the instruments, cleared away the bloody cloths, &c., I proceeded to arouse her. In a few minutes she was fully awake. When she first came to herself, I said some trivial thing on purpose to divert her mind from the wound, she laughed and seemed quite disposed to pass a joke with me. Her attention was now called to the tumour by one of the company who asked her "if her neck hurt her?" She seemed perfectly astonished, and asked me if I had performed the operation. I told her that I certainly had. She then, for the first time, remarked that "she *believed* her neck did smart and burn a little." *She said that she had not the slightest consciousness of pain* *or any sensation during the operation;* nor could she realize it until she saw the tumour itself. The wound healed kindly; much more so than we expected, and is now, *nearly twelve months since the operation*, entirely well. There is no sign of disease returning, either at the same or any other location.

This case is interesting on two accounts; the first is, it shows that the timely removal of a schirrous tumour does give a chance for a final cure; this is the only reason that the case has not been reported before. The other is, *that it is as clear a proof of the truth of animal magnetism as the mind can wish.* I can conceive nothing wanting, connected with the whole matter, that could have made it more satisfactory. The patient herself, a lady of the highest respectability, and wife of the Auditor of Public Accounts. The spectators, among whom were Gen. Clark, State Treasurer, and Chancellor Cock, and George Boddie, all looked on with anxiety, expecting every moment to see her jump up, *for she was not confined*, yet they were delighted to see such perfect success; and every one would swear to the truth of this statement if necessary.

Jackson, Mississippi, June 11th, 1847.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*A Practical Treatise on Inflammation, Ulceration, and Induration of the Neck of the Uterus: With Remarks on the value of Leucorrhœa and Prolapsus Uteri as symptoms of Uterine disease.* By JAMES HENRY BENNET, M. D. Licentiate of the Royal College of Physicians, &c., &c., &c. Philadelphia. Lea & Blanchard. 1846. pp. 146.

This interesting little volume is a re-publication of sundry papers on diseases of the uterus, which appeared in the London Lancet during the year 1846. They are now presented in a "more extended and complete form," and we think should be widely disseminated among physicians in all parts of the country. The lights of modern science in regard to uterine pathology, and the very great improvements in the treatment of uterine diseases, are as yet far from being generally known and practised by the medical profession. The French are entitled to the sole credit for these benefits, and their improvements date from the time the *speculum* was introdueed into practice, an instrument of incalculable value, and one that has had to contend against the powerful obstacles presented by *prejudice and false delicacy*. Such is the prevailing prejudice against this instrument, both in Great Britain and the United States, that it can hardly be said to be in common use in these countries, even at this day. By the leading physicians of the large cities it is frequently used, but chiefly among the middling and lower classes of women. If we are not mistaken, it is only resorted to, amongst ladies of the first respectability, in extraordinary cases, which seem to require extraordinary remedies. It is among this latter class that prejudice chiefly prevails, and in this instance it evidently suffers by it. Whilst we thus condemn the *false delicacy*, which often repudiates the best remedies that have been discovered for the alleviation of the numerous afflictions incident to the frail frame of woman, we would by no means withhold our admiration of her brightest charm—*true and genuine modesty*. Painful and revolting as it must be to the sensitive female, to be compelled to undergo the manipulations absolutely necessary for the treatment of her diseases, it is nevertheless *her duty to submit*; for the preservation of her health and life are paramount to all other considerations. The responsibility that devolves upon the physician in the management of these delicate cases is very great, and should be duly appreciated. Nor is there any thing better calculated to show the necessity and importance of a *pure and unblemished character* in the medical adviser. The relation between patient and physician is as *strictly confidential* as that of the holy confessional. By an imperious necessity we have to be intrusted with secrets involving not only the health, but the temporal happiness of our clients. How important then that we should conduct ourselves in a manner free from reproach; and also how important for the community to encourage the promotion of virtue and scientific attainment among medical men. The community may continue to patronize and encourage the host of mercenary quacks and charlatans that infest our cities, as much as it will, but the occasions will not fail to occur, nor will they be few, on which the physician of profound attainments and pure and exalted character will be required. Men of this stamp are particularly required in the treatment of the diseases peculiar to females. This little work of Dr. Bennet is well adapted for a *vade-mecum*, and should be in the hands of every practitioner. It is devoted to the consideration of the most common diseases of women, such as *inflammation, ulceration, and induration of the neck of the uterus*, and treats of them in the most lucid and practical manner. Seven years of study and service in the principal hospitals of Paris, with the aid of the most eminent instructors, has certainly afforded Dr. Bennet uncommon opportunities for obtaining information. A perusal of this little work will satisfy the reader that he has not neglected these opportunities.

As we are writing this little notice under the heats of August, amidst frequent interruptions and at the commencement of an epidemic of our city's great scourge, *Yellow Fever*, we are compelled to relinquish the task of attempting to give an analysis of Dr. Bennet's practical views. We must, therefore, content ourselves with giving only the table of contents. It consists of 7 chapters on the following topics, viz:—1. *Inflammation, ulceration, and induration of the cervix uteri in women who have not borne children.* 2. *Inflammation, ulceration, and induration of the cervix uteri in women who are pregnant, or have borne children.* 3. *Syphilitical ulcerations of the cervix uteri.* 4. *Cancerous ulceration of the cervix uteri.* 5, 6, *and* 7. *Treatment* of these different affections.

Such are the contents of this little work, and as it is so very cheap, (only 75 cents,) we hope it will obtain a wide circulation.

F.

II.—*Proceedings of the National Medical Convention, held in New York, May,* 1846, *and in Philadelphia, May,* 1847.

We have at length received the proceedings of the great National Medical Association. The business brought before the body during its Convention is of the highest interest to the profession throughout the United States; and we trust, that as almost every section of our widely extended country was ably represented in that body, all who claim an interest in, or belong to the profession, will do their utmost to carry out the views and suggestions of the Convention.

This is but the beginning of a great reform, we trust, which is to regenerate and elevate medicine in the estimation of civilized man.

As it is impossible, in a short notice to give any thing like an abstract of the doings and sayings of this Association, we must content ourselves with recommending the profession to read the proceedings carefully, and do all in its power to carry out the wishes of that body, in order that it may learn how to protect itself, and secure the esteem and confidence of the public.

We regard this Convention as destined, if continued in the proper spirit, to do more for the elevation of the profession than all the legislation in the country. Let us protect ourselves.

In another place may be found an able Report on Medical Ethics, which is all we can make room for in this number of our Journal.

A. H.

III.—*Medical Botany; or description of the more important plants used in Medicine, &c., &c.* By R. E. Griffith, M. D. Philadelphia. Lea & Blanchard. 1847. pp. 704.

Medical botany has hitherto been a neglected branch of medical science in the United States, and it may be said of the members of the profession generally in this country, that they are entirely ignorant even of the elementary principles of this department of their profession. It is true, that individuals in this country have done much to advance our knowledge respecting the indiginous medical botany, and have possessed accurate information respecting the science of medical botany in its most extended sense, but nothing like a general work on this subject has been produced here before. We have now arrived at a new era. In times past we had our Bigelows, Bartons, Rafinesques, &c., giving the profession works of great value, as containing correct information respecting our medicines of home growth; others were studying Foreign productions, and making important discoveries, as to the plants yielding certain medicinal substances; but we were still under awe of Foreign learning, and the profession was allowed to continue to draw its information on these branches from Foreign authors; and Foreign works were our only sources of authority. Times and things have now undergone a most important change. Works of great value on this branch of science are now emanating from our presses, and our authors will now begin to reflect back upon the old world new light in return for that which in early times we drew from them. Those who study medical botany hereafter in this country will have our own works to refer to as sources of information and authority. In place of Lindley's and Burnet's works on medical botany, we have now Dr. Griffith's excellent work; and Woodville's beautiful and excellent, but now antiquated "medical botany," will be exchanged for the admirable work of our accomplished countryman, Dr. Joseph Carson,* in which the learned author has con-

* This work is now in course of publication in Philadelphia. It is issued in parts, of which four will complete the work, at a moderate price.

densed information from every source and quarter, and the elegant colored illustrations of which surpass in excellence the most beautiful works of the kind produced in Europe.

Dr. Griffith's work is admirably calculated for the physician and student. He has adopted Lindley's modification of the Jussian classification, as the basis of his arrangement; and his descriptions are clear and full. To facilitate the acquirement of knowledge, our author has given a most excellent introduction to structural botany, and the principles of the science of classification; and has added a sufficiently full glossary of the terms used.

Taking the work as a whole, we have seen none which promises greater advantages to the profession, if its members will only avail themselves of its use, and devote a few of their leisure minutes each day to the study of its contents.

The profession generally have prejudices against medical botany for several reasons; in the first place, they dislike it in accordance with that general principle of human nature, which leads us to care but little or even dislike those things of which we are almost entirely ignorant; in the second place, they dislike it because they regard it as a branch of knowledge of difficult acquirement; and thirdly, on account of the opinion that it affords no absolute and practical results. An attentive perusal of the work now under notice will remove all these causes of dislike, and the physician or student will soon not only acquire a vast deal of interesting information respecting articles of daily use, but will have the satisfaction of viewing an interesting branch of his profession with a new light.

We most cordially recommend this volume to the profession, with the full conviction that if it were generally read it would give a decided impulse to knowledge and investigation in this department of learning in the United States.

W. M. C.

IV.—*Some Account of the Letheon: or Who is the Discoverer?* By Edward Warren. Boston. 1847.

This is one of those polemical pamphlets that have been published in Boston on the subject of the Letheon and its discoverer. The author, it seems is not a physician, and says he has done what he has "voluntarily, freely, and without *any pecuniary reward whatever.*" He has certainly espoused the cause of Dr. Morton, most zealously, and is unstinted in his censures upon all such as have ever expressed a doubt either as to the value of the Letheon or the exclusive merits of Dr. Morton as its discoverer. By way of rescuing from oblivion and securing to them the condemnation which he thinks they so richly merit, he has collected the names of all these worthies, (private practitioners, medical journalists and dentists,) and inserted them in his pamphlet; thus assigning them over to the *infamy* which he thinks will be awarded them by posterity. Most noble, generous and *disinterested* advocate! defender of the weak! protector of obscure merit! Happy the land that can boast of such a chivalrous and fearless defender of humble but daring genius! It is our misfortune to have fallen under the ban of this bold knight's displeasure, and to be numbered amongst the opponents of the Letheon. Our first impressions in regard to the Letheon are remembered and quoted against us to our eternal shame by this knight of the quill, whilst no notice is taken of what we subsequently said upon the subject. When this remedy was first announced to the profession by Dr. H. J. Bigelow, accompanied by a report of some surgical operations and its effects upon the human system, we expressed our doubts as to the *safety* of its use. Nor did we conceal our chagrin and astonishment that *a patented, secret nostrum* should have obtained the sanction and recommendation of the leading physicians of Boston. Subsequent experiments have tended greatly to enhance its reputation, but we have already been supplied with many instances in which its effects were *very deleterious*, and *some* in which it caused death. This is sufficient to show that our apprehensions were not altogether groundless. The Letheon has now obtained a wonderful repute throughout America and Europe, but we have yet to learn that the conduct of the leading physicians of Boston, in using *a secret remedy*, or that of the ostensible discoverers *in taking out a patent right*, have received the approbation of the medical profession. On the contrary, we think the course taken by both parties has been decidedly condemned, and this very pamphlet affords sufficient evidence that they heartily regret it themselves.

After one or two successful operations with the Letheon, whilst it was yet a secret, the Boston surgeons refused to proceed any farther with it without being informed what it was. This demand was then gratified and other operations were performed, though under the seal of secresy. This is what has been disapproved by the medical profession. As to the conduct of Morton and Jackson, it appears that after having made extraordinary exertions to monopolize the use of the Letheon—sent agents throughout the world to procure and sell *patents*—spent much money—quarreled with one another, and finally failed to accomplish their *selfish purposes*, one of them (Dr. Morton) is now very willing to relinquish all private interest in the article, and will be content with *the honour alone* of being considered *the sole discoverer* of so great a boon. His competitors, Drs. Jackson and Wells, deny him this, and hence the bitter quarrel amongst the trio. But the opportunity for acquiring high and honorable fame has been lost, and the aspirations of these gentlemen have been already sacrificed on the *altar of avarice.* We are sorry that a man of Dr. Jackson's standing should have suffered himself to be involved in this affair, and we particularly regret to see *such hard things* not only said, but apparently proven against him as is done in this pamphlet. We presume he has much to say in his own defence, but we do not feel sufficient interest in the quarrel to examine much farther into its merits. Dr. Morton's apologist and advocate has certainly made out a strong case in his behalf, and it really appears that he is entitled to the chief merit of bringing the Letheon into notice.

As for ourselves, although our first impressions were unfavourable to the Letheon, when it was brought to this city, we felt perfectly willing to see it have *a fair trial.* We have done for it what no one else here would have done—we have reported fully and impartially most of the capital operations which have been performed under its influence.

As we said in our last number, it is certainly *a wonderful agent*, but its *true value* has yet to be settled by experience. We wish, for the honor of our country, that it had been freely offered to the world, without any attempt at monopoly, and so doubtless do Drs. Jackson and Morton. Much more might be said on the subject, but we are compelled to desist.

F.

V.—*Observations on Anuerism, and its Treatment by Compression.* By O'BRYEN BELLINGHAM, M. D., Edin., Fellow of and Professor in the School of the Royal College of Surgeons in Ireland, &c. &c.—London. John Churchill, Princes-street, Soho. 1847. pp. 181.

The following extract from the preface of this interesting little work will afford a sufficient idea of its objects and scope.

"The following pages contain a sketch of the history of the treatment of aneurism by compression, from the rude attempts of its earlier advocates to its present improved state; accompanied by an abstract of every case that has been reported, in which compression has been hitherto used—at least of every case which the author has met in a rather extensive reading. The various instruments which have been employed for making pressure are described; and the theories upon which it has at different times been supposed to effect the cure of aneurism are noticed. The author has also endeavored to point out some of the advantages which compression, as a mode of treating aneurism, possesses over the ligature, when the position of the sac permits of its application; he has added some rules for the guidance of the surgeon in its application; and it has been all through his aim to refer every invention to its proper author, and every improvement, either in the theory or practice of this method of treating aneurism, to its legitimate source."

As we cannot go into an analysis, we will give the author's summary of some of the most material points bearing upon the method of treatment by compression.

"1. The arteries to which compression is applicable being far more frequently the subject of aneurism than those to which it is inapplicable, compression is calculated to supercede the ligature in the great majority of cases.

2. The cure of aneurism by compression upon the artery between the aneurismal sac and the heart, according to the rules laid down here, is accomplished by the gradual deposition of the fibrine of the blood in the sac, until both the latter and the artery at the part are completely filled. The process is in fact exactly similar to that by which nature effects a spontaneous cure of aneurism.

3. Such an amount of pressure as would cause inflammation and adhesion between the opposite sides of the artery at the point compressed is never required.

4. The pressure should not be so great as to interrupt the circulation in the artery at the point compressed; an essential agent in the cure being that a current of blood should pass through the sac.

5. Compression by means of two or more instruments, one of which is alternately relaxed, is much more effectual than by any single instrument, and in many instances the pressure can be maintained by the patient himself.

6. The treatment of aneurism by compression does not involve the slightest risk to the patient, and if persevered in cannot fail of effecting a cure.

7. A cure of aneurism effected by compression, according to the rules laid down here, must necessarily be permanent; and in every case in which a cure has been accomplished, the patients have remained well subsequently.

8. The femoral artery remains pervious after the cure at the point at which the pressure had been applied, and no morbid change of any kind is to be detected in either the artery or vein at the site of the compression.

9. When a cure is effected by compression, the vessel is obliterated only at the seat of the aneurism, and the artery at this part is eventually converted into an impervious ligamentous band.

10. Compression effects the cure of aneurism by more simple and safer means than the ligature, while it is applicable to a number of cases in which the operation is contra-indicated or inadmissible.

11. Compression is not necessarily a more tedious or more painful method of treating aneurism than the ligature, while it is much more certain, more likely to be permanent, and is free from all danger.

12. Compression, according to the rules laid down here, has little analogy with the old method which went by this name; and in fact has no greater resemblance to it than the Hunterian operation had to the operation for aneurism which it superceded."

F.

VI.—*The Half-Yearly Abstract of the Medical Sciences: Being a Practical and Analytical Digest of the contents of the principal British and Continental Medical Works, published in the preceding six months. Together with a series of Critical Reports on the Progress of Medicine and the Collateral Sciences during the same period.*—Edited by William H. Ranking, M. D., Contab, &c. No. 5. January—June, 1847. Philadelphia. Lindsay & Blackiston. 1847.

Just as we were closing our editorial notices, we had the pleasure of receiving this valuable work from the American Publishers, through the post-office. We have only to reiterate our decided approbation of the work, and our desire to exchange with the talented and indefatigable editor. We would thank the American Publisher, to let us know the best method of forwarding our Journal to Dr. Ranking. We should be glad to have our humble labours brought to his notice. As we have said before, Ranking's Abstract is such a valuable production, and the American Edition so very cheap, that it should be in the hands of every practising physician. We usually draw pretty largely from it, and shall avail ourselves of the present opportunity.

F.

Part Third.

EXCERPTA.

1.—*Memoir on the Yellow Fever, or Vomito Negro.* By Dr. Dn. Julio Jacinto Le-Riverend. Translated from the "Observador Habanero,"* by John Forbes Innerarity, M. D., Mobile, Ala.

Before treating of this terrible scourge, it has appeared to us more convenient to present to the reader a slight topographical sketch of this city and its suburbs, the data for which we have obtained from the "Memorias de la Real Louidad Economeia," edited by some friends of the country, from the statistics of the Island of Cuba, published in 1827, by a commission of officers of the Royal Corps of Engineers, and from Baron Humbolt's "Political Essay on the Island of Cuba."

This island, discovered by Cristopher Colon in the year 1492, was at first regarded by the Spanish government as of so little importance, that Dr. Nivolas de Ovando, the then Governor of Sante Domingo did not receive orders to send an expedition to explore it till the year 1508. And then notwithstanding the favorable report of Dr. Sebastian de Ocampo, in regard to the fertility of the soil, the number of the ports, and the pacific and benevolent character of the aborigines, nearly three years were allowed to elapse before Dr. Diego Velazquez was commissioned to occupy and colonize it. Being master of the country in 1514, he founded towns and villages in various parts, and lastly the city of the Habana in 1519.

Originally built without any plan, it is evident its founder was far from foreseeing that, from its geographical position, this city was destined to become, in the course of time, the centre of an immense commerce and the most precious jewel in the crown of Spain. It was at first regarded as a mere port of call for vessels returning to the Peninsula laden with the riches of New Spain and Costa Firme. Having been declared capital of the island in 1543, a regular plan of building, adapted to its future greatness, might then have been adopted, but the government did not occupy its attention with this important point, and allowed the colonists to continue heaping together buildings with as little order as good taste—as may be seen from the description we now submit to the reader.

Situate in 23° 9′ N. lat. towards the confines of the torid zone, the Habana contains, according to the census of 1842, 148,860 souls. Its figure is that of an irregular polygon of an elliptical form ; its greater diameter is 6,300 feet and its smaller 3,600. Built at the level of the sea, to the West of a vast port of 4,500 feet extension, and on a burning soil, it is bounded on the E. N. E. by a chain of low hills, on which are erected the fortifications intended for its defence, and which partly shut out the East wind, the most refreshing and healthy of

* A Spanish Medical Periodical published in the Habana, edited by the author of the Memoir.

those that prevail here. The streets, running E. and W. and N. and S., are so narrow that hardly can two carriages be driven abreast through them; they are not graded nor paved, and the excavations formed by the continuous passages of quitrines, waggons and carts, are filled up with stones so loose in texture as to crumble beneath the slightest pressure: for which reason the streets are filled with dust in the dry season, and with mud during the continuance of the rains.

There are in the Habana* 3,971 houses, the majority of which are low and deficient in space. The rooms situate in the rear of the houses are unhealthy from their proximity to the privies and stables, wherein are not unfrequently kept the rubbish and dirty water for several days. The same thing happens in the houses termed *accessary*, which are small, low-roofed and difficult of ventilation, and inhabited for the most part, by poor mechanics or wretched prostitutes, who struggle in their miserable apartments for a breath of hot air, vitiated by the emanations from their own bodies and from their excrements, of which they have no mode of ridding themselves, save by casting them at night, into the streets.

The water, which supplies the greater part of the inhabitants, is distributed by means of six fountains, proceeding from an artificial creek called *la zanja*—only the more wealthy class make use of rain water preserved in cisterns.—That portion of *la zanja* which passes through the suburbs de la Salud, receives during the wet season, the waters and filth of the streets which cross its course; the little care that is paid to its cleanliness allows also of a constant accumulation within it of vegetable debris at all seasons.

The transition from one season to another not being here so marked as in Europe, the year may be conveniently divided into two, the dry and the rainy seasons. During the first, which begins in November and terminates in May, the prevailing winds are the North and North-east, till towards the end of February when the hot South wind commences to blow with force and frequency. During this period the dawn, the evenings and the nights are cool and sometimes cold, but from 10 o'clock, A. M. till 4, P. M. the heat is great. These atmospheric changes, together with the particles of dust with which the air is surcharged, produce at this period affections of the eyes, throat, larynx, bronchi and lungs, intermittents, neuroses, and neuralgiæ, and cutaneous, muscular and articular inflammations.

During the season of heat and rains, which last for many years back, have occurred at a later period on account of the clearings (*desmontes*) of the Western part of the island, reigns particularly the hot South wind, which blowing across the land arrives at the city laden with the exhalations of the numerous marshes over which it passes in its course. At this time it rains almost daily after mid-day, and to it are well appliable the verses of Boileau:

> "On disait que le ciel qui se fond tout en eau,
> Veuille inonder ces lieux d'un deluge nouveau."

Indeed the water falls in torrents—one portion flows to the sea, and the rest stagnating in the streets for want of gradation, forms puddles which are rendered infectious: 1, by the action of the heat which at this season ranges at from 28° to 29° Reaumur, and which would be insupportable but for the prevailing breezes; 2. by the continuous passage of carriages which unceasingly renew its surface, and 3, by its admixture with dead animals, as dogs, cats and poultry which are cast into the streets.

To these causes of insalubrity may be added:

1. The existence in the interior of the city of a great number of stores for the sale of salted and smoked fish and flesh, as those of tasajo, bacalao, sardines, &c: these edibles, destined for the consumption of slaves and of the poorer

* Entre-muros, or the city proper.

classes, heaped together in low and damp situations, and heated by mutual contact, promptly enter into the putrefactive process, and unceasingly spread around emanations of an insupportable smell.

2. The ill construction of the wharf, built of planks supported by posts or stakes, under which the water is with difficulty renewed, which by its admixture with the excrements of the crews of vessels, moored to it during the time of unloading, acquires deleterious properties, the activity of which is augmented by the ardor of the sun's rays striking immediately upon the planks.

3. The condition of the fish market, a shed, near a stream which receives the filth of the streets in its immediate neighborhood, and into which is cast the refuse fish.

4. And lastly, the market square, known as that of Fernando VII, containing in a very small area very many stalls, on which are exposed for sale flesh, fish, and vegetables, of all which the offals abandoned to the rays of a burning sun, soon fall into putrefaction.

Before entering upon the examination of the direful influence of these numerous causes of infection, we propose to cast a rapid glance over the suburbs known by the titles of Guadalupe, San Lazaro, and Jesus-Maria, which contains a population of 87,519 souls. The first of these, built on higher ground than the Habana, is the healthiest of all. The houses of which it is composed are generally lofty and stand apart, from which it may be inferred that at the epoch of their construction, the exigencies of the climate had begun to be considered in the formation of dwellings. This quarter is much indebted for its beauty to the liberal munificence of Bishop Espada, who greatly labored to render it salubrious—it was he who caused to be drained the marsh which formerly occupied the site of what is now the Field of Mars.

The suburbs of San Lazaro is built, in the greater part, on low ground, and the water, finding no easy exit, transformed its narrow and tortuous streets into infectious muddy gutters. This quarter contains numerous holes and pools of considerable size in which is accumulated the rubbish of the city; besides many guano-thatched huts with floors generally below the level of the streets. Such are the dwellings of the majority of the inhabitants of this suburbs.

In the suburbs called of Jesus-Maria, we have already given a description in our* memoir on Intermittent Fevers.

Such was the condition of the Habana and of its suburbs at the time of our arrival in 1824, and such it continued with little difference till 1834, when General Tacon received the command of the island. Surprised beyond measure to see the numerous causes of insalubrity that the city contained, this chief labored incessantly during the entire period of his government as much to remove these pernicious influences as to embellish the city. He caused the streets to be graded and paved; and established in these, as well as in the houses, sewers to carry off dirty water. The paltry sheds of the fish-market and of those of the squares of Fernando VII and of the Vapor were replaced by spacious and well ventilated stone edifices. Each of these markets was provided with a fountain and its basin. The accumulation of rubbish in the interior or rear of private dwellings was remedied by a system of daily carting. His philanthropical cares embraced the suburbs, the streets of which were, in great part, graded, paved and provided with sewers. Two grand promenades were formed, and the holes and pools in the quarters of San Lazaro and Jesus-Maria were filled up. The number of houses being insufficient on account of the great increase of the population by immigration from Mexico, Costafirme, and Europe, he gave permission to build in these quarters spacious edifices, whose number daily increasing has since caused the disappearance, in part, of

* In a previous number of the "Observador Habanero."

that misshapen heap of wretched hovels, in which formerly vegetated numerous unfortunates, who struggling in them for a full breath of vitiated air, were continuously exposed to a multitude of grave maladies. In this manner were these insalubrious sites transformed into delightful residences.

While the Captain-general Tacon and the principal authorities generously rivalled each other in zeal to render more salubrious and more ornate the city and its suburbs, the Superintendant-general of Royal Revenue, the Conde de Villanneva, concluded the works of the aqueduct of Husillo which carries through the streets the filtered waters of the Almendares by a double line of iron pipes, from which branch smaller tubes to the interior of the dwellings.—This great enterprize has been productive of immense benefit to the city by the multiplication of public fountains, and by the facility which every citizen now enjoys of obtaining a plentiful supply of pure and wholesome water.

Climate.—The climate of the western part of the island is such as belongs to the northern limit of the torrid zone, and almost to the commencement of the tropic of cancer; in which space the constantly recurring inequalities of temperature announce the proximity of the temperate latitudes.

Mean Temperature.—The mean temperature of the Habana, on account of its vicinity to the sea is 25° Centigrade. The extremes being for the hottest months 28° C., and for the coldest 21° C.

Extreme Temperature.—The lowest point to which the mercury has descended in the Habana is 16° C., and the highest to which it has been observed to ascend 31° C.

Hurricanes, less frequent here than in St. Domingo and Jamaica, occur towards the end of August, in September, and even in October.

Of the Causes, Nature and Seat of Yellow Fever.—The improvements which have been successively introduced, within the last fifteen years, into the city and its suburbs have produced notable changes in the frequency and intensity of diseases in general. The numerous cases of pernicious fevers of every type, the adynamic and ataxic fevers which were formerly observed every year among the creoles and the acclimated, have diminished in a sensible degree.—Typhoid fever, phlegmonous erysipelas, and anthrax have become less frequent, as also has dysenteric colitis, endemic in September; but it is certain this beneficent influence has not been extended to yellow fever. Notwithstanding the improvements mentioned, the city still contains many foci of infection, as are principally, the stores of tasajo, the muddy stream of the fish-market, and lastly, the sewers which not having a sufficient current of water to cover the dirt accumulated in them, transform the places in which their gratings are placed into so many centres of contagion.

As we have already observed, there are in the Habana only two seasons, one of drought, and the other of rains. In the first, which begins in November, and ends in May, no case of yellow fever is ever seen.

During the first three months of this season, the prevailing winds are commonly from the E. N. E., and from the N., and rain is very rare. During the succeeding three months the heat gradually increases; the scorching S. wind is interrupted only in the morning and evening by light easterly breezes. The air respired at mid-day is suffocating; the digestive organs become more and more irritable under the influence of the increasing heat, and are extremely prone to take on inflammatory action; this is consequently the period of cerebral and gastro-intestinal inflammations and of eruptive fevers, as the first three months constitute that of affections of the respiratory organs. Lastly, the storms which during the two last months of this period are almost daily observed on the horizon, rendering the heat more intolerable as the atmosphere is more charged with electricity, approach nearer and nearer to the city. At last the rains set in, and with them appears the yellow fever, which seems to start forth from the ground amid the fœtid exhalations which the previously parched up soil, now saturated, gives out. Now only begins the epidemic; and its mortality is the greater directly as the humid heat is the more adapted to the production of gases from putrefaction. In the months of April and May, many cases occur of intense gastro-enteritis and gastro-encephalitis, which frequently assume the appearance of yellow fever, but which are never accompanied by that lesion of innervation which is the distinguishing characteristic of the disease with which we are now occupied. Heat, humidity and miasmata are the three conditions indispensably necessary to the development of yellow fever, whose attack upon strangers is violent in the direct ratio of the predisposing causes which exist in them. Of the individuals who come to the Habana, some are drawn hither by failure in business, and others by ill conduct, and some are driven by the want of the means of sub-sistence in their own country. These unfortunates embark filled with the hopes of here rapidly making their fortunes, (for still El Dorado is believed to exist in the colonies by the people of Europe.) When undeceived on their arrival, every branch of industry completely filled up, what must be the effect upon their *morale?* Reduced to undertake the most laborious and least lucrative employments for a mere subsistence, failing frequently even in these humble endeavors, and without the means of returning to their native countries, they soon become home-sick, (*nostaljicos*) and thus fall the victims of yellow fever.

Others, and these constitute the majority of our immigrants, arrive by hundreds every year from different provinces of Spain, to evade the conscript laws. These young men, accustomed to breathe a pure and invigorating air in their native land, and brought up to agricultural labors, obtaining on their arrival the offices of the lowest servants in the tasajo and provision stores, living in very circumscribed space, breathing a corrupted atmosphere, and discharging laborious duties, are quickly attacked by the disease which then puts on a formidable character.

Lastly, we shall consider the case of sailors. Every one is acquainted with their intemperate habits and their customary food. On arrival here the vessels are moored to the wharf during the discharge of their cargoes. The labors which they have to undergo to effect this, performed during the hottest hours of the day, the abuse of alcaholic drinks taken to quench thirst, the bad habit of sleeping on deck, and the rains which drench them while bathed in perspiration, predispose sailors to the invasion of yellow fever, the severity of which is increased by the negligence of the captains who often do not send the men to be treated till after some days have elapsed, and for the most part not without having previously well dosed them with castor oil and other purgatives. On board the vessels from Buenos Ayres, which come laden with tasajo, the disease generally proves very fatal on account of the virulent exhalations to which the men are continuously exposed.

Whatever may be the inlet by which miasmata producing specific diseases are introduced into the economy, it is certain their presence is not made sensible till they fix themselvss in the solids, no disturbance of the health taking place while they are circulating; but so soon as their action is directed upon the organs, the general perturbation and the development of those inflammatory signs by which the disease announces itself commence.

Before proceeding to the description of the symptoms, course, termination and curative plan of the yellow fever, we think it proper to observe that therapeutical means employed to combat what affection soever, if but little rational, will impress upon it a course different from what it would have pursued, and a character other than it would have exhibited, under the influence of a better indicated plan; hence the discrepancies in the descriptions of authors of the symptoms, march and duration of disease.

Empirical physicians, of which the number is great in the Habana, though more particularly to be found among the Anglo-American and English practitioners, who are imbued in the doctrines of Brown, administer liberally to their sick, emetics, cathartics, calomel, tonics, and principally sulphate of quina, remedies which delay not long in augmenting a slight attack of yellow fever to the highest grade of intensity, and in quickly producing death in consequence of the profound disturbance which they cause in the principal viscera. The symptoms of which they give us the history, are chiefly the fruit of their peculiar system of medication and the very rare cases of cures which they cite, prove merely that their patients have not always died under such treatment.

The initiatory, as well as the subsequent symptoms, vary according to the organs principally implicated. The disease sometimes commences with convulsions, frequently repeated, with pallor of the face, icy coldness and tremulous speech, indicating a violent congestion of the cerebro-spinal axis; at other times it strikes the patient suddenly into a state of general collapse; hardly is he able to support himself, and his head is disturbed by the slightest movement; lastly, and in the majority of cases, all the signs of a violent gastric inflammation are, without warning, developed and act powerfully upon the head and spine.

In each of these cases there is more or less severe lesion of innervation.—Hence the reason that the heart and muscles so early fall into the debility of the adynamic state, which in its turn gives rise to hemorrhages from the mucous membranes, so perilous at this period, and to partial gangrene.

History of the disease when its seat is more particularly in the stomach and duodenum.—Ordinarily, persons attacked by this disease, experience the first symptoms in the evening or during the night, and sometimes also in the morning. It commences suddenly with a violent headache, pain in the lumbar region and in the joints, particularly those of the lower extremities. The face is flushed, the eyes injected or bloodshot, the carotids pulsate with force, the lips are extremely reddened, the tongue of an uniform deep red color, sometimes contracted and dry, thirst is always intense. In some cases occur coryza, dryness of the fauces, the secretion of a thick saliva, difficult of expectoration, sensibility and augmented heat of the epigastric region. We observe also in the majority of cases, visible pulsations of the arteries of the stomach, short respiration, profound sighing, uneasiness, (desasosiego,) constant necessity for a change of posture, dry and hot skin, diminished secretion of urine, which is high colored, constipation, strong and full pulse, and integrity of the intellectual faculties.

When the disease has not been checked at the onset by remedies appropriate to this inflammatory state, to the symptoms already described are superadded others denoting the extension of the inflammation to the brain, nausea, ineffectual retchings, and more frequently mucose-bilious vomitings, repugnance to liquids which excite vomiting, fugacious delirium, somnolence or coma, increasing epigastric pain, meteorism of this region, highly colored urine, burning skin and frequent pulse.

Lastly, when the disease proceeds to a fatal termination, it is observed that the adynamico-ataxic stage sets in about the third or fourth day; the pulse becomes slower and smaller, the face is sunken, the eyes lose their brilliancy, the tongue shrinks and dries, vomiting is more frequent, sometimes hemorrhages from the nose and arms supervene, the skin grows cold, and the urine is suppressed; according to the seat of this sympathetic cerebral irritation, the delirium is low or furious, the respiration becomes more and more slow, each breath is more fœtid than the last, and finally hiccup, sinking, and black vomit commonly close this scene of destruction.

He must be destitute of all medical knowledge, unendowed with any faculty of induction, or bigotted to a bad faith, who cannot see in the picture we have drawn, the certain signs of an intense inflammation of the stomach. Attacking sanguineous subjects, the majority of whom are of athletic constitution, transported without transition from a cold to a burning and humid climate, and surrounded by an atmosphere charged with putrescent particles; the energy with which the heart acts, the rapid movements of the blood in the arteries whose pulsations are difficultly counted, the color of the face, and of the external openings of the mucous membranes, the vascularity of the eyes and the burning condition of the skin, does not this combination of circumstances indicate that all the organs of the economy are affected with a more or less powerful congestion? is it not evident that this state of violent erythism cannot long persist without causing the most profound lesions of the vital organs?—The collapse to which the economy succumbs on the second or third day of this exalted inflammatory state, the hemorrhages, the black-vomit, and the disturbance of the sensorial functions, so much the more marked as stimulants have been abused, announce dissolution as the effect of an inflammation which has not been combatted.

History of yellow fever, when its seat is principally in the nervous centres.—The invasion is as sudden in this as in the former case, the patient being, unawares, seized cold, and convulsive tremors; he suffers a general malaise (*malestar.*) his speech is interrupted, his respiration short and catching, the pulse hardly to be felt, and the face pallid. This state, indicating a profound lesion of innervation, has no determinate duration; this, after a shorter or longer period, is replaced by the following series of symptoms: pallor of the face which wears an expression of uneasiness, yellowish circle around the mouth, divergence of the eyes, or deviation from the parallelism of the axis of vision, (*ojos estraviados*,) supra-orbital pains, the tongue red or white, broad and moist, great or no thirst, little or no pain in the epigastrium, palpitation in the region of the cœliac axis, severe pains in the loins, and in the inferior extremities; patients occasionally complain of an obtuse pain in the precordial region, frequent impossibility of remaining for an instant in the same position, constipation, clear urine, passing horripilation, pulse strong and frequent, skin hot, greater alteration of the features, increase of uneasiness, and of the lumbar and articular pains, which extend to the low motor muscles following the tract of the nervous trunks, exasperation of the headache which some patients compare to the pain that would be occasioned by the stroke of a hammer on the cranium; tongue always moist, broad and red, vehement thirst, nausea, epigastric insensibility, wakefulness, small and frequent pulse, and the skin nearly of its natural temperature, finally death is ushered in by the following symptoms: augmented intensity of those already described, hemorrhage, disturbance of the intellect, the patient, although awake, labors to co-ordinate his ideas; the mucous vomitings of the commencement now become bilious, and vary in color, as blue, greenish or dusky; sometimes the sick vomit nothing but their drinks; sub-delirium, convulsions, sensations of extreme prostration, the pulse becomes momentarily smaller, the skin grows cold, and occasionally turns of a yellow color; finally, all the pains concentrate in one or two points, the arm and the thigh, and force screams from the sufferers: these parts being examined, no sign can be perceived that within a few hours they will fall into a gangrenous state and die.

Typhoid Form.—The disease commences in this case without premonitory symptoms by an attack of vertigo which, passing, leaves the following train of symptoms: purple color of the face, which is sometimes spotted, eyes vascular, intense supra-orbital pain, tongue dry and red, fœtid breath, constant thirst, sensibility of the epigastrium, with increase of heat in this region, pain on pressure in the ileo-cœcal region, borborygmus, fœtid stools, nausea, lumbar, and articular pains, high colored urine, hot and dry skin, full and slow pulse, and somnolency. *Increasing course.*—Agitation, increase of headache, features contracted, axis of vision not parallel, constant nausea, and vomiting of mucus and of variously colored bile, mixed not unfrequently with streaks of blood; tongue dry and red, increasing fœtor of the breath, intense thirst, meteorism, increase of pain on pressure in the epigastrium, and in the cœcal region; dry cough, stools more and more fœtid, skin hot, exhaling a sickening smell, dark urine, and frequent changes of the pulse. *Second period.*—Loquacious delirium, continuance of the vomiting which is sedimentous, breath insupportably fœtid, tongue covered with a dry and rough fur, convulsive movements of the lips, epistaxis, clear yellow color of the skin, meteorism, greater frequency of the alvine discharges and supine decubitus. *Third perid and termination in death.*—Somnolence, indifference or apathy on the part of the patient, renewal of epistaxis, which is more abundant, small pulse, dusky hue of the skin, involuntary alvine discharges, faintings from the slightest movements, meteorism, increased fur on the tongue and gums, which are prone to hemorrhage, ecchymosis on different parts of the body, and most frequently on the sacrum, and finally death, ushered in by gangrene.

These three are the forms under which this disease makes its appearance.—In all, it is worthy of note, that the gastro-intestinal mucous membrane is the seat of the observed phenomena, and that the nervous system is profoundly implicated. This disturbance of innervation is that which constitutes the chief danger of the disease. Death occurs the more promptly and fearfully in the direct ratio of the use of energetic stimulants. What plan of treament soever be adopted, it is not generally followed by such happy results as in other pathological conditions. All physicians, following very different methods of cure, count many unfortunate cases, but those who rely on an antiphlogistic and antiseptic treatment, if very often they find this plan unavailing to achieve a triumph over the disease, have still the consolation to see their patients pass away with tranquility, instead of suffering the convulsive agonies which precede the death of those who have been stimulated.

In making the above classification we do not pretend to assert that the same series of events occurs in each case. Our object has been to call attention to the organ or system most profoundly affected: whatever may be the predominance of the disease over one or more organs during its course, the scene always opens with an attack on the nervous system. The instantaneous pains of the head, of the loins, and of the articulations invariably precede the febrile movement. This inclines us to the belief that the virus or unknown cause of the disease, does not act through the circulation, but intermediately through the nerves.

II. The yellow fever called by Sauvage, Tryteaphya Americana; by Cullen, febris flava, typhus icterodes, and by Pinel, fievre gastro-adynamique, is a disease peculiar to the new world, which has existed in all ages on the Eastern coast of South America and in the Antilles. History furnishes incontestible proofs that it was not imported from the coast of Africa by vessels engaged in the slave trade, as is asserted by M. Andouard, as we recognize in the description of the sickness which attacked a portion of the troops composing the first expedition against the island of Cuba, the distinctive characters of the yellow fever such as we observe at the present day. This terrible scourge, like other morbid affections, is not an entity; we never, indeed, see it reproduced with the identical series of symptoms: besides the modifications impressed upon it by age, sex, temperament, idrosyncracy, habits of life, moral impressions, especially fear of contracting it, and the pathological condition of the viscera, particularly of the heart and stomach, previously to the invasion; it is powerfully affected by the constitution of the atmosphere, which tends to alter its aspect so materially as to have caused it to be termed a truly protean disease. Indeed, if the squalls which form daily on the horizon during the summer, do not soon bring rain to the city, the atmosphere becomes surcharged with electricity, no breeze agitates the stagnant air, the heat then reaches a suffocating degree of intensity from 10 forenoon till 5 o'clock, evening, causing the expanded fluids to exert a continuous force upon their containing vessels. This condition of the atmosphere annoys and fatigues the creole as well as the stranger, producing in every one an invincible repugnance to the slightest exertion. Every disease developed during this atmospheric constitution, but particularly the yellow fever, takes on the hemorrhagic form from the invasion: frequently these hemorrhages cannot be restrained, and the patients are then rapidly plunged into a state of prostration from which only with great difficulty can they be raised.

When, on the other hand, the summer rains are abundant, the hot and humid air facilitates the disengagement of miasmata with which it is loaded and the disease shews itself less openly inflammatory, is accompanied by mucous and bilious discharges and quickly assumes the adynamico-ataxic type. Lastly, if the gases, evolved by the process of decomposition of vegetable and animal matters be very active, persons submitted to their influence, are attacked at the commencement with convulsions and coma, and die frequently without febrile reaction having been excited. We have had the misfortune to lose several patients in this manner, and we have in every case, ascertained that these individuals recently arrived, have resided near the openings of sewers, and have been in the habit of leaving open, during the night, the windows of their diminutive dormitories. In all these cases the signs of more or less profound disturbance of innervation have been met with.

The slight typographical sketch given in the first part of this memoir, obliges us to recognise three occasional causes of yellow fever, heat—moisture and miasmata, especially such as proceed from the decomposition of animal and vegetable matters. These mixed gases are those which, introduced by imbibition into the economy of unacclimated individuals, produce irritative congestions and visceral phlegmasiæ, rapid in their course and accompanied by more or less profound disturbance of innervation, tending to quickly extinguish life.

The predisposing causes are, non-acclimatation, sanguineous temperament, abuse of the pleasures of the table, of alcoholic drinks, of coffee and of venery, muscular exertion during the great heat of the day, insolation, residence near the foci of infection, (almost all the springs of the sewers are such, dispersing around at all hours, but principally at night, so pestilent a stench that the passers by are compelled to hold in their breath.) walking at too late an hour in the evening or too soon in the morning, periods when the miasmata, carried to the superior strata of the air by solar heat, again descend dissolved in a chilling mist.

Though we profess ourself untrammeled with any favorite system, yielding only to our convictions, and bearing in mind the saying of Mirabeau that "there is nothing more easy than to deceive one's self," we must express our opinion that, from the careful study of the facts we have observed during the space of twenty years, the following corollaries can be deduced.

1. The yellow fever is a specific disease of an inflammatory nature; it is one of the numerous modifications of inflammation.

2. Whatever be the system primarily attacked by the septic gases, the inflammatory character of the disease is always evident; and this is the more so in proportion as the attack in the digestive organs takes place at an early period.

3. The yellow fever is not developed except through the agency of heat, moisture and miasmata.

4. The greater or less intensity of these three causes or the predominance of one of them, impresses on the disease its pecular character of malignity or of benignity and gives rise to various epi-phenomena.

5. Like all inflammations produced by septic gases, it runs quickly through its various stages and tends to rapid disorganization. Introduced by imbibition and circulating with the fluids, the gases proceeding from the decomposition of animal and vegetable matters, more active consequently than others, do not manifest their presence except by their effects upon the solids, and these alone give intimation of the infection. We are aware that this explanation of the action of these agents is opposed to the opinion of many physicians of the school of M. Magendie, who attribute the yellow fever and typhus to the altera-

tion of the fluids by the action of these gases mixing with them in the circulation. Broussais opposes to these ideas, which belong to the humoral pathology the fact that, in ten persons exposed to these septic gases, at most only three or four contract the disease, whilst the remainder experienced not the least inconvenience, although the whole have absorbed the same gases which have equally mixed with the fluids and circulated in the vessels of each. This shews that in some cases the poison is tranquilly eliminated by the depurative organs while in others it excites the nervous and sanguineous systems to the point of causing those disturbances constituting the disease peculiar to these gases. According to these data it is not possible to regard the yellow fever as a disease primarily humoral.

We have no doubt that our colleagues, as well as ourself, have frequently met with individuals who, placed in the most favorable circumstances for the developement of the yellow fever, have not contracted it till their second or even their third year of residence. During this period they had certainly absorbed gases which by its mixture with their fluids ought to have affected their health, yet they have continued well till the time when these gases, not being eliminated by the excretory organs, have acted on the solids and have produced the disease, which has not been less severe on account of the delay in its attack. If the blood drawn during the inflammatory stage of the yellow fever be examined, it will be found to differ in no degree perceptible to the senses from that taken in diseases not of miasmatic origin; but it is otherwise in the adynamic stage. Inflammations generally as well internal as external, have a tendency to produce alteration in the fluids and to cause them to run into decomposition; the yellow fever cannot form an exception to this law, and therefore the alteration of the blood and its loss of fibrin, which is observed only in the adynamic stage, can be but consecutive to the alteration of the solids.

Let us now enquire where is the seat of the disease. In almost every case of sickness observed during the hot season within the tropics (whether or not produced by specific causes) the digestive organs are positively affected: this occurs in at least nine-tenths of the cases. These organs are the most exposed to be affected by the direct impression of noxious agents; they suffer most from humid heat; they have the most extensive sympathies, and are most susceptible to be impressed by irritations affecting other parts. These sympathetic irritations are quickly changed into idiopathic inflammation.

The group of symptoms observed as well in the inflammatory as in the adynamic stage, prove that the disease has its seat, in the majority of cases, in the gastro-intestinal mucous membrane, and particularly in its gastric, duodenal and cæcal portions, and that the nervous centres are secondarily and sometimes primarily affected. In these last cases the patients are struck down, as it were, by a flash of lightning, and often die without febrile reaction; or if this be established, the cerebro-rachidian phenomena predominate over those of gastro-enteritis and augment the gravity of the disease.

The course of this disease, whose features we have endeavored to delineate, may be divided into two distinct stages, the inflammatory and the adynamico-ataxic. The duration of the first is from two to three days, which passing, the patient experiences a kind of remission and a feeling of apparent comfort, which seems to announce convalescence, but is frequently only the precursor of death, particularly if at this period the pulse be slow and the lumbar and articular pains disappear leaving a sensation of extreme debility refered especially to the epigastric region. We have seen many, who at this time believing themselves cured, have left their beds, instantly seized with vertigo, followed by black-vomit and death.

The mean duration of the second or adynamico-ataxic stage is from six to seven days; the termination in death ordinarily occurs on the seventh, eighth or ninth day.

(*To be continued.*)

II.—A CODE OF MEDICAL ETHICS.

[From the proceedings of the National Medical Association.]

The Committee appointed under the sixth resolution adopted by the Convention which assembled in New York, in May last, to prepare a Code of Medical Ethics for the government of the medical profession of the United States, respectfully submit the following Code.*

JOHN BELL,
ISAAC HAYS,
G. EMERSON,
W. W. MORRIS,
T. C. DUNN,
A. CLARK,
R. D. ARNOLD, } *Committee.*

Philadelphia, June 5th, 1847.

CHAPTER I.

OF THE DUTIES OF PHYSICIANS TO THEIR PATIENTS AND OF THE OBLIGATIONS OF PATIENTS TO THEIR PHYSICIANS.

ART. 1.—*Duties of Physicians to their Patients.*

§ 1. A physician should not only be ever ready to obey the calls of the sick, but his mind ought also to be imbued with the greatness of his mission, and the responsibility he habitually incurs in its discharge. Those obligations are the more deep and enduring, because there is no tribunal other than his own conscience, to adjudge penalties for carelessness or neglect. Physicians should, therefore, minister to the sick with due impressions of the importance of their office; reflecting that the ease, the health, and the lives of those committed to their charge, depend on their skill, attention and fidelity. They should study, also, in their deportment, so to unite *tenderness* with *firmness*, and *condescension* with *authority*, as to inspire the minds of their patients with gratitude, respect and confidence.

§ 2. Every case committed to the charge of a physician should be treated with attention, steadiness and humanity. Reasonable indulgence should be granted to the mental imbecility and caprices of the sick. Secrecy and delicacy, when required by peculiar circumstances, should be strictly observed; and the familiar and confidential intercourse to which physicians are admitted in their professional visits, should be used with discretion, and with the most scrupulous regard to fidelity and honor. The obligation of secrecy extends beyond the period of professional services;—none of the privacies of personal and domestic life, no infirmity of disposition or flaw of character observed during professional attendance, should ever be divulged by him except when he is

* Dr. Hays, on presenting this report, stated that justice required some explanatory remarks should accompany it. The members of the Convention, he observed, would not fail to recognize in parts of it, expressions with which they were familiar. On examining a great number of codes of ethics adopted by different societies in the United States, it was found that they were all based on that by Dr. Percival, and that the phrases of this writer were preserved, to a considerable extent, in all of them. Believing that language which had been so often examined and adopted, must possess the greatest of merits for such a document as the present, clearness and precision, and having no ambition for the honors of authorship, the Committee which prepared this code have followed a similar course, and have carefully preserved the words of Percival wherever they convey the precepts it is wished to inculcate.—A few of the sections are in the words of the late Dr. Rush, and one or two sentences are from other writers. But in all cases, wherever it was thought that the language could be made more explicit by changing a word, or even a part of a sentence, this has been unhesitatingly done; and thus there are but few sections which have not undergone some modification; while, for the language of many, and for the arrangement of the whole, the Committee must be held exclusively responsible.

imperatively required to do so. The force and necessity of this obligation are indeed so great, that professional men have, under certain circumstances, been protected in their observance of secrecy, by courts of justice.

§ 3. Frequent visits to the sick are in general requisite, since they enable the physician to arrive at a more perfect knowledge of the disease,—to meet promptly every change which may occur, and also tend to preserve the confidence of the patient. But unnecessary visits are to be avoided, as they give useless anxiety to the patient, tend to diminish the authority of the physician, and render him liable to be suspected of interested motives.

§ 4. A physician should not be forward to make gloomy prognostications, because they savour of empiricism, by magnifying the importance of his services in the treatment or cure of the disease. But he should not fail, on proper occasions, to give to the friends of the patient timely notice of danger, when it really occurs, and even to the patient himself, if absolutely necessary. This office, however, is so peculiarly alarming when executed by him, that it ought to be declined whenever it can be assigned to any other person of sufficient judgment and delicacy. For, the physician should be the minister of hope and comfort to the sick; that, by such cordials to the drooping spirit, he may smoothe the bed of death, revive expiring life, and counteract the depressing influence of those maladies which often disturb the tranquility of the most resigned, in their last moments. The life of a sick person can be shortened not only by the acts, but also by the words or manner of a physician It is, therefore, a sacred duty to guard himself carefully in this respect, and to avoid all things which have a tendency to discourage the patient and to depress his spirits.

§ 5. A physician ought not to abandon a patient because the case is deemed incurable; for his attendance may continue to be highly useful to the patient, and comforting to the relatives around him, even in the last period of a fatal malady, by alleviating pain and other symptoms, and by soothing mental anguish. To decline attendance, under such circumstances, would be sacrificing to fanciful delicacy and mistaken liberality, that moral duty, which is independent of, and far superior to all pecuniary consideration.

§ 6. Consultations should be promoted in difficult or protracted cases, as they give rise to confidence, energy, and more enlarged views in practice.

§ 7. The opportunity which a physician not unfrequently enjoys of promoting and strengthening the good resolutions of his patients, suffering under the consequences of vicious conduct, ought never to be neglected. His counsels, or even remonstrances, will give satisfaction, not offence, if they be proffered with politeness, and evince a genuine love of virtue, accompanied by a sincere interest in the welfare of the person to whom they are addressed.

Art. II.—*Obligations of Patients to their Physicians.*

§ 1. The members of the medical profession, upon whom are enjoined the performance of so many important and arduous duties towards the community, and who are required to make so many sacrifices of comfort, ease, and health, for the welfare of those who avail themselves of their services, certainly have a right to expect and require, that their patients should entertain a just sense of the duties which they owe to their medical attendants.

§ 2. The first duty of a patient is, to select as his medical adviser one who has received a regular professional education. In no trade or occupation, do mankind rely on the skill of an untaught artist; and in medicine, confessedly the most difficult and intricate of the sciences, the world ought not to suppose that knowledge is intuitive.

§ 3. Patients should prefer a physician, whose habits of life are regular, and who is not devoted to company, pleasure, or to any pursuit incompatible with his professional obligations. A patient should, also, confide the care of himself and family, as much as possible, to one physician, for a medical man who has become acquainted with the peculiarities of constitution, habits, and predispositions, of those he attends, is more likely to be successful in his treatment, than one who does not possess that knowledge.

A patient who has thus selected his physician, should always apply for advice in what may appear to him trivial cases, for the most fatal results often supervene on the slightest accidents. It is of still more importance that he should apply for assistance in the forming stage of violent diseases; it is to a neglect of this precept that medicine owes much of the uncertainty and imperfection with which it has been reproached.

§ 4. Patients should faithfully and unreservedly communicate to their physician the supposed cause of their disease. This is the more important, as many diseases of a mental origin simulate those depending on external causes, and yet are only to be cured by ministering to the mind diseased. A patient should never be afraid of thus making his physician his friend and adviser; he should always bear in mind that a medical man is under the strongest obligations of secrecy. Even the female sex should never allow feelings of shame or delicacy to prevent their disclosing the seat, symptoms and causes of complaints peculiar to them. However commendable a modest reserve may be in the common occurrences of life, its strict observance in medicine is often attended with the most serious consequences, and a patient may sink under a painful and loathsome disease, which might have been readily prevented had timely intimation been given to the physician.

§ 5. A patient should never weary his physician with a tedious detail of events or matters not appertaining to his disease. Even as relates to his actual symptoms, he will convey much more real information by giving clear answers to interrogatories, than by the most minute account of his own framing. Neither should he obtrude the details of his business nor the history of his family concerns.

§ 6. The obedience of a patient to the prescriptions of his physician should be prompt and implicit. He should never permit his own crude opinions as to their fitness, to influence his attention to them. A failure in one particular may render an otherwise judicious treatment dangerous, and even fatal. This remark is equally applicable to diet, drink, and exercise. As patients become convalescent they are very apt to suppose that the rules prescribed for them may be disregarded, and the consequence but too often, is a relapse. Patients should never allow themselves to be persuaded to take any medicine whatever, that may be recommended to them by the self-constituted doctors and doctresses, who are so frequently met with, and who pretend to possess infallible remedies for the cure of every disease. However simple some of their prescriptions may appear to be, it often happens that they are productive of much mischief, and in all cases they are injurious, by contravening the plan of treatment adopted by the physician.

§ 7. A patient should, if possible, avoid even the *friendly visits of a physician* who is not attending him,—and when he does receive them, he should never converse on the subject of his disease, as an observation may be made, without any intention of interference, which may destroy his confidence in the course he is pursuing, and induce him to neglect the directions prescribed to him. A patient should never send for a consulting physician without the express consent of his own medical attendant. It is of great importance that physicians should act in concert; for, although their modes of treatment may be attended with equal success when employed singly, yet conjointly they are very likely to be productive of disastrous results.

§ 8. When a patient wishes to dismiss his physician, justice and common courtesy require that he should declare his reasons for so doing.

§ 9. Patients should always, when practicable, send for their physician in the morning, before his usual hour of going out; for, by being early aware of the visits he has to pay during the day, the physician is able to apportion his time in such a manner as to prevent an interference of engagements. Patients should also avoid calling on their medical adviser unnecessarily during the hours devoted to meals or sleep. They should always be in readiness to receive the visits of their physician, as the detention of a few minutes is often of serious inconvenience to him.

§ 10. A patient should, after his recovery, entertain a just and enduring sense of the value of the services rendered him by his physician; for these are of such a character, that no mere pecuniary acknowledgement can repay or cancel them.

CHAPTER II.

OF THE DUTIES OF PHYSICIANS TO EACH OTHER, AND TO THE PROFESSION AT LARGE.

ART. I.—*Duties for the support of professional character.*

§ 1. Every individual, on entering the profession, as he becomes thereby entitled to all its privileges and immunities, incurs an obligation to exert his best abilities to maintain its dignity and honor, to exalt its standing, and to extend the bounds of its usefulness. He should therefore observe strictly, such laws as are instituted for the government of its members;—should avoid all contumelious and sarcastic remarks relative to the faculty, as a body; and while, by unwearied diligence, he resorts to every honorable means of enriching the science, he should entertain a due respect for his seniors, who have, by their labors, brought it to the elevated condition in which he finds it.

§ 2. There is no profession, from the members of which greater purity of character, and a higher standard of moral excellence are required, than the medical; and to attain such eminence, is a duty every physician owes alike to his profession, and to his patients. It is due to the latter, as without it he cannot command their respect and confidence, and to both, because no scientific attainments can compensate for the want of correct moral principles. It is also incumbent upon the faculty to be temperate in all things, for the practice of physic requires the unremitting exercise of a clear and vigorous understanding; and, on emergencies for which no professional man should be unprepared, a steady hand, an acute eye, and an unclouded head may be essential to the well-being, and even to the life, of a fellow creature.

§ 3. It is derogatory to the dignity of the profession, to resort to public advertisements or private cards or handbills, inviting the attention of individuals affected with particular diseases—publicly offering advice and medicine to the poor gratis, or promising radical cures; or to publish cases and operations in the daily prints or suffer such publications to be made;—to invite laymen to be present at operations,—to boast of cures or remedies,—to adduce certificates of skill and success, or to perform any other similar acts. These are the ordinary practices of empirics, and are highly reprehensible in a regular physician.

§ 4. Equally derogatory to professional character is it, for a physician to hold a patent for any surgical instrument, or medicine; or to dispense a secret *nostrum*, whether it be the composition or exclusive property of himself, or of others. For, if such nostrum be of real efficacy, any concealment regarding it is inconsistent with beneficence and professional liberality; and, if mystery alone give it value and importance, such craft implies either disgraceful ignorance, or fraudulent avarice. It is also reprehensible for physicians to give certificates attesting the efficacy of patent or secret medicines, or in any way to promote the use of them.

ART. II.—*Professional services of physicians to each other.*

§ 1. All practitioners of medicine, their wives, and their children while under the paternal care, are entitled to the gratuitous services of any one or more of the faculty residing near them, whose assistance may be desired. A physician afflicted with disease is usually an incompetent judge of his own case; and the natural anxiety and solicitude which he experiences at the sickness of a wife, a child, or any one who by the ties of consanguinity is rendered peculiarly dear to him, tend to obscure his judgment, and produce timidity and irresolution in his practice. Under such circumstances, medical men are peculiarly dependent upon each other, and kind offices and professional aid should always be cheerfully and gratuitously afforded. Visits ought not, however, to be obtruded officiously; as such unasked civility may give rise to embarrassment, or interfere with that choice, on which confidence depends. But if a distant member of the faculty, whose circumstances are affluent, request attendance, and an honorarium be offered, it should not be declined; for no pecuniary obligation ought to be imposed, which the party receiving it would wish not to incur.

ART. III.—*Of the duties of physicians as respects vicarious offices.*

§ 1. The affairs of life, the pursuit of health, and the various accidents and contingencies to which a medical man is peculiarly exposed, sometimes require him temporarily to withdraw from his duties to his patients, and to request some of his professional brethren to officiate for him. Compliance with this request is an act of courtesy, which should always be performed with the utmost consideration for the interest and character of the family physician, and when exercised for a short period, all the pecuniary obligations for such service should be awarded to him. But if a member of the profession neglect his business in quest of pleasure and amusement, he cannot be considered as entitled to the advantages of the frequent and long-continued exercise of this fraternal courtesy, without awarding to the physician who officiates the fees arising from the discharge of his professional duties.

In obstetrical and important surgical cases, which give rise to unusual fatigue, anxiety and responsibility, it is just that the fees accruing therefrom should be awarded to the physician who officiates.

ART. IV.—*Of the duties of physicians in regard to Consultations.*

§ 1. A regular medical education furnishes the only presumptive evidence of professional abilities and acquirements, and ought to be the only acknowledged right of an individual to the exercise and honors of his profession.—Nevertheless, as in consultations the good of the patient is the sole object in view, and this is often dependent on personal confidence, no intelligent regular practitioner, who has a license to practice from some medical board of known and acknowledged respectability, recognized by his association, and who is in good moral and professional standing in the place in which he resides, should be fastidiously excluded from fellowship, or his aid refused in consultation when it is requested by the patient. But no one can be considered as a regular practitioner, or a fit associate in consultation, whose practice is based on an exclusive dogma, to the rejection of the accumulated experience of the profession, and of the aids actually furnished by anatomy, physiology, pathology, and organic chemistry.

§ 2. In consultations no rivalship or jealousy should be indulged; candour, probity, and all due respect should be exercised towards the physician having charge of the case.

§ 3. In consultations the attending physician should be the first to propose the necessary questions to the sick; after which the consulting physician should have the opportunity to make such farther inquiries of the patient as may be necessary to satisfy him of the true character of the case. Both physicians should then retire to a private place for deliberation; and the one first in attendance should communicate the directions agreed upon to the patient or his friends, as well as any opinions which it may be thought proper to express. But no statement or discussion of it should take place before the patient or his friends, except in the presence of all the faculty attending, and by their common consent; and no *opinions* or *prognostications* should be delivered, which are not the result of previous deliberation and concurrence.

§ 4. In consultations, the physician in attendance should deliver his opinion first; and when there are several consulting, they should deliver their opinions in the order in which they have been called in. No decision, however, should restrain the attending physician from making such variations in the mode of treatment, as any subsequent unexpected change in the character of the case may demand. But such variation and the reasons for it ought to be carefully detailed at the next meeting in consultation. The same privilege belongs also to the consulting physician if he is sent for in an emergency, when the regular attendant is out of the way, and similar explanations must be made by him, at the next consultation.

§ 5. The utmost punctuality should be observed in the visits of physicians when they are to hold consultation together, and this is generally practicable, for society has been considerate enough to allow the plea of a professional engagement to take precedence of all others, and to be an ample reason for the relinquishment of any present occupation. But as professional engagements may sometimes interfere, and delay one of the parties, the physician who first arrives should wait for his associate a reasonable period, after which the consultation should be considered as postponed to a new appointment. If it be the attending physician who is present, he will of course see the patient and prescribe ; but if it be the consulting one, he should retire, except in case of emergency, or when he has been called from a considerable distance, in which latter case he may examine the patient, and give his opinion in *writing* and *under seal*, to be delivered to his associate.

§ 6. In consultations, theoretical discussions should be avoided, as occasioning perplexity and loss of time. For there may be much diversity of opinion concerning speculative points, with perfect agreement in those modes of practice which are founded, not on hypothesis, but on experience and observation.

§ 7. All discussions in consultation should be held as secret and confidential. Neither by words nor manner should any of the parties to a consultation assert or insinuate, that any part of the treatment pursued did not receive his assent. The responsibility must be equally divided between the medical attendants,—they must equally share the credit of success as well as the blame of failure.

§ 8. Should any irreconcilable diversity of opinion occur when several physicians are called upon to consult together, the opinion of the majority should be considered as decisive; but if the numbers be equal on each side, then the decision should rest with the attending physician. It may, moreover, sometimes happen, that two physicians cannot agree in their views of the nature of a case, and the treatment to be pursued. This is a circumstance much to be deplored, and should always be avoided, if possible, by mutual concessions, as far as they can be justified by a conscientious regard for the dictates of judgment. But in the event of its occurrence, a third physician should, if practicable, be called to act as umpire, and if circumstances prevent the adoption of this course, it must be left to the patient to select the physician in whom he is most willing to confide. But as every physician relies upon the rectitude of his judgment, he should, when left in the minority, politely and consistently retire from any further deliberation in the consultation, or participation in the management of the case.

§ 9. As circumstances sometimes occur to render a *special consultation* desirable, when the continued attendance of two physicians might be objectionable to the patient, the member of the faculty whose assistance is required in such cases, should sedulously guard against all future unsolicited attendance. As such consultations require an extraordinary portion both of time and attention, at least a double honorarium may be reasonably expected.

§ 10. A physician who is called upon to consult, should observe the most honorable and scrupulous regard for the character and standing of the practitioner in attendance : the practice of the latter, if necessary, should be justified as far as it can be, consistently with a conscientious regard for truth, and no hint or insinuation should be thrown out, which could impair the confidence reposed in him, or affect his reputation. The consulting physician should also carefully refrain from any of those extraordinary attentions or assiduities, which are too often practised by the dishonest for the base purpose of gaining applause, or ingratiating themselves into the favour of families and individuals.

Art. V.—*Duties of physicians in cases of interference.*

§ 1. Medicine is a liberal profession, and those admitted into its ranks should found their expectations of practice upon the extent of their qualifications, not on intrigue or artifice.

§ 2. A physician, in his intercourse with a patient under the care of another practitioner, should observe the strictest caution and reserve. No meddling inquiries should be made; no disingenuous hints given relative to the nature and treatment of his disorder; nor any course of conduct pursued that may directly or indirectly tend to diminish the trust reposed in the physician employed.

§ 3. The same circumspection and reserve should be observed, when, from motives of business or friendship, a physician is prompted to visit an individual who is under the direction of another practitioner. Indeed, such visits should be avoided, except under peculiar circumstances, and when they are made, no particular inquiries should be instituted relative to the nature of the disease, or the remedies employed, but the topics of conversation should be as foreign to the case as circumstances will admit.

§ 4. A physician ought not to take charge of, or prescribe for a patient who has recently been under the care of another member of the faculty in the same illness, except in cases of sudden emergency, or in consultation with the physician previously in attendance, or when the latter has relinquished the case or been regularly notified that his services are no longer desired. Under such circumstances no unjust and illiberal insinuations should be thrown out in relation to the conduct or practice previously pursued, which should be justified as far as candour, and regard for truth and probity will permit; for it often happens, that patients become dissatisfied when they do not experience immediate relief, and, as many diseases are naturally protracted, the want of success, in the first stage of treatment, affords no evidence of a lack of professional knowledge and skill.

§ 5. When a physician is called to an urgent case, because the family attendant is not at hand, he ought, unless his assistance in consultation be desired, to resign the care of the patient to the latter immediately on his arrival.

§ 6. It often happens, in cases of sudden illness, or of recent accidents and injuries, owing to the alarm and anxiety of friends, that a number of physicians are simultaneously sent for. Under these circumstances courtesy should assign the patient to the first who arrives, who should select from those present, any additional assistance that he may deem necessary. In all such cases, however, the practitioner who officiates, should request the family physician, if there be one, to be called, and, unless his further attendance be requested, should resign the case to the latter on his arrival.

§ 7. When a physician is called to the patient of another practitioner, in consequence of the sickness or absence of the latter, he ought, on the return or recovery of the regular attendant, and with the consent of the patient, to surrender the case.

§ 8. A physician, when visiting a sick person in the country, may be desired to see a neighbouring patient who is under the regular direction of another physician, in consequence of some sudden change or aggravation of symptoms. The conduct to be pursued on such an occasion is to give advice adapted to present circumstances; to interfere no father than is absolutely necessary with the general plan of treatment; to assume no future direction, unless it be expressly desired; and, in this last case, to request an immediate consultation with the practitioner previously employed.

§ 9. A wealthy physician should not give advice *gratis* to the affluent; because his doing so is an injury to his professional brethren. The office of a physician can never be supported as an exclusively beneficent one; and it is defrauding, in some degree, the common funds for its support, when fees are dispensed with, which might justly be claimed.

§ 10. When a physician who has been engaged to attend a case of midwifery is absent, and another is sent for, if delivery is accomplished during the attendance of the latter, he is entitled to the fee, but should resign the patient to the practitioner first engaged.

Art. VI.—*Of differences between Physicians.*

§ 1. Diversity of opinion, and opposition of interest, may, in the medical, as in other professions, sometimes occasion controversy and even contention. Whenever such cases unfortunately occur, and cannot be immediately terminated, they should be referred to the arbitration of a sufficient number of physicians, or a *court-medical.*

As peculiar reserve must be maintained by physicians towards the public, in regard to professional matters, and as there exist numerous points in medical ethics and etiquette through which the feelings of medical men may be painfully assailed in their intercourse with each other, and which cannot be understood or appreciated by general society, neither the subject matter of such differences nor the adjudication of the arbitrators should be made public, as publicity in a case of this nature may be personally injurious to the individuals concerned, and can hardly fail to bring discredit on the faculty.

Art. VII.—*Of Pecuniary Acknowledgements.*

§ 1. Some general rules should be adopted by the faculty, in every town or district, relative to *pecuniary acknowledgments* from their patients; and it should be deemed a point of honour to adhere to these rules with as much uniformity as varying circumstances will admit.

CHAPTER III.

OF THE DUTIES OF THE PROFESSION TO THE PUBLIC, AND OF THE OBLIGATIONS OF THE PUBLIC TO THE PROFESSION.

Art. I.—*Duties of the profession to the public.*

§ 1. As good citizens, it is the duty of physicians to be ever vigilant for the welfare of the community, and to bear their part in sustaining its institutions and burdens: they should also be ever ready to give counsel to the public in relation to matters especially appertaining to their profession, as on subjects of medical police, public hygiène, and legal medicine. It is their province to enlighten the public in regard to quarantine regulations,—the location, arrangement, and dietaries of hospitals, asylums, schools, prisons, and similar institutions,—in relation to the medical police of towns, as drainage, ventilation, &c.,—and in regard to measures for the prevention of epidemic and contagious diseases; and when pestilence prevails, it is their duty to face the danger, and to continue their labors for the alleviation of the suffering, even at the jeopardy of their own lives.

§ 2. Medical men should also be always ready, when called on by the legally constituted authorities, to enlighten coroners' inquests and courts of justice, on subjects strictly medical,—such as involve questions relating to sanity, legitimacy, murder by poisons or other violent means, and in regard to the various other subjects embraced in the science of medical jurisprudence.—But in these cases, and especially where they are required to make a post-mortem examination, it is just, in consequence of the time, labor and skill required, and the responsibility and risk they incur, that the public should award them a proper honorarium.

§ 3. There is no profession, by the members of which, eleemosynary services are more liberally dispensed, than the medical, but justice requires that some limits should be placed to the performance of such good offices. Poverty, professional brotherhood, and certain public duties referred to in section 1 of this chapter, should always be recognized as presenting valid claims for gratuitous services; but neither institutions endowed by the public or by rich individuals, societies for mutual benefit, for the insurance of lives or for analogous purposes, nor any profession or occupation, can be admitted to possess such privilege. Nor can it be justly expected of physicians to furnish certificates of inability to serve on juries, to perform militia duty, or to testify to the state of health of persons wishing to insure their lives, obtain pensions, or the like, without a pecuniary acknowledgment. But to individuals in indigent circumstances, such professional services should always be cheerfully and freely accorded.

§ 4. It is the duty of physicians, who are frequent witnesses of the enormities committed by quackery, and the injury to health and even destruction of life caused by the use of quack medicines, to enlighten the public on these subjects, to expose the injuries sustained by the unwary from the devices and pretensions of artful empirics and impostors. Physicians ought to use all the influence which they may possess, as professors in Colleges of Pharmacy, and by exercising their option in regard to the shops to which their prescriptions shall be sent, to discourage druggists and apothecaries from vending quack or secret medicines, or from being in any way engaged in their manufacture and sale.

Art. II.—*Obligations of the public to physicians.*

§ 1. The benefits accruing to the public directly and indirectly from the active and unwearied beneficence of the profession, are so numerous and important, that physicians are justly entitled to the utmost consideration and respect from the community. The public ought likewise to entertain a just appreciation of medical qualifications;—to make a proper discrimination between true science and the assumptions of ignorance and empiricism,—to afford every encouragement and facility for the acquisition of medical education,—and no longer to allow the statute books to exhibit the anomaly of exacting knowledge from physicians, under liability to heavy penalties, and of making them obnoxious to punishment for resorting to the only means of obtaining it.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*Report on the present state of knowledge respecting the Surgical, Medicinal, and Obstetrical application of the Vapour of Ether.* By the Editor. (*Ranking's Half-Yearly Abstract.*)

Dr. Ranking deems the introduction of this new remedy into practice, and the various experiments that have been performed with it of sufficient importance to deserve a *special report.* He has, therefore, drawn up one which will be read with interest. As we cannot make room for the whole of it, we will only give the following extracts. He begins thus:—

"The introduction of a new remedy, or a new means of obviating the many undesirable events contingent upon the practice of medicine and surgery, is always regarded in a different light by different members of the profession.—There are some among us, on the one hand, who, contented to move along in the mental "jog-trot" to which they have been long accustomed, look with suspicion or dislike on any innovation upon the ancient opinions with which they have enfolded themselves. These are the men who ridiculed and opposed the introduction of the stethoscope, and who will continue to ridicule and oppose anything else which they had not "dreamt of in their philosophy," and which either threatens to interfere with the usual routine of their thoughts, or necessitates a greater amount of intellectual application than they are capable of devoting to it. There is, on the other hand, another equally mischievous, perhaps, but far more interesting class of practitioners, whose imagination is apt to lead them to expect something great of every chimera which a busy age is continually forcing upon the attention. These men take up mesmerism, homœopathy, and such-like vagaries; become positive as to the curability of incurable diseases; and alternately blow hot and cold upon every medicine with an impossible name, which is ushered into notice by the inventive genius or needy exchequer of the practical pharmacopolist. Between the priggish contempt of novelties exhibited by one class of medical practitioners, and the injudicious favouritism of the other, it is seldom that any new suggestion for the benefit of mankind meets with that dispassionate judgment which the public has a right to expect from those who constitute themselves the guardians of its health; and it is generally not until the lapse of a considerable time that its merits or demerits can be ascertained with anything like certainty.

"The introduction of the inhalation of ether, for the purpose of annihilating pain in surgical operations, and of depriving even the dreaded process of parturition of its pangs, has not been exempted from the ordinary fate of novel propositions in medicine, although it must be allowed that the objectors to the value of this agent form the minority of those who have been led to reflect upon its applicability. Still there have not been wanting, in every locality, some over-cautious or over-timid persons, who are haunted with the idea of the danger which must attach to means so extraordinary, and who look upon an ether inhaler as almost a synonym for apoplexy or asphyxia. These individuals, however, as we have said, are few in number, and their opinions are, therefore, of little consequence as respects the estimation of the agent; the real danger to which it is exposed arises from the precipitate encomiums of its friends, and the reckless manner in which it appears to be made use of, without reference to, and by persons utterly incapable of judging of, the normal or diseased physical peculiarities of the patient.

"In the following report it will be our endeavor, as far as present experience of its effects will allow, to place the inhalation of ether as an anodynic and medicinal agent in a just light; laying before the reader an analytical digest of those communications upon the subject which appear to us to be most worthy of confidence."

He continues his report under the following heads:—1, *Historical Notices;* 2, *Preparation, Application, Physiological Action, &c.;* 3, *Its Application to Surgery;* 4, *to Practical Medicine;* and 5, *to Obstetrical Practice.* It seems that the inhalation of sulphuric ether was used many years ago, as is shown by literary references, but Dr. R. does not think this "as in any degree derogating from the honor which is in common justice to be accorded to the American gentlemen, who at the close of the last year, brought this subject prominently before the medical world." In regard to the comparative claims to the discovery set up by Jackson and Morton, he thinks it "evident that the *idea* was Dr. Jackson's; the merit of *applying* that idea belongs to Dr. Mortin."

As our readers have seen so much about the application of ether to surgery and general practice, we shall confine ourselves to what he says under his last head, viz:

§ V.—*Application of Ehter Vapour to Obstetrical Practice.*

"Many circumstances conspire to render the application of ethereal vapour to the purpose of annihilating the pangs of childbirth the most interesting feature in the history of the discovery; but at the same time, from the number of points to be taken into consideration, in a process so complex as that of parturition, it is, of all the applications of the agent, that concerning which it is the most difficult to arrive at satisfactory conclusions. We have not to determine merely whether the inhalation of ether is capable of suppressing the pain which accompanies parturition, but we have to take note also of other conditions which complicate the problem. The action of ether upon the fœtus, and upon the general economy of the mother; whether the insensibility which paralyzed the voluntary muscles would not likewise abolish the contractility of the uterus and abdominal muscles—were all so many novel questions which it was necessary to elucidate. It required, we think, no small courage to take the first step in solving a problem so important; a problem in which to err would not have been to commit a mere physiological blunder, but, in point of fact, to sacrifice the two lives, the preservation of which was confided to our care. It would be premature at the present time, to make any decided observations as to the general applicability of ether to midwifery; but if it should appear, on further examination, that it is a safe proceeding in that important department of medicine, to Professor Simpson, as the first to make the experiment, the profession and the public will be not less indebted than to the original discoverers of the process itself.

"The communications respecting the application of ether vapour to midwifery, which, in point of number, rank next to those devoted to its use in surgery, consist, for the most part, in the narration of individual cases. We shall recapitulate these as nearly as possible in the rotation in which they appeared.

"24. The first to be noticed is a pamphlet by Professor Simpson*, containing the subjoined cases, which we slightly abridge.

"The first is that of a female in her second confinement, whose pelvis was so contracted as to have rendered craniotomy necessary in her previous labor. Contrary to the advice of her medical attendant, she did not make him aware of her pregnancy until nearly her full time, so that it was too late to have recourse to the induction of premature labor. The labor-pains commenced on the 19th, and in the evening Dr. Simpson caused her to inhale the ether vapour. As she afterwards informed him, she almost immediately came under its influence; but as her insensibility was doubtful, it was continued for twenty minutes before turning was commenced. The operation was performed, and a full-sized still-born child was extracted.

"On questioning the patient after her delivery, she declared that she was quite unconscious of pain during the whole period of turning and extracting the infant, and only became fully aware of her situation by hearing the noise caused by preparing a bath for the restoration of the child; she quickly regained her full consciousness, and talked with gratitude and wonderment of her delivery and her insensibility to the pains of it. On the fifth day after delivery she was dressed, and her convalescence was rapid and uninterrupted.

"The next two are forceps cases. One was brought into the Royal Maternity Hospital, in strong labor, early on the morning of the 3rd of February. It was her second confinement. At her first accouchement (seven years before) she had been delivered by instruments, in Ireland, and had been informed by the attendant practitioner that artificial delivery would be similarly required at her future labors. Dr. Simpson saw her between ten and eleven o'clock, A. M. The os uteri was well dilated, the membranes ruptured, and the pains extremely strong and frequent; but the large head of the child seemed not to enter fully into the brim, and was little affected by the powerful uterine contractions under which the patient was suffering. By three o'clock her pulse had risen to above 125 beats a minute, and it appeared to the medical officers present that it would be improper to allow the ineffectual and exhausting efforts of the patient to be longer continued. She was then, at the request of Dr. Simpson, brought under the influence of ether. Dr. Moir, with great skill, applied the long forceps upon the head of the child. He subsequently was obliged to use strong traction during the pains that followed, and becoming temporarily fatigued with his efforts, Dr. Simpson supplied his place. After the head fully passed the brim, the forceps were laid aside, and one or two uterine contractions finished the delivery. The child was large and strong, and cried vigorously soon after it was expelled. During the whole of this severe operation the patient appeared quiet and passive. The cries of her child speedily roused her from her etherized state, and she subsequently assured Dr. Moir that she had felt comparatively little or no pain during the whole operation and delivery.

"The other case was seen by Dr. Simpson, in consultation with Dr. Graham Weir. The patient was advanced in life, and it was her first confinement.—The waters had escaped early, and the anterior lip of the uterus had subsequently been forced down in a very swelled and œdematous state before the head of the infant. After this obstruction was overcome, the child's head speedily descended upon the floor of the pelvis; but it was there impeded in its further progress by the narrow transverse diameter of the outlet. Under the compression of the converging tuberosities of the ischia, the bones of the fœtal

* Notes on the Inhalation of Sulphuric Ether in the Practice of Midwifery.

cranium soon began to overlap; but, at last, no further progress being made, the patient becoming exhausted by a continous labor of about twenty-four hours, and the soft parts being evidently well relaxed and prepared, Dr. Weir applied the short forceps, and extracted a living infant. For a considerable time before this operation was adopted, Dr. Simpson exhibited the vapour of ether to the patient; under it she speedily became quite narcotized. Its action was kept up, and the pains appeared to be so strong as almost to warrant the idea that nature would yet be sufficient; but ultimately instrumental delivery was had recourse to. The mother did not fully recover from her state of etherization for ten or fifteen minutes after delivery, and then stated that she was quite unaware of what had occurred.

"The foregoing cases, Dr. Simpson observes, point out one important fact, viz: that the uterine contractions in all continued as regular in their occurrence and duration after the state of etherization had been induced as before it; as yet, he states, that he has seen no case in which the pains have been diminished either in frequency or energy. In one case the combination of ether and tincture of ergot appeared to increase the strength of the uterine contractions.

"As might be expected, from our experience of its effects in surgery, the inhalation of ether acts differently on different women. In some a total insensibility is induced, others appear to writhe under the uterine contractions, but when restored to consciousness, have no recollection of suffering; others again remain conscious of what is passing around them, and watch the return of the pains, but seem indifferent to their effects. Dr. Simpson relates two cases illustrative of the above difference. In one the lady knew all that was said around her, but felt nothing amounting to pain, and only expressed her regret for the unnecessary suffering she had endured in former confinements. The other female became excited, and refused to continue the inhalation, but in a few minutes recommenced; she, however, was equally insensible to the birth of her child.

"Dr. Simpson makes the following remarks at the close of his interesting paper:

"A careful collection of cautious and accurate observations will no doubt be required, before the inhalation of sulphuric ether is adopted to any great extent in the practice of midwifery. It will be necessary to ascertain its precise effects, both upon the action of the uterus, and of the assistant abdominal muscles; its influence, if any, upon the child; whether it gives a tendency to hemorrhage or other complications; the contra-indications peculiar to its use; the most certain modes of exhibiting it; the length of time it may be employed, &c. In no case have I observed any harm whatever to either mother or infant follow upon its employment. And, on the other hand, I have the strongest assurance and conviction that I have already seen no small amount of maternal suffering and agony saved by its application. The cases I have detailed sufficiently show its value and safety in cases of operative midwifery. And here, as in surgery, its utility is certainly not confined to the mere suspension and abrogation of conscious pain, great as, by itself, such a boon would doubtless be. But in modifying and obliterating the state of conscious pain, the nervous *shock* otherwise liable to be produced by such pain,—particularly whenever it is extreme, and intensely waited for and endured,—is saved to the constitution, and thus an escape gained from many evil consequences that are too apt to follow in its train. Granting that experience will yet be able to prove its safety and efficacy in modifying and annulling the pains of labor, will (I have repeatedly heard the question asked) the state of etherization ever come to be generally employed with the simple object of assuaging the pains of *natural* parturition? Or (as the problem has not unfrequently been put to me) would we be 'justified' in using it for such a purpose? In conclusion, let us consider this point for a moment.

"Custom and prejudice, and, perhaps, the idea of its inevitable necessity, make both the profession and our patients look upon the amount and intensity of pain encountered in common cases of natural labor as far less worthy of consideration than in reality it is. Viewed apart, and in an isolated light, the degree of actual pain usually endured during common labor is as great, if not greater, than that attendant upon most surgical operations. I allude particularly to the excessive pain and anguish which, in nine out of ten cases, accompany the passage of the child's head through the outlet of the pelvis and external parts. Speaking of common or natural labor in its last stages, Dr. Merriman observes, the pulse gradually 'increases in quickness and force; the skin grows hot; the face becomes intensely red; drops of sweat stand upon the forehead; and a perspiration, sometimes profuse, breaks out all over the body; frequently violent tremblings accompany the last pain, and at the moment that the head passes into the world, the extremity of suffering seems to be beyond endurance.' Or, take the picture of the suffering of the mother in the last stage of natural labor, as portrayed by the most faithful of living observers, Professor Naegele, of Heidelberg: 'The pains,' he observes, 'of this stage are still more severe, painful, and enduring; return after a short interval, and take a far greater effect upon the patient than those of the previous stage. Their severity increases so much the more from the additional suffering arising from the continually increasing distension of the external parts. They convulse the whole frame, and have hence been called the *dolores conquassantes*. The bearing down becomes more continued, and there is not unfrequently vomiting. The patient quivers and trembles all over. Her face is flushed, and with the rest of the body is bathed in perspiration. Her looks are staring and wild; the features alter so much that they can scarcely be recognized. Her impatience rises to its maximum with loud crying and wailing, and frequently expressions which, even with sensible, high-principled women, border close upon insanity. Everything denotes the violent manner in which both body and mind are affected.'

"I have stated that the question which I have been repeatedly asked is this—will we ever be 'justified' in using the vapour of ether to assuage the pains of natural labor? Now, if experience betimes goes fully to prove to us the safety with which ether may, under proper precautions and management, be employed in the course of parturition, then, looking to the facts of the case, and considering the actual amount of pain usually endured (as shown in the descriptions of Merriman, Naegele, and others,) I believe that the question will require to be quite changed in its character. For, instead of determining in relation to it whether we shall be 'justified' in using this agent under the circumstances named, it will become, on the other hand, necessary to determine whether on any grounds, moral or medical, a professional man could deem himself 'justified' in withholding, and *not* using any such safe means (as we at present presuppose this to be,) provided he had the power by it of assuaging the pangs and anguish of the last stage of natural labor, and thus counteracting what Velpeau describes as 'those piercing cries, that agitation so lively, those excessive efforts, those inexpressible agonies, and those pains apparently intolerable,' which accompany the termination of natural parturition in the human mother."

"Having thus given the opinions of Professor Simpson upon this interesting question, we shall next mention the experience and remarks of M. Dubois upon the same point. At a meeting of the French Academy of Medicine, February 25th, M. Dubois stated that his attention had been directed towards the use of ether vapour in midwifery, and that he had more particularly endeavored to arrive at definite conclusions on the following points: 1st. Whether by the aid of ether certain obstacles to parturition can be overcome. 2d. Whether it extinguishes the pains of labor. 3d. Whether its use is innocuous to the mother and child. 4th. Whether it would not, in extinguishing pain, also abolish the uterine contraction. 5th. Whether the consequences were innocent.

"In elucidation of these questions, he related two forceps cases, in which the child was extracted without the least consciousness on the part of the mother; and some other cases of natural labor, the results of which rendered it manifest that the whole process of parturition could be safely conducted under the use of ether vapour, and that neither the uterine nor abdominal contractions were in the least diminished by it. M. Dubois further states that in some of his cases, though the head passed precipitately there was not the slightest laceration of the perineum, and that in none of his patients was there any complaint of the after effects of the ether.

"M. Dubois concludes by expressing his opinion that he could not at present recommend the common application of ether in natural labor, a conclusion which MM. Velpeau and Malgaigne did not think justified by the favorable experience of its effects which he had detailed.*

"M. Bouvier reported a case to the Academy, in which he considered that the inhalation of ether suspended the contraction of the uterus, because the pains subsided from the period of its exhibition, and recurred some time after the restoration of sensibility; but, as M. Roux observed in reference to this case, there is no evidence to prove that the subsidence of the pains was not spontaneous, and would have occurred if ether had not been employed.†

"At the same meeting of the Academy, M. Chailly (Honoré) related the following case. A lady, æt. 43, since the date of her former confinement had suffered from such extreme sensitiveness of the vagina, that intercourse could not be effected without extreme pain. For the same reason when M. Chailly arrived, an examination could not be made without eliciting cries of agony.—When insensibility had been induced by inhalation, an examination was made, and the cause of the delay being found to be impaction of the head, the forceps were applied, and the labor quickly terminated. The woman cried out as the head passed the ostium vaginæ, but declared that she had not suffered in the least degree.‡

"The next case is one reported by Mr. Latham, in which the ether was exhibited by Dr. Lloyd. 'The woman had been in labor for three hours; the os uteri was fully dilated, when inhalation was commenced. In about four or five minutes the patient was insensible, but the uterine contraction continued with regularity, though, as was thought, with some abatement of force. The membranes were now ruptured; the child's head gradually descended into the pelvis. Consciousness having returned, the patient said that she had felt no pain, but had been in a comfortable sleep, and dreamt that her child was born. She now began to feel the pains recur with their usual severity, and earnestly entreated us to give her the ether again; she even seized the instrument from Dr. Lloyd's hands and applied it to her mouth herself. After three minutes inhalation unconsciousness was again established, and though the inhaler was occasionally removed, she was kept under the Lethean influence for about ten minutes; after which time the apparatus was not again applied. The child gradually descended, the head pressing upon the perineum, which became perfectly relaxed, and the head and shoulders were expelled by one strong, continuous uterine effort, in eight minutes from the withdrawal of the inhalation. During the pains I applied my hand frequently to the abdomen, and found that the muscles were much more lax during than previously to the administration of ether. A few minutes after the expulsion of the fœtus a considerable gush of blood came from the uterus, which was now felt to be large and relaxed, but soon contracted again under the firm and steady pressure of the hands; the placenta quickly followed, and a broad bandage was tightly applied. (It must, however, be stated that this patient had suffered considerable hemorrhage after the expulsion of the fœtus in her previous labor.) On questioning her subsequently, she stated that she had felt no pain with the exception of the last, and that she would strongly recommend any one to take the ether under similar circumstances, expresing her gratitude to us for having

* Bull. des Acad.; Nouvelle Encyclogr., Mars 1847; Archives Générale, &c.
† Archives Générales, April 1847.
‡ Archives Générales, April 1847.

applied it. No inconveniences have since followed, and the patient is convalescing as favorably as possible. It is of course impossible, from an isolated case, to draw any just conclusions as to the inadmissibility of this agent in parturition. This case, however, so far as it goes, is satisfactory; and if we can by any means alleviate the first curse, which has for so many ages been incidental to the parturient state, we shall confer the greatest boon on those who are deserving of all our sympathies, and worthy of our best energies.'*

"Mr. Lansdown, of Bristol, has published the three following cases:

"'Mrs. W———, æt. 36, was taken in labor of her fourth child. On the evening of the 8th inst., the pains subsided, until the evening of the 10th, when an arm was found presenting. All attempts to turn being prevented by the extreme rigidity and narrowness of the vagina, ether was administered, and the patient became insensible for two or three minutes, during which time a foot was seized and brought down. With returning consciousness the vaginal contraction reappeared; ether was again administered; but the rigidity of the uterus, which continued during the insensibility, prevented the completion of the labor. Fearing to continue the inhalation, it was abandoned from this time, and the case was allowed to terminate in the usual way.'†

"The next cases are more decidedly successful.

"'Mrs. O———, in labor of her sixth child. Presentation natural, labor progressing favourably, but with exceedingly sharp pains. The ether was exhibited by Mr. Lansdown at the approach of every pain, during which she remained insensible. The contractions of the uterus were powerful; but although she apparently exerted herself during their persistence, she declared that she was perfectly unconscious of them. The head descended precipitately, but the perineum being fully relaxed, no injury occurred; the uterus contracted immediately after the placenta was expelled. The inhalation was continued in this case during three hours. The patient had no after-pains, which had been very severe in her former labors.

"'Mrs. T———, æt. 35, in labor with her eighth child. Ether commenced when the head was in the vagina; three strong pains immediately followed, the last completing the expulsion of the child; the placenta was removed, and the woman remained dozing. In about five minutes she awoke, and began to regret the absence of her pains, being, as she said, convinced that she would have another tedious night unless they came on. When told that her child was born, she scarcely believed it, and affirmed that she had felt nothing but had been in a pleasant dream. This patient likewise had no unpleasant symptoms.'‡

"Three cases of the successful employment of ether in midwifery, are also placed on record by Dr. Protheroe Smith.

"The first case appears to have been a tedious labor with a first child, in a female æt. 40. The ether was exhibited at intervals during a period of nearly four hours, and although the patient to all appearance was sensible to every pain, she distinctly affirmed that she was unconscious of those which occurred during ethereal insensibility, while those which happened in the intervals gave her the usual amount of suffering. It became necessary to apply the short forceps, from the impaction of the head, which was done during a period of insensibility, and a living child was extracted. On regaining her senses, she continually expressed a hope that the child would soon be born. When informed of the termination of her troubles, she burst into an hysterical laugh, exclaiming, 'It is a dream, it must be a dream! what a good thing it is that I had the ether,' &c. Both mother and child did well in every respect.

"The second case was that of a female, æt. 33, in labor with her seventh child. In this instance there were also impacted head from narrow oblique pelvis, and the child was delivered with the long forceps. The effect of the ether in the first instance was imperfect, as the patient did not inhale steadily,

* Med. Times, March 27. † Lancet, April 24. ‡ Lancet, June 5, 1847.

but towards the close complete insensibility was induced, and the labor was terminated without the slightest consciousness on the part of the patient. The after sufferings of this patient are described as less than in previous confinements.

"The third and last case was also a forceps case, and in this complete unconsciousness to pain was the result of the inhalation. Dr. Smith states, that in this instance the effect of the ether was to materially increase the strength of the uterine and abdominal contractions. In this case also the woman uttered the usual cries of the last stage of labor, but positively denied having been aware of its termination.

"The narrator terminates the narration of the above cases by an acknowledgment of the truth of the deductions of M. Dubois, viz:

"1st. That ether prevents pains during obstetrical operations.

"2d. That it does not suspend uterine or abdominal contractions.

"3d. That it appears to lessen the natural resistance of the perineal muscles.

"4th. That it does not appear to exert any bad influence on the life or health of mother or child.

"5th. That it does not retard the subsequent contraction of the uterus.

"The author adds, that he considers it probable that the temporary suspension of the pains, which has been observed in some instances to follow the use of ether, may be caused by the novelty of the means used, and is no more than may be produced by other emotions, as that which is so commonly known to follow the first appearance of the accoucheur.*

"Having thus given a brief account of the several cases in which, up to the present time (end of May,) ether has been employed in practical midwifery, we have in the last place, to mention two or three other communications, which must be considered more or less opposed to the obstetrical use of this agent.

"In a paper published in the 'Lancet' (March 27,) Dr. Tyler Smith investigates the action of ether in connection with the physiology of parturition. He examines the various parts which are taken respectively in that process, by *sensation*, *volition*, and *emotion*, and from the effects of ether upon these functions of the nervous system endeavors to determine its applicability in midwifery. That the pain of parturition may be obliterated by ether the author at once admits, but it does not appear to him so clear that the 'shock' of labor is necessarily diminished, as, in his opinion 'shock' is manifested not in the cerebral system only, but in the true spinal and ganglionic systems. This he demonstrates by the experiment of crushing the leg of a decapitated frog. Upon these considerations, he maintains that the 'shock' of parturition may ensue even though the cerebral system be insensible to pain. This, if true, will, of course, considerably contract the limits of the usefulness of ether; but, if, as we believe this is the case, 'shock,' though not necessarily, is generally attendant upon excessive pain; the annihilation of this pain will go far to diminish the chances of 'shock.' And, moreover, it will not be doubted that another cause of 'shock,' namely, mental depression, will be greatly controlled by the knowledge on the part of the woman that an agent is at her command which has the power of removing that agony, the dread of which is the basis of her despondency.

"Dr. Smith, to our surprise, we confess, also advances the cases supposed to have been fatal from ether as a part of his arguments against inhalation in midwifery. We have already shown that there are no logical grounds for believing that death in those cases was the result of the ether; and it is, therefore, needless to repeat our opinions on that matter. So also with respect to the fluidity of the blood, which Dr. Smith subsequently refers to.†

"Dr. Radford has published a communication, in which he more strongly condemns the use of ether in midwifery, though upon different grounds. His main objection is founded upon the fact that in instrumental deliveries more

* Lancet, May 1, 1847.

† Ibid., March 27, 1847.

particularly, as in lithotomy and lithotrity, the sensibility of the patient is the safeguard against injury of the soft parts; and he instances, as a possible contingency, the non-discovery, by the touch, of the os and cervix expanded over the head of the child, and its inclusion within the blades of the forceps. That such a mistake might occur cannot be denied; but if every measure is to be abandoned because its adoption may precipitate a bungler into error, we fear that many proceedings besides that of ether inhaling must be withdrawn from the list of therapeutical resources.*

"We now bring our report on the subject of ether inhalation to a close, and in doing so beg to guard ourselves against the imputation of a premature admission of all that has been said in its favor. In the criticisms we have thought it right to make upon the various objections which have from time to time appeared, our sole intention has been to point out the *non sequitur* style in which the adverse argumentation has been conducted. We do not deny that other and more logical objections may, upon further experience, be justifiably adduced; but we do maintain that up to the present time no evidence of injurious effects has been brought forward, which ought to weigh against the accumulated testimony in surgery more particularly, which has caused many to regard the introduction of ether inhalation as one of the most merciful dispensations of Providence.

2.—*Compliment to Young Physicians.*

(The editor of the London Medical Gazette, in his exordium to a brief but slashing review of a work emanating from "a surgeon of thirty years' standing in the profession," which he pronounces to be "one of the most singular collections of inconclusive arguments, gratuitous assumptions, ill-grounded theories, and false doctrines which it had ever been his fate to peruse," passes the following handsome compliment on the young physicians of the day. We withhold the name of the unlucky wight who has incurred the penalty of his critical displeasure, but give the compliment for the benefit of those concerned.)

"It is not very long since there existed a prevalent idea that the views and writings of young men engaged in scientific investigations were invariably shallow, speculative, and visionary; and that opinions of true and substantial practical value could only be expected to emanate from those veteran philosophers in whom the observation of a long series of years had become matured and revised by the cool and subdued reasonings of age. And, to a limited extent, this doctrine was true enough, but it will certainly not hold good in the present day. It now seems to be almost constantly observable, that, whenever a book appears which is replete with fine-drawn speculations based upon little or nothing, rash theories, crude fancies, and daring assumptions, the author declares himself to be one who has enjoyed throughout the period of some six or eight lustres the opportunity of applying the test of experience to the accuracy of his crotchets. In the present day, young men have learned to have an absolute horror of everything approaching to a theory, unless they feel tolerably confident that they have the power to convert it into something very nearly approaching to a demonstration by the corroborative evidence of facts. And hence it is, that both here and on the continent the medical institutions everywhere contain men who, although young in years, are really old in experience, and who are continually adding to our store of scientific and practical knowledge by the closeness of their observation and the caution of their inferences. On the other hand, we are almost monthly receiving works by old stagers, replete with theories, a large proportion of which were evidently the cherished fantasies

* Lancet, April 7.

of the author's youth—opinions which these gentlemen were either too diffident or too wise to publish at the time when they were first conceived, but which have remained fixed and unchanged in their minds during all the revolutions which medical science has undergone in intermediate years, to be given to the world at a time when long experience and grey hairs are supposed to confer the highest degree of authority upon matured convictions."

3.—*Value of the Patent for the Inhalation of Sulphuric Ether.*

To the Editor of THE LANCET.

SIR,—As notice has been given of a patent for the inhalation of ether in surgical operations, and as such notice may deter surgeons and dentists from using it in the mitigation of pain, I beg to ask your insertion of the following letter, which I have received from one of her Majesty's council "learned in the law."

Yours, &c.,

Gower-street, Jan., 1847. F. BOOTT.

January 4, 1847.

"MY DEAR MR. BOOTT,—In answer to your question with respect to the patent alleged to have been obtained for 'a process for procuring insensibility to pain by the administration of the vapour of ether to the lungs, I beg to say, that I am clearly of opinion no patent can be valid, giving the patentee the exclusive privilege of *administering the vapour of ether to the lungs*. If the word 'process' is used to denote some particular apparatus for the convenient administration of the vapour, then the validity of a patent for such apparatus will depend upon whether the patentee is the first inventor, and the apparatus was not known to, or in use by, the public before the granting of the patent. The power of the Crown to grant patents is defined by the 21st, Jac. I., chap. 3, which was passed to put an end to the abuse of the power of the Crown to grant monopolies. This power was, by this statute, limited to the granting patents *for any manner of new manufactures*. In commenting upon this statute in Rex *v.* Wheeler, (2*d Barn. and Ald. Reports*, 245,) Lord Tenterden, describing the sort of inventions for which patents can be granted, says,—'The word *manufactures* has been generally understood to denote either a thing made, which is useful either for its own sake, and vendible as such—as a medicine, a stove, &c.,—or to mean an engine or instrument to be employed either in making some previously known article, or for some other useful purpose, as a stocking-frame, or steam-engine for raising water from mines; or it may, perhaps, extend also to a new process to be carried on by known implements or elements acting on known substances, and *ultimately producing some other known substance* in a cheaper or more expeditious manner,' &c.

"I could cite abundance of other authority to the same effect, but it must be sufficiently plain that *no construction* of the word *manufactures* can give the Crown the power to confer by patent the exclusive privilege of administering the vapour of sulphuric ether for the purpose of producing insensibility to pain; or of *administering* any particular drug, or compound of drugs, for the purpose of producing any particular effect, although an inventor may have a patent for the *manufacture* of particular medicinal preparations. But who ever heard of a patent for the performance of a new operation in surgery (as, for example, that by which squinting is cured?) I can see no distinction *in principle* between such a patent and the patent supposed to be claimed for the administration of ether. If this patent could be supported, the patentee might grant a monopoly to any particular surgeon of all operations to be performed with the assistance of the ether, or he might grant it to such surgeons in each town as he might please to select, to the exclusion of others; for although he might sell licenses to *all*, he would not be *obliged* to do so. He might almost be said to hold in his hands, in some cases, the power of life and death. Upon the whole, I am satisfied you may safely advise your professional friends to continue the use of ether in their operations, without the slightest fear of legal consequences. Whether the instruments which are manufactured for the purpose are an infringement of any valid patent will be a question between the patentee and the manufacturers; but the operators can have nothing to do with this; and it would be most deplorable to have any interruption to such a mitigation of human suffering.

Believe me, yours faithfully,

"——— Q. C."

4,—*New Methods for Using Castor Oil.*

"The mildness and certainty of operation of this cathartic give it peculiar advantages in the treatment of many diseases; very often, however, its tendency to produce vomiting prevents it from being employed. To remedy this inconvenience, M. Parola proposes the substitution of an extract, an ethereal, and an alcoholic tincture of castor-oil seeds, for the oil itself. The result of his experiments on himself, and on numerous sick and convalescent individuals, is as follows:—1st. That the ethereal and alcoholic tinctures have a purgative action four times as strong as the oil obtained by expression, and that they are not so apt to produce vomiting, nor so irritant, as the ordinary oil.—2nd. That these new preparations remain unalterable for a long period without reference to climate or season. 3rd. That the ethereo-alcoholic extract possesses a purgative action comparatively weaker than the marc or pulp from which it is extracted, proving that the seeds contain a principle which is insoluble in alcohol or ether. 4th. The advantage of the new preparations, so far as relates to their not causing vomiting, is easily explained by the smallness of the dose in which they are administered."—*Dublin Quarterly Journal.*

5.—*On the Vapour of Ether in Asthma and Hooping-Cough.*

Dr. Willis makes the following observations on this subject in the *Medical Gazette:*

"Ether, given by the mouth, has long been familiarly employed in the treatment of asthma. I have for many years been aware of the fact that it is vastly more efficacious administered directly in vapour by the breath. My plan of using it is extremely simple. I have had recourse to no kind of apparatus for this purpose, but have been content to pour two, three, or four drachms of the fluid upon a clean handkerchief, and to direct this to be held closely to the mouth and nostrils; a single short and difficult inspiration is hardly made before the effect is experienced; and I have occasionally seen the paroxysm ended in six or eight minutes, the respiration having in that brief interval become almost natural.

"It is not otherwise with hooping-cough; the paroxysms of coughing are positively cut short by having the ether and the handkerchief in readiness, and using them when the fit is perceived to be coming on. So effectual have I seen their immediate application, that I have even found it neeessary to suffer the patient to have an occasional fit of coughing to its natural termination, with a view to clearing the chest from accumulated means."

AMERICAN MEDICAL INTELLIGENCE.

1.—*The Mexican Surgeon General's letter to his Government, after the Battle of Cerro Gordo.*

[We take the following interesting document from "*The Picayune,*" one of the ablest newspapers of our city. The Editor's remarks are so appropriate that we insert them in place of any that we wish to offer. We will state, however, that Dr. Vander Linden is mistaken in the name of the officer who struck up the guns of our brave Volunteers.—It was *Captain Pugh,* instead *Pion.*]

"One of the most interesting documents which we find in the Mexican papers lately received is the report of Dr. Vander Linden, who served as surgeon general to the Mexican army at Buena Vista and again at Cerro Gordo. It bears noble testimony to the humanity and kindness of our gallant volunteers at Cerro Gordo. We had before learned from Capt. Hughes that many cases occurred within his own observation of touching acts of considerate and gentle kindness to the Mexican wounded, shown by our volunteers, even while their blood was hot and they were panting for an enemy with whom to engage in deadly strife. We needed no confirmation of the statement of Capt. Hughes, for we well knew the stuff of which a true American soldier is made; but we have here the testimony of the enemy's principal surgeon, which we hope may teach the misguided Mexicans that the reports made of the barbarity of our volunteers are the fictions of those interested to deceive them and to inflame their passions against the United States.

In Dr. Vander Linden's report a striking and beautiful anecdote is related, which illustrates as well his own firmness and sense of professional duty as the prompt humanity of our officers and men. We could wish that the doctor had given as full an account of those wounded on the 18th as of the victims of the affair of the 18th which Santa Anna claimed as a victory. Here is the report:

Army of the East, Medical Department,
Jalapa, *April* 19, 1847.

Inspector General.—About mid-day, on the 17th inst., an action took place between some of our light troops in a part of the mountain to the left of the public road and below Cerro Gordo, to which I ascended immediately, accompanying his Excellency, the commander-in-chief. From this point we could distinctly see our men endeavoring to repulse the enemy, who on his part disputed the point with obstinacy. At about 2 o'clock my aid, Tarbe, came to inform me that a considerable number of wounded had collected round my tent, (the fifth *ambulance* or camp hospital.) Being ignorant whether the action had become general I could not dispose of any of my subordinates, who were all at their respective posts, and therefore was compelled to descend immediately to the assistance of these unfortunates. At 7 o'clock in the afternoon 133 wounded had come to my hospital, of whom 103 had been attended to, five amputations having been performed and thirty-one balls extracted. Fearing, if the battle should continue the next day, to be encumbered with the sick and wounded, I dispatched immediately those whose wounds had been dressed in eight large wagons, and ordered Dr. Felix Orellane, surgeon of the army, to accompany the convoy that same night to the hospital at Jalapa, and he started at 8 o'clock. I continued attending to the few wounded remaining, and at 12 o'clock at night we laid down, worn out with fatigue. At 6 o'clock on the morning of the 18th some more wounded arrived. Having ascertained that the enemy had rendered himself master of the wood and mountain which he had attacked the evening previous, I considered that my camp hospital had become useless and called Dr. Rivadeneira to my assistance. I despatched two more wagons with wounded to Jalapa, but without escort, as there was none disposible.

As the enemy advanced, turning the Cerro Gordo, few wounded could reach my hospital, and as besides two congreve rockets fell, one close to my tent and the other in the park of artillery, which was only at the distance of a pistol shot, but fortunately without bursting, I concluded to avoid this danger and to go nearer to the rear guard of our forces. Accordingly I transported my camp hospital higher up on the public road, a distance of about six hundred yards, to a hut fronting the commissary general, where, at that moment, several wounded arrived, and among them the sergeant of artillery, Antonio Bustos, whose left foot had been carried off by a 4-pound ball. All our cavalry was extended on the public road; the brigade of infantry of Dr. Ortega was descending at a rapid pace, so that it was with extreme difficulty I could reach the house with my medicine chest. Dr. Dominguez, who came with this force, reported himself to me and I detained him to assist in performing the amputation on Bustos. We made the necessary arrangements for the amputation in the entry of the house, Drs. Tarbe and Verde holding the patient and Dominguez and Rivadeneira assisting me. I had commenced cutting the flesh of the patient when a shower of balls coming from the woods in the rear of the house and passsing through its feeble walls made our cavalry retreat in the direction of Corral Falso. A woman, an old man and a child of seven years were wounded in the kitchen of the house. The balls followed in every direction. Our cavalry continued the descent with impunity, while our horses, tied to the posts of the entry, attracted the attention and the balls of the enemy. But honor would not permit us to abandon the sergeant, whose amputation was only half finished, although our death appeared inevitable, but a Divine Providence saved us.

I continued attending to the various stages of the amputation in the midst of balls and the cry of the enemy, and at last finished an operation which appeared to me to have lasted an age. The serenity and resignation of my companions in this crisis were admirable, and is above description. All remained around the patient, attending to the part of the operation which fell to their share, in the midst of the whistling of balls and the cries of death; and when we rose, looking to Heaven with gratitude for our salvation, as we thought, a new peril came to dismay us. A number of volunteers presented themselves in front of our entry, and seeing our uniform, cried—"Death to the Mexican officers," and presented their guns to our breast. I do not know what sentiment inspired me in the resolution which I took, but I rushed to the muzzle of their rifles—I showed them my hand, dripping with blood, and holding a piece of the mutilated leg, cried—"Respect humanity or a hospital of blood—we are surgeons." My words produced a magic effect. In an instant an officer, whose name I have since learned to be Pion, stepped between the volunteers and ourselves, raised their guns with his sword, and these men, animated by victory, thirsting to avenge the loss of their General, mortally wounded, as I have since learned, became from that moment our friends, our protectors. While these events were passing in my hut, which will never be erased from my memory, our firing had ceased; the troops in the redoubts, finding themselves cut off from the public road, surrendered or capitulated; those on the slope of the Cerro Gordo retired through the ravines, and the enemy remained master of all our positions and of an immense *materiel.*

The volunteers of the enemy commenced bringing in, without distinction, their own and our wounded, and we dressed their wounds according to the dictates of humanity and our instructions. We performed various amputations on some real giants, which succeeded in gaining their good will to such an extent that they refused us nothing that could be useful to us or our wounded.

Although two of their own surgeons had arrived, the body which I have the honor to command had the satisfaction that from their number was chosen one member to assist in some grave cases, even in that of Gen. Shields, who had been traversed by a grape shot.

On the 19th all my companions of the camp hospitals of the redoubts, where they had remained at their post, with honor to their corps, until they received permission from the enemy to retire, presented themselves to me, and considering their presence in this place indispensable, I have retained them near me. God and Liberty! PEDRO VANDER LINDEN.

To Col. D. Luis Carrion,

In charge of the Inspect'n of Med. Milit'y Dep't.

2.—*The Cause of Disease in St. Louis.*

That the diseases of warm weather prevail to a great degree in St. Louis no one can deny, and that our bills of mortality are high and startling every one knows who has examined them. St. Louis has been termed "the grave-yard of children." It is a sad truth that our infant mortality is great—but this we think, in part, attributable to the great number of children relatively to the population. Our population is composed mostly of young married emigrants. *Verbum sat.* We have not set down to go into a detail of all the causes of disease in our city, many of them are common to other cities, and the country generally, and cannot be well avoided; of these may be mentioned atmospheric changes, excessive heat—and its opposite dampness and dryness of the atmosphere, &c., &c. Of the modes of living in St. Louis we having nothing to say.

We have often asked ourselves the question: Is St. Louis by virtue of its location and other uncontrolable circumstances necessarrily more unhealthy than other cities in the same latitude? We have no hesitation in answering in the negative. The city is situated on an elevated ridge sloping to and from the river—the position is therefore favorable to draining. It is surrounded by no stagnant marches from which pestiferous effluvia might emanate, but on the contrary by a beautiful undulating country for miles, covered not with decaying forest but with a green carpeting of grass, and thriving flocks. The suburbs of St. Louis strikingly remind us with their charming meads and apparently comfortable and sometimes stately residences, of the cultivated and undulating champagne which delights the eye in the neighborhood of Paris, London, and other cities of the old world.

Some have thought that the American bottom on the opposite of the Mississippi, might account for the unhealthiness of St. Louis. We cannot believe this for the following reasons:

1st. According to the best authorities marsh miasmata cannot pass over a body of water so large as that afforded by the Mississippi.

2d. During the early history of the city, malarious diseases did not so generally prevail as at present.

3d. The American bottom is but little more unhealthy than the city itself.—We should be loth to acknowledge this condition of things did we regard it irremediable; we do not thus regard it. St. Louis can be made as it used to be, as healthy a city as any in the Union. How is this to be done? By removing the factitious causes of disease. What are they? In a great degree the stagnant pools within the environs of the city; the filthy streets, which amount to about the same thing; and, above all, and more than all the rest put together in our opinion the thousand (at least) local marshes—dens where the evil genius of death and decomposition manufactures the agents of destruction —*the cellars of St. Louis!*

We have said that there are a thousand cellars containing water in the city; we are not above the mark. Most of these cellars are used as wood-houses and as depots for various vegetable substances. To say nothing of the tax on comfort which such a state of things occasions—to say nothing of the coldness and dampness of the houses under which they are situated, which coldness and dampness deserve no little consideration.

To say nothing about all the other causes of disease in St. Louis these *thousand* stagnant pools, operated on by the heat of summer, are sufficient to account for the major portion of the diseases which affect us. According to the weight of authority three circumstances are necessary to the production of miasmata. Heat, moisture and *vegetable decomposition*, all these circumstances are formed in our cellars. In vain may our medical police recommend the sweeping and washing of streets, whilst the cellars are stagnant pools! In vain may we clear the streets and highways of the enemy, whilst he is allowed to fire on us from the houses! And now why is it that the children are more unhealthy than adults? Many causes perhaps, but we would offer as one reason—that they are more confined to the houses than adults. Our remedy is plain. Let the Hercules of St. Louis if he can be found drain these Lernean marshes, and thus cut off the hundred heads of the hydra of disease generated by them.—(*St. Louis Medical and Surgical Journal.*)

3.—*Value of Life among the Children of Charleston, illustrated by statistics of the Orphan House.*—I lay before the readers of the "Southern Journal of Medicine and Pharmacy" the following table, which I procured many months since, for a purpose of my own. It seems to me well adapted to confirm and illustrate the views of Dr. Nott, contained in his able essay on longevity, insurance, &c., recently published in this Journal. I requested my friend, Dr. G. Logan, the venerable Physician of the Institution, from whose books it was taken, to examine it and pronounce upon its accuracy. It was prepared for me only to October 18th, 1845; he declares it correct, and adds the last year.

On its very face this statement contains a tribute highly honorable to all parties concerned in the management of the Institution: the trustees, who exert so benevolent a superintendence over its concerns; the stewards, matrons and nurses, who have so well watched over the little destitute creatures committed to their charge; and very specially to the medical officer, who has, for the third of a century, had the management of its Hygienic and Hospital Department.

It should be observed, in regard to this table—

1. That the children referred to are of the lowest class of our white population—speaking generally.

2. They are likely, therefore, to present all the impairment, imperfection and deterioration of constitution and physical and moral health, naturally and necessarily connected with the condition of their parents, hereditarily transmitted, and impressed by circumstances.

3. That the parental management of children in families, is. at least, as favorable to life and health as their mode of living in the best conducted public institutions.

4. That inferences drawn from the proportion of mortality among the children of an alms house or orphan house can hardly be deemed unduly or deceptively favorable, when applied to the community in the midst of whom it exists.

5. That the chances of life are as good, and the proportional mortality as small, in the young children of Charleston, and its population under puberty, as any where else. No where else do we hear of a less proportion of deaths among children under seven years of age than three in a hundred!

Dr. Nott has proved that the acclimated adult population of our Southern cities, on the sea-coast, is somewhat specially exempt from disease, and that their expectation of long life is comparatively good.

The table given extends over sufficient space of time to include all the epidemics to which we are liable: yellow fever, bilious fever, small pox, or varioloid, scarlatina, measles, hooping cough, mumps, and even Asiatic cholera.

From all this, I think, we are reasonably entitled to the gratifying conclusion that our city is as healthy as any other in the world, and that her children enjoy an equal prospect of reaching maturity, and her men and women of attaining a ripe old age, as the denizens of any civilized region.

Year.	No. Child'n.	Officers.	Serv'ts.	Tot. Inmates.	Deaths.	Remarks.
1825,	172	14	14	200	—	No death from sickness—a boy was drowned.
1826,	156	14	13	183	3	Varioloid and Influenza prevailed.
1827,	154	14	13	181	2	Mumps, Yellow Fever, and Bilious Fever, were epidemic.
1828,	130	12	12	155	1	
1829,	130	12	12	154	—	No death.
1830,	130	12	12	154	3	
1831,	136	12	12	160	4	Scarlatina and Pertussis prevailed.
1832,	165	11	11	187	—	No death.
1833,	152	11	11	174	2	Pertussis epidemic.
1834,	143	10	11	164	2	
1835,	137	17	11	165	2	
1836,	120	15	11	146	4	Asiatic Cholera prevailed.
1837,	112	14	11	137	3	
1838,	114	12	10	136	3	Pertussis—Yellow Fever prevailed.
1839,	118	12	10	140	3	
1840,	136	12	10	158	—	No death.
1841,	129	12	10	151	1	
1842,	135	12	8	155	3	
1843,	115	12	8	135	—	No death.
1844,	109	11	7	127	3	Scarlatina—Pertussis prevailed.
1845,	109	11	7	127	1	
1846,	103	11	7	121	1	Mumps and Sore Throat.
Totals,	2905	273	232	3310	41	

Average annual number of Inmates, 150. Deaths, if divided, to include all these, 1 in 75, or $1\frac{1}{3}$ per cent.

Average annual number of children, 132. Deaths, if divided among these only, 1 in 66, or $1\frac{1}{2}$ per cent.

Average annual number under 7 years of age, 30. The rest between 7 and 14.

If half these deaths be supposed to occur in the fraction under 7, (less than $\frac{1}{4}$ of the whole number,) it would not amount to more than about three in the hundred.

(*Southern Journal of Medicine and Pharmacy.*)

DR. HARRIS *versus* THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

We learn from the *Western Journal of Medicine and Surgery*, that one of its contributors, Dr. Harris, of Wetumpka, Alabama, complains of injustice having been done him in a review of his paper on "*the Pathology and Treatment of Fever*," which appeared in our Journal for May last. The editors say—

"We have not received that number of the New Orleans Journal, and are not informed as to the extent or character of the strictures, except as adverted to in the reply of Dr. Harris. What he particularly complains of is, that the editors accuse him of "inexperience and want of candor." We can undertake to say, if such charges have been preferred against Dr. Harris, that they are unjust. Dr. H. has been engaged in the practice of medicine for more than a dozen years, and in that time has seen much of those diseases which abound in southern latitudes. In respect to candor, no one who has known him ever questioned that, however much he may be mistaken in his views, he is always honest in the entertainment and expression of them. For the rest, differences of opinion among physicians, both as to pathology and modes of practice, must prevail, and every writer who submits his thoughts to the profession may make up his mind to have them fully and freely canvassed. We cannot for a moment entertain a suspicion that the able and impartial editors of the New Orleans Medical Journal would willingly do Dr. Harris injustice, and therefore decline publishing his reply to their notice of his paper."

This is extremely clever in the liberal editors of the Western Journal, and we take pleasure in rendering our grateful acknowledgments. They do us but justice in repudiating the "suspicion" that we "would willingly do Dr. Harris injustice," and we think a reference to the remarks in question will satisfy the candid reader that Dr. H. has taken offence *without sufficient cause.*

P. S. This acknowledgment was prepared for our last number, but unavoidably excluded.

A NEW MEDICAL JOURNAL.

We have received two numbers of a new medical Journal, from Memphis, Tennessee; edited by James Conquest Cross, M. D. *Professor of the Institutes of Medicine and Medical Jurisprudence in the Memphis Medical College; assisted by his colleagues.* It is a *monthly,* each number containing about 50 pages. We welcome our new cotemporary into the Southern field of labor, and trust it will do good service.

NEW ORLEANS, SEPTEMBER 1, 1847.

HEALTH OF THE CITY.

The present number of our Journal is sent forth under the harrassing cares and painful anxieties of a severe epidemic of our city's great scourge, *Yellow Fever.* Since the commencement of this Journal, we have had to announce the prevalence of yellow fever upon two separate occasions, viz: in 1844 and 1846, but never before as an *epidemic,* in our September number. In our last we alluded to the prevailing apprehensions and flying reports on the subject, but stated that there had been no well marked case of yellow fever in the city. Very soon after we went to press the disease actually made its appearance in different parts of the city, about the same time, and the cases have steadily increased ever since, until the present moment, when it completely engrosses the attention of the community. As is generally the case, the rumours which go abroad respecting the amount of sickness are greatly exaggerated, but we would not disguise the fact that the sickness is already great, and constantly increasing. Nor can we hope for any material mitigation until the appearance of *frost.* It is yet a long period, shadowed with the most gloomy forebodings, in which the pestilence may be expected to reign over our devoted city, and none knoweth who shall survive to tell the tale of sorrow, affliction and death.

A general epidemic of fever has hardly ever been known to prevail in our city at so early a period as at present. We have endeavored to trace the commencement and gradual spread of what is called *yellow* *fever*, as carefully as possible, and have collected some facts and memoranda which we think will tend to throw light upon the subject, but they must be reserved for a future occasion, as our engagements will not allow us to dilate on them at present. So far as we have been able to ascertain, there has been no connection whatever between the first well marked cases that occurred; one happening in one place, and another quite distant. Within a week after the first man died with black vomit at the Charity Hospital, cases were occurring in all parts of the city.

On the 12th of July, the Board of Health report the number of interments in the city for the week ending July 10th, as having been 138, and make the first public announcement of the appearance of yellow fever, in the following communication:—

MEETING OF THE BOARD OF HEALTH.

Monday, 12th July, 1847.

"Nothing has occurred to change the opinion of the members of the Board of Health respecting the nature of that form of Typhus Fever called Ship Fever, in this climate, since their publication about three weeks since.

"In the hospital, a ward crowded with Irish immigrants, suffering with the aforesaid fever, has invariably become a focus of disease, where any person exposed for a sufficient length of time to the influence of the vitiated atmosphere, might contract the disease. Four persons employed in the Charity Hospital have died of this fever, so contracted. During the last month but one vessel has arrived from Liverpool with Irish immigrants—the "William and Elizabeth." She had on board about sixty, in fine health. One man died during the passage, of chronic diarrhœa. In the mean time, the disease is diminishing in the hospital; many have been discharged; very few have died. Other cases are still under treatment, but should no other immigrant vessel arrive, it cannot be long before the disease will disappear from the Charity Hospital.

"Five deaths from yellow fever have occurred in the Charity Hospital, and two or three cases are still under treatment in that institution. They appear to have originated in the city; and no facts have come to light to prove any connection between these cases and the fever prevailing at Vera Cruz, or other foreign ports.

"The following resolution was unanimously adopted by the Board:

"*Resolved*, That the physicians of the city be requested to report to the Board of Health, through the Secretary, any cases of yellow fever that may fall under their notice—to trace the origin, progress, and, indeed, every thing connected with the history of the disease."

(Signed,) W. P. Hort, Chairman.

A. Hester, Secretary.

In their report for the week ending July 17th, we find the whole number of deaths 143; of which 6 were of yellow fever.

In their next weekly report, up to July 24, the whole number of deaths is 131, of which 16 were from yellow fever.

In the next report, week ending July 31st, the whole number of deaths is 177; of which 47 were from yellow fever. In this report the Board announce the existence of the *epidemic* as follows:—

Board of Health, August 2d, 1847.

"It having been well established by the observations of the physicians of the city, that the yellow fever is now prevailing in nearly every part of it, and further appearing by the reports of the medical men, of the public and private hospitals, and of the cemeteries, that the cases of this disease have been numerous during the past week, and forty-seven thereof fatal—it becomes the duty of this board to apprize the public, and particularly the unacclimated, that we are on the eve of an epidemic, that the latter may prepare to absent themselves in time, and avoid such exposure and imprudence as may increase their susceptibility to the disease."

WARREN STONE, Chairman.

A. HESTER, Secretary.

From this time daily reports were published in the city papers, and the number of deaths from yellow fever for the next week was 133. Since that time the number of cases and the mortality have continued to increase, until at the present writing, (August 23,) the disease pervades all ranks of society, and the number of deaths for the past week from yellow fever alone amounts to 324.

As usual, the lower class of society has suffered first, and to the greatest extent. When the deaths from the fever at the Charity Hospital amounted to from 8 to 15 a day, but few physicians had more than four or five cases under treatment in private practice. It has been remarked that there have been more cases than usual among negroes, but the disease is evidently lighter among them than the whites. It is worthy of remark that every form and grade of Summer fever may be seen in our city at the present time—as mild intermittent and remittent fever, dysentery, mild and grave yellow fever, and congestive or pernicious intermittent, sometimes terminating fatally in the second chill. All these forms of fever may occasionally be seen *blended or running into each other*—the milder into the grave, and *vice versa.*

As usual, the Third Municipality or lower part of the city suffers more than any other. Owing to the cheapness of rent, the greatest number of poor people reside in that quarter, and besides, the principal markets and shipping are in that vicinity, which were shown by a report of the Board of Health last year, to be always offensive in hot weather.

The quarters which suffer in the next degree, are the upper part of the Second Municipality, and the town of Lafayette immediately above. The First Municipality contains a greater number of long settled and acclimated inhabitants than any other, and consequently suffers less from the epidemic. Whatever the influence of locality, it appears that the lower class of society are far the greatest sufferers, and there has been an immense influx of this class within the last five years.

In regard to the connection between the sickness of this city and Vera Cruz, it is worthy of special remark, that notwithstanding the constant intercourse between the two cities, and the great number of discharged soldiers returning from the seat of war, *very few cases of yellow fever have been brought to this place.* Nor have the army invalids, during their confinement in hospitals here, appeared to contract the disease with readiness, notwithstanding their close proximity to yellow fever patients. Dr. McCormick, the medical Purveyor for this place, informs us that among five or six hundred sick and wounded soldiers who have returned from Vera Cruz, there have not occurred more than half a dozen cases of yellow fever. Now, there were from 60 to 100 of these invalids entertained at the Charity Hospital after cases of yellow fever were numerous in the house; and there are from three to four hundred at Dr. Luzenberg's Hospital, in the suburbs of the city, where there are usually from thirty to forty cases of yellow fever. True, the fever patients are kept in separate wards, and mostly in a separate building; yet they are close together.

As to the *Ship Fever*, of which we spoke in our last number, it has nearly disappeared; the tide of immigration from Ireland to this city having been checked. When it was so bad at the Charity Hospital, many of the inmates who had entered for other complaints, and several of the House Students contracted it. We also heard of a number of cases in different parts of the city. Some of our physicians are fully satisfied of its communicability from person to person.

The Weather.—The summer thus far has been unusually cool and remarkably wet. By reference to Mr. Lillie's Meteorological Report it will be seen that it rained nearly every day in July, and a good deal in the early part of August. There has been a great deal of thunder and lightning; several houses in the city have been struck, amongst others, the St. Charles Exchange. But no injury was done.

The River is very low. There was something of a rise after the Spring freshet mentioned in our last number, but very moderate.

We cannot close this hastily written notice of the prevailing sickness, without a passing allusion to the ample means of relief provided for *the poor* by our liberal and benevolent community. The Municipal Councils have all appointed physicians and apothecaries to visit and furnish medicines gratuitously to the sick; and the different religious and benevolent societies, such as the Odd Fellows, the Masons, the Howard Association, the Hebrew Benevolent Society, &c., &c., have all established funds, and appointed committees to seek out and minister to the necessities of the afflicted. These poor creatures, (almost exclusively Foreigners,) seem to be incapable of appreciating the active benevolence displayed in their behalf, and often display a disgusting want of sympathy with each other, yet this does not stay the hand of charity; it bestows freely and expects no reward but the gratification of benevolence. At a future time, if spared, we may furnish a more dilated account of the epidemic.

F.

RIVER QUARANTINE.

It is hoped that the towns on the river and bayous above New Orleans will keep a strict watch on the progress of yellow fever, and that those which have established quarantine regulations will have them rigidly enforced.

HEALTH OF THE COUNTRY.

The following communications have been received from our obliging correspondents, for which we return thanks.

Montgomery, Ala., August 15th, 1847.

Gentlemen:—We have had considerably more wet and cloudy weather for the last two months than is usual with us at this season of the year, there having been about 36 rainy days, between the 10th of June and the 9th of August. Besides this, on many of the days in the same interval, which were fair in town, it rained in some part of the surrounding country, within a few miles of us.

We have as yet had a smaller number of cases of fever of the different types than it is usual for us to have at this season.

Below is the list of cases, made up from the same sources as the others.

Abscess 3, Apoplexy (pulmonary) 1, Asthma 1, Abortion 1, Bronchitis (acute) 1, Croup 1, Cholera-morbus 4, Cholera-infantum 3, Colic 13, Cataract 1, Dirt-eating 1, Dyspepsia 2, Diarrhœa 23, Difficult Dentition 3, Dysentery 10, Enteritis 2, Epilepsy 1, Eczema 2, Erysipelas 1, Fracture (of cervical vertebra) 1, Foreign body in ear 1, Fever (ephemeral) 3, do. Intermittent 38, do. Remittent 79, do. do. Infantile 4, do. Pernicious 5, Gonorrhœa 3, Gastro-duodenitis 1, Hæmorrhoids 1, Hæmaturia 1, Hæmoptysis 1, Leucorrhœa 1, Laryngitis 1, Jaundice 2, Menorrhogia 1, Meningitis 1, Marasmus 1, Neuralgia 9, Prolapsus uteri 1, Pneumonia (acute) 3, Parotitis 2, Parturition (nat.) 3, Rubeola 25, Rheumatism (acute) 1, do. chronic 1, Retention of urine 1, Synovitis 1, Syphilis 7, Sprain 1, Scirrhus (of rectum) 1, Spinitis 1, Tinea capitis 1, Urticaria 2, Vertigo 1, Wounds (lacerated) 2, do. (contused) 2, do. (punctured) 2, do. (incised) 3, Worms 1, Whitlow 1.

Making 293 cases in all—of which 9 proved fatal, 1 from pulmonary apoplexy, 2 from cholera infant., 1 from Dysenterey, 1 from fracture of cervical vertebra, 1 from Meningitis, 2 from acute Pneumonia, and 1 from Rubeola.

In conclusion I would observe that an intelligent medical gentleman, residing about 14 miles South of this place, recently informed me that Typhoid Fever was prevailing to some extent in his neighborhood, and though I have never myself in Alabama, met with a single case which I was entirely satisfied was Typhoid Fever; still his account of the cases of which he spoke, corresponded so well with the description of Dr. Bartlett, and of others who have made this disease as it were a subject of special investigation, that I should feel exceedingly reluctant to doubt that they were such, notwithstanding a degree of scepticism which I have for some time felt, in regard to the existence of this disease, to any extent within the State. The gentleman however made no post-mortem examination.

W. M. B.

Memphis, August 10th, 1847.

Editors New Orleans Medical and Surgical Journal.

Gentlemen:—Herewith you will receive the list of cases that have occurred in my practice in the last two months.

Cases.—Angina Pectoris 1, Asthma 2, Abortion 2, Bronchitis 2, Cerebro-spinal meningitis (acute) 1, (chronic) 1, Convulsions (infantile) 5, Colic 5, (painters) 1, Carbuncle 1, Colera-morbus 4, Colera-infantum 14, Croup 2, Cynancha-Tonsilaris 1, Dysentery 18, Dysenteric-diarrhœa 1, Diarrhœa 33, Dysmenorrhœa 1, Dislocation (elbow) 1, Dropsy 1, Dyspepsia 6, Fever (intermittent) 12, (remittent) 79, Gonorrhœa 1, Gastro-enteritis 2, Hepatitis 2, Hemoptysis 2, Hysteria 2, Hemaplegia 1, Hemorrhoids 2, Jaundice 1, Hydrothorax 1, Lumbago 1, Leucorrhœa 3, Mania-a-potu 1, Menorrhagia 1, Neuralgia 3, Ophthalmia 2, Pleurisy 3, Pneumonia 3, Purtussis 1, Phthisis 4, Prolapsus uteri 3, Parotitis 4, Rheumatism 6, Rubeola 3, Syphilis 1, Spinal irritation 3, Spleenitis 1, Urticaria 1, Wounds (incised) 2, (contused) 2, (lacerated) 6, Hydatids-uterine 1.

In all 263 cases, and 9 deaths. One from Chronic Cerebro-spinal Meningitis, 1 from Croup, 3 from Dysentery, 1 from Gastro-enteritis and 3 from Phthisis.

Compared with the cases reported the two months previously, it will be seen that measles and mumps have subsided; and that the only form of disease that has increased much, is remittent fever. All the cases of remittent fever were mild. No death having been produced from that form of disease.

For the past three or four weeks, the city and surrounding country have been gradually improving in healthfulness, becoming exempt from measles and its secondary complications; and now may be reported as unusually healthy for this season of the year.

The spring, and the summer thus far, have been unusually cool; and the amount of rain that has fallen has been unusually great.

During June and July, the quantity has not been so great, but the showers unusually frequent. Seldom more than one or two days have passed without rains.

The crops both of corn and cotton, are unusually well grown and promising; and the quantity of vegetable matter of every kind, never was greater in this region of country. Almost every kind of vegetation is still in a growing state, and presents a large absorbing surface for the purification of the atmosphere. The weather for a few days has been dry; and the large amount of vegetable matter, of every kind, with which the country abounds, will soon be matured, and its decomposition commence, when a great increase of sickness will no doubt be produced.

Very respectfully,

J. S.

Pattersonville, La., August 17th, 1847.

Messrs. Editors:—This region of Louisiana has been comparatively healthy this whole season, up to within two weeks past. Diarrhœa has been the prevailing disease up to the beginning of this month. It has been confined most generally to the slave population; the recent cases yielding pretty readily to suitable treatment; but the chronic ones difficult to manage, and recurring again and again upon the slightest error in diet or drinks, or exposure. The best treatment as yet instituted is the internal use of quinine, opium and tannin, and a mixture of chlorate potass in hydrochloric acid, together with vesication over the abdomen. Since the beginning of August there have been some fever cases, both among the white and colored population. They have been of the congestive, intermittent and remittent type; some presenting considerable gastric and enteric irritation. Though requiring prompt measures, they have not been difficult to treat, and I think for the last six or eight days have rather been on the decline. This has been a very wet season, and continues so up to this time. Should it now become dry, we are expecting much sickness; it being generally thought, and probably with much correctness, that wet Springs and summers, with dry hot autumns, are generally associated with much sickness in this climate.

Very respectfully your ob'dt. servant,

R. H. D.

HOSPITAL REPORTS.

If time had been allowed us, we might have obtained interesting reports from all the hospitals in our city for the present number of our Journal, but under existing circumstances it has been impossible to attend to it. All of them have been full. In the month of June last, there were admitted into the Charity Hospital 1,216 patients, the greatest number that ever entered that hospital in the same time.

The first death from yellow fever that occurred at the Charity Hospital, was on the 7th of July. From that time to the 22nd of August, the number of deaths from yellow fever at this institution was 362. It is now the predominant disease of the house, and the deaths have been as high as 27 a day. This hospital will furnish some interesting fever statistics when the epidemic is over.

MAIN BUILDING.

June—Admitted: Males, 879: Females, 346. Total 1216.
Discharged: Males, 665; Females, 247. Total 912.
Died: Males, 117; Females, 53. Total 170.
Remaining on the 1st of July, 578.
(Lunatic Asylum, 101.)

MAIN BUILDING.

July—Admitted: Males, 802; Females, 166. Total 968.
Discharged: Males, 754; Females, 145. Total 899.
Died: Males, 126; Females, 37. Total 163.
Remaining on the 1st of August, 483.
(Lunatic Asylum, 100.)

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1847.

By D. T. LILLIE, at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1847.	THERMOMETER. Max.	THERMOMETER. Min.	THERMOMETER. Range.	BAROMETER. Max.	BAROMETER. Min.	BAROMETER. Range	COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10	Rainy Days.	Quantity of Rain — Inches.
June - 20	84.5	72.0	12.5	30.16	30.00	0.16	N.E.	2-3/5	3	0.331
" - 27	84.7	71.5	13.2	30.20	29.96	0.24	S.W.	2-4/5	6	1.452
July - 4	85.5	73.0	12.5	30.13	30.00	0.13	W.	2-4/5	7	7.545
" - 11	87.0	73.0	14.0	30.24	29.99	0.25	W.	2-6/7	6	2.330
" - 18	89.0	75.5	13.5	30.20	30.07	0.13	S.W.	2-3/7	4	2.960
" - 25	89.0	71.0	18.0	30.16	29.87	0.29	S.W.	2-3/7	5	4.625
August - 1	91.5	71.0	20.5	30.10	29.79	0.21	N.	2-6/7	4	5.015
" - 8	87.0	74.0	13.0	30.17	30.00	0.17	S.E.	3	1	0.050
" - 15	90.0	73.5	16.5	30.18	30.04	0.14	S.W.	3	3	0.385

Remarks.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

THE

NEW ORLEANS MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE AND THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.

NEW-ORLEANS CHARITY HOSPITAL.

NOVEMBER, 1847.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1847.

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☞ The above constitute at present the only authorized Agents for this Journal. All monies or remittances may be paid or sent to either of the above as may be preferred—and to them only. We mention this fact for the benefit of our Subscribers many of whom have sent monies to other persons not authorized to receive the same and in many cases the amounts have failed to reach their destination. We therefore repeat that we will consider no payment as valid, should it fail to reach us, if addressed to any other persons than those mentioned above.

New Orleans, May 1, 1847.

NOTE BY THE PUBLISHER.

Subscribers are respectfully requested to give due notice to the Publisher of any change of residence that they may contemplate or may be making, in order that their numbers may be properly mailed to their address. We mention this in consequence of having numbers sent back to us from various points with the post office endorsement of "moved away" or something of that sort, besides the expense of postage. The same suggestions we would likewise offer to those who may desire the work to be discontinued, as in consequence of the great increase in our subscription list, we have no numbers to throw away by sending them where they are not wanted.

We would remark in connection with the above, that in consequence of the repeal of the law granting to Post-masters the privilege of drawing drafts upon each other for amounts of subscription, we shall be much obliged to our patrons if they will remit the amount of their dues by enveloping a good and solvent bank bill, and addressing it by mail as below. Due credit will be given as in the next page of the present number.

S. WOODALL, Publisher
N. Orleans Med. and Surg. Journal,
No. 49 Camp street,
New Orleans.

TO READERS AND CORRESPONDENTS.

A communication has been received from W. M. B.

A communication from Dr. Leslie, which has been for some time mislaid, will appear in our next number.

Our usual list of exchanges have been received. In addition we have received "*The New Jersey Medical Reporter, and Transactions of the New Jersey Medical Society.*" Edited by JOSEPH PARRISH, M. D.

The following works have been received from the publishers:

Carpenter's Physiology. 3rd edition, by CLYMER.
Watson's Practice of Physic. 3rd edition, by CONDIE.
Chelius' Surgery; in 3 vols. By SOUTH.
Wood's Practice of Medicine.
Latham on Clinical Medicine.

Reviews of the above works will appear in our next number.

LIST OF RECEIPTS

ON ACCOUNT OF SUBSCRIPTION, SINCE THE PUBLICATION OF THE LAST NUMBER.

Dr. Autenreith, (N. O)	vol. 4	Dr. Montgomery, E.	vols. 3 & 4
" Buegnott, " "	" "	" Naylor, N. B.	vol. 4
" Bier, H. " "	" "	" Penniston, (N. O.)	" "
" Bevins, A.	" 3	Hon. Isaac T. Preston,	" "
" Chinn, R. H.	" 4	Dr. Peyton, V.	to Jan'y, 1848
" Cobb, B. E.	" "	" Ridgley, J. (N. O.)	vol. 4
" Caldwell, J.	" "	Sickles & Co.	vols. 3 & 4
" Colmer, G	" "	Dr. Spurlock, T. J.	vol. 4
Prof. Dickson, S. H.	" "	" Sunderland,	" "
Dr. Despriz,	" "	" Townes, James M.	" "
" Gans, D. S.	" "	" Vaughan, A. S.	" "
" Holt, A. C.	" "	" Wederstrandt, (N. O.)	" "
" Hasmer, W. B.	" "	" Wood, Geo. B.	vols. 3 & 4
" Hogan, J. D.	" "	" Williams. S. J.	vol. 4
" Kelly, Wm.	" "	" Waters, W. W.	" "
" Law, Wm.	vols. 3 & 4	" Waters, W. A.	" 3

JOSEPH COHN, PRINTER, 31 POYDRAS STREET.

CONTENTS

OF

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

Vol. IV. No. III. — FOR NOVEMBER, 1847.

PART FIRST.

ORIGINAL COMMUNICATIONS.

PAGE

Art. I.—Criticisms and Controversies relating to the Nervous and Muscular Systems. By Bennet Dowler, M. D. - - - - 279

Art. II.—Obstetrical Memoranda. Partial occlusion of the Os Uteri during Labour, treated successfully by Incision. Rigidity of the Os Uteri treated by Injections of Belladonna. By A. H. Cenas, M. D. 312

Art. III.—Four Cases of Cerebro-Spinal Meningitis. Reported by Charles Chester, of Union County, Arkansas. - - - - 314

PART SECOND.

REVIEWS AND NOTICES OF NEW WORKS.

Art. I.—The Medical History of Alabama. By P. H. Lewis, M. D., of Mobile. - - - - - - - - - - - - - - 318

Art. II.—A Treatise on Malignant Fever and Vomito Priesto. By William Ingalls, M. D., Fellow of the Massachusetts, Rhode Island and New Hampshire Medical Societies; formerly Professor of Anatomy and Physiology in Brown University.—*Venienti occurrite morbo.* Boston: 1847, p.p. 108. - - - - - - - - - - 349

Art. III.—New Orleans Literary and Scientific Miscellany. - - 351

Art. IV.—Wood's Quarterly Retrospect of American and Foreign Practical Medicine and Surgery, from April to July, 1847. New York, Richard and George S. Wood. - - - - - - - - - 351

CONTENTS.

PART THIRD.

EXCERPTA.

PAGE

ART. I.—History of Histology. Translated from the Traité D'Anatomie Générale ou Histoire des Tissus, etc. By J. HENLE, Professor of Anatomy and Physiology in the University of Zurich. - - - 353
ART. II.—Dr. McWilliam's Report. - - - - - - - 361
ART. III.—Report on Fever at Boa Vista. - - - - - 365
ART. IV.—An Address delivered in the Medical College of Chemistry, Hanover Square, on Wednesday evening, June 3. By JOHN GARDINER, M. D. - - - - - - - - - - 367
ART. V.—Fermentation ascribed to the growth of Fungi and of Infusoria. 376

PART FOURTH.

MEDICAL INTELLIGENCE.

FOREIGN.

ART. I.—A Clinical Lecture on Hypochondriasis, delivered at the York Medical School. By THOMAS LAYCOCK, M. D. Physician to the York Dispensary, and Lecturer on the Theory and Practice of Medicine. - - - - - - - - - - - - 382
ART. II.—Medical Evidence in a Case of Poisoning by Arsenic. - 389
ART. III.—The Sequels of Scarlet Fever and Measles. By J. A. HIGGENSTON, Esq. - - - - - - - - - - 391

AMERICAN MEDICAL INTELDIGENCE.

ART. I.—Removal of Parotid. - - - - - - - 395
ART. II.—Vesico Vaginal-Fistula. - - - - - - - 396
ART. III.—Adulteration of Medicines. - - - - - - 396
ART. IV.—Osteo-Sarcoma of the Lower Jaw. Removal of the body of the bone without external mutilation. By J. MARION SIMS, M. D., of Montgomery, Alabama. - - - - - - - - 400
ART. V.—On the consequences of rising too soon from bed after Confinement, with two cases in illustration. By WM. M. MCPHEETERS, M. D. - - - - - - - - - - - 402
ART. VI.—Oath of Hippocrates. - - - - - - - 403

EDITORIAL.

Appointments in Medical Colleges. - - - - - - - 404
University of Louisiana. - - - - - - - - 404
Health of the City - - - - - - - - - 405
Health of the Country - - - - - - - - - 406
Hospital Report, - - - - - - - - - - 410
Meteorological Table. By D. T. LILLIE, - - - - - - 410

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

NOVEMBER, 1847.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*Criticisms and Controversies relating to the Nervous and Muscular Systems.* By Bennet Dowler, M. D.

(*Concluded.*)

As the present paper is but the hasty sketch of a rambler in the realms of neurology, and not a systematic effort, I may with the more propriety introduce, here, a few supplemental observations belonging to an era anterior to that of Whytt, Prochaska, and Haller, which will serve to illustrate the earlier doctrines of contractility and of the nervous centres, with a glance at the ganglionary system of nerves as explained at a later period by the illustrious Bichat, and by the learned Dr. Copeland, a quarter of a century ago, and still adhered to by him, together with some experiments performed while these sheets were passing through the press.

A correct history of the medical theories of the 17th and 18th centuries would be little more than an account of the doctrines based on muscular contractility, under the terms contractilitas, irritabilitas, vis vitalis, vis insita, vis motoria, etc.

Glisson, (born 1597, died 1677,) studied the muscular system with great care, and was the first to discover and name its most characteristic and peculiar principle, namely, *irritability.* From the muscular fibre he ascended to the principal organs, noticing the distribution of irritability to each, generalizing the whole into "natural, vital, and animal. 'It is hardly comprehensible,' says Sprengel, 'how this lucid and excellent notion was not accepted with greater alacrity, and further unfolded by cotemporaries.' It has, however, since, been universally adopted; though the explanation hitherto offered, of the way in which the nerves operate on this irritability, and discharge their other offices, present only a series of hypotheses. Glisson assumed the existence of certain vital spirits,—a mild, sweet fluid."* Cuvier,† in reviewing Glisson's researches on this subject, regards him as the founder of nearly the whole system of physiology of the 18th century. It is not a little curious, that the illustrious Frenchman himself, whose vast erudition and massiveness of thought would seem guarantees against theoretical puerilities, should have adopted Glisson's doctrine of innervation. For although Cuvier does not use the words "vital spirits, mild, sweet fluid," he adopts the hypothesis of "a nervous fluid,"‡ adding to this several other assumptions, still more incomprehensible, as will be seen hereafter, forming the strongest possible contrast to that ingenious, but in no respect peculiar, generalization of his, namely, "Natural History has a principle on which to reason, which is peculiar to it—that of the *conditions of existence*, commonly termed *final causes*—nothing can exist without these ;—the component parts of each must be so arranged as to render possible the whole living being, not only with regard to itself, but to its surrounding relations. The analysis of these conditions frequently conducts to general laws, as demonstrable as those which are derived from calculation or experiment"§—a proposition which rests chiefly upon *à priori* and synthetic reasoning, but which at the same time borrows or rather steals much from the experimental and inductive method. At least, few can make any sure progress in philosophizing in this manner, without being deeply learned in the experimental school. Cuvier, for example, infers the structure of the viscera of an unseen animal, from its claws or teeth, upon the doctrine of adaptation, or what he calls the conditions of existence, implying contrivance, purpose, end.

The authors of the Bridgewater Treatises, who, under the assurance of a heavy golden consideration, wrote to order, to prove the power, wisdom, and goodness of God in the Creation, proceeded generally upon this principle of adaptation,—a principle which is displayed in the muscular system to an extent not equalled in scarcely any department of nature they have investigated, and which even Sir Charles Bell, in his treatise on the Hand, its Mechanism and vital endowments, as evincing Design, does not enter upon as fully as its importance demands. It is not a little curious, that St. Hilaire who wrote about the same time, maintaining the *Theory of Analogues* and the doctrine of Unity of Plan, in even the Organization of Monstrosities, (views that would seem to harmonize with those of Cuvier, as well as of those given in Bridgewater Treatises,) repudiates, nevertheless, all such purpose, end, aim, intelligence, &c., as hypothetical : "I take care, says St. Hilaire, not to ascribe to God *any intention—Je me garde de prêter à Dieu aucune intention.* I ascribe no intention to God, for I mistrust the feeble power of my reason. I observe facts merely, and go no further. I only pretend to the character of the historian of *what is.* I cannot make nature an intelligent being who does nothing in vain, who acts by the shortest mode, who does all for the best." Now from the muscles of the foot to those of the eye, the adaptation of the means for the attainment of the

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* Whewell Hist. Induc. Sci. iii. 427–8.
† Hist. Sci. Nat. ii. 434.
‡ An. King. 23.
§ An. King. 23.

end, is indubitably clear; the intention or purpose of nature is obviously developed. The most complicated motions of the hand, for instance, can be traced to the combined actions of appropriate muscles, while in neither the individual nerves of the part, nor in the assumed imponderable fluid, can there be traced by experiment or analogy, any organization or conditions of existence, specially adapted to the infinitesimal varieties of muscular motion. In fact, the function and the organization go hand in hand. If the fibres of a particular muscle be radiated, penniform, orbicular, or rectelinear, the contraction will correspond, whether excited by a blow, a cramp, or a volition, while the nervous cords present no organization, no action, no adaptation of this kind. It is here, if any where, that what is, serves to conduct to the wherefore, the result, the end, the purpose.

Glisson more than a century before the era of Haller, developed the great principle of muscular *irritability*, while an Italian cotemporary made great advances in unfolding its mechanical details or effects.—This was the celebrated Borelli,* who maintained that the muscular fibres were hollow cylinders like a chain of minute bladders. He was the first who seriously applied mathematical calculation to explain and to estimate the muscular force. He was the first to demonstrate the principle, then little known, namely, that nature had guarded *against* that arrangement of the muscles which *economises* the muscular power, so that much of this power is necessarily lost, and that it is, in fact, much greater than it appears to be,—the muscles being inserted, not perpendicularly, but obliquely, at an unfavorable angle, and into the most disadvantageous points or ends of the bones or levers to be moved; the power being remote from the resistance,—all of which is now well known to every physiological anatomist acquainted with animal mechanics. Borelli showed, nevertheless, that this arrangement of the muscles combines advantages greater and more varied than is attainable by any other. His generalizations gave a new impulse to the Mathematical School of that day,—a school that sought to explain, upon mathematical principles, not only the animal forces but the whole science of medicine—a school which Pitcairn,† a Scot, subsequently labored with zeal to advance, particularly in a work of his under the imposing title of *Elementa Medicinæ Physico-Mathematica!*

Haller,‡ a Swiss, the morning star of modern physiology, recognised under the terms contractile power, irritability, *vis insita*, &c., an enumeration of phenomena, throwing a greater luminosity over this entire field of inquiry than any other preceding writer. It is evident, that he knew nothing of post-mortem contractility in the manner and form which I have described. The *vis insita* "is according to him excited by a *sharp* instrument—*oscillates to and fro*; at one moment it contracts itself towards the middle and at the next, extends itself from the middle towards the extremities, and so on for several times." (*Phys.* cccc.) In a word, Haller knew, that with a sharp instrument, a delicate ridge could be produced, the summit of which oscillates, but soon flattens down, and may be reproduced, as he says, "for several times." Now this cannot be called a functional action, as the flexion of a limb. The

* 1608–1679. † 1652–1713. ‡ 1708–1777.

action which he describes as being "more powerful than any other is the stimulus of electricity," and says, that violent *convulsions* can be produced in the muscles through the nerves of the spinal cord. (CCCCIII.) Now this *vis insita* or *irritability* as he knew, it was refered not to the nerves or "nervous power," as he termed it, but to the muscle itself. Here lies the merit of Haller, not so much in proving and developing the phenomena, as in announcing the true principle. He relies, it must be confessed, almost wholly upon the heart and intestines for his proofs, asserting that "they are exceedingly tenacious of their *vis insita.*" (CCCCII–CCCCIX.)—organs wherein I have never been able to detect, in the human subject, any thing of the kind, except a delicate ridge from scratching the surface of the heart. From half an hour to later periods after death I have searched often, but in vain, for any other motions. Bichât, during the Reign of Terror, received from the authorities decapitated criminals, for experiment, in from 30 to 40 minutes after death, but was never able, by even Galvanic electricity, to produce motion in these organs.

Cuvier criticises Haller for ascribing irritability to the muscle, as a property independent of the nerves—a theory which he pronounces very weak.* Yet Cuvier the at same time admits, that in many animals wherein no nerves can be discovered, muscular motion exists, as among the *Zoophytes*, and especially the *medusés*. The weakness in this case is not with Haller. Cuvier with all his greatness, sometimes adopted theories not at all tenable, especially in neurology. Thus he considers, "the nerves the cause of [muscular] contraction." He next assumes the existence of "a nervous fluid derived from the blood and medullary matter which secrete it," and says, this "cannot be doubted!" He then finishes his circle of assumptions by stating "that it is by an imponderable fluid that the nerve acts upon the [muscular] fibre."† Here, without a particle of proof, one improbable assumption is offered to prove and explain another still more improbable, until the subject becomes absolutely inconceivable, not to say absurd, and judging by analogy, impossible. For, by no law, by no analogy, no adaptation, no plan, can the imagination itself connect as cause and effect any fluid whatever, in locomotion, respiration, sensation, thought. To say water will run down an inclined plane to seek its level, or that caloric will cause expansion is comprehensible, but to aver that an imponderable fluid, that is, a fluid the weight of which cannot be appreciated by our present imperfect instruments, can in virtue of any special organization it possesses, communicate to the nerve, and the nerve to the muscle, all the motory variations in painting, in dancing, in running, in fencing, in singing an opera, &c., is to adopt a method of explanation which is directly opposed to that profound intellection and rigid induction which Cuvier generally displayed and for which he is justly admired. Haller wisely refers "the motive cause to a law established by the Creator;" (CCCCVIII,)—though philosophically speaking, this is only a provisional reference—a confession of ignorance—a leaving of the question open, unencumbered by theoretical trammels, and not as some imagine to silence inquiry, or to intercept further efforts to rend the veil which now

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* Hist. Sc. Nat. IV. 233. † An. King. 23, Lond. 1840.

hides the truth and which prevents further advances. But the most extraordinary part of Cuvier's series of assumptions remains to be told, namely,—"sensativeness and muscular irritability, *are so much the stronger* at every point, in *proportion* as the exciting cause is more *abundant*, and this agent *is the nervous fluid*, &c." Now, I have shown that the muscular force is not even *diminished* by destroying the nerves.—There is, indeed, no ratio between the amount of nervous matter and the amount of the muscular forces. I could illustrate this principle in the living body, in its normal and diseased conditions: one example will suffice. Many anatomists, especially before Scarpa's time,* maintained that *no nerves whatever entered into the tissue proper to the heart.* Professor Dunglison† regards the nerves, especially the cerebral, as exercising but a very limited and indirect action over this great organ. Digitalis may diminish—exercise, fever, and mental perturbations increase its action, but from the first to the last moments of the longest life, its motion is perpetual. Answer, ye who slight the muscles and idolize the nerves, how comes it to pass that the very organ which is the *poorest in nerves* should be *the richest* in the muscular *force?* Has any dissector ever shown any morbid change in the nerves of the heart, even in angina and maladies unmeaningly called nervous? Are not nearly all diseases of the heart, as induration, softening, hyperæmia, atrophy, dilatation, enlargement, muscular? Ask the morbid anatomist. Probably most cases of palpitation, angina pectoris and the like, are attributable to spasms or convulsive cramps of the muscular tissue of this organ.

Certain it is, that the heart's action is not in a ratio corresponding to its nervous matter. Borelli's estimate, (doubtlessly an exaggerated one,) represents the force, in the left ventricle alone, as equal to 180,000 pounds! The muscular force of the uterus is in no respect proportioned to its quantum of nervous tissue. Its irregular actions in rupture, abortion, hour-glass-contraction; its hyperæmias, its scirrhous, and other degenerations, belong to the muscular rather than to the nervous texture.

The beau Ideal of the physical man, the Sampsons of our race, possess brawney masses, prominent muscular developments, with the brain and nervous matter and the intellect and the sensibility minimized—without longing after immortal fame—without genius and without taste in the fine arts—having much animal, but little moral courage. On the other hand, a great development of the nervous system, large brains, intellectual superiority, exquisite sensibility, taste and genius seldom appear in a man possessing Herculean muscular strength. With respect to this particular, who can believe that Homer and Milton, Shakspeare and Voltaire, Rousseau and Pope, were equal to the ancient *Athletæ* as wrestlers, or to the modern boxers and tumblers in muscular action? The Cuvierian doctrine, that muscular action or irritability is proportion to the nervous matter, its agent, is the reverse of the general opinion and the general experience of mankind, and contrary to Cuvier's own statements elsewhere. He asserts that no quadruped approaches man in the magnitude of his brain,‡ but that he is inferior to animals in

* 1746–1826. † Phys. ii. 146, et seq. ‡ An. King. 46.

strength and swiftness,* that is, in muscular force. Hence the rule, to which the exceptions are few, namely, the muscular and nervous systems are in the inverse ratio to each other, and not correspondent.—Coleridge asserted that in the features of every man of genius there is something feminine.

The obliquities in the logic of the nervous system, but too truly represents those in its anatomy, physiology, and morbid action. M. Sarlandière, in one and the same page, says that "the principle of motility resides in the spinal medulla, which is the reservoir of *innervation*, of nervous power for the ganglionic system"—of which latter, he maintains, that "all the nerves of the life of relation entering the ganglia are sensible—all passing from the ganglia are insensible; that they intercept the cerebral nervous influence, and are found in all animals having a distinct nervous system, constituting that of the invertebrated exclusively."† Now if we turn to Cuvier's work on the Animal Kingdom, his second great Division will be found devoted to these animals, which he groups in six classes, with many orders and families, and, which he describes thus: "their nervous system does not unite in a spinal cord, but merely in a certain number of medullary masses dispersed in different parts of the body. Their irritability is very great, and is retained a long time in parts after they have been amputated."‡ Neither the existence of the spinal cord, nor a great quantum of nervous matter is necessary to a great muscular force.

Prelusory to a few remarks concerning the sympathetic nerve, including the ganglia, plexuses and their supposed functions in presiding over the involuntary muscles, circulation, nutrition, secretion, and absorption, it may be proper to say, that while Glisson was engaged in disseminating the doctrine of *irritability*, his compatriot, Willis,§ was engaged in propagating the Doctrine of the Nervous centres,—the ganglionary masses, and, indeed, the distribution and configuration of the entire nervous skeleton.

As the summit of the loftiest mountain is the first to receive and reflect the morning light upon the dark plains below, so Bichat's towering mind received and reflected the medical glories with which the Nineteenth century opened, and which the bright emanations of his own genius increased and concentrated. But his brief life passed away like a meteor, leaving however an enduring track of light in the medical heavens.||

Paradoxical as it may seem, it is nevertheless true, that Bichat's most erratic speculations and the least sound of his generalizations are more or less valuable, because they are suggestive, and serve as starting points for the inquirer. An original observer, an orginal thinker, the massiveness and the multitude of his ideas, like a crowd hastening to pass through a narrow door-way, embarrassed each other. An enemy to mere words and metaphysical abstractions as exponents of things, he generalized without limit. Was there ever before any book so excessively generalized as his "*upon Life and Death?*"

Bichat's theory of the nerves, epecially of the ganglionary system—its insensibility, independence, and involuntary action, is of great impor-

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* An. King. 45. † Anat. passim. ‡ 335. § 1612–1675. || 1771–1802.

tance, as serving to classify a very peculiar and extensive group of phenomena, over which he supposes the sympathetic nerve presides.—Now if the heart or some other organ shall be found to exercise this assumed control, the leading conception is not the less due to Bichat.

According to Bichat,* the nervous mattter constitutes two distinct systems, whose functions have but little in common. The Sympathetic is a separate system, emanating from the ganglions, each of which has a distinct and independent action. This system he calls the organic, ganglionic or sympathetic which has numerous centres in the ganglia, greatly differing from the cerebro-spinal which has but one centre, that is, the brain. Whether the marvellous influences of Ether, lately discovered, act through the blood, the nerve, or the muscle, may not be evident, but one fact appears to be, if not established, quite probable—a fact favoring Bichat's views of the independence of the sympathetic, namely, that the organs supposed to be under the influence of the latter, are the least and the last to suffer, from etherization;—though many of the reflex school are making haste to appropriate these etherial phenomena to their theory. The reflex anatomy and function of the spinal arcs are alike obscure, especially in parturition, where they are supposed to be the most clear. If the uterus be exclusively under the control of any great division of the nervous system, it ought not to be that of the cerebro-spinal, but that of the sympathetic, whence it derives its principal nerves, that is from the renal and hypogastric plexuses. If the nervous texture of this organ be nothing else but a portion of the spinal arcs, why does not etherization intercept its motory as well as its sensory function, as in other arcs? If the ganglionic and every other nervous influence as the primary controlling power of the affiliated organs of the centre, be rejected,—if the doctrine of a special, yet common organization and function be assumed, as necessary to and inherent in all, as an ultimate fact beyond which it is impossible to look,—in either case, the fundamental idea of Bichat, that is, independent action is, to a considerable extent, countenanced by etherization. I may remark, however, that the great precision—the mathematical exactness with which the action of ether has been traced and described, must be viewed with the greatest distrust by all, not partizans, who have studied with care the uncertainty with which such physiological, morbid, or medicinal actions are enveloped.

Dr. James Copeland's exposition of the ganglionary system, (differing but little from Bichat's,) given more than a quarter of a century since, and now reinforced by the late approval of the author, becomes the more important from the fact that it contains a reflex doctrine. Dr. Copeland says, "the phenomena which Dr. Hall has assigned to a reflex function of the spinal cord, were fully recognized by Whytt. Reflex actions, I denominated many years ago, (1824,) *reflex sympathies.*" Dr. C. contends that Dr. Hall has only dignified with the term function what had long before been called reflex sympathy. The former attributes reflex action to the ganglial, the latter to the spinal system.† Dr. Copeland so far from limiting reflex sympathy or action to the brain or spinal cord, or to both conjoined, refers it chiefly to the organic or

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* Life and Death.

† Lancet, for January and March, 1846.

ganglial nervous system, affirming from experiment, that after the division of the spinal cord, "galvanic currents through the sympathetic ganglia, affected the muscles both below and above the divided portion of the cord." "As the ganglia of the great sympathetic form, says Dr. Copeland, an independent system, presiding over certain functions which are essentially vital, consequently. they may be viewed as the system and seat of organic life, and may therefore be denominated the vital system of nerves, whose centre is in the semilunar ganglion. The vital influence being thus produced from the centre of the body, and reinforced and modified by subordinate ganglia, allotted to the individual organs, according to their functions, is propagated along the distributions of the system on which it depends and is inherent, throughout the whole body."* "Contractility," he continues, "is essentially a vital phenomenon. This property may be divided into *insensible organic contractility*, into *sensible* organic contractility or irritability, and into cerebral contractility," or "the contraction occasioned by the will in the voluntary muscles. The first and second species result from the ganglial distribution and influence."†

Dr. Copeland maintains that "the ganglionic nerves do not originate either from the brain or spinal marrow :—because they are observed in the lowest animals which possess neither brain nor spinal cord,—because they may be distinguished in embryos before either one or the other nervous mass can be traced, and because they are never wanting in the fœtal state,—whereas not only have the brain and spinal marrow been individually wanting, but the same fœtus has been found entirely without both."‡

Dr. Copeland denies "the existence of a peculiar structure or mechanism set apart for the reflex or excito-motory actions, as Dr. Hall believes."§

Of the Reflex discovery, the Lancet affirms, "there is nothing so original, diffusive, developmental, grand,—it will deliver our art from the hands of the ignorant—bring certainty out of chaos—revolutionize physiology, pathology, remedies—and is the key, the corner-stone, the basis of scientific midwifery," while Dr. Hall asserts that this system "is as definite as the action of the ordinary ray." These are great discoveries, if any body could understand them. It is with despair that a plain man reads in the Lancet, that these things are too high for any intellect save that of Dr. Hall. "We know, says that journal, of *none* except Dr. Hall himself by whom the spinal functions, and the spinal marrow are *fairly* treated of." Now this kind of argument is well known among the Indian tribes of North America. No one dares to know as much as the Medicine Man. His Medicine-Bag is more wise than any other. There is, fortunately, a greater doctor than either of these, namely, common sense. What he cannot understand is no discovery at all.—Nor can he be frightened the by words *physical*, *dynamical*, *imponderable*, *reflex*, *excito-motory*, &c.

In the dark ages, words were more potent than at the present era. In the 11th and 12th centuries, the State, the church, the learned—were divided between Plato and Aristotle, or the Realists and the Nomi-

* Doc. 166–7; Read, 1820. † Ib. 169,170. ‡ Ib. 156. § Lancet, 1846.

nalists. The former believed that words, or rather ideas have a proper or independent existence, being realities, true entities, but without souls; the latter, that general ideas are nothing but abstractions, or intellectual results deduced from sensations. Bitter was the dispute—fiery was the persecution—and violent were the shocks, when one half of christendom arrayed itself against the other, about a word, an abstraction,—whether an idea was an entity, a reality, or but *flatus* vocis—whether, qui in rebus, non in *vocibus, verum positam esse?* The Nominalists failed according to D. Stewart, because they had no experiment or palpable example by which to illustrate their doctrine of the real and universal function of words, in opposition to things. The same failure awaits reflexism. One of the greatest merits of Bichat, was his rejection of metaphysical abstractions as exponents of physiology. The Reflex abstraction, under all its metamorphoses, throws no light whatever upon, nor has it any connection with, any branch of medical science, being an idea, a word, not a thing,—not physical, but phantasmagorial,—not light, but a cimmerian darkness. "The Germans, says a wit, possess the faculty of making the sciences inaccessible." So does the reflex school.

A true history of the world would be, not so much a relation of the acts of kings and cabinets, of generals and of armies, as of *Words.*—Aristotle's ten categories reigned longer, more extensively, and more despotically than the Twelve Cæsars. "The majority of mankind pay an habitual veneration to words, and this species of adoration is not exempt from fanaticism. It would not be difficult to find men who would willingly suffer any privations and tortures, an even death, for *words.* And it is almost always for want of attaching *the same ideas* to the *same words*, that men misunderstand each other, dispute, and sometimes come to blows." Words, in every age, have reigned in Medicine.

"Allow a man, says Berkeley, to make his own definitions of common words, and it will be no hard matter for him to infer conclusions, which in one sense shall be true and in an other false, at once seeming paradoxes and manifest truisms." Admitting, provisionally, the truth of the Reflex discovery,—does it belong to Nominalism, or to Realism? Is it a real, proper, independent entity, or an abstraction—a thing, or a word—Dr. Hall is aware that the *true* Spinal System is but a cadaver without a soul, when separated from the word *reflex.* He, therefore, idolizes that potent word. I will not say that he would, the law permitting, like the Realists put dissenters to the torture, though he and his friends do even now, in this age of toleration, go as far as the law will allow—nay, a good deal beyond it.

In a late communication in the Lancet, having for its title, "The *Anatomy* of the Excito-Motor *System*," Dr. Hall exclaims, "how much, then, is conveyed or implied in that one word Reflex!" Without intending any persiflage, I may mention a true story of a certain clown, who was unacquainted with the reflex laws of light, and who, having as he supposed, a real moon in a bucket of water, killed his jackass, for drinking up the same, though the poor beast had done nothing but drink the water which reflected the moon. "But how much is implied in that one word *Reflex!* It performs in the excito-motory system, all the mysterious functions of the Zodiacal Man, pictured in the Almanac. Whether "internality or externality, subjectivity or objectivity," or some other Germanism can reveal its hidden meaning, is doubtful. But the most difficult part of the inquiry is, to ascertain whether it applies to a material, or an immaterial entity, as constituting this discovery, the evidence being about equally balanced: For, although, "the material studies of medical men, as humoralism, morbid anatomy, pathology, organic chemistry," are denounced as pernicious, the language used by that school is of the most materializing character; "*true* spinal marrow, *distinct anatomy* of the excito-motor system, *arcs*," and many terms, tests and properties indicative of materiality, are used. The spinal cord is surely as material as the great wall of China, but a *true* spinal cord is not quite so clear in meaning, but Mephistopheles says, "we must not be too anxious about that, for where the meaning fails, a word comes in most opportunely." Is not that *esse substratum* or *something* which goes, comes, has incidence, reflection, "which is physical in its nature," which has curves, a distinct anatomy," and many attributes of matter, as much matter as the pyramids? It is to no purpose to talk of particles, fluids, and the like, or even of "dynamics." Mere abstract power which is described as being of a physical nature—as being also fully discovered, mapped out, and made known, without having at the same time substance or a substratum in which it inheres as an attribute, is about as obsure an idea, as ever turned up in the sea of dreams.—Power as a separate entity, is not as "distinct in its action as the ordinary ray." "Power, says Dr. Reid, is not an *object* of our external senses, nor even an object of consciousness, but a relative conception,—a quality, and cannot exist without a subject to which it belongs. That power may exist without any being or subject to which that power may be attributed, is an absurdity, shocking to every man of common understanding." Though this may be an extreme view, yet whatever may be the reflex thing, entity, light, or power, its material or immaterial form, its course, or curves in going to, being pictured upon, and returning from the reflector, or *true* spinal cord, none of these could be seen or appreciated by the senses, inasmuch as "the mysterious messenger" would be masked and concealed in the midst of opaque muscular, bony, and nervous masses. Its exact route, could not be seen nor mapped off. The inductive philosophy is too *naïve* to permit "the lynx-eyed Dr. Hall," as the Lancet calls him, to impose on her a mere abstraction, for "the second great discovery," having "a *distinct* anatomy, reflex arcs, animal dynamics, mysterious messengers," which by the new *regulæ philosophandi*, are immaterial yet "physical in their nature, reflex in their action." Now when a plain man cannot comprehend all this, he is gravely told that "the difficulty lies in the inductive nature of the thing itself," or, in other words, that he is ignorant of the inductive philosophy, that is, of common sense, and is only fit for the execration of future generations, as "a laggard and an obstructor of the truth."

It has been already shown, that the excito-motory "meaning of the word *reflex* is not *figurative*, as in politics and morals," but *literal*, "*physical*," and that no one had discovered or used it, in this sense, before Dr. Hall. This he insists on in his last paper in the Lancet: "The terms incident and *reflex* imply a *real phenomenon* of the *most remarkable* kind." "There is, in these nerves, some *extraordinary* *recondite* connection." "The superior laryngeal sends forth some *mysterious messenger*,—returns in the *just channels*." "Reflex function, with its *fulness* of meaning, had been used, *could have been used by no one*," [other than Dr. Hall.]

No one can tell whether the *true* spinal marrow is the reflex-acting matter itself, or a mere passive reflector, a receiver of impressions,—not *figuratively*, but *literally*, and if literally, how does the seal traverse "the reflex arc" to make its stamp on the medulla? Is the seal a self-moving one? Is its impression, one that can be seen? or must it be divined? Does it travel among transparent, or opaque tissues? Can one see "the mysterious messenger," going, and "returning in the *just channels*," and not in bye-paths? How does he travel? What is his velocity? Being a physical, not a metaphorical personage, what are his characteristics, distinctive features, natural history? Whatever this entity may be, "it claims," according to the Lancet, "to be a great discovery, and cannot descend to a lower title;" [though]—"the writers of the profession still go on writing, and their so called thinkers still go on thinking, as though the spinal marrow, as we now know it, had no existence." But let no one cry Eureka! He cannot be sure, so long as the question of entity and non-entity, the literal and the metaphorical, the ranine and the human are jumbled together, and the more so, as "all material studies render men's minds *inept*" in this new science. Never was a system more obscure—data more inconclusive, hypothesis more unwarrantable;—in none have criteria, direct experimental proof, rational analogy, coherent, philosophical deduction, been more constantly avoided. Its "arcs," like mechanical arcs, serve but to illustrate the law in mechanics, *that nothing is stronger than its weakest part.*

It may be proper to say something of the luciferous argument, (*argumentum ad lucem,*) of the reflex school.

After a considerable slumber, the doctrine of Goethe—doubtless, at first, an ideal reference of his poetical mind, is attracting universal attention among physicians and naturalists;—I allude to Metamorphosis or Morphology.* The reflex theory seems to have taken for its *mor-*

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* Morphology, with numerical averages, promises the greatest advantages to medical science, in solving many problems, beginning as it does, with elementary types, noticing progressive development from the lowest to the highest formations, it seeks to appreciate the circumstances which modify the primordial law of organization, the range of which is more limited than is generally imagined. One of the most remarkable, and, as yet, unapplied facts, going to show how a single circumstance may triumph over the apparently inexorable purpose of nature in maintaining the unity of organization, is found in that subterranean wonder of the world, the Great Cave of Kentucky. In the New York Journal of Medicine, about two years ago, will be found an anatomical discription of Eyeless Animals, inhabiting that cimmerian region. The fishes of the river Styx, in this cave, have *no eyes*. An unbroken Night for countless ages, has, as it were, assumed the functions of the Creator! has annihilated one of the most important organs, because it was useless!

Would the great *Archæus* of the Kentucky Cave, restore the visual organs of these eyeless animals, upon laying open that vast realm to the light of day? The Kentucky philosophers have within their reach, in the great cave, what Goethe would call "*the sacred riddle*" of metamorphosis. If Darkness annihi-

phological type or analogue, Light—a very lucid point of departure. If the germ be so bright, how much more so the fully developed form! The language is that of optics—*incidence*, *reflection*, *&c.*, and if the word *refrangibility* is not used, the word *curve* is. One would naturally suppose that these terms were used in a *figurative* sense, but such is not the fact. The Lancet says, "The term *reflex* was, it is true, used by some," [before Dr. Hall,] "but it was in the same sense as the word is now used in morals or politics, as something secondary or dependent." Dr. Hall says, "How much then, is conveyed or implied in that one word, *reflex*,—*incidence*, *reflexion*," &c.; "the ordinary reflection of a ray of light, or the polarization of a ray of light, is not more *definite*;" "the ray of light, which is now incident, and immediately afterwards reflected, is the same ray, modified, directed, and returned by the *reflector*, whether it consist in locomotive particles, or in vibration."

With respect to this new light, there cannot be much propriety in investigating its laws, so long as its *reality* as a discovery, is questionable.

Berkeley has well said, "it is to no purpose for *explaining nature* to bring forward what is neither *open* to the *senses*, nor can be understood by reason."* Sir Charles Bell, (much as it was against his theory,) quotes and adopts Dr. Reid's observation concerning the nerves, namely, that they are *unhandy engines* for carrying images, elastic ether, animal spirits, vibrations, tonicity, &c.† How much more "*unhandy*" is the "*true*" spinal marrow for a looking-glass,—not a *figurative*, but a *literal reflector!* "definite as the ordinary ray." By which of the five senses may this "mysterious messenger," the morning star, the harbinger of that effulgent day long prayed for by the Æsculapians, be recognized? Can the material eye trace its self-luminous path, its rectelinear, refrangible, and reflex actions—its divergencies, intensities, velocities, composition, decomposition? This light or discovery "is physical in its nature, and reflex in its action," but is *not* figurative. Now the literal meaning of *reflection*, (the one recognized in the excito-motory system) is thus given: "Reflection, the return or progressive motion of a moving *body*, occasioned by some obstacle which prevented it from pursuing its former direction."‡ Now this spinal luminosity, being "as definite as light," like optics, ought not to be incomprehensible to the Royal Society, nor to any other sane society. If there be any truth in Dr. Hall's discovery, it is of such a nature that every man of education can attest it—can depose to its reality before "a court medical," civil or military—not being the result of prolonged and deep mathematical calculation, such as the *Méchanique Céleste* presents—not such as was required in the case of Le Verrier's late discovery. If there is really any thing "as definite as light," which goes *to* or returns *from* the spinal marrow of a frog, it is a simple *fact*, which, whether it have any connection with Dr. Hall's doctrines

lates the most complex and complete organ of the animal body, would the Light create or restore the same, that is, the eye? This problem might perhaps be solved in a few years, by the removal of some of these animals to the light.

* Wks. ii, 89. Lond. 1843. † Anat. ii, 221. Am. Ed. 1834.

‡ Ency. Brit. xix, 87.

or not, can be proved as easily as the movements of a ship or a locomotive,—and to make a mystery of the *reality* of the *act itself*, is an insult to the common sense of mankind. It is in vain for Dr. Hall to write of "the *mysterious messenger*;—his *message* may be mysterious, but his arrivals and departures, are *facts* susceptible of *proof*. Dr. Hall's discovery, says the Lancet, "*is physical*." Now which physical fact has he proclaimed, the existence of which "*no one*," but himself is competent to recognize *as a fact*, leaving the *explanation* out of view? Is the loss of "*resiliency, contractility*, or of *shape* in the muscles" of a frog, as the *immediate* consequence of the destruction of the spinal cord, *such a fact*, as no one can perceive but himself? If his excito-motory-system be as clear and as "definite as ordinary light," the laws of which constitute the most certain of all the sciences, why can no one comprehend it?

Was there ever before a discovery which, having been completed, systematized, epitomized in the form of tables, maps, and axioms, and published, no one but the discoverer could comprehend. Columbus, Harvey, Galvani, Jenner, Franklin,—all made discoveries;—Drs. Jackson and Morton, of Boston, discovered the new uses of ether in suspending the painful sensations hitherto incidental to surgical and some other operations, but, as in all other *true* discoveries, intelligent cotemporaries possessed themselves not only with the whole discovery, but confirmed, extended and perfected the same—an easy achievement, after genius has made known the true path to knowledge. In this reflex discovery alone, the contrary rule holds good. No one can understand or apply it. This failure is charged to envy, jealousy, and malice!—The Lancet says, "There can be no doubt, whatever, that Dr. Marshall Hall is half a century in advance of his cotemporaries." A popular English Author says, "woe to the man who precedes his age; awful is the duel between Man and the Age in which he lives."

The reflex school reproaches England with being behind other countries in adopting this discovery. If this be so, it is one among many proofs, that Englishmen are very much prejudiced in favor of common sense or mother-wit. From Shakspeare to Scott, and from Bacon to Brougham, this it is which constitutes the charm of their literature.—The following enumeration, by the Lancet, gives ten true and two doubtful disciples, as "early advocates" of the discovery, namely, Muller, Flourens, Sharpey, Smith, Barlow, Simpson, Grainger, Clark, Van Deen, Budd,—with two later converts, of whom it is said,—"it would be *well* for Dr. Carpenter and Mr. Newport, if they could wipe out their *former opposition* by their *later conversions*."

Never having seen the newly discovered agent of the excito-motory-system, "physical in its nature," I cannot say that it is, or is not identical with, or analagous to, light, but, I presume it is only the *Fata Morgana*, or that sort of light described in Hudibras:

"Whate'er men speak by this New Light,
Still they are sure to be i'th' right.
'Tis a dark-lantern of the spirit,
Which none can see, but those who bear it:
An ignis fatuus, that bewitches,
And leads men into pools and ditches."

Sternutation or sneezing is, evidently, a most important branch of reflex-science. It is a *dernier resort*, especially with the *élite* of that school; the sneezing influence is their epidemic arguement. To sneeze is a great matter; its import is stupendous. But this is nothing new. One of the earliest and most universal customs of mankind was that of praying to the Deity for the safe deliverance of, and a benediction upon, every one who happened to sneeze; and, this too, by people, who, perhaps, never prayed upon any other occasion.

Homer considered *sneezing* as belonging to *Astrology*:

> * * * "Telemachus then *sneezed* aloud,
> * * His nostril echoed through the crowd,
> The smiling queen the happy *omen* blessed."

Puck, and other fairies, in the mid-summer night's Dream, regard it as belonging to *comedy*—

> "And waxen in their mirth, and *sneeze* and swear."

The Miltonian explanation is *a physiological* or rather a *sanitary* one:

> "Harmless, if not wholesome as a sneeze."

But the reflex school regards sneezing as the dispeller of all doubts. Sneeze and believe. In order to know the eternal reasons, the hidden mysteries, the inscrutable secrets of nature, it is only necessary to sneeze. A learned Professor and a worthy citizen of New Orleans, does me the honor to say—"Surely, our author, during the course of his life, has taken a pinch of snuff, and had a good sneeze therefrom." Most true, but whether this good sneeze was a reflex, a direct, or a rectelinear operation I neither affirm nor deny positively, not knowing. It is, however, a circular argument, to affirm that the sneeze itself is a competent witness of its *own modus operandi*, or the very thing in dispute. Is it not surprising, that, as this sort of evidence always existed, the discovery was not made long ago? The number of sneezes must have been immense before Dr. Hall's era. While writing this page, I was called to aid at the *début* of an infant, which during the first ten minutes of its extra-uterine life, performed the three principal reflex acts almost simultaneously, and with prodigious force; it cried, coughed and sneezed, but I could not discern any *physical* agent marching to or from the true spinal cord, in an arched manner, "as definite as the ordinary ray."

All nations, and all genders, ought now, since the discovery has, at last, been made, to be able to testify as to the reflex nature of sneezing, &c.

For my own part, I am a *Nosarian*, as a *noseless* man could not sneeze, though "the respiratory arcs" might be in the best possible condition. In the life and opinions of Tristram Shandy, is an excellent account of a long nosed stranger, whose arrival at Strasburg created an excitement and controversy very like the present one. The disputes of the vulgar, though intense, were not more so than those of the faculty. The doctors could not admit that the stranger's nose could be as large as was represented, because, it "would have destroyed the statical balance of the fœtus *in utero*, and have thrown it plump upon its head nine months before the time." Another party argued, "that there was no cause in nature, why the nose might not grow to the size of the man himself." This was answered, by showing, that there could not be nutrition for both;—"mortification would ensue, the nose would fall off from the man, or the man inevitably fall off from his nose; that there was a just and geometrical arrangement and proportion of the human frame to its several destinations, offices and functions." "The logicians began and ended with the word *Nose*; and had it not been for a *petitio principii*, which one of the ablest of them ran his head against in the beginning of the combat, the whole controversy had been settled at once. A nose, argued the logician, cannot bleed without blood,"—[and, is necessary to a "good sneeze."] "God's power is infinite, cried the *Nosarians*; he can do any thing. He can do nothing replied the *Anti-nosarians*, which implies contradictions." One maintained that a nose might be as big as the steeple of Strasburg. This was answered, by showing that a middle-sized man could not wear a nose 575 feet long; but no one argued that the spinal marrow was the exclusive seat of "a good sneeze," nor, that the nose itself was a mere superfluity in that operation.

As to pathological sneezing, I will help the reflex school to a case, which is, for their views, the most favorable one that I know,—though by no means damaging to mine. A worthy printer, a patient of mine, had frequently suffered from sudden and prolonged paroxysms of sneezing, which sometimes seemed to endanger his life, and which alternated with hæmorrhoidal attacks. The premonitory symptoms were fulness, stuffing, and engorgement of the nasal passages. How the *eccentric* morbid reflex action "of the respiratory arcs," and of the anal "arcs" mutually compensated each other, acting at remote points of the "true" spinal marrow, leaving all the intervening "arcs" unaffected, while "the mysterious messengers" were running up and down "by two lines," most perpendicularly, is *not* quite so "definite as the action of the ordinary ray." Besides, it must be borne in mind, that the *modus operandi* of the sternutation is, as already mentioned, the precise *question in dispute*, in which the sneeze itself cannot be a competent witness. When the credibility of the witness is questioned, do we take *his* testimony in order to decide whether he ought to be believed? Is a sneeze in the United States bound under the constitution to criminate itself? By which of the five senses is the physical matter, with the inward and outward course of the sneeze traced, that is clearly seen, going first fiom the nose to the spinal cord, &c., then back, before the sneezing explosion can take place?

The reflexians take for granted—for proof they give none—that the whole science of obstetrics is but an embodiment of their system. "Dr. Marshall Hall, says the Lancet, has himself declared that it will one day form the very basis of scientific midwifery—constitutute an entire department of the medical art—the key—the corner-stone,* etc.; now, all this, "physical in its nature, reflex in its action," is done, not by the common material spinal cord known to anatomists, but by a *true* spinal cord known to Dr. Hall alone. A lecturer on Midwifery, in London, Mr. Smith, who has, according to Dr. Hall, made out of this reflex

* Lancet, Nov., 1846.

discovery a department peculiarly to his own, testifies in effect, that the whole science of obstetrics comports or adopts itself precisely to Dr. Hall's theory, and is, therefore, the reflex Ideal, Actualized—an incarnation of "the imponderable, the physical, the dynamical." Mr. Smith deposes as follows: "The act of parturition never had been and never could be studied *properly* as a motor function, until the *discovery of the spinal marrow* by Dr. Marshall Hall! The spinal marrow is the central presiding organ. All the chief physiological uterine motor actions are reflex in their nature."* What is the proof? Nothing but a few obscure, meaningless experiments upon frogs, turtles, and salamanders, not even in the act of parturition, but in the last *agony—in extremis* and under the stimulus of thunder, &c. Call you this the *discovery* of the spinal cord?

Without pretending to know anything of parturition in the latitude of London, I can affirm, after having witnessed many acts of this nature in Virginia, and in New Orleans, that if anything reflex—any "mysterious messenger" ever travels to or from the *true* spinal cord, "as definite in its action as the ordinary ray," I never could see it. This phenomenon must be peculiar to London. It is believed that no one in America will testify to this fact—"*physical in its nature*"—before any court medical or obstetrical. There is, however, a very inexact method of testifying to '*physical facts*,' suggested by Mephistopheles to Faust, who hesitated to bear testimony as to the reported death of Martha's husband: "Is it the first time in your life that you have borne false testimony? Have you not confidently given definitions of God, of the world, and of whatever moves it? And looking fairly at the nature of things, did you—you must confess you did not—did you know as much of these matters as of Mr. Schwerdtlein's death?"

Assertions are easily made, but when unproved and improbable, they ought not to have much weight. The spinal marrow, not less than the brain, heart, and soforth, is doubtlessly necessary to natural labor, though both reason and experience teach that the expulsive power in that great hollow muscle, the uterus, is not *reflex*, but the *direct inherent* act of its muscular tissue and function, other organs contributing only in a secondary manner. The uterus, like other muscles may act after death: in Moreau's late work on Midwifery, he relates the case of a parturient woman, whom he was called to see;—but not arriving until after her death, he proceeded to turn and deliver. The uterus *contracted perfectly*, and expelled the placenta completely. I have observed, not only an apparent resistance to the introduction of the thermometer into the rectum, but a partial expulsion of that instrument, not to mention defecations in cadavera, under circumstances not admitting of a very satisfactory explanation from the expulsive power of post-mortem gases and the like, though, it must be confessed, that this kind of proof is equivocal.

Early in the last century, when midwives were more ignorant than they are now, they attended only to the placenta, which, immediately after accouchement, was forcibly pulled away, destroying thereby many lives; Ruysch directed that the practice should be discontinued, declar-

* Lancet, May, 1846.

ing that he had discovered in the fundus of the uterus an orbicular muscle, (*detrusor placentæ*,) whose special business it is to expel the placenta,* drawing the surface of the uterus by a gliding motion from the corresponding surface of the placenta, thus separating it and casting it off;" the existence, as well as the function of this muscle, Bell admits,† because Ruysch saw it, adding, that he had "nearly an absolute reliance on the observations of that author. The editor of Cuvier's work,‡ denies the existence of any such muscle, and considers it as altogether incomprehensible that nature should place a muscle in an organ, which is *itself* only a muscle.

The reflex school does not seem altogether exact in their neurography, in claiming the uterus for the *true* spinal marrow exclusively, for anatomically speaking, this viscus is vastly dependent on the sympathetic system for its nerves. The operation for the removal of *placentæ* retained by hour-glass contraction which I have several times performed without pain or ill consequences, illustrates Bichat's notion of the little sensibility of that system of nerves, with which the great organs of the centre are connected.§

There can be no doubt that the expulsive power in parturition, is essentially a muscular effort. The nervous system, including of course sensation, its great function, usually dies in advance of the muscular—an opinion, which, independent of experiments, it would not be difficult to support. It may be sufficient to say, that many writers could be quoted, proving, as above stated, that after the death of the mother, the uterine action has continued, resulting in post-mortem births—the children being usually dead. It is out of the question to regard such deliveries as the result of post-mortem gases, or of any merely mechanical force.

Reflexism affects an admiration for Numerism, though, as yet, nothing has appeared to justify this Arithmetical pretension,—no exact histories, no numerical analyses. Dr. Hall dedicated his Lectures on the Nervous System to the father of numerism, M. Louis—a book of 2,000 or 3,000 sentences, with about one thousand divisions, duly marked with numerals, but not a single case or experiment in a tolerably complete form to compensate for a great many assumptions. The British and Foreign Review,|| justly remarks, that Dr. Hall's researches "are in absolute *contrast* to M. Louis." "There is not, we repeat, the slightest attempt at a physiological or numerical consideration" etc.—Dr. Hall must, therefore, admire Numerism by way of contrast to Reflexism. Coleridge must have had a glimpse of this kind of admiration, when he asserted of connubial bliss, that 'a *contrast* of character

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* Hist. Sci. Nat. ii, 405. † Anat. and Phys. ‡ Hist. Sci. Nat. ii, 515.

§ It is the *cervex uteri*, not the *fundus* which possesses acute sensibility, if I may judge from obstetrical experience. In asserting that no ill consequences, or even pain has resulted from this operation, I by no means dissent from Dr. Fenner's views of the management of retained *placentæ*, set forth in the last number of this Journal—a few lucky cases do not form a rule. My first case of hour-glass retention was in the country. I was forced to act in order to escape, if not a *lynching*, at least the condemnation of a dozen of ladies. After passing the *os tincæ*, no sensibility was experienced.

|| January, 1847.

is essential to happiness." M. Louis' facts relate to man;—Dr. Hall's to reptiles. M. Louis gives appropriate names to his books, as Yellow Fever of Gibraltar, Phthisis, Typhoid; Dr. Hall calls his experiments on the most inferior animals, Human Physiology,—The Nervous System,—The True Spinal System.—Excito-motory System,—Pathology and Therapeutics of the Nervous System,—Scientific Obstetrics! M. Louis gives facts, physical changes, an account of all the organs, without bias or selection; Dr. Hall gives opinions, confines himself to *one* organ chiefly—to imaginary reflex functions, arcs, lines, curves. M. Louis generalizes his facts,—Dr. Hall his opinions. The former gives analyses of his histories,—the latter of his hypotheses. Louis builds upon arithmetical averages; Hall, upon reflex abstractions. Louis copies from nature,—Hall from Hall. Louis can be understood by others—Hall by Hall only. Louis philosophises upon the thing,—Hall on the word Reflex. Louis' studies being material, lie within the realms of sense—Hall abhors these as pestilential entities, "rendering the mind inept" in comprehending the reflex discovery. The former numerizes, reasons, and makes a free will-offering from his scientific treasury, without attempting to coerce his *confrères;* the latter dogmatizes, anathematizes, and would, the law permitting, "crush as vipers"* all dissenters who believe not in him, or rely on any other reflex name but his.

As illustrative of the *morale*, as well as of the method of argumentation in the reflex school, I subjoin, with reluctance, the whole of Dr. Hall's famous communication† in the New York Journal of Medicine for January, 1847, enclosing the words of the editor in brackets. I do this, because the following, as well as some preceding remarks, cannot be otherwise intelligible, and, because this is an average specimen of the means resorted to in order to intimidate oppositionists.

Hitherto the reflex thunder has been directed against others, rather than myself. This is remarkable, because the denunciation of European oppositionists was based, not on their dissent to the reflex system, which,

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* Dr. Hall gives an anonymous letter from Paris, in the Lancet, from "the *first* physician of our day"(?) averring that "the reflex actions of the spinal marrow are regarded in the Academy of Medicine as an established fact"—whereupon Dr. Hall breaks forth against Her Majesty's United Kingdom, thus: "In the midst of the attempts at detraction here, it is a relief, &c. The viper detraction is only just crushed; being cold blooded, it has been very tenacious of life." (*July*, No. '47.)

† [MARSHALL HALL AND DR. DOWLER.—We have received a communication from the able author of the "reflex doctrine," complaining of the language employed by our correspondent, Dr. Dowler, in the May number of our Journal, and especially of the imputation of "Materialism," imputed to him by Dr. D. This charge is denounced as] "cowardice and calumny, refuted by anticipation, by § 9, &c. &c. &c., of my work on the Diseases and Derangements of the Nervous System. The rest is as little founded in truth, moral or scientific. Many have been the attacks on the poor reflex doctrine, but none to compare with this last, and not one with *one* fact, or sound argument against it, Dr. Dowler's inclusive. Many and great are the efforts being now made here to sustain the *Status* of our profession. I am sorry that you should have sanctioned by your name, in any way, what must be certainly considered an attempt in a contrary direction."

indeed, they generally admitted, but on their denial of Dr. Hall's assumed claim as the discoverer thereof. My experiments, taking the same point of departure with Dr. Hall's, show that his fundamental doctrine cannot be true; it was reasonable, therefore, to suppose that I should have incurred the maximum of displeasure—I say reasonable, because in the logic of this school, the most conscientious men, who cannot believe in the reflex dogma, are denounced as felons, as the numerous readers of that able periodical, the Lancet, must have observed; the Royal Society, for example, is represented as a band of conspirators, allied together for the criminal purpose of preventing the extension of "the second great discovery—a discovery which is destined to revolutionize the whole science of medicine," and to bring in that more than millinnial glory, which, the most imaginative Æsculapian, never yet dared to predict, since "man's first disobedience which brought death into our world with all our woe."

What does Dr. Hall mean by "the Status *of the Profession*, to sustain which many and great efforts are now being made?" Does he mean that dynamical spiritualism, that moral force and beauty, known to erudite Heathens, as *honestum*,—Τὸ καλὸν? Στάσις, *status*, *state*, *standing*; the moral sublime—the eternal fraternization of all faithful Æsculapians,—is this the enterpretation? Is this the *Status?*

The very best rule that can be adopted in doubtful cases, in verbal and doctrinal criticism, is after giving the literal meaning, to give examples showing how the writer applies the rule himself. The *Status*, then, by this rule signifies in its practical, that is its *reflex* sense, that Dr. Hall and his adherents are not amenable to the law of libel,—but have a right, or a *ca te blanche*, to call about nine tenths of the medical professsion, that is all dissenters from the reflex doctrine, the following names—sometimes in the singular, but generally in the plural: "Laggards, obstructors of the truth, consummately mean, impudent, ignorant, physiological Dogberrys, worms of the Nile, foiled disappointment book worms, calumniators, abominable and false slanderers, wicked, foolish, malignant, bitter, envious,—like the kiss of the ancient lady, to all comers,—cowards,—unable to comprehend the reflex function,—damning serpents biting the file after the teeth are all gone—deserving to be tried before a court medical,"—not to mention other epithets without number; such epithets as bad men seldom, if ever, apply to the greatest felons, even after sentence has been pronounced against them,—yet these are the choice phrases used to designate that brilliant constellation of worthies now shining in the medical heavens. In the reflex vocabularly alone, is found such an exhibition of the STATUS, of the moral sublime.

"Many and great have been the efforts to sustain the *status* of the profession, here, says Dr. Hall. It may be so. But the success seems "to grow small, and by degrees beautifully less." The word *status*, in its *reflex* meaning, is therefore but little better than that by which Goethe characterizes the French language, in his Wilhelm Meister: "is a language of reservations, equivocations and lies; it is a *perfiidous* language. Heaven be praised! I cannot find another word to express this *perfide* of theirs. Our poor *treulos*, and the *faithless* of the English are innocent babes beside it." The excito-motory style was not known to Goethe.

The following propositions are, it would seem axioms with the Lancet, (and here the style changes:) "Dr. Hall stands alone in the modern history of real and legitimate discovery." "Of cotemporary names, who by their own egotism or the vanity or partiality of their friends have been placed in competition with our author, it is scarcely necessary to say a word. For any actual competition there never was *any chance.* Dr. Marshall Hall has always been too lynx-eyed, and too far in advance of his cotemporaries for this."* Let the reader always remember that this discovery relates to frogs and turtles, and can be as well tested by a "first course student," as by any man, howsoever learned he may be, as "it is physical in its nature,—reflex in its action"—a discovery, which if true of frogs, does not prove the reflex doctrine even among them, excepting in a special state, &c., and if it did, must be limited to the single, artificial condition of the particular vivisection, and which can no more be received into human physiology, than the other peculiar habits and functions of those animals.

Why should dissenting physicians, who cannot conscientiously receive "this as the second great discovery," be nothing but "cowards, laggards, obstructors of truth," whose very memories shall be accursed by succeeding generations? Do these propagandists remain ignorant of the fact, that medical faith is an involuntary act? If, in the language of Dr. Hall, the discovery is "as clear as the ordinary ray," disbelief is impossible. Can any one doubt the existence and action of "the ordinary ray?" If, as I contend, Dr. Hall's experiments have no more connection with his doctrine, than they have with the ebbings and flowings of the tides, are not some doubts allowable, nay, unavoidable? In offering fifty or sixty histories, not of frogs, but of *bona fide* men and women,—an humble offering, it is true—have I done Dr. Hall any personal wrong—committed a mortal sin? If my experiments nullify his on frogs for all the purposes of human physiology and pathology, am I to be blamed? Had I anything to do in forming the laws of Nature? A lover of scientific truth does not regard as a calumny, or a *caning*, an attempt to investigate the physiology of the muscles, even though the result might show the fallacy of frog-experiments, when applied to man. Is it a virtual assault and battery, to show that all that Dr. Hall can do *with* the *true* spinal marrow, aided by *electricity*, I can do ten times better *without?* Is it a crime to show the traveller that he has mistaken his way?

The great efforts made, and means used, to pass this assumed discovery upon the world for a *bona fide* one, naturally begets a suspicion that it is only a counterfeit: "M. Say relates a story of a woman, who for a wager stood the whole day on one of the bridges of Paris, offering

* This kind of philosophy is very common among our Indians. If the MEDICINE MAN, such as Tecumseh, the Great-Bear, Black-Hawk, or Walk-in-the-Water, consult his MEDICINE-BAG, every body must submit: "For, (in the language of the Lancet.) actual competition there never is *any chance;*" the medicine man is too lynxed-eyed, too far in advance of his cotemporaries for this," and his Medicine-Bag is equally potent with Dr. Hall's true spinal marrow.

to sell a five franc piece for one franc, and (naturally) not finding a purchaser."

The spinal cord, to say nothing of its equivocal physiology, is the obscurist of all organs in its pathology, and the least certain source of diagnosis, except to *Dr. Hall*, who at a late meeting of the London Medical Society,* informed that Body, that "the spinal system had become the source of all diagnosis—a gift and boon conferred on the art of knowing and curing diseases—taught the nature and value of symptoms—was our sole diagnostic;"—and then kindly added, "for the correction of a certain weak and ignorant set of persons who wrote and spoke foolishly upon the subject," that "no one knew or understood the subject, who had not seen with his own eyes *the* actual experiments"—all of which the society accepted with meekness—no dissenting voice was raised; but speech after speech was made, in favor of Hallism and nothing else. I will not call this fanaticism, nor orientalism, but *sciolism* it must be; for if Dr. Hall's most accredited expounders can be relied on, "there is not another person who understands, and fairly treats of the *true* cord, its discoverer excepted." The doctrine is here reiterated, namely, that *no one can know* or *understand* the discovery, unless he sees with his *own eyes* Dr. Hall's reflex experiments! All other believers, therefore, must believe *without* any evidence whatever; and still worse, *no* one but the *discoverer* can *comprehend* these experiments, even after *seeing* them! Hence all physiologists must conduct themselves like sheep. Here, a little explanation may be necessary for city doctors, who have not observed the habits of these animals. In the hilly portions of the country, where the fences are generally *reflex*, consisting of a series of salient and retiring angles, it often happens on the slope of a steep hill, that the BELL-WETHER, in jumping a fence, strikes the top rail, and thereby precipitates, perhaps twenty pannels into the plain below, especially if the fence be wet at the time. Now, although not one rail be left upon another, each sheep of the entire flock, instead of relying on his own senses, and walking quietly over the prostrate fence, will jump just as high as his Leader jumped, to the great danger of breaking his neck.

As to my charging Dr. Hall with Materialism, and which he bravely calls calumny and cowardice, I can only say, that it is a pure fiction of the John Doe and Richard Roe kind, serving only to give him an occasion to refer to a work of his. I have quoted Dr. Hall as saying that the *true* spinal cord "*is the sole seat of all the emotions, passions, and appetites.*"† But I have not said that he was a materialist.‡ The Pantheists, materialize the Divinity whom they consider literally as the All, the Whole, the Aggregated Universe. Berkeley denied the existence of Matter altogether, maintaining that the mind with its ideas, alone, had any positive existence. Yet Pope ascribed to

"Berkeley, every virtue under Heaven,"

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* Lancet, Aug. 1847. † Nervous System 4 to. 96, 71.

‡ Whether a materialist or an immaterialist in physiology be the greater heretic, is a quiddity, worthy of the study of one

Who can "distinguish and divide
A hair 'twixt South and South-west side."

though he was an *immaterialist.* A German writer approved Diderot for saying, that the *Monades* would one day unite and form a God, if one did not already exist. I fully acquit Dr. Hall of atheism, pantheism, materialism and monadism; but, at the same time his *spinalism* is equally absurd. His *distinct anatomies*, physical dynamics, spinal arcs, mysterious agents, passions, reflections, incidences, curves, *true* spinal cord, &c., seem at times both material and immaterial. On the whole his system inclines, not to materialism, but to *immaterialism;* indeed it is declared expressly, that "all *material studies* render men's minds *inept*" to Dr. Hall's studies!

The reflex school greatly mistakes the proper method of making converts. Does Dr. Hall think that the medical public can *be coerced* into a belief of his hypothesis? Does he really adopt the logic of Hudibras?

> "Some have been wounded with conceit;
> And died of mere opinion straight;
> Others, though wounded sore in reason,
> Felt no contusion, nor descretion.
> * * * * * *
> But since no reason can confute ye,
> I'll try to force you to your duty,
> For so it is, howe'er you mince it,
> As ere we part I shall evince it,
> And curry (if you stand out) whether
> You will or no, your stubborn leather."

The Falstaffian philosophy, of giving no man a "reason on *compulsion*" is more rational. There is, it seems, but one method of escaping the wrath, and of securing the good will of these fiery reflexians, who swear by Dr. Hall's theory;—it thus explained by Dean Swift: "It is an easy and short way to obtain the reputation of a wise and reasonable man, wherever any one tells you his opinion, to agree with him."

The whole of Dr. Hall's argument concerning the "*Status* of the profession," when interpreted according to its reflex meaning, is this: to proclaim Dr. Hall's few frog and turtle experiments as constituting that great discovery "destined to revolutionize medical science," is to "*sustain the status* of the profession"—to offer an host of experiments upon men and women which completely nullify his discovery, "is certainly an attempt in a *contrary direction.*" With the present paper, I will have published about sixty individual histories of contractility, not to mention a vast number not yet published—the individual acts of contraction, of these sixty cadavera, amount by estimation to one thousand; —amputation, the division of every discernible nerve, the destruction of the spinal marrow and evisceration, were performed in a sufficient number of these to establish the law to which there is no exception, and which overthrows the fundamental doctrine of Hall, namely, that the spinal cord has no influence whatever upon the contractility of human bodies recently dead. Now if these cases had been given, however falsely, as so many proofs of Dr. Hall's discovery, nothing would have been heard of this "*status*," &c.

In a word, I say with all the fearlessness of truth, that my experiments disprove, as far as the human body is concerned, the reputed discoveries of Bell, Hall, and others, who ascribe to the anterior roots or to the *true* cord, or to the sympathetic, or to any other portion of nervous matter, the *force* necessary to muscular contraction. My experiments are direct—the force I apply, on an extended arm, is towards the centre of the earth—the effect produced is in the opposite direction—is contrary to every other known force or motion, and is that appropriate to the function of any given muscle;—all is clear, definite, unequivocal, and unlike that obscure *mélange* of frogs, and convulsions, and electricity, which constitute "the second great discovery."

The Lancet charges Bell with "the singular and deplorable weakness of not mentioning, in all his works, the name of Marshall Hall." Is there any weakness in this? The veteran vivisector had labored long in the field of experiment, without obtaining definite and positive results illustrative of human physiology and pathology;—was he bound, therefore, to rely on inferior evidence, and to believe the "physical nature" of reflex impossibilities?* Having failed in the experiments necessary to establish his own "NERVOUS CIRCLE," was he bound to replace it with "arcs, lines, distinct anatomies, the mathematics of the nervous system, Euclid, definite as the ordinary ray"—the mere mystifications, physiological travesties, kaleidoscopic views and dissolvent scenes of his own labors, baptized by the attractive names of Excito-motor-system—Reflex system—a system which localizes in the *true* spinal cord, with an host of healthy, morbid, curative, obsterical agents, entities, actions, and functions, including "the emotions, appetites, and passions," and which must be a perfect pandemonium—

"Black spirits and white,
Red spirits and grey,
Mingle, mingle, mingle,
Ye that mingle may."

As Harvey's great discovery nearly ruined his private practice, owing to the opposition which it brought from his professional brethern, some condemning it as an innovatien, others maintaining that it was known before,—so Dr. Hall's potent abstraction seems to have had a similar effect. The Lancet says, "we have heard Dr. Marshall Hall say, that if he had been devoted to physiology before he had established himself in practice, he should never have succeeded at all. Sir Charles Bell had the same story to tell; he has left it on record, that after every step in his discovery he was obliged to work harder than ever, to preserve his reputation as a practitioner." "This discovery met a better fate" [than Dr. H's.;] he had to complain not of black balling, but that his papers appeared without exciting attention; but his time came, and suddenly, as he says, after reading a paper no better than the rest, a cumulative reputation, worthy of all his labors, burst suddenly upon him, and he stood confessed the head of anatomy and physiology at that time. Knighthood followed."† And it may be added, that VICTORIA I. pays his widow a pension for this same *discovery*.

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* This word is not too strong. Had I space, I could show, that, until the laws of nature be changed, much in reflex experiment and deduction is physically impossible, being not a discovery, but a *revolution* in the constitution of nature.

† Dr. Copeland has lately remarked, that "Dr. Hall's doctrines have been more advanced by his supporters than by himself, as the opinions admitted by

It is pretty evident that the "second great discovery" is looking up for "the honors of knighthood." Hence, the intolerance towards all who oppose the excito-motory system.

Post-mortem contractility presents an important point of departure—a dynamical type for studying the physiology of motion, serving at once to ascertain and fix the uses of each muscle, and to develope the leading but peculiar force inherent in all the contractile tissues. It may seem contradictory to study a living function or principle, in a dead body.—Did Goethe intend a satire on the physiologist where he says in Faust—"He who wishes to know and describe anything *living*, seeks first to drive the *spirit out of it;* he has then the parts *in his hands;* only, unluckily, *the spiritual bond is wanting?*" In the present instance, however, the general or popular death, leaving as it often does the muscular force alive, aids this investigation, in several respects, chiefly by insulating this force from an host of vital and modifying complications derived from the mind, sensation, circulation, respiration, &c.; thus preparing the inquirer to rise to higher and more complex laws in physiology. The study of the muscular force will probably end in, or serve as the prelude to, a great dynamical discovery in the organic, like that of gravitation in the inorganic world. Professor Whewell says, "many anatomical truths have been discovered, but no genuine physiological principle. All the trains of physiological research have begun in exact examination of organization and function, and have ended in wide conjectures and arbitrary hypotheses. Hitherto we have had to tell of the *failures* of physiological speculation."*

It has been already said, that the flexions of the forearm afford the best myological type for physiological post-mortem dynamics: Thus the biceps and brachialis being inserted into the most unfavorable ends of two levers, must act to the greatest disadvantage,—that is nearly all the forearm, not to mention the hand with weights in the palm, lies beyond the point where the force is applied; in other words, the resistance is remote from the force. This resistance is aided by friction at the fixed end or elbow, by incipient rigidity in some, and by the antagonistic muscular force and physical elasticity or resiliency of the opposing muscles in all cases. The small angle at which those muscles are inserted, diminishes, to a great extent, their force, percussion probably does not excite all portions of the muscle at once; besides it is presumed that great obliquity in the insertion is a bar to the equal and the simultaneous action of all the fibres, particularly in such a muscle as the deltoid. Now if we admit the dynamical principle that as much force is lost on the fixed, as on the moving end of the fore-arm, it follows, that, if the fore-arm and a body placed in the palm shall weigh ten pounds, the force exerted to carry these to the perpendicular must be equal to about two hundred pounds placed at the point of insertion. The physi-

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him were either obscure as if he had himself seen them only through a haze, or they were successively inconsistent with, or different from, those previously published. May they no longer be tortured by their overfond parent, in hopes of bestowing upon them that decent form which they are incapable of receiving." (Lancet, 1846.)

* Hist. Ind. Sci. iii. 431-2.

ological antagonism of the muscles, is really no hypothesis: I have known it more than once to extend the arm spontaneously, immediately after the subsidence of the flexor-paroxysm of contraction. The few cadavera on which I have experimented the present year, have not presented this most curious phenomenon, and I find my notes, as to this particular, very indefinite in former years;—whether the extension retrograded by the same route in which the flexion advanced—whether any massive knot took place in the triceps, with other questions, require further elucidation.

It may be doubted whether these and similar questions, will be satisfactorily solved by electrical experiments upon animals, now strangely named *Electro-Physiology*—a study to which the distinguished Professor Matteucci, of the Univerity of Pisa, has given much attention. In a recent communication to the French Academy, he terms the eight propositions which comprehend the whole of his speculations, nothing more than "*some hypothetical ideas*," and which I reduce to a single sentence, namely: *There is a* NERVOUS FLUID *existing in, and generated by the* MUSCLES, *whence it is diffused* TO THE NERVES *from their extremeties to the brain, where it is subjected to the will.* Here all is reversed, neurography, myography, physiology,—the origin, direction, and distribution of the nervous force. Now I propose, not as "a hypothetical idea," but as an indubitable verity which I have fully established, to reject all circumlocutions, and to name this force, what it really is, a peculiar and inherent property of the muscular system, which, in the voluntary muscles, during life is subject to the will, which often remains for hours after death,—which may be called into appropriate action by percussion, and which, if I may judge from the published statements of electro-physiologists, cannot be proved in a satisfactory or natural manner by exciting, or combining it with an electrical force. What a perversion of language, what an inversion of logic, to call the nervous fluid a muscular product, or to call the muscular force a nervous fluid!

This is infinitely more than I could ask in behalf of the muscles; but, without admitting that the electro-physiological method is at all conclusive, I must confess that this theory is still more simple and much nearer the truth, than the usual one—namely, a hypothetical fluid which does nothing but hand over its power to the nerve,—which latter can do nothing only to hand over *its* power to the muscle, by afferent and efferent routes, &c.

The following supplemental observations on post-mortem contractility, were made with the utmost care, amid the excitements incidental to an epidemic, which, for many weeks has every day struck down as many victims as might be expected from the daily shock of hostile armies.*—

* This season, meteorologically speaking, is healthy—historically, mortal; ceaseless breezes, loaded with ceaseless sighs; a balmy temperature, with fiery plagues; refreshing showers, with scalding tears; starry nights, with cheerless lights flickering in the chambers of the dead. The morning breaks brightly; the afternoon is overcast with thunder-bearing-clouds, which evening rolls away in banky masses, fringed with red, shimmering in the setting sun; while, funeral marches blacken the streets—while the dead carts, loaded with victims, coffin rumbling upon coffin, pass on, without a single mourner. Even the strong mind of Johnson, quailed at the idea of dying unwept. Philanthropy

No dissection was deemed necessary. I take for granted, that in no case does the destruction of the brain, the spinal cord,* the great sympathetic, the nerves, viscera, produce any diminution of the muscular force. Having arrived at this result by direct experiment, nothing remains but to investigate the physiological dynamics of the muscular system. This I do not propose to engage in at present, but simply to offer a few new experiments, with some remarks, not having time to copy old ones, much less to entertain speculative views upon the subject. It may be proper to state that the cadavera experimented on, were the victims of the now prevailing epidemic which has already more than decimated the unacclimated portion of the pôpulation of New Orleans.

I.—August 8th, 1847. J. F., resident 15 days—sick 9 days—dead 30 minutes—experiments lasted for one and a half hours after death. The mercury soon reached 101° in the axilla, and remained stationary. During this period about twenty flexions of the forearm took place from percussing the biceps, after having placed the limb at a right angle with the cadaver; the fingers rested over the centre of the chest or abdomen. The contractility after having been apparently exhausted, was, after some time, reinforced spontaneously without frictions. A blow upon the middle of the pectoralis major, caused a rapid, but slight jerk of the middle portion of the shoulder joint downward and forward—a rare phenomenon; for although this muscle is very contractile, the weight of the cadaver makes the shoulder a fixed point. The muscular *nisus*† or effort is generally strong, but for motory purposes unavailing. If friction, pressure upon the upper end of the humerus, and the weight of the forearm could be obviated, the complex action of this muscle could readily be demonstrated. On semi-flexing the thigh, so as to bring the knee nearly to the vertical, the leg hanging down, clear of the floor, and percussing the rectus femoris, the foot and leg were instantaneously carried nearly one fourth of the distance towards the vertical, but returned as quickly by its gravity. The cadaver was now turned over on the face;—the flexors of the leg were percussed—a strong *nisus*, but no flexion took place.

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and Science, busied "in the labors of love," are mute as to the *essential cause* of the epidemic. No changes of the weather can be designated as satisfactory causes of the prevailing mortality.

The highest temperature at sunrise in the air, was for July, 78°, for August, 79°—in my office 82½° and 83½°; in the ground, in a grass lot, 6 inches deep, at 3 P. M. 83° and 82°;—the river at sunrise, 85¾°, and 86°; at 3 P. M. 85¾ and 86°. The River in 1845 and 1846, reached in August, ¾° higher than in 1847. The diurnal range of the river is hardly appreciable—generally a fraction of a degree. There occurred in the present year the most extraordinary change of temperature that I have ever noticed in the river: on the 9th of August, at sun rise, the river gave 86°—the Levee 71°—the street 79°; on the 13th the river was 83°—the 18th and 22d, 83½°—the 29th 85°; Sept. 1st, 84½°.

* It might be well for the reflex school who spinalize almost every function, to call to mind that competent authorities might be cited, showing that children have been born with neither brain nor spinal cord. Can they point out a case in which the entire Ganglionic system of nerves was wanting?

† It is necessary to use some such term to prevent confusion, that is, to distinguish the effort, from its accomplishment. The effort may be intense, yet many causes, chiefly rigidity, may prevent motion.

II.—At the same time, J. W., born in Boston, aged 34, resident 9 months, dead two hours, had rigidity of the neck and abdominal muscles, with a temperature for half an hour of $102\frac{1}{2}°$ in the axilla, and $105\frac{1}{2}°$ in the rectum, while the muscular *nisus* in the pectoralis and biceps was strong, producing massive knots, but no functional motion.

III.—On the same day—J. E., a German, aged 30, resident 18 months—died at 4 P. M. The experiments began in half an hour, and lasted an hour and three quarters. *Caloricity.*—Axilla, 5 minutes 102°—5 m. 103°—10 m. 103°; rectum, 13 m. $104\frac{1}{2}°$; axilla, 5 m. $102\frac{1}{2}°$—5 m. 102°; rectum, 5 m. 104°. *Capillary Circulation.**—The veins of the arm were collapsed. A ligature was applied as in ordinary blood letting, though somewhat tighter—the veins became distended as in the living body, though the position of the arm was varied by elevating and depressing it, and finally, by turning the cadaver over; at all elevations, the distention continued with but little variation, though it was greatly augmented by moving the muscles of the forearm, as in ordinary blood-letting. The arm without ligation presented no venous distention. *Contractility.*—The arm was extended; the biceps was percussed with the ulnar edge of my hand—the arm arose to the perpendicular; the handle of the hatchet was used at intervals upon the same spot, and with a similar or rather increased effect, three or four times, after which the muscles appeared to be completely exhausted. These blows covered about one inch of the length of the flexors. I then took a piece of plank, wide enough to extend about one inch on each side of the exhausted part of the muscle;—each blow for a considerable period caused much more perfect flexions, the hand being always quickly placed on the breast; finding at length that the force was declining, I took a wider piece of plank, covering the whole length of the biceps,—upon using which, the contractions were more powerful than ever, until about twenty flexions took place. Exhaustion now quickly ensued. At $5\frac{3}{4}$ P. M., the neck became rigid. A blow caused strong and prolonged *nisus*, with a large dense knot, lasting nearly a minute before relaxation occurred, but without changing the position of the limb. The thigh, as in case I, was semi-flexed;—a blow over the rectus femoris, produced similar effects.

IV.—August 20, 3 P. M.; air of the house 88°. N. B., born in France, aged 45, resident 18 months—dead one hour. *Cadaveric hyperæmia*,† well marked—on turning the right or left cheek towards

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* See an allusion to this subject near the close of this paper.

† This was comparatively a slight case of *post-mortem hyperæmia*, scarcely transcending the average, and falling short of many. This change often begins, even before death and during the agony in dependent parts. If the cadaver be turned on the face, in a few minutes after death, it will in many cases become so discolored as to look nearly black, especially in that portion of the tissue which was the most flushed or injected in the early stage of the disease. The vast import of this change, which in many bodies allows the blood immediately after death to run through the capillaries like a seive, flowing and re-flowing from side to side, by simple gravity, has never been appreciated in pathology nor in *morbid* anatomy. A pathologist, wedded to the gastric theory of fever, cannot see without a profound sensation, the faintest discoloration of the mucous membrane, even one or two days after death, while

the floor a dark red congestion of the skin took place in the dependent parts, in from three to five minutes, and so of the limbs and body. *Caloricity.*—Axilla, 3 m. $102\frac{1}{2}°$—5 m. $102\frac{1}{2}°$—5 m. $103°$—5 m. $103°$;—rectum, 5 m. $103\frac{1}{2}°$—2 m. $103\frac{1}{2}$—30 m. axilla, $102\frac{1}{4}°$—10 m. $102\frac{1}{4}°$ *Contractility.*—Flexion of the right arm was made artificially, the hand of which was made to rest on the floor, between or beyond the left ear and shoulder, the elbow having been elevated nearly over the wind-pipe, so that the arm could not return, except by overcoming the force of gravity equal to the entire forearm, not to mention the incidental friction, which the result showed to be great. A blow with a piece of plank upon the upper part of the triceps and the outer third of the deltoid, caused the extension of the arm, though considerably short of a right angle with thebody; the forearm was dragged obliquely over the breast, flexed upon the arm, the semi-flexed fingers raking the chest; the hand came to rest on the floor near the axilla. The experiment was repeated several times with a similar result. The motion was probably due chiefly to the outer portion of the deltoid. The flexors, (perhaps always stronger than the extensors in the dead body,) did not act with much force;—a blow with the hand raised the forearm about ten degrees above the floor—with a plank ninety,—but the elevation became less and less from repetition. Three hours after death, a *nisus* only remained.

V.—August 21. A. J., born in England, aged 45, resident 9 months —dead three quarters of an hour;—observations ended the 3d hour after death. *Caloricity.*—Axilla 5 m. $104°$—5 m. $104\frac{1}{2}°$—5 m. $104\frac{3}{4}°$ —3 m. $105°$—2 m. nearly $105°$; rectum 5 m. $102\frac{1}{2}°$—5 m. $102\frac{1}{2}°$; axilla 10 m. $104\frac{1}{2}°$—5 m. $104\frac{1}{4}°$—2 m. $104°$. *Capillary circulation,* the same as in III. *Contractility.*—A blow with the hand caused a complete, but slow flexion, occupying a number of seconds;—the hand was carried to the outer end of the collar bone, where it was allowed to remain five minutes, when the limb was again extended. A bar of iron about six inches long, weighing exactly twenty-one ounces, was tied in the palm, extending to the tips of the fingers—percussion was followed by a slow, but uniform motion—the arm arose to the perpendicular—the hand with the weight descended gently to the middle of the breast bone. The force was so constant, the motion throughout its semicircular path so equable, that it was comparatively easy to estimate its velocity. It might seem incredible, that the hand, the arm, and the iron, should not be greatly accelerated by the force of gravity, after passing the vertical; it might be expected that they would, from the two forces coinciding, fall heavily and with increased velocity upon the chest, and not gently, as was the case:—Explanation is scarcely comprehended within the scope of this paper; yet, some of the phenomena in this case call for one. The following is offered as probable, if not demonstrative. The distribution of the muscular force tends to antagonize mus-

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his subject, if turned on the face for a short time, say from two to ten minutes, will, at a few paces distant, look nearly like a negro—a discoloration which is despised, because it is in the skin! In morbid anatomy, as in practice, an exclusive theory ends in sciolism. The subject here alluded to requires a distinct essay, being a pathological and an anatomical as well as a post-mortem and a physical alteration.

cles whose functions are opposite, giving a tone or passive contraction, so as to form an equilibrium so long as this power is latent and equal. Percussion destroys this equilibrium, this latency, causing an accumulation of free force like free caloric or positive electricity. This excitation induces contraction,—flexion, for example. In the mean time, the latent muscular force as well as the elasticity of the antagonistic muscles, the extensors, resist this excited force, more especially after the hand has passed its culmination, and is descending upon the breast, by which these antagonists, are put upon the stretch, so that their physiological or muscular force, combined with their physical, elastic or resilient force, may so augment as to equalize or modify the induced and gravitating forces resident in the flexors. Hence, the sum of all these forces or the resulting force, is uniform; the hand passing through equal spaces in equal times. Herein is seen a beautiful combination of physiological and physical dynamics upon the principle of mutual compensation—an aggregation of forces maintaining towards each other definite ratios—elasticity and gravity, a latent and a free force, resulting in an uniform motion. Many circumstances, however, often occur to derange this harmony of forces. It is sufficient to mention rigidity.

After an interval of ten minutes, percussion was repeated—the iron bar being still in the palm:—the arm arose towards the vertical about 45 degrees,—the induced force was insufficient to complete its orbit; but instead of falling back to its original position, agreeably to the law of gravity, the hand was *deflected* diagonally towards the hip, which it came in contact with near the floor, illustrating the same law of compound forces, that is, the free or positive force was modified by the latent antagonistic force, and by the forces of elasticity and gravitation, resulting in a physico-vital force, the mean of all these motory elements conjoined.

In less than five minutes all motion ceased in the arm; but in half an hour its contractile power had returned, and the arm was raised as in the preceding experiment, but each succeeding elevation diminished, until the force appeared to be entirely expended again, when the experiments ceased, though, possibly, the contractility may have been reinforced afterwards.

VI.—August 28. A. R., dead half an hour. *Caloricity.*—Axilla 5 m. 106°—5 m. 107¾°—5 m. 108° nearly. *Contractility.*—This was moderate, but increased for a time. In two hours it declined considerably. The right arm was now carried over the throat to the left, and was placed as in case IV; the deltoid and a portion of the triceps were struck. The arm, dragging the flexed fore-arm across the chest, was extended, but lacked twenty or thirty degress of forming a right angle with the body. This experiment was repeated several times.

VII.—August 23. J. F., born in Ireland, aged 19, resident 9 months; well proportioned, the bony and muscular systems rather predominant. *The Agony.*—Nearly pulseless; eyelids slightly parted; eyes upturned; the pupils contracted; mouth open, the under jaw forcibly drawn downward; respiration irregular, with loud stertor, causing the larynx and wind-pipe to descend and ascend to a great distance, with much force; the breathing became more difficult, and more limited to the bronchial tubes and trachea; total insensibility. In about half an hour the eyes opened with a stare, which continued until death; the globes oscillated to and fro with great rapidity, doubtlessly from the involuntary contractions of the *recti* and *obliqui* muscles, but so quickly that it was impossible to decide at any instant which set acted. These oscillations or tremblings of the eyes were preludes to a general rigidity or tetanic stiffness of the neck and trunk; in about ten minutes, the muscular equilibrium was for an instant broken, that is, the antagonism of the right side was overcome by the muscles of the left; the body curved laterally, and the eyes lost their parallelism at the same time. This muscular agony lasted from thirty to forty minutes, during which a series of indescribable wave-like contractions of the muscular fibres were seen to pass beneath the integuments, chiefly on the trunk. No entire muscle seemed to act at the same instant, but portions of its fibres oscillated convulsively and irregularly; sometimes in undulating lines, which trembled along their summits. These agitations, which were the most striking in the muscles of the eyes and of the chest, continued to augment; the respiration became more and more limited to the air tubes, in which, mucosity accumulated, causing rattles. The skin of the face and neck became congested and cyanosed. The muscular convulsion having reached its acme, gradually declined, each fibre trembled less and less. The respiration was now wholly tracheal. The pupils now dilated enormously. The impresssion produced on the observer's mind was that of a muscular convulsion, in which each muscle, if not each fibre had a particular agony of its own. The death of the lungs and heart, that is, the respiration and the circulation seemed to have been caused or at least accelerated by this general muscular spasm. The breathing and muscular agitations ceased for more than a minute, when an universal muscular convulsion or rather a shock ran over the entire body—a few respiratory gasps, not extending to the lungs, followed. The muscles ceased to quiver. The agony had lasted three quarters of an hour. Death was complete.—*Ante-mortem caloricity*: Axilla 10 m. 110°—10 m. 110½°; hand 5 m. 107°; axilla 10 m. 110°. Death.

Ante-mortem rigidity.—The under jaw was depressed by an apparently constant force, during the entire agony, keeping the mouth open. This was not a relaxation or falling of the jaw, but an active contraction of the muscles which depress it—a phenomenon, which I have seen several times in both adults and children, hours before death from fevers, not to mention a similar appearance in infantile lock-jaw. The moment the breathing ceased, the nurse, a strong man, attempted to bring the jaws into contact; the fingers were placed on and near the base of the nose and on the cheeks, the thumb under the chin;—force was applied several times for about a minute, but with little effect. The rigidity had evidently set in *before* death. I have more than once observed in the abdominal, especially the recti muscles, a similar *ante-mortem rigidity*, which seemed to have reached nearly to its maximum during the agony.

Post-mortem caloricity.—Axilla, 25 m. 110½°—10 m. 110°; bend of the arm 107°. The cadaver was now carried to the dead house—stripped—laid out in a sheet. Pupils now but little dilated; general rigidity; the muscles of the abdomen were rigid; thumbs strongly flexed within the semi-flexed index finger; the other fingers were strongly bent into the palm, requiring great force to straighten one at a time. After a number of efforts, during which the muscles and ligaments made a cracking noise, the rigidity, which was great, was finally overcome at both the elbow and the shoulder joints; the limb was artificially flexed and extended several times. The arm was now extended—the biceps struck. The flexion was perfect. The hand was carried to the abdomen with an uniform motion, three times. The fourth blow produced no effect. The heat of the axilla, an hour after death, had descended to 107¾° only.

This case may appear to contradict an assertion in the preceding part of this paper, namely, that the *rigor mortis* and contractility are contraries. Perhaps this language is too strong, or rather not sufficiently explicit. For although rigidity is an insuperable barrier to contraction, it does not always imply the absence of the contractile force. At the same time, the occasional coincidence or co-existence of these forces furnishes no proof whatever of their being *identical* in nature. Many facts and reasons could be adduced to show that they are wholly different, even when contemporaneous. But this is not necessary in a matter which is self-evident. I have on a former occasion published cases illustrative of the aberrations, not to mention the spontaneity of contractility, showing that it may decline in the cadaver for a time and then revive; and that the muscular *nisus* of the biceps may be strong without moving the forearm, owing to the *rigor mortis* prevailing simultaneously among the pronators, supinators and extensors, fixing the elbow joint. Rigidity and the contractile force, in its latent state, may for a time run parallel with each other, without affording any presumption of identity. This coincidence in point of time is supposed to be a fact wholly new, and opens a field for speculation. Broussais regarded *contractility as the fundamental principle of life:* "Contractility and sensibility are the evidences of the living state: contractility belongs to *all* the fibres; sensibility is one of the modes of action of the encephalo-nervous apparatus."* The *rigor mortis* has been viewed as the extinguisher of contractility, and, therefore, as the most certain sign of real death, putrefaction excepted. Is not this erroneous? The *rigor mortis*, like a strong man, *binds* the weaker hand and foot, but this does not necessarily *kill* him. The cords will prevent the latter from walking, but do not prove him to be dead or without motory force. In the same way the contractile force may be bound by the *rigor mortis*, so that percussion and electricity shall utterly fail to develope the natural phenomena inherent in the muscular tissue. If, therefore, contractility be the test of life, or life itself, its absence cannot always be inferred from the fact, that contractions do not follow the application of galvanism; or in other words, the great majority of the learned, in asserting that contractility, especially when excited by galvanism, is the test of life, or rather its absence the test of reality of death, assert a hypothesis only. It is remarkable, with respect to the nature of many recondite principles or ultimate facts, as death for example, that the learned and unlearned stand on a perfect equality. Philosophy may without shame

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* Princip. Phys. Med. 10. Prop. vi.

or reproach adopt, in many instances, the popular notions concerning the nature, if not the laws of life, death, etiology, gravitation, matter, mind, force, muscular power, &c.

Dr. Reid regards as self-evident, that active power exists as an attribute, but "whether it can exist in a subject which has no thought, no understanding, no will, is not so evident. Des Cartes thought matter, and a certain quantity of motion given to it by the Almighty at first, to be all that is necessary to make the natural world. Leibnitz conceived the whole universe, even the material part of it, to be made up of *monades*, each of which is active and intelligent, and produces in itself, by its own active power, all the changes it undergoes from the beginning of its existence to eternity."* No force in nature approaches the muscular force in the light of a independent and positive existence; in fact, it may be not a mere attribute, but an entity (though imponderable and invisible) in which attributes inhere, many of which have been already mentioned; motion is one of its conditions;—it may be increased, diminished, exhausted;—it may oppose or coincide with that simplest law of matter, *inertiæ;* it bears no resemblance to chemical attraction or gravitation; both of which are attributes of matter, constant and simple. But far be it from me to call such speculations, discoveries.

[†In uterine diseases, the organs secondarily affected, are for the most part, those affiliated with the ganglionic masses and their cords. A young married woman, now convalescent from an attack of yellow fever, afflicted for several months with prolapsus uteri, complains chiefly of gastric and æsophageal symptoms. Not long since, I was called to see a negress, for seventeen years a slave in the family of a merchant of this city. During her treatment, (for cholera,) it was ascertained that she had prolapsus uteri, which had existed many years. This woman, aged about 35, can give no account of the origin of the prolapsus; and never having made known her situation, she probably thought it was natural. I have not met with any case of reported prolapsus so complete as this, not even in Madame Boivin's works. The vagina is completely inverted; its tissue nearly white and dry like the skin, as is the uterus. The latter is of the usual size, and is quite as external as the male scrotum. Menstruation is regular, the mammæ normal. Her mistress, (an accomplished and humane lady,) had observed that this slave, though generally healthy, occasionally became suddenly indisposed, and once fainted. Doctors were called in, and the girl was treated for disease of the heart. The symptoms did not indicate *eccentric or other disease of the spinal cord*, but disturbances among the organs associated with the ganglionic system. I have never seen in any disease vomitings so obstinate, as in some cases in the advanced stage of cancer of the uterus. Now according to the reflex system, all these maladies are, or ought to be *eccentric* diseases of the cord. *Eccentric* tetanus, *eccentric* convulsions, &c., might be expected, or at least, strongly developed secondary spinal affections.]

Cases I and II, in which the phrase, *Post-mortem capillary circula-*

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* Es. I.

† This paragraph was omitted in the proper place. It relates to the reflex speculations on obstetrics.

tion is used, by no means give a tolerable illustration of the doctrine indicated, in relation to which, I possess ample experimental proof.—Although I am engaged in preparing a medical work which absorbs my time, yet, the reception which my "Researches on Post-mortem Caloricity"—"Researches on Post-mortem contractility," &c., have met with, is a strong inducement with me to offer before many months, "Researches on Post-mortem Capillary circulation," not so much to establish its physiological truth, as to develope its fundamental importance in the pathological anatomy of fevers, and, indeed of most other maladies of an acute character, in which hyperæmia, congestion, inequilibrium of the blood, constitute a leading principle;—all of which may in a few hours, nay a few minutes after death, be modified, or even obliterated by the post-mortem action of the capillaries. Whether the experiments and the doctrines of the post-mortem capillary circulation, which I may offer, will draw upon me another controversy, time will show.* Lest a question of priority should arise, I now state, that my experiments began in 1841;—the germ of the doctrine, illustrated by a number of cases, will be found in the Western Journal of Medicine, for April, 1843.

There is a grand hiatus to be filled up in physiology, pathology, and morbid anatomy, comprehending the agony, the general death, and the first hours thereafter—a brief era, which, nevertheless, presents a concentration of phenomena, not to be learned thirty-six hours after death, (the usual period of European dissections)—a period which presents three principal points of departure, namely, caloricity, contractility, and capillary circulation, not to mention gravitation, imbibition, coloration, and other changes antecedent to putrefaction.

I intended to conclude with a rapid survey of the functional and structural diseases of the contractile tissues; but this paper is already too extended.

I will only add, that among the earliest symptoms of yellow fever, is a muscular aching like that which follows excessive exercise,—described by the patient as soreness of the flesh, as if tired all over, particularly in the great muscles along the back which sustain the trunk, and in those of the legs. In the latter, cramps are not uncommon. This muscular *malaise* is not simply a loss of force or debility. The patient's strength often is considerable until the close of life; nevertheless, as a preventive and as a means of cure, *perfect muscular quietude* is of the utmost value. Muscular apoplexy sometimes takes place in yellow fever, impeding motion, and causing much uneasiness, and even preventing the extension of the limbs. Masses of coagulated blood are found among the fibres, but more generally in the interspaces of the

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* I can scarcely hope that the reader will admit, to the fullest extent, my dislike to controversy, seeing that I have been so much engaged in it; yet, I have never acted without the advice of those who were more competent to judge than myself; though, my friends are not, of course, answerable for the manner in which I have acquitted myself. The controversy, as to the originality of my researches on Febrile Caloricity has resulted in the magnanimous concession of the chief point contended for by me, in opposition to Dr. Ranking, of London, as may be seen in that gentleman's valuable Abstract;—(ii. 246, June, 1846,)—thanks, to the disinterestedness of Professor Lee, of New York, and to the Editors of the New Orleans Medical and Surgical Journal.

muscles, varying in quantity from minute points to several pounds. The fibre is never pale; on the contrary its color is usually increased. No lesion of the human body is more curious than that of the *muscular tissue of the bowels*, in fevers of an acute character in New Orleans, and although I have the materials for ascertaing its proportional, or rather its approximate frequency, I have not the leisure to *count*. This lesion, chiefly of the cæcum, colon and rectum, consists in *the firm contraction of the bowel into a round cord, elastic, white, bloodless, obliterating* not only the *cavity* of the great intestine, but even its *notchings, puckerings*, and *sacculated pouches*. Another lesion, that is, *intus-susception*, though less frequent, is doubtlessly a *muscular* disease or *irregular action* of the muscular coat of the small intestine. A careful dissector will occasionally, perhaps frequently find from two to six complete *intus-sussceptions* in the same subject—one portion of the bowel having descended within another several inches.

I give a case from memory, not having time to search the original MS. A stout, young Scot, taken with the yellow fever in the evening, was soon after bled largely by an apothecary, and was freely purged with senna and salts. Next morning his physician ordered that blood-letting should be repeated, until fainting supervened. This required fifty-four ounces of blood. In the evening I saw the patient. He said he was sinking and would die, that he had a strong *nisus* or straining in his bowels since the blood-letting, but could pass nothing. His strength was such that he got up, and endeavored in my presence, for ten or fifteen minutes, to evacuate the bowels, but in vain. He died during the night. The post-mortem examination, which I made the next morning, showed that the bowels were completely empty, but obstructed by six intus-sussceptions.

II.—*Obstetrical Memoranda. Partial occlusion of the Os Uteri during Labour, treated successfully by Incision. Rigidity of the Os Uteri treated by Injections of Belladonna.* By A. H. Cenas, M. D.

Rigidity of the os and neck of the uterus during the course of labour, is by no means a rare occurrence, and offers sometimes no inconsiderable obstacle to its safe accomplishment. But it is not, generally speaking, a dangerous complication, and is in a great majority of cases easily overcome by appropriate general or local means. There is, however, a source of rigidity sometimes met with, more serious in its character and exercising a more important and sometimes fatal influence upon the progress of labor; I mean rigidity with more or less complete occlusion of the os uteri. This rare condition may be congenital, but it is more often the result of previous disease of the parts, occurring as a consequence of protracted or instrumental labor—and when met with must be boldly treated by free incisions—otherwise exhaustion, laceration, or extensive and fatal sloughing may take place, either before or after the labor is accomplished. In the course of my obstetrical practice, I have met with but one such case—and in which

I pursued the bold practice above alluded to with the most satisfactory results.

The patient was a young woman of about 25 years of age, who had been the subject of instrumental delivery in her first labor, the result of which was inflammation and sloughing to a serious extent—I found her advanced to about the 4th month of her second pregnancy, and suffering under active uterine contraction, with considerable hæmorrhage.—Examination per vaginam detected a preternatural hardness of the whole neck, and the os uteri which was dilated to about the size of a *dime piece*, conveyed to the finger the sensation of a firm structure—the uterine contraction having not the slightest effect upon it. As her pulse was firm, and skin hot and dry, I bled her "*ad diliquium*," and kept up the impression with nauseating doses of tart. emetic for nearly six hours, without effect. The pains, however, increased in violence, and an arm of the fœtus was actually cut off by the sharp and firm edge of the os uteri and expelled; fearing the worst from such powerful and unavailing contractions, I determined to incise freely the structured os uteri, which with the assistance of my friend, Dr. Le Monnier, I effectually accomplished, and had the satisfaction of seeing the balance of the fœtus expelled in a short time afterwards. My patient suffered very little from the operation, and soon recovered her usual health.

Simple rigidity, as I said above, does not often resist general means, such as venesection, when the patient admits of it, or nauseating doses of tart. emetic either with or without opium. But sometimes even these means fail, and then we have a precious recourse in belladonna, which in my opinion and experience dilates the os uteri as rapidly and effectually as it does the pupil of the eye.

CASE 1.—I was called to Mrs. G., aged about 19 years, in labor with her first child, she had been suffering already nearly 12 hours, and the os uteri thick, hard and dry was dilated scarcely more than the size of a half dollar piece—pulse being full and skin dry, she was bled to fainting, and the impression kept up with tart. emetic. But discovering little or no effect after the lapse of six hours, though the skin had become cool and relaxed, I applied belladonna freely, and in little more than one hour the labor was over.

CASE 2.—Was also a primi para, and the patient, aged about 20 years, had been in labor about 14 hours before I was sent for. I found the contractions active, and the os uteri about the size of a quarter dollar, thick and hard, though the head was low down in the pelvis. As general means seemed here indicated also, I bled usque ad diliquium and gave solution of tart. emetic, without effect for nearly 4 hours. A liberal application of belladonna, as in the above case, terminated the labour in less than one hour after its use.

CASE 3.—Was a premature labor. 7th month of gestation, and was brought on by a fall. The patient had been in active labor under the charge of a midwife before I was sent for. I found the os uteri dry, hard and diffused, and open to the extent of about half a dollar. Tart. emetic used as in the other cases, producing no effect, I resorted to belladonna, and as pains seemed flagging, gave ergot—under the combined influence of which the case was terminated in little more than an hour.

Case 4.—Was also a premature labor—patient a prime para and term of pregnancy about sixth month. Uterine action was brought on by a fall, and the contractions had been pretty active for several hours before I was called. On examination, found the common dryness and heat, the head descending rapidly in the pelvis, covered by the hard and rigid neck, and the os uteri felt so small and wiry, that I feared incisions would be necessary.

The bleeding and tart. emetic to nausea and the effects kept up for some time, seemed to produce some influence upon the parts—but after a few hours, finding no further benefit resulting, I applied the belladonna; which in this case seemed to act more readily than in any previous one in which I had used it, for the labor was terminated in little less than one hour after its application—showing its rapid influence not only upon the os uteri, but also upon the soft parts below, viz: the perineum and os externum.

The foregoing cases are only a few of many similar ones occurring in my practice. They are not given as novelties, for the same results obtain every day where the belladonna is efficiently used. But as the profession is by no means agreed upon the obstetric value of this agent, and as some high authorities have even pronounced against it, I am disposed to think that its mode of application has a good deal to do with its efficiency, for we know that the usual method of applying it is either in extract or ointment of the extract, carried on the end of the finger to the parts to be acted upon. But this plan is uncertain, and for this reason, that the medicine is wiped from the finger during its passage through the vagina, and little or none of it reaches its destination—hence, in my opinion, the frequent failures and disappointments complained of. Now, in order to obviate this inconvenience and secure a direct contact of the article, I have been in the habit for some time back of employing it by injection, either in the form of a watery solution of the extract, or as an ointment sufficiently fluid to answer the same purpose. The instrument used should be an ordinary vagina syringe. Employed in this way I am pretty sure that very few disappointments would result.

In relation to the above method of applying belladonna by the os uteri, viz: by injection, I would remark that I was under the impression until lately that it was peculiar to myself. But I have within a short time had occasion to learn that Professor Eve, of Georgia, has recommended precisely the same practice. Although I find myself shorn of the merits of originality in this matter, yet I am highly pleased in having the testimony of such excellent authority in behalf of its value.

July 12, 1847.

III.—*Four Cases of Cerebro-Spinal Meningitis.* Reported by Charles Chester, of Union County, Arkansas.

The following cases occurred in my practice upon the plantation of Mr. S. Harper, in this county, which I have diagnosed *Cerebro-Spinal Meningitis.* If you think them worthy of notice in your Journal, they are at your service.

The first case, a negro boy, aged 12, was taken about 11 o'clock, A. M., February 11th, with rigors and violent pains in the head. Supposing it an ordinary case of chills, (which were prevailing on the plantation at the time,) no attention was paid to the boy until night, when he was found in a comatose state; head flexed back upon the spine, followed by convulsions and death in a few hours.

This case I did not see, but learned the particulars on being called to the plantation the following day to visit the overseer.

CASE 2.—February 20th. Called to see negro girl Ann, aged 20, on the evening of the third day of her sickness—learned that she was taken on the 18th with a chill, followed by high febrile reaction and delirium—found her in the following condition: pulse 60 and irregular—extremities cool—tongue thickly coated and dark—partial delirium—intolerance of light—uttering loud and repeated cries, "*my head, my head.*" *Pres.:* Cups to back of the neck—cold applications to head—hot mustard pediluvia and stimulating purgative enema—at bed-time cal. 20 grs., Ipecac. 6 grs.

21st. Medicine operated well—passages dark, viscid mucous. Gave ol. ricini, applied sinapisms to extremities and spine—continue cold to the head—still complains of pain in the head.

22d. Head somewhat relieved—violent pain in nucha, convulsive mobility of the muscles of the face—double vision—tetanic spasms of the opisthotonos variety. *Pres.:* Blister to nucha, to extend some distance down the spine—calomel 20 grs.

23d. Pain in nucha relieved—tongue commenced to clean—head still flexed back—convulsive mobility of the muscles of the face still continues—gave cathartic of sulph. magnesia.

24th. Much better—head relieved—tongue nearly clean. Patient continued to improve for about a week—when she was attacked with gonorrhea—complained of aching in her bones, headache. some return of convulsive mobility of the muscles of the face—tongue became coated and dark. The disease assuming a typhoid character, with evening exacerbations of fever—in which condition she remained until about the middle of March, when she again became convalescent. The treatment during the typhoid stage consisted in revulsives in the form of blisters and setons—alterative doses of blue pill and the sulph. of quinine.

CASE 3.—Parthena, a negro girl, aged 18 years, taken with a chill on the 22d of February, followed by very slight febrile reaction—saw her on the morning of the 23d; found her with pulse slow and irregular—skin normal—tongue furred and dark—nausea and vomiting of bilious matter resembling the yolk of eggs—uttering the most distressing cries—my *head!* my *head.* Prescribed emetic of Ipecacuanha—cups to back of the neck—hot mustard foot-bath and cold to the head—at night cal. 40 grs.

24th. Had a restless night—bowels constipated—no operation from the medicine—still vomiting—vomited 5 large lumbricoid worms—gave stimulating enemata—sinipisms to epigastrium and extremities, and repeat cal. 40 grs.

25th. Violent pain in the nucha—tetanic spasms of the opisthotonos variety—bowels still constipated. Gave oil ricini which produced some small evacuations of a dark viscid mucous.

26th. Still complains of pain in nucha—has some small operations from the bowels—pulse more frequent and regular—applied blister to nucha, and gave blue pill, 8 grs.

This case assumed rather a typhoid character, with slight pain in the head and nucha—pulse about 100—extremities and skin cool in the morning, with slight evening exacerbations of fever, when the skin was rather above the normal temperature without much variation in the frequency of the pulse—no relaxation in the muscles of the neck—when interrogated how she felt, her uniform reply was, *right smart*, she continued in this condition until the 12th of March. The treatment during this stage consisted of blisters to the posterior parts of the scalp, and down the spine. and to the lower extremities—alterative doses of blue pill and sulph. of quinine during the remissions of fever. On the 12th of March, she spent nearly the whole day and night in singing.

13th. The muscles of deglutition were paralyzed—the jaws firmly clenched—eyes constantly rolling about in their sockets, without manifesting any signs of consciousness—the pulse was slow and weak—extremities cold. Prescribed stimulating applications to extremities and gruel enemata.

14th. Same condition. 15th, no change—repeat prescription.

16th. No change—had the whole body immersed in a warm red oak bath—poured boiling water upon the ankles, and applied the actual cautery to the spine, when she manifested some signs of consciousness.

17th. Has continued in profuse perspiration since the bath—ankles finely blistered, pulse better.

18th. Still continues in warm perspiration—respiration growing shorter—dies in the evening.

Post-Mortem appearances on the 19th: The substance of the brain normal. The pia mater was deeply injected over its whole surface, and the posterior portion gorged with blood—between the pia mater and arachnoid there was a bloody serosity and an effusion of bloody serum in the ventricles. The abdominal and thoracic viscera were not examined.

Case 4.—Bella, a negro girl, aged 7, was taken on the morning of the 28th of February with a chill, followed by high fever and violent delirium—holds both hands upon her head, uttering the most distressing cries, "*my head, my head;*" tongue coated and dark—pulse full and hard. Prescribed venesection at the arm—cathartic of calomel 12 grs.

March 1st. Medicine operated freely, passages dark, viscid mucous—delirium still continues, pulse irregular and frequent, extremities cold. *Pres.:* put her up to the hips in warm mustard bath—at the same time shaved head and applied cold—cups to back of the neck—at night repeat cathartic of cal., 12 grs.

2d. Medicine operated well—head relieved—great pain in nucha—tetanic spasms—intolerance of light, &c. *Pres.:* applied blisters to back of the neck and ankles.

3d. Great pain in nucha, pulse irregular and full, extremities cold—gave carthartic of castor oil, and keep warm applications to the feet.

4th. Pulse more regular—tongue improved—pain still continues in the nucha—blisters on the ankles did not vesicate. *Pres.:* applied blisters to inside of the thighs—warm applications to the feet, and repeat cathartic of cal. 12 grs.

5th. Pain still continues in nucha—head still drawn back—pulse regular and 80—medicine operated well, wants to eat.

6th. Still violent pain in nucha, and some febrile excitement—tetanic spasms still continue—appetite good.

This patient continued to have some pain in nucha, her head drawn back upon the spine with evening exacerbations of fever for about two weeks, when she gradually recovered her health—during this time her appetite was good, her bowels regular, and dejections normal.

The symptoms in each of these cases were various—yet there were certain general features which the most careless observer could not fail to recognize. In every case on the 3rd or 4th day there was an eruption upon the face, which in a few days again disappeared. The cases all occurred among negroes who had recently arrived in the country.

With regard to the treatment, it requires to be energetic and varied according to the symptoms present. In the first stage free blood-letting, followed by mercurial cathartics—sinapisms to the extremities—with local bleeding by cups and leeches, would seem to promise more benefit, than any other class of remedies—and if these were energetically applied, might not the second or typhoid stage be avoided?

With regard to the sulph. of quinine, I would remark that although in the second stage the fever was regularly intermittent, and I exhibited it for several successive days, (20 grs. during an intermission,) yet I could see no benefit derived from its use.

Union County, Arkansas, August 16th, 1847.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*The Medical History of Alabama.* By P. H. Lewis, M. D., of Mobile.

This is a paper of 73 pages, to which a silver cup offered by the Alabama Medical Society, for the best essay on the subject named in the caption, was unanimously awarded in December last. It was originally published in the May, July, and September Numbers of the New Orleans Medical and Surgical Journal, and now appears in a separate form.

The undertaking we are sure will be considered a most laudable one, and no little credit is due the society for their correct discrimination in regard to matters of most interest to the physicians of the South, displayed in the selection of the subject for emulation, as well as to the author, for the able manner in which he has accomplished the task.

Dr. Lewis has collected, at no small amount of labor, a vast number of facts, and much interesting information relative to the character of the diseases of the State during its earlier settlement, the sources of which must necessarily in a few years more have been in a great measure lost or unattainable. The paper is one not only of much immediate interest to the physicians of Alabama, and indeed of the South generally, but must prove also of great value for reference to others who may hereafter engage in the investigation of Southern diseases.

Dr. Lewis's information, besides what he gives from his own experience, has mostly been gathered from the recollections of some of the elder and more accurate medical men of the State,—indeed in some instances seemingly from reliable persons not connected with the profession,—from the few publications which have appeared in the various medical periodicals, or in a separate form from the pens of the medical men of Alabama at various periods;—and in some instances from the passing remarks of the newspapers of the day, in which notice may have been taken of the more malignant epidemics.

Having now expressed our opinion of the merits of the paper in general terms, we will proceed to make occasional extracts; which, while they may be presented as fair specimens of the author's style, will also afford occasion for a passing comment. It is possible that in so doing we may in more instances select those, in which we may differ in opinion with the author, than those in which our sentiments accord with his, while in the expression of such difference, we do not think there is any inconsistency with our general opinion of the high merits of the paper.

Dr. Lewis says, "We find in Alabama, that although the diseases may belong to the same family, yet they are distinguished by certain differences, as apparent, as are the physical characteristics peculiar to the regions where they respectively prevail.

"Finding that this connection and dependence every where exist, and the influence which physical geography exerts, independent of latitude, is not less apparent, the writer is forced to the conclusion, that there will be no further advance in the science of ætiology without the aid of geology and chemistry." Dr. Lewis is perhaps rather hasty in the conclusion, at which he here arrives, for although few will be found to deny the influence of locality and peculiarities of soil, &c., in modifying what may be called the standard characteristics of diseases, the number is still smaller of those who would be willing to admit "that there will be *no* further advance of ætiology, without the aid of geology and chemistry," nor do the premises at all warrant so broad an assertion. There are several other inconsequent conclusions in the paper, which perhaps, however, are less indicative of a correct understanding of the subject—and of a logical mind, than of the fact that the *paper had to be finished by a given time.* Simply these we look upon as evidences of haste in the weaving together of the authors facts,—the material of the paper, after collection.

The author gives a brief, but seemingly correct description of the geological peculiarities of the State. Describing the characters of the soil of the different regions into which it is divided; to which, as may be inferred from what has just been observed, he attaches considerable importance, and not without some reason perhaps, as producing a modifying effect upon diseases, or rather, indeed, as giving to those of each region any peculiar features which they may possess.

Dr. Lewis divides his history into three periods, during each of which the diseases prevalent were characterized by distinct and peculiar features, the causes of which he attempts to account for; but we fear that in this, his explanation will not be considered entirely satisfactory. We may recur to this again. He says "after a careful review and study of the regular gradation of diseases, we will so far anticipate inevitable conclusions, as to divide the proposed history into three distinct epochs, viz: the Ataxic, Phlegmonic, and Adynamic."

The first epoch—the ataxic—dates from the first commencement of emigration to the State, which it seems was very inconsiderable prior to the year 1812, down to 1819, a period of about seven years.

Having always ourselves looked upon the word ataxic as exceedingly indefinite when used as characterizing a state of disease, we deem it the better way to give such extracts from the paper, as make allusion to the diseases and the symptoms marking them during this epoch, that the reader may himself judge as to the fitness of the term used, and its accordance with the symptoms named.

"The character of fever prevailing in these years consisted of the various types of intermittent, with now and then, in mid-summer, the occurrence of bilious remittent; quotidians in the Spring and first of the

Summer, remittents towards the close of Summer, with tertians and quartians in Autumn, and frequently continuing through the Winter."

"The remittent fevers of this day were exceedingly mild, yielding readily to the operation of one emetic, and not unfrequently to the spontaneous discharge of bile and copious sweats that usually occurred in the first paroxysm. In the most serious and obstinate cases, the intermissions or remissions were irregular and incomplete, the hot and sweating stages rapidly succeeding each other, and the chill or cold sensation becoming less distinct with every revolution."

"If in the neglect, so often attending the treatment of non-malignant diseases, these fevers were suffered to continue, they not unfrequently glided into a continued irritative type, attended with great *nervousness*, twitching of the muscles, general emaciation and debility, constituting what in those days was termed "*nervous fever*." We imagine that these latter cases were not unlike many that occur in the present day. Indeed it is no very uncommon thing in some parts of the State to see cases of remittent fever, which at first perhaps were rather of a mild character, by neglect or maltreatment assume the appearance here described, and in fact, the terms "nervous fever," "typhus fever," and "typhoid fever," are not unfrequently applied to them in this stage;—though, on careful inquiry, the peculiar characteristics of remittent fever may always be discovered to have marked their early stages.

But to proceed. "The cold stages of these intermittents continued for many hours, attended with gaping, stretching and incessant shaking. It has been stated that in these fevers there was no danger in their primary stages;—inquiry has, however, brought to light a few exceptions, to which attention is here invited. After several hours continuance of a hard ague, the rigors and shaking would suddenly cease, and instead of the usual sign of reaction and fever, the physician would be surprised to find the body bathed in a copious perspiration, slow compressible pulse, *cool*, bluish tongue, sunken, careless expression of countenance, with a cessation of all pain and anxiety. In a few hours, the tendency of this condition was made manifest by a deepening of the foregoing symptoms, together with insensibility, swelling of the abdomen, and involuntary discharges."

During this *epoch* the country was thinly inhabited, and it seems was "inadequate to the support of scientific physicians." We are not therefore surprised to learn that "tartar emetic, given to emesis, was extensively used; as was also, especially in remittents, the Boneset or eupatorium perfoliatum. But Calomel and Jalap to "pass off the bile," and peruvian bark to prevent the occurrence of the paroxysm, was the most uniform and successful system of practice."

So much for the symptoms and therapeutics of the ataxic epoch. We will now examine some of the circumstances which, in the opinion of Dr. Lewis, gave to the diseases of this epoch their peculiar features, by which they are distinguishable from those of the succeeding epoch.

"As a consequence of these changes (spoken of in the paper,) of atmospheric and terrestrial temperature, the rapid increase and decay of vegetable and animal matter, together with physical causes not enumerated, such a change in the gaseous emanations is doubtless produced, as tends to designate the character of diseases at various periods of time."

"Connected with this matter there is at the same time an altered condition of the constitution and temperament, as man advances from a state of native simplicity, to the refined and luxurious habits that wealth can command. In that primitive state of existence the mind was unfettered by corroding cares, the articles of food simple in their nature, and the oaken couch was as soft to the wearied husbandman as a pillow of down—then the robust frame enjoyed an almost uninterrupted condition of health, and great longevity marked man's pilgrimage on earth."

"But when refinement slowly and stealthily creeps into his habitation; when the rude cabin obscured in the tangled vines of the wildwood, is exchanged for the more modern mansion of the exposed and cultivated plain; when the unlicensed freedom of sensual indulgence, with the pampered appetite and excess of libation reign supreme,—then is forcibly marked the cause of disease, tending to reduce the once stalwart man to a state of decrepitude; and the prattling urchin, instead of the glow of health that once sat upon his cheek, now presents the aspect of refined infirmity, laying the foundation of an early grave."

"To these various causes can be traced a gradual change in the character of disease from the first occupancy of our country to the present day."

"The early settlers came mostly from Georgia, Carolina, Virginia, and Tennessee. Owing to the fact that these immigrants brought but few laborers with them, they chose the uplands and hammocks, rather than encounter with a feeble force the rich and heavy timbered alluvion of the river swamps."

Simply then we must infer, that the circumstances principally which contributed to the comparative good health of the earlier settlers of the State were their avoidance of the "rich and heavy timbered alluvion of the river swamps," the small extent of lands as yet brought under cultivation "exposing the virgin soil to a tropical heat," and consequently the comparative small amount of poisonous material disengaged; as also the absence of that enervation of the system, produced by sensual indulgence, and "the refined and luxurious habits that wealth can command."

Though the facts, relative to the character of the disease, prevalent during the first and second epochs, the "ataxic" and the "phlegmonic," cannot be thereby invalidated, we must deem the explanation of Dr. Lewis, anything but satisfactory. Thus the diseases of the first epoch we must of course suppose to have been characterized by ataxic symptoms; while those of the second, as the name would imply, are described and spoken of as of a highly inflammatory character, and yet we are reminded and forcibly impressed with the idea, that during the ataxic epoch the inhabitants were generally possessed of the finest constitutions; that in this "primitive state of existence the mind was unfettered by corroding cares;" and that "then the robust frame enjoyed an almost uninterrupted condition of health, and great longevity marked man's pilgrimage on earth." Surely in such subjects as are here described, we would naturally expect disease to assume a high *inflammatory* character; though the fact seems to have been otherwise—according to the information collected by Dr. Lewis. Still, on what grounds he offers this condition of stalwart health, in explanation of, or as among the causes of the *ataxic* character which the diseases of this epoch assumed, we cannot comprehend.

In regard to the *phlegmonic* epoch, we are informed that by this time "the rude log cabin had in many instances given place to the stately mansion," and it is to this period that the author alludes, when he speaks of the *enervating* habits and sensual indulgence at the command of wealth," tending to reduce the once stalwart man to a state of decrepitude; and the prattling urchin, instead of the glow of health that once sat upon his cheek, now presents the aspect of refined infirmity, laying the foundation of an early grave."

Another quotation;—"And again, we should not forget, that there is a principle well established in the progress of society, that as wealth accumulates, so luxury creeps into the favored domicil, and under the imperious sway of sensual enjoyment the wealthy inmate is tortured with a protean disease, unknown to the tenant of an humble mansion. From the causes thus briefly alluded to, *we may distinctly trace, on rational conclusions, that marked change in the character of disease, so fully exhibited during this important epoch.*" Now, we can conceive it possible, that circumstances might conspire to produce a tendency to high inflammatory action, even in the prevalent diseases of a period, or in a condition of society, in which luxury and sensual indulgence had tended "to reduce the once stalwart man to a state of decrepitude"—but must think the author decidedly at fault, as advancing indeed a doctrine contrary to one of the most firmly established principles of medicine, when he offers this very state of decrepitude and deteriorated health in explanation of, or at least, as among the causes of the high inflammatory symptoms, characteristic of the disease of this epoch.

We cannot but look upon it as a matter to be regretted that the Dr. could not so have arranged his *facts*, that those circumstances "tending to reduce the once stalwart man to a state of decrepitude, &c., should have been in operation during the ataxic period; and equally so, that it could not have been so arranged that during the "*phlegmonic period*" the mind should have been unfettered by corroding cares;" and that "then the *robust* frame should have enjoyed an almost uninterrupted condition of health, and great longevity marked man's pilgrimage on earth." But facts, of course, cannot always be bent to meet our wishes. *En passant*, we must recollect, that this ataxic epoch during which "*great longevity* marked man's pilgrimage on earth," (if we have not misunderstood our author's statements)—extended only through *a period of about seven or eight years*, from the time emigration to the State first commenced, which was very limited prior to the year 1812,—as the Doctor informs us,—down to about 1819.

We will here point out a slight seeming inconsistency, but in doing so must again remind the reader of the *vigorous constitutions*—according to the statements of Dr. Lewis, possessed by the inhabitants of the State during the first epoch; "that then the robust frame enjoyed an almost uninterrupted condition of health, &c.; and yet the Doctor, in accounting for a condition somewhat resembling congestive fever, which occasionally followed a protracted ague during this epoch, says, "we find in these cases the mischief to consist in the shock given to the system by the violence of the ague, when in an *atonic* condition." No other explanation is given, no reference whatever to any circumstances in which previously these patients differed, as regards health or constitution, from others; and we are consequently at once thrown back upon the condition of constitution which characterized the epoch. We are irresistibly impelled to the remark, that by a few paragraphs of the essay before us, we are forcibly reminded of the "cross readings" with which newspaper editors occasionally amuse their patrons.

In the commencement of this notice it was observed, that our extracts would be such as might be presented as fair specimens of the author's style. This to a certain extent we must now retract, for the quotations which have formed the basis for the last few remarks are not such. Indeed we are perfectly convinced, it was not owing to an *effort* on the part of the author, that such sentences, (of which there are several others of a similar character, more nearly allied to the inflated style of modern romance, than to the simple and concise manner suited to the investigation of subjects of scientific interest,) crept into the essay; but to a circumstance to which we have already alluded; the hasty, the "*Currente Calamo*" manner in which the material of the essay was woven together after its collection.

The second epoch, the phlegmonic, embraces the period between 1818 and 1833. We are told that "about the years 1817, 1818 and 1819 the town of Cahawba increased very rapidly in population." It is altogether probable that it was about this time, too, that the towns and villages generally throughout the State began to spring up or increase in population—the agricultural settlers, prior to this time, having constituted much the larger proportion of the inhabitants.

It is a fact well known, that while the towns and villages of the State have been peopled principally by emigrants from the Northern States and Europe—the agriculturalists, without exception almost, were during the first epoch, at least, from other Southern States; as for instance Georgia, Carolina and Virginia, where the prevalent diseases, though mitigated in severity, were of a character corresponding with those of Alabama, produced by similar causes in diminished force;—for we cannot agree with Dr. Lewis in his remark. that in those States "the seed of infection had been exhausted."

Dr. Lewis seems to have been aware of the difference in the localities from whence emigrated the principal portion of the settlers of the first epoch, and of those of the towns and villages at least of the second; for in speaking of the first epoch, he observes, that "the early settlers came mostly from Georgia, Carolina, Virginia, and Tennessee;" while in speaking of the second epoch, he says, "the population of these towns was composed in part of emigrants from the Northern States of Europe," —yet he attaches much less importance to these facts combined, than they would seem to merit, as exercising a modifying influence in the amount and character of disease during the two epochs;—no doubt owing to his belief, that "the seed of infection had been exhausted" in the States from whence emigrated the principal part of the settlers of the first epoch.

In confirmation of the opinion which we have expressed, that the seeds of infection were not exhausted at this period in the States from whence principally the settlers emigrated—we have but to refer to the accounts which are given in the periodicals of that day, and subsequently of even severe epidemics of Autumnal fever in these parts. Indeed, scarcely a year has elapsed since the commencement of emigration from the other States to Alabama, for which we cannot find in the medical periodicals distinct accounts of Autumnal fever, as occurring in some of them.

In 1821 and 1822, in Cahawba, we are informed that the "mortality was not less than twelve per cent. of the entire population, which far exceeded that of the country settlements."

The remittents of this epoch "were ushered in with a sensation of coldness, slight in degree, but often long continued, with restlessness, thirst, and *burning of the muscles;* this condition soon passed into one distinguished by intense heat and dryness of skin—excruciating pain in the head and back, white furred tongue, frequent *tense* pulse, restlessness and irritability of temper. In 18, 24 or 48 hours, during which time the lancet and antimony were used freely, a moderate perspiration with a decrease of heat and moderate pulse ensues. This abatement of distressing symptoms, called a remission, rendered the patient so comfortable when compared to his previous suffering, as often to flatter him that a happy crisis had approached, but in a few hours there was, without any sensation of coldness, a renewal of the febrile symptoms even more intense than before. After the second or third paroxysm, which were irregular in duration, the disease gave way under the influence of copious evacuations of thick black matter, and a general perspiration. In some cases, however, after one imperfect remission or a momentary calm, the symptoms became indicative of the most aggravated character of disease, the pulse becoming strong and corded, delirium of the most extravagant character attending. These cases continued many days—yielded in the end to the heroic practice of the times, spontaneous active hæmorrhages, or unfortunately resulting in organic disease, or after the departure of the fever leaving the patient in a fatal collapse."

"In other cases, again, after the second or third paroxysm, the tongue becomes fiery red, pulse small, frequent and wiry, extremities cool, constant thirst, retching to vomit, great restlessness and delirium."

"But the most common form of obstinate and dangerous fevers throughout this period, was the continued. They took place usually without chill or sensation of coldness; the pulse, slow at first, gradually became corded and bounding; the skin increased in temperature till it became burning hot; eyes injected and suffused; pain in the head, back and joints; restlessness, thirst, and some times nausea took place. Towards the evening after the first day, delirium made its appearance. * * * * * * These fevers were usually called bilious inflammatory—and owing to the violence of the vascular excitement, local inflammation often occurred. Spontaneous hæmorrhage from the nose, and black grumous discharges from the bowels were frequent, and always attended with happy consequences."

The blood was cohesive, and "not unfrequently the crassamentum was covered with the buffy coat, as in cases of local inflammation." * * * * * "The liver was distended with blood—the spleen engorged and sometimes softened, and the membranes of the brain and stomach giving evidence of pre-existing inflammation."

The treatment during this epoch "was bold and prompt, corresponding perfectly with the violent undisguised character of disease which the physician had to combat. Blood-letting, emetics, cathartics, calomel and jalap, with a constant *stand by* of the pulvis antimonialis were then the Sampsons of the art."

Peruvian bark we are told—"which had been in high repute in the treatment of the diseases during the preceding epoch, was given during the remissions of these fevers with unsatisfactory results."

Dr. Heustis, the most authoritative writer of his day, perhaps, in Alabama, gives an account in the American Journal of Medical Science, of an epidemic which occurred in Cahawba in the Autumn of 1831, being in the "phlegmonic epoch," which we think does not exactly sustain the author's views relative to the highly inflammatory character of the diseases of that day; nor in the epidemic spoken of, at least, do we find him speaking in any thing like high terms of the use of the lancet. He says, "the fever this season was of the congestive character, with a great disposition to an unequal distribution of febrile action and development. In many instances the head, and particularly the extremities would be cold, while the trunk was hot: whilst at the same time there was considerable palpitation of the heart, with a pulse small, weak and frequent, sometimes after a transient state of febrile excitation, collapse took place at an early stage of the disease, with little impairment of the general powers of the system; this was more especially the case when depletion had been too freely practised. * * * * * The protraction and collapse are for the most part confined to the vascular system, while such is the strength of the muscles of locomotion, that the patient is able to rise and set up, and even to walk about. The cases in general did not admit of the *free and liberal extraction of blood*, and for the most part the loss of four or five ounces was sufficient to produce a reduction and softening of the pulse." Even in the exceptional cases in which Dr. Heustis supposes that bleeding was highly useful—he admits that "the quantity required at any one operation was small, seldom exceeding 8 or 10 ounces. If the extraction was carried much beyond this, there was danger of sinking and alarming prostration."

These remarks of Dr. Heustis, we will add, were relative to the general characteristics of the epidemic of 1831, as it occurred at Cahawba.

Before entering upon the third or adynamic epoch, we will turn back to make an extract from the first epoch.

"After tracing disease from its mild incipient action of early days, through the high toned phlegmasial of later times, until we arrive at the low state of typhoid affection that marks the character of all disorders at the present day, it will be discovered that the above division, so far from being the result of an hypothesis, is naturally and imperiously demanded." We would ask what does Dr. Lewis mean here by "typhoid affection." He cannot, of course, we must believe, have reference to the disease known under the names of the "typhoid affection," "typhoid fever," "dothinenteritis," "follicular enteritis," &c., for no one who is at all familiar with the diseases of Alabama, could be so far led astray, even by a favorite hypothesis, as to discover among our more prevalent diseases anything at all answering to the description of this affection. Or does he wish to convey the idea by the word *typhoid*, that the diseases of the present epoch are characterized by symptoms resembling typhus. If this be the case, we consider the application of the term equally objectionable. Most certainly, in a few rare and exceptionable cases of fever, which have become protracted, from neglect at first, an injudicious treatment, or perhaps an unusual degree of obstinancy in the disease itself, symptoms of a typhoid character do supervene; but from this small proportion perhaps not over one in every two or three hundred cases, it would be incorrect to draw the characterizing features of the diseases of the epoch.

Further on we shall have to state, deriving the impression from another part of the essay, that Dr. Lewis has assumed the congestive fever as the type of the diseases of the adynamic epoch. How then can he reconcile this with the idea of typhus or a typhoid state? Has he recently compared the symptoms of either typhus or typhoid fever, as given by any practical writer, who has had personal opportunities of observing these diseases frequently, with those of congestive fevers? Certain we are, and all we think will agree with us, that no febrile diseases are more diametrically opposite in the manner of their development and progress, and in their symptoms generally.

The third, the adynamic epoch, embraces the period from 1834 up to the present time.

"Causes of a prominent nature have been assigned for the change which took place in the character and temper of disease about 1818. * * * * * * * But for the change which we are now contemplating, the mind cannot fix upon any tangible or supposed cause, with certainty or satisfaction. In many parts of Europe, as well as America, it was noticed about this time that diseases assumed an adynamic type. * * * * * * Watson and other observers seem to hold the opinion that it was in some way influenced by the epidemic cholera, which immediately preceded." Dr. Lewis, however, is inclined to the opinion, that its "development in Alabama was owing to local causes, rather than any inexplicable foreign atmospheric agency."

We make a few extracts in which Dr. Lewis's description of the peculiarities characterizing the diseases of the present epoch is conveyed; premising, however, the expression of our belief,—that the picture is drawn from exceptional cases, rather than from the mass collectively.

"In 1834, the change in type and character, as contrasted with 1828, was complete and striking. During the summer and autumn of this year, the red and scarlet livery of past years had disappeared, and disease henceforth robed itself in darker and more gloomy colors. The cold stage of fever had heretofore attracted no other attention than as ushering in the stage of preternatural excitement, the intensity and violence of which alone marked the degree of danger in the case. To local inflammation, or that sinking and collapse which ensue on the breaking up of such high unnatural excitement in the system, was directed the attention of the physician for the issue of these evils. Disease then was bold in its approach, open and undisguised in its conflict with the constitution. But in 1834 we find its approach was insidious and unobserved, giving no serious warning of its proximity, until the unconscious victim was secure in its grasp. The patient first complained of depression, *heat and burning*, when to the touch the surface was icy cold; that cold, that first stage, is now the stage of disease and peril, and that reaction which in past days was looked to with fear and trembling, would now be hailed as the messenger of returning health and vigor."

* * * * "Diseases of every form, season and locality in the bounds of the State, became at this particular time characterized by a low, enfeebled state of the circulation." * * * In the disease of this epoch, "there was a display of certain specific differences, in various sections and localities of the State, * * * * * * owing to some peculiarity attaching to the organic nature of each locality and region."

Dr. Lewis being unacquainted with the topography and diseases of the north-west section of the State, "leaves the subject to other hands."

"Before entering upon a description of the diseases of those sections adverted to, we will take a brief notice of other causes and circumstances influencing disease, than those arising merely from soil. In the "circumstances" enumerated, however, we perceive but little that is new or of special interest. They are merely repetitions of statements made frequently and long since by others; as, for instance, the agency of mill-ponds; of the eddies of rivers, the removal of trees or their foliage from around marshes, *sloughs and habitations, &c., as exerting a local noxious influence.* The bad quality of the water in some parts of the State is also alluded to, and it is stated that "the water from the artesian wells throughout the prairie region frequently contains saline properties, the constant use of which tends to change the healthy functions of the system." The ground on which this conclusion is based, are not given, but in the next paragraph we are informed that it is not "unfrequently the case that those individuals who suffer with chronic disease of the stomach, have been restored to health by the use of the prairie waters."

We fear that we do not understand exactly what Dr. Lewis means in the following paragraphs.

"It is the generally received opinion, that living vegetation protects the human system from the deleterious effects of *malaria*, and reasoning by analogy, it would appear that experiments made by scientific men, have satisfactorily explained the mutual dependence of the animal and vegetable kingdoms on each other for support."

"It has been ascertained, that if air rendered pernicious by respiration be confined in a bottle, in which some green plant has been introduced, and exposed to the action of the sun, the *carbonic acid* will be absorbed, and the air restored to its original condition. The putrefaction of animal matter, and the decomposition of vegetable substances, would cause a sufficiency of carbonic acid vapor, when united with the atmospheric air, to destroy every living being, were it not for this wise provision of nature."

"This gas which is poisonous to the human as well as animal species, is a source of nutriment to every variety of plant, and thus, it would appear, vegetable absorption exercises a benign influence in protecting man from the deleterious effects from poisonous vapours."

We presume, however, that no other inference can be drawn from what we have quoted, than that Dr. Lewis believes in the exploded opinion,—so exquisitly ridiculed by Professor Caldwell, in the American Journal of Medical Sciences, for August 1831,—of the identity of carbonic acid gas with the malarious poison. This we say is the only literal inference that can be drawn from the matter quoted; but as the position is so absurd, so extremely ridiculous and untenable, we are willing to believe that Dr. Lewis really did not mean to express any such opinion, but merely, that reasoning analogically,—as living vegetation absorbed the carbonic acid gas,—it probably absorbed also the miasmata productive of autumnal fever; and this exercised a protective influence. This idea is by no means of recent origin.

The Doctor also speaks of the effects of the winds from different points; of the different results produced by large quantities of rain in different localities; of the influence of dews; and informs us that one of his correspondents "hazards the conjecture, that further investigation will establish the fact, that the nearer the dew point approximates the temperature of the summer and autumn, the greater will be the prevalence of every form of miasmatic disease.

Our author next enters upon the diseases of the third epoch. He says: "Having presented the general outlines of the physical characters of each section of the State, we will now endeavor to give a brief description of the prominent diseases of each division. It is too much the custom with medical men to view disease in the mass, without directing attention to its individual character. Diseases that are dissimilar in the manner of invasion, unlike in symptoms, complication, duration and mode of termination, either in recovery or death, are *generally* looked upon as one and the same affection." This indeed is new to us and startling in the extreme. Before Dr. Lewis informed us otherwise, we at least had remained under the impression that diseases, that were "dissimilar in the manner of invasion, unlike in symptoms, complication, duration and mode of termination, either in recovery or death, were *generally* looked upon "as different affections." We can call to mind but one instance to the contrary, and that occurs in the paper now under consideration; nor are we certain that in this we are not mistaken. If, however, it is not an instance of the kind, it is a nearer approach to it than any thing we can now recollect. We allude to that part of the paper in which Dr. Lewis speaks of "that low stage of *typhoid affection*, that marks the character of all diseases of the present day," bearing in mind at the same time, that he has taken congestive fever as his type of the diseases of the present epoch. That this is really the case we will refer the reader again for proof, to his picturesque account of the change in character between the diseases of the second and third epochs, so marked in 1834, commencing with "during the summer and autumn of this year the red and scarlet livery of past years had disappeared, &c." Now, in the sentence quoted before the last, if Dr. Lewis does not mean to say that congestive fever and typhoid fever are the same—he must, to justify his use of the term typhoid,—find a striking *resemblance* between congestive fever and typhus; and we venture the opinion, that there are few acute febrile diseases more "dissimilar in the manner of invasion, unlike in symptoms, complication and duration."

Dr. Lewis proceeds. "This want of analysis in investigating disease in the abstract, leads to great confusion in medical literature, particularly in the South, where remedies are prescribed through the medical journals, in varying doses for disease under some one head, that may be totally dissimilar in character." We presume, though we do not pretend to understand precisely the meaning of this sentence, that a grave imputation is conveyed in it, as to the qualifications of the physicians of the South; and all must admit,—we make the acknowledgement in sorrow, that our "medical literature" in *every instance*, is not of the very highest order.

We continue the paragraph. "It is true that disease, to a great extent, may prevail in some one locality in the same season, presenting precisely the same symptoms as in the various types of intermittent, remittent, congestive and yellow fevers; yet in other sections of the country, the symptoms may be so unlike in many respects as to demand an opposite method of treatment." Here again we are at a loss as to the precise meaning of the author. If he intends to say that in any one season or locality the "*various types* of intermittent, remittent, congestive and yellow fevers" are found to present *precisely the same symptoms*," we fear that his position will not be generally sustained by the members of the medical profession of the South, who are not yet sufficiently illuminated to recognize a *difference in diseases* or in the "various types," of a disease from an identity of symptoms. On the other hand, if he merely intends to say that any one of the diseases named, may, during the same season, even in different localities, differ somewhat in their symptoms, and require a modified treatment accordingly, he is certainly advancing nothing new, nothing which he on consideration himself must not acknowledge that even the physicians of the South generally seem to understand. Since the time of the Father of medicine himself, this opinion has generally been admitted, and is most forcibly inculcated by Baglivi, to a partial extent at least, in speaking of the use of the Cinchona under particular circumstances at Rome. "*Aliis forsan in urbibus egregrium est remedium, hic noxium experior.*"

We would here, with all respect, suggest to Dr. Lewis that words may or may not express ideas, according to the manner of their arrangement.

In entering upon an account of the diseases of the three different sections into which the author divides the State—he first directs attention to the summer and autumnal fevers of the coal and granitic or hilly portion.

"Notwithstanding the *extreme vicissitudes* of the weather, incident to this region, are sufficient to give rise to *an endless variety* in the type of fevers, yet the general character of disease is not such as would come under the head of active or open phlegmasial, but like those of other sections, they are attended with small frequent pulse, moderate degree of heat on the surface, and in a majority of cases there is a constant tendency to prostration."

* * * * * * * "The warm days proper of summer, are some 20 less in number than in the middle and Southern sections. For two or three weeks in July and August, the thermometrical range is higher than is exhibited in the prairies or on the seaboard."

"In the upland country, fevers of an intermittent and remittent type usually make their appearance about the first of July, increasing in number and becoming more violent in the month of August, with occasionally one of a typhoid character, and by the first of September they have attained their maximum point."

"For some years past in low moist localities, (of the upland country also,) the fevers have assumed a more *grave* type—what the physicians in those regions term "congestive intermittents." They are mostly quotidian, the chill continuing from two to six hours, and attended with coldness, shivering and the usual phenomena of an ordinary chill."

"The cold stage is not attended with the same difficulty of respiration —cold clammy skin, sense of oppression, *heat of the internal surface*, extreme thirst, together with that marked depression of all the vital forces, that characterizes the true congestive fever; the continuance of the cold stage and moderate febrile reaction seeming to depend on the general tendency to a state of adynamia that so peculiarly marks every form of fever at the present day, *rather than any vital or important derangement of the functions*."

"In the course of these as well as all other acute affections, it is not unfrequently the case that from some morbid derangement, by the administration of harsh and irritating medicines, vomiting, watery dejections and a state of collapse may suddenly supervene. Under these circumstances, the medical attendant unaccustomed to the *fevers of the prairies*, or not having studied the individual nature of Southern fevers, is perfectly assured that he has encountered a case of *congestive fever*. This character of fever is rare in this region, and is simply referred to here, because it is the only description of cases which has been denominated congestive, and the difference of the two diseases will become apparent when we have described those of the prairies."

Dr. Lewis here for the first time hints at a position, which further on he enters upon more fully, to wit: that there is a radical and total difference between our ordinary autumnal fevers and congestive fevers, or, as he terms it, "the fever of the prairies;"—indeed that the latter is a distinct and separate disease, a disease *sui generis*, and not, as seems to be the more general impression among physicians, a mere modification of the former.

As we look upon the determination of this question as one of no trivial importance to the Southern physician, we must beg the indulgence of the reader, should we dwell more especially upon all that the Dr. has to say upon the subject; as—seeing that he confidently and boldly takes the position named;—we may reasonably expect that he has cogent reasons, if not the most absolute proofs to advance. At present he promises us that "the difference between the two diseases will become apparent when we have described those of the prairies;" though he has already drawn a contrast in part, which we have quoted a short time since, and which for the sake of clearness and precision we will again repeat.

"The cold stage is not attended with the same difficulty of respiration, cold clammy skin, sense of oppression, heat of the internal surface, extreme thirst, together with that marked depression of all the vital forces that characterizes the true congestive fever. The continuance of the cold stage and moderate febrile reaction, seeming to depend on the general tendency to a state of adynamia, that so peculiarly marks every form of the present day, rather than any vital or important derangement of the functions." We will look forward to Dr. Lewis's explanation of this "vital or important derangement of the functions" with some interest, as he, of course, bases his distinction of the two diseases mainly upon it, and intends to show that such exists in the congestive or "prairie fever,"—different in character,—and not merely different in degree,—from that existing in the other forms of autumnal disease. Now, so far as we may judge by what has already been said, the conclusion might readily be drawn that the difference is not one of a radical nature, but of degree merely; seeing that those "congestive intermittents" must have been of a very mild character, (scarcely deserving the title, we think, though the Dr., in speaking of their advent, says, that "in low, moist localities, the fevers have assumed a more *grave* type,") and consequently we would expect that the cold stage would *not* be attended with the same difficulty of respiration, cold clammy skin, sense of oppression, &c.," as in what Dr. Lewis is pleased to term the true congestive fever; seeing that in continuation of his account of the fevers of the same region,—in the very next paragraph, indeed, he says that "the summer and autumnal fevers of this region are principally intermittents and remittents of a *mild* nature, attended with little fatality; the only *severe or grave* form of febrile affection being of a continued or *typhoid* nature." Thus far then the Dr. has given us no reason to believe that what is called congestive fever is not a mere modification of our ordinary autumnal fevers.

But in continuance of the account of the upland country, Dr. Clark, one of our authors "correspondents," remarks, "In August, September and October of 1840, in Benton County, typhoid fever prevailed as an epidemic, assuming frequently a malignant, obstinate and unmanageable character. *It attacked indiscriminately individuals of all ages*, without regard to sex or color. During the prevalence of this fever we had also every grade and variety of intermittent and remittent fever throughout the summer months, but all the *fatal cases* were of a typhoid character."

The author goes on himself to observe, "notwithstanding the occurrence of this and other partial epidemics, idiopathic typhoid fever cannot by any means be regarded as the prevailing disease of summer and autumn; it more properly belongs to the winter and spring months, yet as the general tendency of the remittent is to the continued typhoid, taken with those that are essentially so, and that is the only type which is considered grave or dangerous, we cannot err in stating that typhoid fever prevails in this section of the State to some extent at least." We are exceedingly puzzled to know what to make of the above sentence. We find, however, the conclusion—that "typhoid fever prevails in this section of the State to some extent at least." A conclusion which we think, without a clearer case than is here made out at least, the author need not have put himself to the trouble of repeating, as he had already informed us in a quotation from a correspondent, that in "August, September and October of 1840, in Benton County, typhoid fever prevailed as an epidemic, &c." On what authority or grounds the Doctor bases his opinion that "the general tendency of the remittent (fever) is to the continued typhoid," he has not informed us.

"In the summer of 1839 and 1840 the writer witnessed many cases of fever of various types in this section of the country; some of which, *termed* "typhoid congestive," were attended with a coldness of the extremities, profuse perspiration and a strong accelerated pulse. Contrary to the usual course of fever, the perspiration increases with the arterial excitement, and subsides with it. In other cases again, there is bilious vomiting, soft feeble pulse, moist yellow tongue, stupor, or coma."

"In these forms of disease the fever generally lasted from ten to fifteen days, and may be comprehended under that variety described by Chomel as not being essentially Typhoid fever." Now we do not know whether the Dr. retracts here the opinion just given, "that Typhoid fever prevails in this section of the State to some extent at least," or wishes to be understood as persisting in it, contrary to what he says, is the opinion of Chomel in regard to such cases—that they are not "essentially Typhoid fever,"—nor do we exactly understand whether he means to convey the idea, that Chomel pronounces a disease Typhoid fever. and then declares it "not essential Typhoid fever," or not. If so, he should have referred us to volume and page.

Next we have a description of Typhoid fever. "Typhoid fever, whether remittent at the first or continued from the onset, is attended with extreme debility and great prostration of body; nervousness, irritability, perversion of the senses, stupor, some times delirium, and pain in the head and limbs, thirst and heat of skin, are frequently the variable although prominent symptoms. The pulse is small and frequent, the bowels are slightly tympanitic and sore on pressure, the tongue is usually round and lengthened, dark in the middle, inclining to red at the edges and *always dry*, but when it *becomes moist* it is generally a sign of convalescence which may occur in 8 or 10 days, but the usual duration of the fever is from fifteen to twenty-five days."

"*This fever* is not so strongly marked in Autumn as in winter, and separated from the latter would not deserve the name; as it does not comprehend the strict definition of Typhoid fever." Which meaneth we suppose when "literally interpreted" that typhoid fever in Autumn is not typhoid fever.

It cannot be questioned, that there is much confusion and some contradictory statements in regard to this typhoid fever of the "upland region." First we are told, that cases first spoken of as typhoid, are not "essentially typhoid," and next, after urging the existence of the disease again,—that typhoid fever in Autumn does not deserve the name. Is not all this calculated to strengthen the doubts, which many physicians of Alabama already entertain, as to the existence of typhoid fever at all, within the limits of the State—especially when we recollect also that this typhoid fever of the "upland region in 1840," "attacked indiscriminately individuals of *all ages*." We fear in this particular instance that, "investigating the nature of disease in the abstract," has led to "great confusion in Medical literature."

"In the treatment of the continued fevers of *this* section of the country, whether they are idiopathic or supervening upon a remittent type, local depletion cautiously practiced, diaphoretics, sinapisms, emollient poultices, diffusible stimulants and mild alteratives, are the remedies usually employed by judicious practitioners. Quinine has been used at different periods, in every variety of dose and form, but always tending to aggravate the disease, and increase that disposition to local inflammation which usually exists in the cases of *that* region."

The author's account of the Southern region and its diseases is so brief—and withall so interesting, that we quote it almost entire :—

"In the Southern portion of the State, pointed out as the tertiary, the fevers are less uniform in their type than in any other region, differing in every respect with the varying features of the country and the peculiar nature of the soil, for in this section we find every variety of cause that would tend to generate malaria.

"Here you meet with the dark lagoon that is overshadowed by the cumbrous foliage of *timeworn* forest-trees ; here the brackish marsh abounding in animal and vegetable deposits—the accumulated mass of ages lies exposed to the sun ; and here the wide spread creek and overflowing river leave their slimy deposits of decomposed matter over a vast extent of adjacent soil, to furnish material for the generation of poisonous gases ; all combining with their teeming stench to induce disease of a grave malignant nature."

"As any attempt at a description of all these *types* (varieties ?) is incompatible with the limits of this paper, we will confine ourselves to those cases which occur in the vicinity of the swamps, extending from the junction of the Alabama and Tombigbee rivers down to the city of Mobile. The fevers here are strikingly dissimilar to those now prevailing in other parts of the country. The poisonous cause of disease it will be remembered arises from swamps and marshes of recent formation, in which vegetable matter greatly preponderates."

"The cold and hot stages of the intermittents are characterized by the most violent extremes. The chill lasts from four to six hours, often amounting to a shaking ague, the fever then ensues and continues with great violence from 10 to 20 hours. The sweating stage is scarcely formed before another chill is announced, the paroxysms occurring at irregular intervals ; and it must be noted that these intermittents are exceedingly obstinate, usually continuing from 8 to 10 days, without any change."

"The remittent, or according to a strict definition *continued bilious fever*, (we cannot perceive the propriety of the change of name, nor the correctness of the "strict definition"—Rev.) most usually forms, and is commonly known as "swamp fever." It is attended with a full bounding pulse, from 110 to 130 in the minute—a dry and hot skin—a pale flattened tongue covered with white fur, which in the course of the disease becomes yellow or dark-brown. The pain in the head, back and limbs is very violent ; some thirst, nausea, and bilinary derangement. Once in 24 or 48 hours, there is a tendency *to remit, manifested usually by less frequency of pulse, abatement of pain, and moisture about the head and chest.* The bowels are usually constipated ; all the secretions becoming diminished or suppressed. The blood very soon loses its rich tenacious qualities, becoming thin and pale, and the disease is from 6 to 14 days in running its course."

"We have been treating this fever for several years, and seldom succeeded in cutting it short. Large doses of quinine change the character of the symptoms, but never cure; convalescence is painfully protracted or interrupted by relapses, until the approach of frost."

"In the examination of four bodies made in the hospitals of Mobile, the liver was atrophied in *one*, *being also dry and brittle*, *as in those dying of yellow fever*, *and of a pale straw color*. In the other three this organ presented nothing peculiar as a lesion of disease; in fact, there was no lesion sufficiently prominent any where to attract attention, except in the serous condition of the blood, and soft spongy condition of the cellular and muscular tissues."

Dr. Lewis thus enters upon his accouut of the *fevers of the prairie region.*

"To ascertain that malady which for gravity is the maximum in the chain of morbid sequences—that malady which most excites the fears of the people, and absorbs the attention of the medical man, it is only necessary to be brought to the bed side of one laboring under congestive fever, and the search is at an end."

After some preliminary remarks, the author observes "that the combined testimony before us points unerringly to the following conclusion, that the poison of the disease is confined mostly to humid prairie, or low swampy lands, abounding in organic remains; that it does not spread broad cast over extensive tracts of country, or rise to elevated situations like the ordinary marsh poisons; that the more elevated prairie soil, which has in the process of cultivation given out its excess of humidity, does not produce the disease, and that with an equal exposure, all ages and sizes of the white population are alike susceptible."

"In looking back at our sketch of the first and second epochs, the regular gradation of disease will be observed as follows: intermittents and irregular bowel affections prevail in the spring and early part of summer; as the season advances these are replaced by remittents; late summer and autumn find that the latter have attained their maximum, and the fevers are *continued*, being either of a *nervous*, *inflammatory* or *malignant* character. Here is presented a regular gradation of morbid action, from its mild incipiency to a high toned or malignant action, without any superadded complication or change of character, save in degree."—We must pause here a moment.—Although a few might be found unwilling to cavil at the "regular gradation" from intermittents and irregular bowel affections to remittents, and from these to continued fevers; yet when they are spoken of as "being either of a *nervous*, *inflammatory* or *malignant* character," the impression will force itself on many, that there must have been a "superadded complication or change of character."

"We find that the congestive fever is preceded by the intermittents and remittents, precisely in the same manner that the continued fevers were in times past. This apparent connection and dependence afford a strong argument in favor of the identity of the congestive and other fevers so closely associated with it,—but while we admit its force we cannot subscribe to its conclusiveness. He who does not know, that the most deadly maladies, maladies wholly and essentially different in form, are often found engrafting themselves on simple intermittents, has observed to little purpose. This, however, as before remarked, is not the place to consider the identity or connection which may exist; allusion is made to the question, for the simple purpose of calling attention to the importance of a rigid scrutiny and investigation, not only of connecting circumstances, but the symptoms pathognomonic of congestive fever." We are not a little pleased with the conclusion of this paragraph,—for, now, we have reason to expect, that in addition to an explanation, relative to the "vital or important derangement of the functions"—formerly alluded to, as existing in *the* congestive fever, and not in other forms of malarious fevers, we have also the hope held forth, that we are to have the *pathognomonic symptoms* of the congestive fever given us, and then, of course, if in reality there exists the distinction for which Dr. Lewis contends, it will be made clear and manifest. The line of demarcation will be plainly drawn, and "he who runs may read."

We turn back a page. "A correspondent residing for many years in Montgomery, a town situated amidst diluvial elevations of the prairie region, informs us that he has not yet seen any case of congestive fever, such as has been described by medical men, as prevailing in other sections of the State. That he has "seen cases of simple fever collapse or become complicated, (under the influence of injudicious treatment,) resembling in that condition the congestive state." From abundant testimony it would appear that this town is exempt from the disease in its true characteristic form. Take for instance the paper of Dr. Boling on the fevers of that vicinity. He describes no case of febrile affection, which physicians of certain localities in Green, Marengo, and Dallas would recognize as congestive fever."

A question of no little importance is involved in the preceding remarks. It is, whether there really exists a disease in Alabama, distinct and separate in its nature from our ordinary autumnal diseases, —more especially deserving the name of congestive fever, and radically differing from the various aggravated shades of the former disease, which have been described frequently, and spoken of under the latter term. Whether indeed physicians, in speaking of congestive fever, have included under this term and in their descriptions, unknowingly, with a disease distinct and peculiar, also certain aggravated shades of other diseases of an entirely different character in some instances; and in others, have erroneously spoken of the latter merely, as congestive fever.

Dr. Lewis, it will ere this be distinctly perceived, assumes the position, that there is a "congestive fever," a disease *sui generis*, distinct and separate from our ordinary malarious fevers; while, as has already been observed, the mass of the profession in the South, if we are not in error, is inclined to the opinion that the latter is a mere modification of the former, or, rather, that the cases termed congestive are mere modified shades of the former, produced perhaps by different degrees of concentration of the malarious poison itself, with certain circumstances connected with the invasion and progress of the disease; as, for instance, the extent of predisposition, the kind and degree of exposure; irregularities of diet, more especially a recently eaten indigestible meal, the condition of the system in regard to strength or debility, perhaps even the remedial agents first resorted to; and we may also add, the temperament of the patient. Under this latter view of the case, it would be expected, as a matter of course, that in different localities, and in different seasons—as also in different individuals in the same locality and season,—there would be some variation in the symptoms,—as is the case in regard to all other diseases, whose different shades also would be dependent somewhat on the organs bearing the principal onus of morbid action, and the extent and degree of the latter;—but still, to justify the term congestive, as applied to and embracing them, possessing a general resemblance and an identity in some important features.

The term, we believe, is intended to express a general tendency of the blood from the surface to the central organs, of which a small and corded or feeble pulse, cool, shrunken and often shriveled condition of the extremities, are among the principal indications,—these coming on rapidly and unexpectedly in many instances, and when seemingly the result of depletion; supervening upon such measures, carried to an extent inadequate to the production of any thing like a similar condition of surface, pulse, &c., or even marked debility in any respect, in an individual whose nervous system has not previously been impaired or modified by the malarious poison. Although we consider, that all of the different shades of congestive fever very properly come under the description of pernicious intermittent and remittent fevers, as given by the Italian and other physicians practising in malarious regions, yet we do not think that all the forms described as pernicious fever, could with equal propriety be called congestive. For instance, several of the shades of comatose fever, included by these writers among the pernicious fevers,—present none of the evidences of central determination of the fluids. We see for example, occasionally, cases of fever marked by periodical coma, in which there is a full and moderately firm pulse, heat, and even a slight general redness of the surface, which is full and plump, in contrast with the shrunken state, and pallid or pale livid color, marking the congestive fevers. Again, there is another shade of the comatose remittent, in which there is a periodical suspension of the animal functions, without any perceptible febrile excitement or determination of blood to the brain;—the skin, respiration, pulse and pupils, the state of the secretion and the tongue, to all appearance being indicative of a perfectly healthy condition of the system. During the paroxysm, the patient seems to be in a natural and healthy slumber, and the only thing giving evidence that this is not the case, is the impossibility of rousing him. Such cases as these, we repeat, though classed among the pernicious fevers, cannot properly be called congestive. In the last described, there cannot be a doubt, we imagine, that the affection of the brain is entirely nervous, "the functions of the organ being probably suspended by the noxious agent, in a manner analogous to that, by which local neuralgic affections are produced by it," without local determination of blood. In regard to the shade first named, the presence of coma with febrile excitement would lead at once with many to the supposition of cerebral congestion, and though there may exist more or less preternatural fulness of the brain, it is not improbable that in these cases also the suspension of the animal functions is dependent upon a nervous cause, and merely coincident with febrile action. At any rate, there is not present that central determination, with shrunken surface, &c., which are looked upon as characterizing mainly the various shades of fever termed congestive. All these, however, we have exhibited in some of the shades of the comatose fever.

We proceed to an examination of what is meant by congestive fever by Southern physicians, and the symptoms by which it is is recognized; and in so doing, will wander somewhat from the essay before us, as in the fulfilment of our object we deem it the better method to present extracts from different writers, who at least supposed, when writing, that they were giving an account of the only forms of disease recognized under the term. We make our first extracts from an article on congestive fever in the Americal Journal of Medical Sciences, for July, 1843, by Charles Parry, M. D., of Indianapolis. Of course we must abbreviate.

"In the majority of instances the symptoms of the first paroxysm are such as occur in an ordinary intermittent attack. * * * * There is a general coldness of the surface of the body and extremities, felt by the physician, but not by the patient, as there is a lessened endermic sensibility, * * * * * the patient in the interval (subsequent to the first paroxysm) complains of malaise only, or some debility, being in other respects perfectly well."

The second paroxysm, "is always severe, not so much in the violence of the rigors, as in the extreme coldness, and in the approaching death like hue of the face and extremities."

"In the chill, gastro-intestinal irritation is very violent, the vomiting and purging being almost incessant. * * * * The discharges do not resemble those of cholora; they have more the appearance of water, in which a large portion of recently killed beef has been washed."

"The patient complains of a sense of weight and burning heat in the stomach. * * * * * The respiration is often peculiar. It consists of a deep drawn double inspiration (or double sigh) with one expiration."

"The pulse, even from the beginning of the second paroxysm, is very small, thready, very frequent, 120 to 150 in a minute."

"Restlessness is very great, the patient constantly tossing about from one side of the bed to the other; throwing his arms and legs around; incessantly endeavoring to get out of bed; and he is able to do so, even to walk across the room if permitted, until within an hour or so of dissolution. I have positively seen persons get out of bed and stand in the doorway, hours after it was impossible to detect any pulse at the wrist, though the carotids could be felt plainly. Such is the intense desire of the patients to get cold air, that they frequently express themselves determined to have it at all hazards."

"The mind is usually undisturbed until the agony. * * * * * * Increased coldness, clamminess and wilting of the skin on the hands and arms; a sticky unctuous sweat from head to foot, collecting in drops the size of a half dime, if not wiped away. Hippocratic sharpness and anxiety of look, without corresponding sensations (for even here the patients may speak as if there was little the matter with them); seldom any subsultus or hiccough, or convulsions; and death takes place easily, as if without cause."

We will next quote from an article by Dr. R. G. Wharton, (of Grand Gulf, Miss.,) in the American Journal of Medical Sciences, for April 1844. After speaking of the gradual initiation of the disease, he proceeds to the more marked symptoms. "By degrees the heat of skin passes off with profuse perspiration; the hands and arms become rather cool, the pulse becomes quicker, the breathing still more oppressed, the patient every few seconds making a deep inspiration; the restlessness increases; the perspiration becomes more profuse, standing in large drops on the head and face; gradually it feels sticky and cold, especially on the extremities; the hands now become shriveled and very cold, and disagreeable to the touch, feeling like a corpse; the coldness rapidly spreads up the limbs to the body; the pulse is very small and quick, beating from 120 to 140 per minute; the patient is so restless that he cannot remain in one position for a second, his great complaint being that he cannot get his breath; sometimes he will get out of bed a very short time before death; the pulse becomes imperceptible at the wrist often eight or ten hours before death. * * * * * The patient has usually great thirst and complains of a feeling of inward heat."

"It often commences as a simple intermittent, and the reaction not taking place, the cold stage is rapidly merged in the most violent form of congestive."

We have given these extracts from the two papers above named, not only because to us they seem to convey a pretty correct idea of what we have always considered congestive fever, but also because we have heard them spoken of by others as giving a most accurate description of the disease;—and the truth of the pictures has also been acknowledged in some of the medical periodicals.

We will here give the symptoms of the disease as it occurs in Montgomery and its immediate vicinity,—as represented by Dr. Boling, reminding the reader at the same time, that Dr. Lewis says, "he (Dr. B.,) describes no form of febrible affection, which physicians of certain localities of Green, Marengo and Dallas, would recognize as congestive fever," and also, that a "correspondent," of the Doctor's from Montgomery, says that "he has not yet seen any case of congestive fever, such as has been described by medical men as prevailing in other parts of the State." The extracts which we have given above descriptive of the disease, are not taken from the accounts furnished by the physicians of Alabama, and the reason will be presently apparent.

Now, should it appear, that the description of Dr. Boling corresponds in all important points with the statements of Doctors Wharton and Parry, and others, who have supposed they were describing congestive fever, and that the account of the disease by Doctor Lewis differs in no essential feature from these, we think it will be but a fair conclusion, not only that there is a more general and correct accordance among physicians as to what constitutes congestive fever, than Doctor Lewis would lead us to believe; but also that he has also failed to make out his position, that there is a congestive fever of a distinct and separate character. If Doctor B. describes none such, and we should be able to discover no important difference; no constant diagnostic symptom, in the account of the disease by Doctor Lewis, distinguishing it from the disease described by Doctor B., surely he describes none such.

Here are the symptoms as given by Dr. B.

"There is a form of malignant remittent fever, known in many parts of the Southern country as "congestive fever," in which there is rarely, till a short time preceding death, any affection of the brain. The principal characteristic of this form of fever is the absence of any great degree of febrile heat on the surface generally, with extraordinary coldness of the extremities. The pulse is small, corded and very frequent, varying even in the first or second exacerbation from 130 to 140, and should a third or fourth exacerbation occur, without being mitigated or modified by treatment, it still increases in frequency till it cannot be counted, and becomes small, thready, and occasionally imperceptible sometimes before the death of the patient. The extremities with each exacerbation become colder and more clammy, while a proportionate increase in the temperature of the chest and abdomen frequently occurs. The conjunctivæ are white and pearly,—except in those instances in which the surface becomes jaundiced,—when they partake of the yellow tinge. So susceptible are the bowels, to the action of purgatives, that the smallest doses produce hypercatharsis, the stools generally being a thin serous matter, sometimes tinged of a yellow colour with bile ; but more frequently they are of a dirty reddish color. The respiration is sighing, interrupted, and at times disproportionately slow, compared with the pulse. The stomach is excessively irritable, the matter rejected being principally a transparent ropy mucus, in which is suspended a small quantity of a grass-green flocculent matter. The surface is frequently of a slightly livid tint, that of the extremities shriveled up and clammy, and covered with an exudation, cold, and so profuse as to have been likened by Senac to the "*Sudor Anglicanus.*" In this form of fever, the remissions are indicated more by a tendency to a natural temperature, than in any decided mitigation in the other symptoms."

"Notwithstanding the small and thready state of the pulse in this variety of pernicious fever especially, the action of the heart will be found strong, as indicated by the loudness of its sounds and the force of its impulses. * * * * * * Muscular prostration is rather apparent than real, as evinced by the force with which the heart beats, and the fact that there is "oftentimes surprising muscular strength, until within an hour or two of dissolution, the sufferer being able to jump out of bed and walk about, while he is pulseless." * * * * * *

"There is a modification, a shade of this variety of pernicious fever, produced by injudicious treatment, in cases where there is an original tendency to assume a bad character, and sometimes indeed it is to be feared, where this tendency is not very great. It is most frequently brought about by drastic purgatives. It differs from the last only in a greater feeling of weight and anxiety about the epigastrium ; more heat and distension of the abdomen, jactitation, a deeper colored and more parched state of the tongue, a more rapid respiration, an indescribable expression in the countenance of anxiety and apprehension,—and where the abdominal distension is not too great to admit of a satisfactory examination,—more violent throbbing of the descending aorta will be found."

The description of the disease as given by Dr. B., it must be admitted, is defective ; nevertheless, so far as it extends, it coincides so exactly with the accounts of Doctors Wharton and Parry, we are disposed to think, as to show conclusively that he was speaking at the time of the form of disease which they have described as congestive fever. The accounts of no two physicians in picturing a disease, even from the same case, would be identical—as certain symptoms would probably strike the mind of one with more force than that of the other, and would therefore be represented more prominently in the foreground; while a variation might be expected also, in consequence of the different stages of the disease which might be held principally in view at the time. How much less could we expect exact identity when the descriptions are drawn from the general impressions made by a number of cases, each one varying no doubt in some of its features from the rest.

We have said that the description given by Dr. Boling is defective and imperfect. We find no mention made of the intense thirst with which the patient is harrassed—the disposition to get out of bed, and move to a window or door, or from one bed to another,—the wish to be constantly fanned—requiring frequently the services of several persons at once for this purpose, to satisfy the great desire for fresh air; and as is frequently observed to relieve measurably the insupportable feeling of weight and oppression, faintness and deadly nausea about the præcordial and epigastric regions. Neither do we find any mention made of the exceeding restlessness and jactitation, inducing the patient constantly to toss his limbs and head about, as also suddenly to change his position from side to side, perhaps as often as every few minutes. No mention either is made of the great internal heat of which the patient often makes such loud complaints. This extreme internal heat we will here observe has a real existence, for in no other disease have we ever found it so great, during post-mortem examinations as in congestive fever—we have a most distinct recollection of one case in particular, of a post-mortem examination of a patient about three hours after death from congestive fever, in which we assisted our friend Dr. W. O. Baldwin of Montgomery, and in which the heat of the viscera, was not only extremely disagreeable—but almost insupportable to the hand inserted among them.

We will now give the *only* account of congestive fever, which we are able to find, as it is "described by medical men as prevailing in other sections of the State," which was published prior to the presentation of the essay of Dr. Lewis before the medical society, expressing at the same time our regret, that the "correspondent" from Montgomery did not refer us to the accounts of the disease, to which he alludes, as otherwise we are disposed to think it will be a somewhat difficult task to find them. We quote from a paper to which we have already referred, on the autumnal remitting fever of 1831, by J. W. Heustis, M. D., who at that time wrote from Cahawba; which it will be recollected is in Dallas, one of the counties to which Dr. Lewis more especially refers as the site of congestive fever.*

"The fever this season was of a congestive character, with a great

* There may also be found, however, a few passages descriptive of the disease in an article on the cold dash in congestive fever, published by Achilles Whitlock, M. D., of North Alabama, in the Western Journal of the Medical and Physical Sciences, for January, 1837.

disposition to an unequal distribution of febrile action and development. In many cases the head and particularly the extremities were cold, while the trunk was hot;—whilst at the same time there was considerable palpitation of the heart, with a pulse small, weak and frequent. * * * * * Sometimes after a transient state of febrile excitation, collapse took place at an early stage of the disease, with little impairment of the general powers of the system; this was more especially the case when depletion had been practised too freely. * * * * The prostration and collapse are for the most part confined to the vascular system, while such is the strength of the muscles of locomotion, that the patient is able to rise and sit up, and even to walk about." So far as it extends can any thing be in more perfect accordance with the description of Dr. Boling?—and if Dr. Heustis has not here spoken of the same form of disease as the writers whom we have before quoted, we most respectfully request Dr. Lewis, or the "correspondent," to point out in what particulars it differs.

We will now present the reader with extracts from accounts of congestive fever from several of our authors "correspondents," which he himself quotes as descriptive of the genuine disease,—that an opportunity may be afforded of detecting, if possible, the difference between this and that which Dr. Lewis says is not congestive fever; and also, of judging, whether or not in most instances those who have thought they had under their care congestive fever, and have attempted its description, have been deceived, and that it was reserved for Dr. Lewis to separate the true from the false; to distinguish the reality from the counterfeit.

Before presenting the views of his correspondents, however, Dr. Lewis observes, "as a general rule this *disease has become modified* since 1835; still, however, maintaining in some localities its original marked violence. As it is the most formidable and interesting disease of summer and autumn that is known in the bounds of the State, the impressions of medical men practising in different localities will not only assist in *revealing its true character*, but are absolutely necessary to the end we have in view, a fair and correct picture, so far as we go, of the prominent diseases of the State." It will therefore be perceived that Dr. Lewis here endorses the descriptions of his "correspondents"—and he in no other part of the essay expresses a dissent.

The first extract we shall give, is from a quotation made by the author, from a prize essay (in manuscript yet) on congestive fever, by Dr. Mabry, an intelligent and estimable physician of Selma, in *Dallas* County.

"Its invasion is often sudden, coming on after a few hours premonition with a chill, which continues for several hours, during which time the patient is harrassed by difficulty of breathing, nausea and repeated efforts at vomiting; the pulse is frequent and feeble, skin cold and clammy, countenance distressed and shrunken; not unfrequently attended with sero-mucous discharges. The duration of this stage varies in different cases, seldom passing off, however, in less than three or four hours, and often continuing much longer. The hot stage is characterized by a feeble reaction of short duration, after which it gives place to the sweating stage. About the same hour the next day, or perhaps the day after, a second paroxysm takes place, which differs but little from the first, except in severity."

Our next extracts are from a quotation made by Dr. Lewis, from a communication to him, of "Dr. Bates, a resident of the prairies, not very far from Selma."

"The skin of the whole body is pale and cooler than natural, shriveled, and where pressure is made, as on the back and sides,—motley and livid; frequently the whole body is covered with clammy exudation. The nose and ears become icy cold, and the lips perfectly livid. There is a great diminution of sensibility, and oftentimes the patient does not manifest any feeling on the application of the most powerful rubefacients. The countenance betrays the greatest anxiety, the eyes are red and suffused. The patient turns frequently in bed, or tosses from side to side, uncovers himself, complains of great heat, intense thirst, and says, if water is not given him, he "will burn up." The respiration is more frequent than ordinary; sometimes it is slower, but very difficult, frequent sighing with expiration like one out of breath. The pulse is small and more frequent than natural. In the most violent cases it is imperceptible, except just above the elbow. The heart is felt beating tumultuously, as if struggling to free itself of a load. * * * * * Oftentimes the dejections are copious, and after one or two evacuations become watery, containing small flocculi of mucus. The intellectual faculties are often retained to the last."

Dr. Lewis next gives three cases communicated to him by Dr. H. V. Wooten, an accurate observer, residing in Lowndes County,—in the prairie region also. Previous to quoting them, Dr. Lewis observes that Dr. Wooten, "reports none but cases of a distinct intermittent type; his notes will serve, not only to show the modified character of the disease of the present day, but convey a correct description of this variety." The Dr. then, it will be observed, admits of varieties and modifications of the disease. After giving the cases, he observes that they present a view of that simple variety known as "*congestive or intermittent chill.*" Our space will not permit us to present before the reader the cases of Dr. Wooten in detail—but we will merely observe that among the symptoms present were, in the first: an inability to "speak intelligibly, moans, rolls about the bed, making frequent efforts to get up. * * * * * A return of all the depressing symptoms of the morning previous, attended with profound stupor,—death." In the second: the patient in the second paroxysm,—the Dr. found "lying with her head thrown back, eyes protruded and fixed, face livid, breathing hurried and laborious, pulse scarcely perceptible at the wrist—and too frequent to be counted; extremities cold, trunk and head very hot." As the third case is short we will, contrary to our first intention, give it nearly entire. "A., aged 24, of good constitution, complained for the last two mornings of languor and aching in the bones, but would so improve in the afternoon as to attend to his ordinary duties. On the third morning, at five o'clock, I was summoned in haste, and found him totally insensible to surrounding objects; would not answer questions, open his mouth, or move the least from his position. With the exception of the chest, the entire surface was cold, that of the extremities intensely so,—respiration laborious, pulse small and 140 in the minute; found much difficulty in getting him to swallow. At 7 o'clock there were signs of improvement—as an increase in the volume of the pulse, restlessness, warmth of skin, &c." He recovered in a few days. Such are the cases which Dr. Lewis says present "a view of that *simple* variety, known as congestive or intermittent chill." We would ask here, how the word "simple," can apply in connection with such symptoms, as are enumerated by Dr. Wooten?—and by whom any form of disease is called "congestive or intermittent chill;" or where in "medical literature" the author finds authority or precedent for the use of the term? Dr. Wooten's cases we think might have received a more appropriate name; indeed they are most graphic representations of some of the shades of what has been called comatose remittent, cerebral remittent, &c.,—but of a congestive character. In other words, congestive fever, in which the brain is one of the organs sustaining the principal force of the disease. Indeed, cases very similar, differing in but the slightest shades,—and from these not more than from each other further on, it will be found, Dr. Lewis recognizes as "cerebro-congestive" fever.—We allude to the cases of Dr. Ames.

We think it will be perceived, that the greater part of these quotations are descriptive of the same disease, with slightly varied phases only,—as that spoken of by Dr. B. as congestive fever; at least we cannot discover upon what particulars or peculiarities to base a diagnosis. Consequently, so far, it is our impression, that our Author has not succeeded in establishing the existence of a disease, with symptoms distinctive and peculiar, as entitling more especially to the term "congestive fever, and differing in important features from that which has generally been recognized by others under this term. Yet Doctor Lewis says that Dr. B. does not describe the disease, and that Montgomery is entirely exempt from it. The opinion of Doctor Lewis, as also of his "correspondent" from Montgomery, who says "that he has not yet seen any case of congestive fever," &c., is singularly at variance too with the statements of Doctor Ames, one of our author's correspondents, who likewise resides in Montgomery, and who furnishes Doctor Lewis with several cases, which he inserts in his essay, under the title of "*cerebro-congestive fever*." They are all cases in which the brain seems materially to have suffered, presenting each in a greater or less degree, evidences of congestion. In passing we would also remark, that Doctor Ames in his communication to Doctor L. quoted in the essay before us, speaks incidentally of the abdominal congestive remittent, and in such a manner as to prove to us, that he at least, more fortunate than the "correspondent," has seen congestive fever in Montgomery, "such as has been described by medical men as prevailing in other sections of the State." Speaking of the "cerebro-congestive" cases, he says: "the coolness of the surface in these cases is never the coldness of collapse, nor is there even the profuse sweating, vomiting, diarrhœa, oppression of the epigastrium, sighing, jactitation and general restlessness *of the abdominal congestive remittent*." Surely when Doctor Lewis inserted this brief but correct description of congestive fever from Doctor Ames, together with his five cases of "cerebro-congestive fever," he had forgotten, that but a few pages before he had said, that Montgomery "from abundant testimony was free from the disease in its true characteristic form."

We would remind the reader, that Montgomery is within the bounds marked out by Doctor Lewis as the "prairie region," which he considers so emphatically the site of congestive fever, that he frequently uses the term "prairie fever" as synonymous with the other term; that the prairie lands reach on one side within about a mile of the city; that the city is situated on the Alabama river; that on the opposite side from the city in an extensive body of "low swampy land abounding in organic remains," the character of soil most favorable according to Dr. Lewis;—next to the prairies, for the production of the congestive fever poison,—that within a few miles of the city in another direction runs the Catoma creek, almost touching at certain points, the prairies; and that along its borders also are extensive bodies of "low swampy land, abounding in organic remains;" and finally that,—as is the case perhaps in every other place,—the practice of the physicians of the city extends for a considerable distance into the surrounding country.

Having failed so far in discovering in the cases and views of the "correspondents" of Doctor Lewis,—as presented by the Doctor himself, in his essay,—any distinctive phenomena characterizing any form of disease spoken of as peculiar and separate from those various modifications or shades of our autumnal fevers, recognized generally as congestive, entitling it to be considered a distinct disease, we turn to the Doctor's own remarks, and will transcribe verbatim the only case given from the records of his own practice, with the hope that here at least we may be able to discover the "vital or important derangement of the functions," and the "symptoms pathognomonic of congestive fever,"—with which he has intimated that we are to be favored,—distinguishing *the* congestive fever of Doctor Lewis, from the forms which others have heretofore spoken of, and treated under that name. Here is Doctor Lewis's picture of the disease.

"Mr. A. aged 24, native of the State, of robust constitution, came to my office at 9 in the morning for advice. Says he has been for the last week engaged in superintending "some work" in an adjoining prairie swamp; went to bed last night feeling well, awoke this morning at daylight, since when he has been weak or languid, not particularly sick, but is unable to shake off a restless, desponding and uneasy feeling. Found his pulse not exceeding 100, but small and deep seated; skin cool and damp, which was attributed to exposure, the atmosphere being warm and damp. Says he is thirsty but *does not feel chilly;* his difficulty mainly consists, to use his own language, "in an "inability to get my breath." In two hours after he returned home, some two miles from my office, he wrote and despatched the following note: "come and do something for me or I shall die." Did not see him till 3 o'clock in the afternoon. His condition then was nearly as follows: skin cold, of a pale blueish color, muscles soft, unless put upon the stretch by exertion, profuse perspiration over the entire surface, standing in large drops on the chest and forehead, tongue cool, pale and inclined to a livid hue, pulse frequent, small and thready. Action of the heart changed to a *tremulous flutter* with now and then a violent pulsation, causing the patient to start; urine abundant and colorless; bowels torpid; slight nausea, countenance haggard, expressing the deepest agony or physical trouble, very restless, walks rapidly over the floor for a moment, then sinks exhausted; wants an emetic to relieve his breathing; is perfectly sensible. About 10 o'clock at night the perspiration sensibly diminished, pulse improved and the breathing became easier. This partial improvement continued till 6 o'clock in the morning. At 10 o'clock in the morning found him much worse. Pulse imperceptible at the wrist, tongue and lips livid, skin icy cold, dependent portions wilted and shriveled; other symptoms about as yesterday. After a doubtful struggle of 6 or 8 hours, during which time there *is no complaint of chilliness*, but great heat and burning, the pulse again becomes perceptible, a slight degree of warmth returns to the surface, and hope again enlivens the household. The next morning I was summoned early. He began to grow worse at 2 o'clock, made several efforts to vomit; *had not been chilly*. He now speaks in monosylables, says he is "sensible but has no breath to talk." Is extremely restless, skin cold, bluish and mottled about the back—each expiration is attended with a harsh distressed murmur, cannot bear any covering. Finally, in a paroxysm of wild despair, rises from the bed, rushes to a window and whilst holding to the facing, is seized with a convulsion. He expired in a few minutes, after being laid upon his couch; having been ill but 52 hours."

The Doctor it will be perceived does not give the treatment of this case, and we are therefore at a loss to know how far the symptoms peculiar to the disease itself may have been modified by the remedies used. He observes, however, at the close that "notwithstanding the most *active means* were used, there was no evacuation of fæces from the bowels."

Dr. Lewis has not pointed out in the enumeration of the symptoms of this case such as he considers "pathognomonic of *the* congestive fever"—but from the fact that every thing relative to the absence of a sensation of chilliness, as regards the feelings of the patient, is put in italics, we presume this must be one of them. Nothing, however, is said relative to what were the symptoms from 9 o'clock of the first morning till 3 o'clock in the afternoon, and this is the period during which a chill would most probably have occurred, and have been *felt* by the patient,—prior to the entire loss of "the endermic sensibility." We presume also that this absence of a sensation of coldness on the part of the patient,—when much of the surface is cold to the feeling to another,—is considered by Dr. Lewis among his "pathognomonic symptoms," because further on he goes to the trouble of pointing out certain distinctions between the cold stage of an intermittent or remittent fever, and congestive fever, in which the complaint of coldness in the former and its absence in the latter are especially dwelt upon. Does the Doctor really mean to say that congestive fever is never ushered in by one or more distinct rigors—in which the patient is sensible of, and complains of coldness? Does not Dr. Mabry,—as quoted too by Dr. L. himself, to "assist in revealing its true character,"—say distinctly that it comes on "after a few hours premonition with a chill;" and is it not also stated in some of Dr. Wooten's cases, quoted for the same purpose, that the patients were seized with chills? We would not do such injustice to these intelligent gentlemen, as for a moment to suppose, that they would call that condition of coldness, (the patient not complaining of cold,) in the further progress of a case, which has become congestive, and in which stage the patient is so devoid of sensibility of the surface, as to be insensible to the most powerful rubefacients,—a "chill." Is it not a common thing also in cases of remittent fever, not even of a grave character, or deserving the term congestive,—after the first two or three exacerbations, for the subsequent ones to be ushered in by actual coldness of the fingers and toes; perhaps indeed extending some distance up the wrists and ankles, without the perception of their real state on the part of the patient, who is oppressed, and annoyed, and complains of heat, on the application of additional covering to the feet by the nurse, to moderate this cold stage? Is there not in such cases evidence of an irregular distribution of nervous influence or perverted innervation of the cutaneous surface, differing only in degree, from that so characteristic of the various modifications of the malarious fevers denominated congestive, by the mass of Southern practitioners?

"*The muscles were soft unless put upon the stretch by exertion.*" We believe this is generally the case, in health, or disease, and can scarcely therefore be considered a "pathognomonic symptom." The torpor of the bowels spoken of,—although we think they have seldom been found in this condition, in what others have considered congestive fever,—cannot be one of the pathognomonic symptoms, because Dr. Lewis subsequently says there are cases, in which there are "frequent stools of a sero-mucous matter." The next italics we find are these, "action of the heart changed to a *tremulous flutter*"—more of this anon. We acknowledge our incompetency, in the case given, to discover the "pathognomonic symptoms"—distinguishing it from the modified forms of remittent, which have been denominated congestive fever by others, and which may be traced in every degree, from the most simple remittent to the most malignant form, passing into each other by imperceptible shades,—unless indeed we admit that one is presented in the last sentence quoted.

We will next quote from Dr. Lewis's own general observations regarding the symptoms, and though in a condensed form we will if possible leave out nothing referring to any symptom upon which we have not already remarked.

"In many cases there was pain in the head, flushed face, watery injected eyes, sensibility to light and an early supervention of delirium. In this variety, labored and difficult respiration was not so prominent. This, together with the absence of symptomatic gastric complication, caused practitioners to refer to the brain as the seat of mischief.—Again there were cases (and probably a plurality were of this description) where early nausea, sinking at the stomach, vomiting and frequent stools of a sero-mucous matter, were the most prominent and urgent symptoms of the disease. Here the stomach and intestines were pronounced to be the points more especially involved, although one or the other of these determinations may have existed in one case, or they all may have been manifest in another, producing death in 24 hours; yet as a general rule, the assemblage of symptoms characterizing the disease were very uniform. * * * * * * * * * * * * * * * * During the improving stage, evacuations of a bottle green color, and about the consistence of grape jelly, took place; often as much as a gallon passed in 48 hours. This remarkable colluvies was inodorous, devoid of bile, breaking into pieces (mirabile dictu—Rev.) like the coagula of blood."

"Our attention thus far has been directed to the graver and more malignant cases, about one third of which proved fatal between the second or third day. These were those of a much lighter and milder grade, distinguished by the same phenomena—such as cool skin, continued perspiration, thirst, quick thready pulse, interrupted respiration, uneasiness and occasional sighing. These symptoms would deepen and become more urgent in the morning, a partial reaction taking place in the afternoon. In these, as well as those described, there was no marked remission or intermission; the patient continuing in a pathological condition until a *dryness or permanent warmth of skin*, announced a restoration of the functions of the body." Is not then a patient laboring under an attack of remittent fever of whatever form, in a "pathological condition" all the time prior to convalescence?

We confess we were for a moment *startled* into the belief that we had discovered one of the "pathognomonic symptoms" when we reached the passage descriptive of the evacuations. On more mature deliberation, however, we are impressed with the belief that such can scarcely be the case, seeing that not one of the "correspondents" makes mention of "this remarkable colluvies," and it is not probable that so extraordinary a phenomenon would have escaped the notice of some half a dozen medical gentlemen, all seemingly pretty accurate observers,—when especially questioned too relative to the disease in which Dr. Lewis found it,—if such stools were by any means of sufficiently frequent occurrence, to be considered characteristic of the disease. We are therefore led,—legitimately we believe,—to the conclusion, that this cannot properly be considered a "pathognomonic symptom," seeing that it is by no means of common or general occurrence in the disease.

We continue our quotations. "Notwithstanding the exhibition in this disease of *apparent strength*, (regarded by many as real,) we are firmly convinced that great muscular prostration exists. The patient is capable of these surprising efforts only at occasional intervals. In a moment of extreme agony, with his lungs and heart oppressed with dark blood, like the victim of asthma, he exclaims "I cannot breathe, I am smothering," and by an instinctive struggle his nervous energy is rallied for a moment—he starts up—rushes for the open door or window, and falls powerless on the floor. It is said by some that the action of the heart is "loud, strong and tumultuous," hence they conclude there cannot be diminished nervous power. True it is "loud and tumultuous" and often beats violently against the thoracic walls; but it is that tumultuous irregular action, (often seen from depressing poisons,) when the heart painfully labors to force on the stream of blood which flows in too fast for its exhausted powers. The patient often complains of fulness and oppression, and one ear placed over the heart, conveys to the mind the idea of a distended organ, laboring in vain to free itself. We have here nothing of the bold, distinct measured pulsations which belong to the heart in the simple phlegmasiæ."

It will be perceived in the above extract, that Dr. Lewis admits that the action of the heart is "loud and tumultuous, and often beats *violently* against the thoracic wall." Whether this statement is reconcilable with a "tremulous irregular action" or a mere "tremulous flutter," we leave to others to decide. This we do believe, however, that as a general rule the strength of the heart's action is judged of by the *loudness of its sounds* and by the *violence* with which it beats against the thoracic walls—in other words, its impulse. We most heartily concur with our author in the belief that the "heart painfully labors to force on the stream of blood," but at the same time also believe that it is not so much from "exhausted power" on its part—as from an increased centripetal tendency of the blood,—from what cause produced we cannot say,—that this is rendered necessary, and that the unequal distribution results. That there is then perverted innervation, irregular distribution of nervous influence, diminished nervous power, if the Doctor will have it so—we fully admit,—but, in regard to the heart and vascular system —in the capillary network, and smaller vessels alone. There can be most certainly no diminution of nervous influence to the heart, when its action is "loud," and it beats "violently against the thoracic parietes," at a time when the patient's pulse is a mere thread, or perhaps entirely extinct at the wrist. That we have not the "bold distinct *measured pulsations* which belong to the heart in many of the simple phlegmasiæ," so far as the latter qualities are involved at least, is easily explained. In these the heart is perhaps beating at the *measured* rate of from 90 to 120—while in the disease under consideration it strikes at the rate of 120 to 160 in the minute. That nervous influence may be unequally distributed,—that a diminished supply may be sent to the cutaneous surface, and to the capillaries, and an increased supply to the heart and larger vessels, will not appear improbable or unreasonable, when we reflect on the many anomalous exhibitions, connected with the nervous system in the various shades of our malarious diseases.

The Doctor is convinced that in this disease "great muscular prostration exists." If by this is meant that the patient is incapable of the same degree and continuance of physical exertion as in health, nothing is more true. If in comparison with other diseases, nothing we believe can be more erroneous. Is there any other form of disease, in which,—perhaps not more than a few minutes before death, as he himself has shown in the case quoted, and hours after the pulse has become extinct at the wrist,—the patient is able to rise from his bed, and walk across the floor, to a window or open door? Is it not most surprising—and in striking contrast with what occurs in almost all other forms of disease,—that the patient under such circumstances should be capable of these "surprising efforts,"—as our author himself most truly characterizes them, even at occasional intervals? But instead of being at occasional intervals only, is not the patient in most instances able to, and does he not actually change his position in bed, turning quickly from one side to the other without assistance—every few minutes from the time the disease puts on its malignant garb, till almost the last moment,—now, and then too, walking across the house from one bed to another, or to a window, and frequently without sinking exhausted—or "falling powerless on the floor,"—without in many instances, indeed, his respiration being appreciably quickened thereby, or giving other evidence that his powers have been diminished by the effort? How is it with regard to all other diseases—Typhoid fever for instance, or those of the most violently inflammatory character? Of what manifestations of muscular strength are the patients laboring under these, capable during the hours of impending dissolution, and when the pulse can be no longer felt at the wrist? And in these, under similar circumstances, is the sound of the heart "loud"—or does it ever "beat violently against the thoracic walls?" Even the "victim of asthma"—with "lungs and heart oppressed with dark blood," ceases to be capable of any great physical exertion before the pulse ceases to be felt at the wrist. We think that Dr. Lewis has here denied the existence of symptoms, which with more probability of successful argument might be presented as "pathognomonic," than any that he has named.

We do not think that it is by any means a common occurrence for the patients after such exertions as we have spoken of to "fall powerless on the floor,"—our own experience is against it, and we believe that we can show by Dr. Lewis himself that he did not regard it as such, or at least did not keep the fact at all times before his mind's eye;—else, we know not what language means. We commence at the end of our last quotation.

"The cold adynamic condition which so essentially characterizes this malady, has been likened to pathological states occurring in other diseases, after injuries, loss of blood, &c.; we are unable to trace such analogies. We have under treatment at the present time two rare cases, caused by subjects falling into a dry heated steamboat boiler, some five days since, in which the condition is not unlike that of congestive fever. The extremities are wilted and shriveled; the skin cold and damp, (but not perspiring profusely,) the action of the heart tumultuous and oppressed, the pulse in one small and thready, in the other imperceptible, expiration harsh and violent, much internal heat, oppressed breathing, and yet *muscular strength sufficient to walk* 100 *feet.*" Up to this point, we think it will be admitted that there is a very striking analogy between these cases and congestive fever as described by our author. Here, however, we presume, he means that all analogy is lost or rather destroyed by what follows—"*but the exertion is followed by sudden and complete prostration.*" Were it a common occurrence for patients after similar exertions in congestive fever to "*fall powerless on the floor*"—would not the analogy be most complete and striking?

W. M. B.

(*To be continued.*)

II.—*A Treatise on Malignant Fever and Vomito Priesto.* By William Ingalls, M. D. Fellow of the Massachusetts, Rhode Island and New Hampshire Medical Societies; formerly Professor of Anatomy and Physiology in Brown University.—*Venienti occurrite morbo.*—Boston: 1847, p.p. 108.

Dr. Ingalls informs the reader in his *preliminary remarks* "that this treatise on yellow fever was ready for the press in 1841; but having consulted a physician in whose judgment I placed great reliance, in the propriety of publishing it, who gave it as his opinion, that as the work of *Baron Louis* on the same subject, translated by Dr. Shattuck, Jr., had recently made its appearance, a *similar* production was not at that time required.

The foregoing remarks, aside from the bad grammar which they contain, display great respect for the reputation of M. Louis; and no doubt the noble Baron, when this book meets his eye, will feel himself under lasting obligations to Dr. Ingalls for delaying the publication of a work *similar* to his own on the same subject.

Respect for the grey hairs of our author, as well as the antiquity of the facts which he reports, (being collected in 1798 and 1819,) will compel us to be much milder in our criticisms, than the facts and reasoning here set forth deserve. The motives which induced Dr. Ingalls to publish these few disconnected observations, were doubtless good; and it is on this account that we shall treat him with that consideration and respect due his character as a practitioner of a past century.

Dr. Ingalls regards yellow fever like small pox, as a contagious disease; nor do we denounce him for embracing such a doctrine, since many highly respectable physicians of the present day believe in the transmissibility or contagious nature of yellow fever. On this point we are at issue with our author, but this is not the place and time to discuss the question.

Dr. Ingalls' pathology of yellow fever is too good to be overlooked.—He says, "the cause of *yellow fever* produces an inflammation of the mucous membrane of the stomach, intestines and pori biliari. That the inflammation partakes, at least in one respect, of the nature of erysipelas, appears from its frequently commencing in one portion of the alimentary canal, and being diffused over its whole extent, either uniformly or in successive patches."

The course of practice such pathological views of yellow fever would lead Dr. Ingalls to adopt, must be detailed in order to be accredited.—Says the Dr., "I used to carry with me pills containing two grains of tartrate of antimony, to prevent the delay that might be occasioned by sending a recipe to the druggists store. With regard to the dose, I was guided by the circumstances of the case; sometimes giving a pill every ten minutes till vomiting was produced; sometimes two pills at first; if they did not operate in ten minutes the third was given; in cases of extreme urgency three were administered at once; six grains proved to be a sufficient portion (to kill does the Dr. mean to say?) in every instance but *one*."

How our author can make his pathology and treatment of the disease harmonize, is indeed strange to us, who have been taught both by observation and a *little* experience, to believe that an inflamed gastro-enteric mucous membrane was intolerant of an irritant like tartar emetic.

We can but regret that an old and respectable practitioner like Dr. Ingalls should have given to the world a small work so full of crude and incorrect notions of the yellow fever—as, however, his aim was not to gratify his ambition but to enlighten the profession, we are disposed to commend his writings to the kind mercies of the critic and the intelligent reader.

A. H.

III.—*New Orleans Literary and Scientific Miscellany.*

We have received from Dr. Macauly, the editor, a prospectus for publishing in this city a literary periodical with the above well-chosen title. The object of this monthly publication is to *disseminate valuable knowledge—to cultivate a literary taste*, and to *advance the interests of morals and education*. With these high aims in view, we trust the able and worthy editor will receive such encouragement and assistance as the enterprise deserves. Why should not New Orleans be represented in the literary, as she is already in the commercial world? Have we not the material in abundance, and the talent to fashion it into beautiful and lovely forms? If commerce is *King*, let literature henceforward be indeed the *Queen*, that shall guide and direct with her gentle influence the wealth and commerce of New Orleans.

Commerce and literature may march, like twin sisters, hand in hand. The first may be made subservient to the diffusion and interest of the last. Let a printed sheet go abroad with every bale of cotton and barrel of flour that leaves our wharf, and receive in exchange from abroad, the *best* literature of the day. "Man was not made to live by bread alone;" he has other than his animal appetites to gratify.

This scientific miscellany will not be the champion of any particular religious creed. The editor, on this subject, says: "Whilst nothing will at any time be introduced which can prove offensive to the most scrupulous conscience, or be deemed an attack on any peculiarity of creed or opinion, it is proper we should distinctly state, that this is not intended to be a religious publication, nor is it identified with any sect. It is intended for the people, and all may inspire its contents as innoxously as they do the vital air."

The miscellany will be issued in monthly numbers of 104 pages, at $5 per annum, *payable on delivery of the first number.*

The editor has already engaged some of the best writers of the day, as contributors to the pages of the Miscellany.

IV.—*Wood's Quarterly Retrospect of American and Foreign Practical Medicine and Surgery, from April to July*, 1847. New York, Richard and George S. Wood.

This is a new publication of 64 pages, somewhat on the plan of Braithwaite's Retrospect. We give the publishers' prospectus below, that our readers may judge for themselves of the work. It is certainly cheap enough.

PROSPECTUS.

"The present enterprise is an attempt to meet the demands of this "high-pressure" age, by furnishing the physician and surgeon the means of keeping pace with the progress of knowledge in their respective departments of science, at *the least possible cost of time and money*. It consists of condensed reports of cases and their treatment, with occasional remarks, and abstracts of the medical literature of the day, collected from the whole field of medical science, American and Foreign, with announcements of all new publications of interest to the profession.

Its plan is, in the main, that which has been so much approved in "Braithwaite's Retrospect" and "Ranking's Abstract;" with the super-added advantages of a fuller view of American Medical Literature and Science, a more frequent emission, and reduction of price.

The project, which was suggested by some members of the profession as a desideratum in medical literature not yet supplied by any journal, was decided upon too near the day fixed upon for publication, to enable the publishers to carry out their plan as fully in the present number as they could wish. They trust, however, they have done enough to show its great utility; and they promise that, if well sustained in the undertaking, they will spare neither pains nor expense to make it worthy of the most extended patronage.

It will be seen at once that, at a price so low, it can only be supported by a very extensive circulation; but the advantages offered are such, that this is confidently anticipated; and they request all to whom this number is sent, who approve the plan, to aid them by bringing it to the notice of their professional brethren.

TERMS—*One dollar per annum, payable in advance.*

Those wishing to subscribe, are requested to send their names immediately, that the publishers may know how many to print of the next number. They will please write their names legibly, and at full length, adding their respective titles, and the names of the town, county, &c., of their residence.

By the new Postage Law, subscribers to Periodicals are allowed to save postage and the risk of transmission by paying their own postmaster, requesting him to give notice thereof to the postmaster where the publisher resides, and remitting his receipt, instead of the money, to the publisher. We hope they will take advantage of this.

Authors and publishers wishing their works reported, will forward copies."

We hope the editor will hereafter be more particular in crediting his articles. We observe that the extract from Dr. Boling's "Remarks on Remittent Fever complicated with symptoms of Tetanus," is not placed to the credit of this Journal in which it first appeared.

Part Third.

EXCERPTA.

1.—*The following interesting 'History of Histology,' is translated from the Traité D'Anatomie Générale ou Histoire des Tissus, etc.* By J. Henle, Professor of Anatomy and Physiology in the University of Zurich.

Introduction.

The human body is composed of a certain number of organs. Each one of these organs, considered separately, may be reduced into parts which have no analogy with each other. But it is easy to perceive that these parts are reproduced in different organs, either because they really constitute together one body, and represent one continuous whole, as the nerves, the vessels, the layers of the cellular tissue, or because they resemble each other in certain characteristics which we consider essential, and differ only with regard to properties less important, such as form, volume, and others of a similar nature.

The science, whose object it is to investigate the similar parts in the different organs, to compare them together, and to assign to them their appropriate characteristics, is called *General Anatomy* or *Histology*. The parts which constitute the organs are called *Tissues*.

Development of Histology.

Histology is as old as the science of the structure of the human body in general; for the most ancient observers had seen themselves that certain bones, tendons, vessels, &c., appeared in all regions, with the same properties; and the physicians of the remotest ages supposed the identity of certain parts, different as to form and relative position, when they laid down, for example, for the treatment of a fracture, general rules applicable to all the bones. But there was not as yet any histological system, and the principles were not known by virtue of which such and such parts were regarded as possessing the same nature. Fallopius, to whom we are indebted for the earliest treatise on general anatomy, established, indeed, rules for the classification of the tissues;* he also divides them, for example, according to their origin, into parts which proceed from the blood, and into parts which are derived from the principle of production; or, according to their form, into tissues warm and cold, humid and dry; but he does not follow a single one of these divisions, and contents himself with passing in review, one after another, a certain number of tissues, of which he develops the texture and the uses. Before and since his day, the science had the benefit of a multitude of disconnected observations upon the intimate structure of certain organs and systems, particularly upon the subdivision of the smaller blood-vessels; but we must come down to the beginning of the present century in order to find histology reduced to a system, and presented in the scientific form which it still retains, and which has exerted the most decisive influence upon physiology and medicine. The author of this form, and to speak properly, the author of General Anatomy, was Bichat.

* *Lectiones de partibus similaribus humani corporis ex diversis exemplaribus.* Nuremberg, 1775.

Bichat's mode of examining histology had been preceded immediately by the discoveries of Haller. Haller attributed a special force, irritability, to those of the animal fibres which contract by the effect of contact with external bodies: the greater the irritability, the greater the contraction. He denominated sensible fibres, those which, on being touched, transmit an impression to the mind.* He and almost all the physiologists of his epoch studied especially the parts and tissues of the body with reference to their sensible and irritable nature. But he thence deduced that to the organized living fibres belong certain determinate forces, which are called into action by the most diversified external influences, by the aid of which the organic fibres are distinguished as well from all inorganic bodies, as among themselves. It was easy to conceive the idea of a physiological force of the tissues, and to discover that particular physiological operations are the result of special animal substances, irritable, and reacting in a manner peculiar to each one. The reflections of Pinel upon the analogy of pathological phenomena in the membranes of the different organs exerted considerable influence upon Bichat, as he himself acknowledges. "Of what consequence is it," said this great physician, "that the arachnoid, the pleura, the peritoneum, are situated in different parts of the body, since these membranes have a general conformity in their structure? Do they not undergo analogous lesions in a state of inflammation, and must they not be reunited in the same order, forming only different species?"† It was an idea as bold as it was fruitful to compare diseases of the mucous membrane of the stomach with catarrh of the pituitary membrane and blennorrhagia. Pinel established thereby the first basis of the natural classification of diseases according to their anatomical characteristics, a classification of which our epoch is so proud; but he rendered double service to histology by interesting physicians in its progress, and by teaching them to avail themselves, for the distinction of the tissues, of the manner in which they were affected in a morbid state. Finally, we must not omit to mention the part which the advancement of the physical sciences already so marked took in the labors of Bichat. He himself observes, in order to find fault with it, how much the method pursued by the physiologists differs from that adopted by philosophers: the philosopher sees every where phenomena of weight, elasticity, &c.; the chemist ascribes all those which he witnesses to affinity; but the physiologists had not yet ascended from the phenomena to the properties of matter which produce them. Consequently the first thing to be done in physiology is to investigate the properties, organic and vital, of animal substances.

The different tissues are then, according to Bichat, so many different substances, endowed with particular forces, by the concurrence of which the organs are formed, and with properties upon which depends the action of these organs, almost in the same manner as the motion of a machine is the consequence of the elasticity of metal and the weight of water. He describes each tissue according to its chemical and physical characteristics, its vital properties and its morbid changes. The materials necessary to fill up this outline were supplied to him almost entirely by his own researches, by vivisections, by dissections, by separating the tissues by the aid of the knife, by maceration, and by chemical re-agents.

In France where Bichat propagated his doctrines himself by public teaching, and where his death, the consequence of every species of excess,‡ caused general emotion, his opinions soon took deep root. They were known in Germany

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* *Mémoire sur la nature sensible et irritable des parties du corps animal.* Lausanne, 1756, t. 1, p. 7.

† *Nosographie philosophique*, 6th edition, Paris, 1818, introduction, p. xvii. The first edition appeared in 1798.

‡ I am told that this is not true, and that Bichat led a most exemplary life. Be that as it may, the expression used, "suite d'excès de tous genres," is susceptible of no other meaning. (M.)—*Trans.*

by the translation of the General Anatomy, published by Pfaff; but they did not begin really to flourish there until Walther had animated them somewhat with the spirit of philosophy which prevailed at that time in the countries beyond the Rhine.

Nevertheless the system of Bichat fell short of the end which the author had so clearly in view, and which he made such efforts to attain. The tissues which he considers as simple, and which in their quality of elements of organic bodies he compares to hydrogen, carbon, nitrogen, &c., are the following:

1. The cellular tissue.
2. The nervous tissue of animal life.
3. The nervous tissue of organic life.
4. The tissue of the arteries.
5. The tissue of the veins.
6. The tissue of the exhalant vessels.
7. The tissue of the inhalant vessels and of their glands.
8. The osseous tissue.
9. The medullary tissue.
10. The cartilaginous tissue.
11. The fibrous tissue.
12. The fibro-cartilaginous tissue.
13. The muscular tissue of animal life.
14. The muscular tissue of organic life.
15. The tissue of the mucous membranes.
16. The serous tissue.
17. The tissue of the synovial membranes.
18. The glandular tissue.
19. The cutaneous tissue.
20. The epidemic tissue.
21. The pilous tissue.

Among these tissues there are very few which are simple and homogeneous. The greater part of them are organs, some composed, as the arteries, the veins, the lymphathics, the serous and mucous membranes, of many tunics of a different structure and endowed with different vital properties; others resulting from particular elements combined with the cellular tissue and the vessels. Some organs of a formation evidently specific, are omitted, such as the yellow ligaments, the crystaline lens, and the cornea. Some tissues of a similar nature are found subdivided into two or three classes. Many of these errors were speedily discovered, and the histologists who succeeded Bichat suppressed certain tissues, for instance that of the exhalant vessels, united others under a common appelation, and added some new ones, as the erectile system of Richerand, and the elastic tissue of Cloquet. The attempt was also made to group the tissues; for example, by dividing them into general and simple (Meckel,) or into simple and compound (Rudolphi, R. Wagner,) or into simple, complex, and compound (E. H. Weber.) All these systems were modifications of that of Bichat; but their authors abandoned little by little the principle which had served as a starting point for the founder of the system; and although many of them arranged the materials in a more convenient order, nevertheless it was impossible with the insufficient means employed, to arrive at a vigorous classification. It is neither the external appearance nor chemical action of the tissues which can furnish the essential characteristics by which to distinguish them from each other. Their physiological functions are important, no doubt; but the functions of many of the tissues are uncertain, and it has happened much more frequently that the identity of function has been admitted on account of the analogy of structure with other known tissues, than that the morphologic identity of two tissues deduced from a real knowledge of their functions. Thus, for instance, the contractility of the middle tunic of the arteries was denied, because great weight was attached to a superficial resemblance between this tunic and the elastic tissue, whereas a more profound investigation of its physiological resemblance would have led to a comparison between it and the muscles of organic life. An acquaintance with the structure of the tissues, properly speaking, upon which must be based every good classification, is impossible without magnifying them greatly; for certain organs appear homogeneous to the naked eye, which are really composed either of fibres or of granulations, or of both, while other organs which are formed of elements totally different, resemble each other in their purely physical qualities. The following investigations will furnish abundant proof in support of this assertion.

In fact, the microscope had already been in use for a long series of years; but it was confined to the hands of a small number of persons, who pursued a separate course. It was at first the simple pleasure of seeing wonders concealed from the naked eye, that induced Leeuwenhoek, Ledermuller and Gleichen to make observations. He first mentions frequently, in his letters, how the idea occurred to him one beautiful morning, to examine such and such substances, to-day the tartar on his teeth, to-morrow the deposit in his wine. This first period of infantile curiosity all those passed through in their turn, into whose possession a microscope fell. Leeuwenhoek was also frequently led by one discovery to a series of methodical observations, and often he made the happiest applications of his instrument to the physiological functions, for example, to the circulation of the blood, and to generation; but he never thought of comparing together the elements of the different organs. It is only in pursuance of his own good pleasure that he describes the fibres sometimes as tendons, sometimes as muscles or vessels, and the cellules as granulations, vesicles or scales. Toward the close of the preceding century excellent microscopic observations of some liquids had been made in England, in Holland, and in Italy; we may mention particularly in this connection Hewson, Muys, and Fontana; but it was not until 1816 that Treviranus undertook to resolve the tissues into their simple elements, easily discovered by the aid of the microscope, that is to say, into parts of a legitimate form, by reference to which we see clearly that they are not accidental fragments, and of which each one possesses the properties of the whole. These were called the elementary parts. Treviranus and most of the contemporary observers admitted three kinds of elements: 1st, homogeneous or amorphous matter; 2nd, cylinders or fibres; 3rd, globules. In place of the tissues of Bichat's system, figured here and there at that time the elementary parts. We find sometimes in the works of histologists the expression, "the muscular tissue," "the osseous tissue," "the vascular tissue," &c., replaced by those of "the muscular fibre," "the osseous fibre," "the vascular fibre." But this was the period in which men preferred to build up systems rather than to search for facts, and in which they selected from among the actual observations, not the most certain, but those which were best adapted to their own views. Was a general anatomy possible while the most erroneous ideas prevailed with regard to the intimate structure of the most extensive of all the tissues, of that which entered into the composition of almost all the parts, the cellular tissue, which the majority considered as an amorphous mucus, indeterminate, but susceptible of the most diversified developments? It was necessary to begin with the study of this tissue, and since it was described almost simultaneously (1834,) and nearly in the same manner, by Krause, Lauth and Jordan, we see discoveries succeeding each other with such rapidity, that the zeal for making observations does not leave time now for the establishment of a system. May this state of things continue for a few years longer! We have many materials to collect before it will be necessary, before it will be even prudent, to arrange and to classify them. The only thing is not to lose sight of the end in view, and to advance continually, sustained by the hope of attaining it. In fact, it becomes more and more manifest every day, that the same tissues regulate the same functions in all the organs, that the different physiological phenomena depend upon elementary parts morphologically and chemically different, and that a time will come when we shall be able, as Bichat desired, to reduce organism to a certain number of simple tissues, the names of which will recall the idea of determinate vital actions, in the same manner as an inorganic body is connected inseparably with the idea of specific gravity, of friability, of elasticity, &c.

But microscopic studies have produced other fruits still. The human mind has always been disposed to bring back the different forms of creation to a small number of simple primitive parts. It is to this innate tendency that the systems of Epicurus and of Leibnitz owe their origin, both of whom conceived their atoms, their monads, without reference to observation and without the slightest hope that it would ever confirm their views. Urged knowingly, or unwittingly, by the same instinct, many moderns have endeavored, arming themselves with the microscope, to reduce the body into particles of similar form. The first which were observed before people had learned to distrust the instrument, were optical illusions, undulatory filaments and globules, which under certain circumstances are to be seen in every transparent object. Oken considered the infusory and spermatic animalcules as real monads. According to him the superior organisms, animal and vegetable, are composed of smaller animated beings, which have renounced their independent existence for a certain period of time. Dœllinger and his school constructed the body of globules of blood, put in motion in the unwalled fissures of matter, susceptible of being united with the latter, to be afterwards separated from it, and to which C. Mayer even went so far as to attribute a particular life, sensibility and spontaneous motion. Heusinger explained in the following manner how the fibres and tubes may proceed from spherical elementary particles: the sphere is the expression of an equal contest between contraction and expansion; it is on that account that all the organisms, all the organic parts, were originally globules; when the forces experience a greater tension, the vesicle is seen to emanate from the globule, which has often only the appearance of homogeneity; when the globules and the amorphous mass meet in organism, they are arranged into series, according to the laws of chemistry (?), and form fibres; when the vesicles are placed one after another we have canals and vessels. We see that this theory singularly approximates the truth, although the facts alledged in support of it are some of them inaccurate, and others badly explained; for Heusinger classes, for instance, among the simple vesicles, not only the adipous and mucous follicles, but also the serous membranes, and he considers the valves of the lymphatics as traces of the previous separation of the vesicles which have united to form these vessels.

What Raspail says of the formation of molecules or organic atoms, of their configuration and of the forces by which they are animated, rests already upon a better foundation. At the moment of its formation, the organic molecule, reduced still to its most simple chemical expression, results from a combination of hydrogen and carbon; it is liquid and oleaginous, and enjoys already the faculty of aspiration; placed in atmospheric air, it absorbs principally oxygen, and like all liquid molecules, it assumes the spherical form as soon as it is suspended in water. At the same time that it absorbs atmospheric gases, it has a tendency to combine with inorganic bases. When this combination has once become intimate, the sphere is composed: 1st, of a vesicular envelope permeable to certain gases and to certain liquids, capable of developing itself and of enlarging; 2nd, of a liquid which continues to be organized within this envelope. The vesicle is then an organ endowed with the faculty of reproducing itself *ad infinitum*, and organizing according to its type the liquid which fills it and animates it. Whenever we have under our eyes the wall of a simple cell, in its fresh state, it is impossible, however much it may be magnified, to discover in it the least structure, that is to say that it seems to be homogeneous; but analogy leads us to believe that this membrane so simple in appearance, is composed of primitive globules, ranged spirally around the ideal axis of the cell. It must be admitted then, necessarily, that the wall of the maternal cell results from globules of the same nature and of the same aptitude for development, so that we can conceive of a cell formed, and so to speak, paved with globules all touching each other at six points of their equator, and whose axis is confounded with the radius of the sphere of which their union delineates the envelope. These globules are all equal, all endowed with an equal aptitude for development; but those only are developed, which meet at the points of intersection of two spires moving in a direction opposite to each other. Raspail compares these cells to crystals, atoms of organic creation, and gives to organization the name of crystalization in vesicles, vesicular crystalization. The organic cell is a crystal which absorbs gases and liquids, in order to convert them into internal organs; it is enlarged by organs of the same nature and the same aptitude engendered within itself, while the inorganic crystal increases only in surface, by the aid of successive juxta positions, and end to end. As soon as the chemical elements are united in the form of cells, they acquire particular forces, and constitute a separate kingdom, the organic kingdom. Give me a vesicle capable of absorbing, exclaims this author, parodying Archimedes, "and I will make you an organism."

Raspail refers, in support of his theory, to the cells of starch in the vegetable kingdom, and to those of fat in the animal kingdom. He examined profoundly those tissues which are really most likely to give rise to the idea that vegetables and animals resemble each other in their elementary parts. As it had already been demonstrated, with respect to the tubulous and fibrous tissues of vegetables, that they are produced by cells extended or confounded together, Raspail adopted these views for the animal fibres also. Dutrochet arrived at the same results by a comparison of the intimate structure of the animal, with that of the vegetable tissues. He discovered that the elements of the salivary glands, and the gray substance of the brain are utricles, of which those of the cerebral matter present, upon their walls, a multitude of opaque punctuations, exceedingly small, which he compared, improperly, to the numerous punctuations of the vegetable cells. He concluded, moreover, that the globules which compose, by their agglomeration, the greater part of the animal organs, are small membranous vesicles containing a liquid. This consideration induced him to reject the former distinction established between the solids and the liquids of the body: the solids are aggregates of globules having a certain solidity; the liquids, such as the blood, are equally aggregates of globules, but disconnected; and in animals there are certain component parts in which the globules are so slightly connected, that we do not know whether we ought to consider them as liquids, or as solids. There is but a single organic solid, the membrane of the urticle, or of the cell; the contents of this latter may very well become solid too, but life exists, at least with a certain degree of activity, only so long as the substances contained in the cells are liquid; the solid contents of the cells that have grown old have generally become strangers to life. The muscular fibres, and the other animal fibres, are only cells very much elongated, as we find them among the vegetables. Nature follows then a uniform plan in the intimate structure of all organized substances, as well animal as vegetable. The one and the other agglomerations of utricles, sometimes globular, sometimes elongated. The elementary utricles, as Dutrochet calls them, all have a general resemblance, and differ only in the nature of the liquids they contain. Nevertheless, the difference in the liquids they contain establishes a difference in the intimate nature of the membrane which forms the elementary utricle, for it is this membrane which secretes the liquid contained in the interior of the cavity which it forms.

Neither Raspail nor Dutrochet has attempted to establish the laws of the organic development which they have announced with so much boldness, and, it must be confessed, with such beautiful simplicity, for the different animal tissues. Their observations are deficient in this respect; their theory has therefore remained stricken with sterility, and has passed unnoticed. Besides, neither the one nor the other observed, or at least has mentioned, an organ which plays a very important part in the development of the cells; I mean the nucleus.

As early as 1831, R. Brown had discovered the nucleus in the cells of Vegetables; but it was reserved for Schleiden to assign its uses. Schleiden demonstrated that the utricle, round or oval, which lies in the wall of the vesicle, is a sort of plastic organ of this latter, since it begins by completing its own development, and then the cell itself, which rests upon it at first as upon a watch glass, is formed and enlarges little by little. The ancient observers knew already some microscopic vesicles, belonging to animal organism, which were furnished with a speck or nucleus, to wit, the corpuscles of the blood. But within a few years past, similar elements have been discovered in a great number of other liquids and tissues, in the lymph, the mucus, the pus and humor of Morgagni, in the epidermis, the black pigment, the cartilages and the central organs of the nervous system, in the glands, and even in some pathological productions. The reproductive vesicle itself, at the expense of whose contents the animal is developed, has been recognized as a cell furnished with a nucleus. Some authors supposed an analogy of these cells with each other, and several, such as Purkinje, Valentin, and Turpin called attention to the resemblance between these and the vegetable cells. The pre-existence of the nucleus and the gradual formation of the cell around it were demonstrated before the appearance of Schleiden's work, by Valentin in the pigmentary utricles, by E. H. Schultz, in the corpuscles of the blood, by R. Wagner, in the egg, by myself in the cells of the epidermis. The formation of new cells in the older had been observed by Armand de Quatrefages and Dumortier, in the embryo of testaceous animals. Valentine had found even in the muscles and the substance of the crystaline lens, instances of fibres developed at the expense of vesicles or granulations. But it was Schwann who first advanced the proposition that the cells furnished with a nucleus are the base of every formation, animal as well as vegetable. He develops this proportion in a work devoted specially to that purpose, and it was received with so much the more favor, as it furnished a key to many known facts, and indicated the direction which should be given to further researches conducted with reference to a uniform plan. Schwann himself examined according to this principle the development of the greater part of the tissues, availing himself of the observations already made, and endeavoring to supply the deficiencies by his own researches. Although there remain yet, in the details, many doubts to be removed, more than assertion which must be rectified, and it seems that the cells furnished with a nucleus are but a species or secondary form of the elementary organic parts, nevertheless, our contemporaries should always be grateful for the influence exerted by the labors of Schwann.

The most obscure ideas continued to prevail in treatises on physiology, with regard to the nutrition of the organs, and the forces by which their growth, secretion, and reproduction are accomplished. Some represented these operations as depending upon the nervous system, others attributed them to the blood-vessels, although they ought to have arrived at other conclusions from seeing the germ produce, with a homogeneous substance, not only organs, but also their nerves and their blood-vessels. It is one of the chief merits of Schwann that he has shown the presence of the vessels does not establish an essential difference in the growth, that it determines only certain modifications which are explained by the distribution of the nutritive liquids and the greater or less facility thus acquired for the reproduction of materials, while on the other hand, the study of the functions of the nervous system led to a more correct appreciation of the part which this system performs in the circulation of the blood, and thereby in nutrition. I shall discuss this point of doctrine in the chapters devoted to the vascular and nervous systems.

We have arrived at this result, that organism is composed of a certain number of elementary parts, monads or organic atoms, which, controled and kept together by some power beyond our means of investigation, are arranged and developed conformably to a certain type. These monads are endowed with particular forces, for it suffices that they should have a common origin, either the yelk or the blood, in order to form, and to nourish all the cells, each one in its order. General anatomy, in order to constitute the science of the efficacious elementary parts of the body, ought now to divide these monads, to begin by studying their structure, formation, forces, chemical and physical properties, and afterwards to form them into tissues, which are nothing more than aggregates of a multitude of homogenous elementary particles. A rational system of histology ought to assume as the basis of its divisions, the metamorphoses of the cells, so as to form groups of tissues, according, for example, as the cells remain separate, or are arranged into series one after another, or are, formed into stellated ramifications, or are divided into fibres, &c. But the facts are neither sufficiently numerous, nor sufficiently conclusive as yet to enable us to follow this method with certainty, and the attempts which have been made thus far, offer us but little inducement to imitate them.* I prefer, therefore, to pass in review one after another the tissues and the organs, as they have for a long time been distinguished anatomically and physiologically, confining myself to a description of their intimate structure, and their vital properties, and contenting myself with mentioning, incidentally, the resemblance existing between their elementary parts. The order of succession of the chapters was a matter of no consequence; nevertheless I have endeavored as much as possible to avoid anticipation, and to commence with the tissues a knowledge of which seemed to be useful in further researches. One, and the first section, will contain all that it is possible to observe or to conjecture with regard to the development and the life of the cells in general.

* Schwann divides the tissues into five classes: 1st. The isolated independent cells: corpuscules of the lymph, the blood, the mucus and the pus, &c; 2nd. independent cells united into coherent tissues; he places in this class the epidermis and the other productions called corneous, the black pigment, and the crystaline lens; but we find cells confounded into fibres in the hair, feathers, nails, and the crystaline lens, and besides there are some ramified pigmentary cells which correspond with each other; 3d, cells of which only the walls are confounded together: cartilages, bones, and teeth; but the walls of the cells are not confounded in the spongy cartilages, and the dental bone is composed principally of cells arranged one after another in a right line, like the fibres of the hair; 4th, fibrous cells: the cellular tissue, the tendinous tissue, the elastic tissue; here, the cells should, according to this author, be divided into bundles of fibres; the cellular and tendinous tissues, which moreover do not differ from each other, cannot absolutely be connected with the elastic tissue when considered with reference to their development; 5th, cells in which the walls and the cavities are confounded together: muscles, nerves, capillary vessels. We must oppose to this last class the fact that what are called the muscles of organic life do not differ from the cellular tissue, with respect to development, and that in reality the cellular tissue and the organic muscles pass from the one to the other by insensible gradations; the muscles of animal life, on the contrary, and the nerves, appear to be, as I shall show hereafter, complicated organs, the development of which is probably not the same thing as the primitive wall of the cell. Schwann treats of fat in connection with the cellular tissue, and of ganglions with the nerves, although these parts are entirely different, in a morphological point of view. He does not speak of the glands, nor of many other particular organs. Valentin (R. Wagner, *Lehrbuch der Physiologie*, t. 1, p. 133,) has proposed another classification, and established a greater number of species; we cannot give an opinion with regard to his labors without descending into details which would lead us much too far. This classification contains imperfections analogous to those which were urged against Schwann's system, many of which it has even adopted. Valentin also classes all the corneous tissues among the tissues with separate cells; he groups together the cellular tissue, the elastic tissue, the muscular fibres, &c. Gerber (*Allgemeine Anatomie*, p. 18,) has given a sketch, in the form of a table, of the elementary animal parts; but he has paid too little attention to their mode of development, and attached too much importance to slight differences in the form of the tissues developed. Thus he distinguishes the flat filaments, hollow filaments, and round filaments, comprehending among the latter the cellular tissue, the muscular fibres, and the fibres of the fibro-cartilages, while he refers to the class of hollow filaments the nerves and the small channels of the teeth.

2.—*Dr. McWilliam's Report.*

There are a few facts connected with the report on the Bona Vista fever which still require notice. Among these may be mentioned the reappearance of the disease at Moradinha, on the 30th May, 1846. Dr. McWilliam gives us the following description of the symptoms, as they appeared in a girl, 14 years of age, and in a man, about 35 :—

" The countenance of the girl, a dark mulatto, was of a dirty lemon colour, shining through the natural darkness of the skin, made it resemble very much that of a light bronzed statue. A very strong fœtor issued from the body, which tainted the room and drove us back from the door until the window was opened. She had complained much of pain along the spine, and still pointed to her head as the seat of pain. She had been bled in the arm by one of the neighbors, and all around the wound was of a greenish color, swollen, and putrid. In the angles of the mouth there was dark frothy blood. She had had black vomiting; but this symptom had for some hours ceased. The urine was black,* as were also the fæces. The former had been voided in very small quantities. Pulse small, irregular. She had been ill seven or eight days.

" The man had nearly the same symptoms, but in a milder degree, and he had not been affected with black vomit.

" Being persuaded that the disease had, during the epidemic, manifested highly infectious qualities, we at once requested Senhor Librao, Administrador do Cancelho, to have the sick put into as large a house as the village afforded ; to cause a temperature of 140° Fahrenheit to be introduced into all the houses ; to have the clothes of two persons who had already died burnt, and to have the village surrounded by soldiery; sending all supplies under proper precaution, and preventing ingress or egress to all except the medical attendants. On our visit to the village early the following morning, the girl was just expiring ; but the man was not worse, and was reported to have rallied somewhat during the night. The nausea had in a great measure subsided under the use of small and repeated doses of quinine. The urine gradually became lighter, and increased in quantity ; the headache was relieved : he took some nourishment, and was convalescent in five or six days."

The mode in which the disease was brought into this district, was clearly traced :—

" A girl called Maria dos Prazeres was the first person attacked at Moradinha on this occasion. She had visited Joao Gallego and the other eastern villages on the 15th of May, and returned to Moradinha, where she was laid up with fever on the 20th of the same month. Her mother, who slept with her one night after she was taken ill, was also attacked on the 26th, and died on the 29th, after three days' illness. The girl whom we saw was called Perpetua. She had visited Maria, soon after which she was seized with fever, and died with the symptoms which have been already described, on the morning of the 1st of June."

The strong resemblance which the disease prevalent in the island bore to that which spread among the crew of the Eclair, justifies the inference that they were identical.

" Dr. Almeida, who was present during the early period of the epidemic, and Messrs. Moraes and Leao, surgeons in the Portuguese navy, who arrived in February, informed me that in the majority of the cases *there were yellowness of skin, suppression of urine, and black vomit.* The two former symptoms existed in the cases I saw at Joao Gallego in April, but there was no black vomit.—*Suppression of urine was so common a symptom, that it has been mentioned to me by all classes* with whom I spoke on the subject of the fever. It is worthy of

* Circumstances prevented a chemical examination of the urine ; but it seemed to owe its color to the same cause as the matter vomited in black vomit, namely disorganized blood.

remark that, according to Louis, this was an extremely rare symptom in the epidemic at Gibraltar in 1828. *It seems then hardly to admit of a doubt* that the disease with which the crew of the Eclair were afflicted, that which proved fatal to the European soldiers at the Fort, and that which soon afterwards prevailed on the island of Bona Vista, were identical."

As additional proofs of the importation of this disease, and of its propagation by contagion, we quote the following from Dr. McWilliam's summary :—

"After most minute and repeated investigations at Portô Sal Rey, at Rabil, Estacia, as well as the Eastern villages, I have failed to discover that there was disease of any kind on the island until after the arrival of the Eclair. If to testimony such as this be added the fact that Moradinha, which is situated in the Rabil Ravine, and Estacia de Baixo, which overlooks it (both of which places have always come in for a great share of remittent fever at any time that disease was prevalent,) were in a most unaccountable degree exempt from fever during the late epidemic, then I think that the source of the disease is not to be found on the island, and therefore is to be looked for among the sick crew of the Eclair.

"The previous healthy state of Bona Vista will, I think, not now be questioned, nor will the occurrence of the disease in the two European soldiers at the Fort soon after the departure of the Eclair's people. If, then, the absence of all local cause on the small island, and the identity of the diseases that proved fatal to them and to the sailors be admitted, the inevitable conclusion is, that the fever was propagated to the soldiers either directly by contagion from the bodies of the sailors, or by an infectious matter left in the room which had been occupied by the sick crew. The same reasoning applies to the island of Bona Vista, to which it seems beyond any doubt that the fever was conveyed by the negro soldiers, and by them transmitted to Anna Gallinha and their other visitors at Pao de Varella."

At the time that the reporter was engaged in collecting these observations, Dr. Bowring was contending for the abolition of the quarantine laws, on the ground that there was not "the slightest evidence" that this disease (African fever) was communicated from one person to another. In support of this argument, he quoted the authority of Dr. McWilliam! As the subject will again come on for discussion in the Houses of Legislature, we may put in juxta-position with this bold assertion, another quotation from the Report. This is the more necessary, because the facts stated by the reporter tend to show that cutting off all intercourse with the sick had a remarkable influence in preventing the diffusion of the disease in the island. We have seldom met with a case in which the prudence and utility of maintaining quarantine restrictions, were so clearly demonstrated.

"In fact it may be said that in each town and village on the island the disease first appeared in a single house, which became an irradiating focus for its dispersion in all quarters. The exemption in persons who removed in time from infected places was clearly shewn in many instances. Dr. Almeida, by changing the residence of his family from place to place, succeeded in keeping the whole of them intact. The same gentleman for some time prevented the introduction of the disease into Fundo das Figueiras, by the establishment of a sanitary cordon, and afterwards retarded its progress by the imperfect means of segregation of the sick which he had in his power. Boaventura was for some weeks without a case of fever, although the disease was raging at Cabeçada, a few hundred yards only from it, by the adoption of measures to prevent intercourse. When making the circuit of the island in May, I fell in with many families who had fled from the several villages at the outbreak of the fever. At a place called Espinguera, near Mount Broyal, on the North side of the island, there were three families, amounting to twenty-four persons, who had left Joao Gallego early in November, and they had all escaped. At Cantor, on the North-east base of Mount Ochello, there were fifteen people from

Cabeça dos Tharafes and Joao Gallego, who had left these places when the fever appeared. The original number that had taken refuge here was twenty-three or twenty-four, but eight persons had imprudently returned to the villages in January; of these all had fever, and four died. At a small hamlet only one mile and a half to the Eastward of Cabeça dos Tharafes, there were twelve persons from that village who had been completely exempt during the whole of the epidemic, merely by never leaving their place of refuge, and allowing no one from the villages to come near them. Other instances of complete immunity from seclusion are on record in the evidence appended to this Report.

"From the above statements it is evident that the fever at Bona Vista possessed the properties which are usually attributed to a contagious disorder; and, connecting this fact with the time and circumstances of the seizure of the soldiers at the Fort by fever, and of the appearance of the disease in Porto Sal Rey, will, I think, leave no doubt of its introduction into the island by the Eclair."

The supposition entertained by Sir W. Burnett, that the fever had an indigenous origin, and depended on a vitiation of the air, is thus swept away:—

"It is thus evident that all of these islands are within a few hours' sail of each other with the North-eastern trade, which blows in this quarter with little variation throughout the year. Now, had the disease depended upon general vitiation of the atmosphere over the Cape de Verds, why did Sal, San Nicolao, San Antonio, and the other islands escape, while the pestilence was spreading far and wide upon the devoted Bona Vista? The supposition (if there be such) that there was an unwholesome condition of the air over the islands and corresponding portion of the African coast, cannot be reasonably entertained; for it is not in the usual order of nature that a cause so diffused would produce an effect so merely local. An attempt has been made in a former part of this report, to show that the origin of the fever on the island of Bona Vista, was not attributable to an indigenous cause, but that it was the result of the visit of the Eclair."

The main conclusions at which Dr. McWilliam arrived, are the following:—

"1. That the fever on board the Eclair was primarily the remittent of the African coast, which is not a contagious disorder, but that the disease acquired contagious qualities in virtue of a series of causes.

"2. That although there exists on the island of Bona Vista a physical cause capable of producing remittent fever, yet it does not appear that that cause was in action when fever broke out in September 1845, and that the island was quite healthy when the Eclair arrived there.

"3. That the disease of which the Portuguese soldiers died at the Fort (Duke of Braganza) on the small island, was that which afterwards ravaged Bona Vista, and the same as that which prevailed among the crew of the Eclair.

"4. That the fever was propagated throughout the island almost exclusively by direct intercourse with the sick, there being only two cases in which there appears any probability of persons having been infected in any other way.*

"5. That although those who had passed through the fever were much less liable to the disease than those who had not, yet it would appear that a person having had one attack, possesses no absolute protection against a second attack.

"6. That connecting the whole of the circumstances attending the arrival and stay of the Eclair at Bona Vista with those under which the disease appeared on the small island, and afterwards on Bona Vista itself, leaves no doubt of its having been introduced by the Eclair.

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* In one of these, the fever was supposed to have been conveyed by a blanket which the man had in his possession.

"7. That in all probability the mortality from fever on the island was much increased by the want of proper nourishment for the people, as well as by the total absence of medical assistance for some months."*

The important questions which Dr. McWilliam had to solve for the information of Government, are therefore conclusively settled in the affirmative.—The first conclusion is, we consider, open to exception; and so far as it bears on the main subject of inquiry, quite superfluous. That medical writers should agree about the essence of a particular disease, is not to be expected. Whether the fever in the Eclair was from the very first a contagious disorder, as Sir William Pym believes, or whether it only acquired a power of propagating itself by contagion when the ship arrived at Bona Vista, as Dr. McWilliam thinks, is really immaterial to the issue; because the Doctor has already proved most satisfactorily that, but for the arrival of the ship, the island would have been spared this terrible calamity, and this is all that the advocates of contagion and quarantine need regard. If Dr. McWilliam's first conclusion be sound, it makes the case even more fearful than we could possibly have anticipated; because it is admitted thereby, that a comparatively mild and tractable disorder may, by a vessel touching at an island, become converted into a formidable and destructive plague, against which nothing but the strictest seclusion can protect the inhabitants! This hypothesis of "acquired or contingent contagion," where the "circumstances," although stated to be "peculiar," can neither be defined nor foretold, would, if well-founded, have a most dangerous influence on the intercourse of nations. Governments must look to practical results. The Governor of Goree refused all intercourse with the Eclair, and the island escaped;—the Governor of Bona Vista following medical theories which were placed before him in a plausible form, allowed such an unguarded intercourse, as to lead to a sweeping mortality, and to his own island being placed for a long period under strict quarantine! If, therefore, Dr. McWilliam's views are to be made the basis of future precautions in the Cape de Verd and other islands, the authorities are bound to place, in the strictest quarantine, every vessel that happens to have on board a case of the remittent fever of the African coast, which is commonly admitted not to be a contagious disorder! There is obviously no alternative when the "series of causes" which render a mild disease intensely contagious, cannot be defined or described.—The facts, however, do not appear to us to bear out Dr. McWilliam's theory, *i. e.* that contact with Bona Vista first rendered this disease contagious: especially as it is admitted that the island was at the time in a very healthy state. So far as we can perceive, the reporter had before him no proofs to justify this view. At the most, he could only judge from a perusal of the documents furnished by the officers of the vessel and hearsay evidence. Therefore, in this respect, he can claim no advantage in forming his opinion over others who are at a distance. Indeed the mode in which he expresses himself in one part of the Report, conveys the impression that he was without the means of coming to a correct decision. "There is *no proof* that the fever in question was in any degree contagious before the vessel reached Bona Vista."† By using these terms, the question is obviously left open; they certainly do not bear out the conclusion at which the reporter has arrived; because this could be justified only by his having discovered clear *proofs* that the disease, as it existed in the

* "The treatment adopted by the Portuguese surgeons, including Dr. Almeida, the resident medical man on the island, was of the most simple kind. They rarely bled generally, sometimes cupped and blistered, administered diaphoretics and anti-spasmodics, and relied chiefly upon quinine and mild purges and enemata.—Mercury was not exhibited in any form. As experience has, alas! too generally taught on other occasions, the bad cases were little, if at all, amenable to treatment of any kind; but great dependence was placed upon quinine in checking vomiting, whether bilious or black."

† Page 110.

Eclair before reaching the island, was certainly *not contagious.* On this point we cannot agree with Dr. McWilliam. If our readers will turn to our report on the state of this vessel in 1845, we think they will concur with us, in the opinion, that the disease, if not contagious from the first, speedily became so; and that the landing of the crew on an island, admitted to be healthy, was, *pro tanto*, likely rather to mitigate than to aggravate its malignancy. On the assumed convertibility of endemic into malignant, remittent, or yellow fever, we shall express no opinion. Countries may be decimated while medical men are settling the types of diseases, or determining in what latitude or at what degree of the thermometer they may acquire special malignancy. Discussions on subjects of this kind merely touch the fringe of the question: the great fact for a government to consider is,—under what circumstances and to what extent it may be advisable to allow of intercourse with countries where disorders are prevailing, which are either avowedly contagious or likely to become so by a series of peculiar but unknown circumstances!

Although we have taken leave to differ on this question from the learned author of this valuable Report, we must do him the justice to say that the mode in which he has performed the highly responsible duties entrusted to him by Government, reflect upon him the greatest credit. There can be no doubt that before his departure from this country, his views, consistently with the experience that he had then acquired, were rather adverse to, than in favour of contagion. He has had the honesty and candour—somewhat rare qualities with those who take up medical theories—to state the truth boldly, even although it might conflict with former opinions, and would effectually prevent him from being hereafter quoted as an authority by the anti-quarantine members of a House of Commons! Nevertheless, he will have the conviction that he has performed his duty conscientiously, and that he has been able to contribute valuable evidence on a subject in which the well-being of nations is eminently concerned. It will be a great satisfaction to us to know that Sir W. Burnett, the Director-General of the Medical Department of the Navy, has recommended this worthy member of our profession for that promotion which he so well deserves. His services in the Niger are not likely to be soon forgotten, and alone deserve a better reward than has yet been meted out to him. Although the Report, which we have here examined, does not accord in all respects with the well-known opinions of the Director-General, we cannot believe that this will be any obstacle to the recommendation of the author for promotion.

London Med. Gazette for July, 1847.

3.—*Report on Fever at Boa Vista.*—Boa Vista is a small island of the Cape de Verds, of some importance to England. During the latter part of 1845 and beginning of 1846, a fever appeared there, attended with a singular fatality, which was believed to have been introduced by H. M. Steam Ship Eclair. Such was the sacrifice of human life, and the terror of the inhabitants, that the subject reached the ears of the Lord of the Admirality, who at once made proper inquiries, and instituted a commission to make a thorough investigation of the facts in the case. I. O. McWilliam, M. D., a surgeon of the Royal Navy, was selected to visit the Island, to gather such information as would be essential, and report the same on his return. On examination of the document forwarded by Dr. McWilliam, and by the admirality transmitted to parliament, and subsequently ordered to be printed, ample evidence is found of the confidence so worthily reposed in the author by that body; and, while reading the details of the report, which is exceedingly minute, we came to the conclusion that but few men would have the patience, even had they the scientific ability, to collect such materials, and arrange them with the scrupulous care and tact observable in every page of this work.

In form, the report is a folio, containing 112 pages, accompanied by two maps, illustrative of the position of Boa Vista, in the group to which it belongs, together with other points memorable in the progress of fever. There is also added a letter by Sir William Pym, relative to the report, addressed to the Lords of the Council. He gives a synopsis of Dr. McWilliam's views, and at the same time presents his own on the character of the two distinct fevers known to exist on the coast of Africa, viz., the yellow, or *Bulam* fever, known to the Spaniards as the *Vomito Prieto;* and the *Welcheren* fever, or remittent, from which seamen suffer in ascending rivers. The latter is the malaria of the Levant, the jungle fever of India, and is developed in all warm climates, in moist, uncultivated lands. Had Sir William been familiar with the diseases of sailors and boatmen on the rivers of our Southern States, and on the Mississippi, even high up, when the water is low, he would doubtless have also referred to this country for a locality in which that wasting disease reigns with almost uncontrolled activity in the summer months.

Boa Vista lies in lat. 16 deg. 5 m. N., and long. 22 deg. 55 m. W. When the steamer Eclair arrived, Aug. 21, 1845, the crew were suffering from a fever that appeared on board in June, while the vessel was stationed at the mouth of the Shebar river. On arriving at Sierra Leone, they were employed in cleaning the hold of the steam ship Albert, which had been employed in the famous Niger expedition. The seamen went on shore during the rains—some remained over night, and others were brought back by the police. Next, the Eclair visited Gambia, Aug. 10, towing the Albert. On the way to Boa Vista, she called at Goree to deliver despatches, but, on account of sickness on board, free intercourse with the shore was interdicted. "Meanwhile," says the report, "the fever was gaining ground. Three of the men, who slept on shore, died. A merchant passenger from Sierra Leone also died, and, Aug. 21, the day of arrival in port, fifteen new cases appeared—seven proving fatal." After some little formalities, free intercourse with the shore was established, and from that time the disease was rapidly and extensively propagated to Porto Sal Ray, the town of Rabil, Estacia, Provoçao, Velha and the villages of Joao Gallego, Fundo das Figueiras and Cabeca dos Tharafes. Without tracing the progress of the disease from one station to another, in the order of the report, it is enough to say that it was fully recognized to be highly infectious, a point placed beyond the possibility of doubt. The symptoms were those of a formidable type of fever, which Dr. McWilliam has left without a name. There is one fact not readily understood, however flexible and yielding it may be in the hands of the theorist, viz., the gradual alteration in the type of the fever, from mild to severe ;—from a simply disturbing sickness, to a fatal, uncontrolled malady.

According to Dr. McWilliam's conclusion, the fever on board the Eclair was, primarily, the remittent of the African coast, which is not a contagious disorder ; but it acquired contagious qualities in virtue of a series of causes.

Here, in America, there is a school of physicians who would be slow to believe that the laws of disease are as flexible as this view of the Eclair pestilence represents. They certainly do not often run from one form to another, but maintain their original identity. Thousands may die in a prevailing epidemic, yet the character of the complaint remains the same. Its fatality by no means indicates that its essential elements are altered—but simply an unusually extensive prevalence. Sir William Pym, more bold and confident than Dr. McWilliam, calls the Boa Vista sickness, yellow fever, without apology, and gives an unshackled opinion that the steamer carried to that place two diseases, the marsh or river, and the genuine yellow fever. Whereas the medical commissioner, being non-committal, as we gather from the narrative, twists the remittent of the coast into a terribly frightful engine of death; and by a process quite new to the expounders of the laws of disease, alters a non-contagious disease into an actively contagious one.

With regard to the manner of conducting the inquiries at Boa Vista, the methodical preparation of the report, and the earnestness with which Dr. McWilliam prosecuted the business of the medical mission, he is deserving of a full measure of praise. No man in England could have done it more acceptably, in all respects save one, already mentioned, viz., the theoretical metamorphosis of a governable disease into an ungovernable one. Leaving the subject here, our thanks are tendered to the author for his politeness in sending a copy of the report to our address.—*Bost. Med. Journ.*

4.—*An Address delivered in the Medical College of Chemistry, Hanover Square, on Wednesday Evening, June* 3*, BY JOHN GARDINER, M. D.—The establishment of this institution, towards which your assistance has been so kindly and liberally afforded, must appear, to every one fully who considers its design and object, one of the most important enterprises ever contemplated in this country. The dependence of Great Britain for her prosperity on arts and manufactures, based upon physical science, in the absence of all systematic efforts to foster and advance our knowledge of nature and her laws, has often called forth expressions of astonishment. You have determined to terminate this state of things, and to form a school for inducting the student of Nature into a profound acquaintance with chemistry—that branch of natural science which most needs at this time to be studied and promoted, and which is the key to the greatest variety of professions, trades, and economic arts. This being the general design of the Royal College of Chemistry, I am instructed by the Council to place before you, somewhat in detail, although as briefly as possible, the means by which they propose to accomplish it, and to sketch out in outline the several subordinate points amongst which their efforts should be distributed, in order to accomplish their main purpose.

I need not remind you of the popularity of the science of chemistry; you are well aware of the fact, and of its causes, the beauty of its laws and principles, and the attractive character of the experiments usually exhibited to illustrate them. The chemical lecturer commonly commands overflowing audiences, and nothing is more easy of attainment, or more accessible, in this country, than a speculative acquaintance with its leading facts and theories. You know, too, how often it has happened, that amongst the spectators of the brilliant phenomena of chemistry, some have been excited to a love of the science, and a desire to pursue it, and have, by their own efforts, under great difficulties, and after a considerable length of time, distinguished themselves by becoming original discoverers. A history of the difficulties met with and surmounted by those whose names are eminent in science, would be, perhaps, the most powerful plea for this institution. It is difficult to fix a limit to the achievements of genius, but he who would entrust science to individual effort, must have forgotten that the patient collector of facts, the labourer on details, is as necessary to progress as the bold propounder of theories; that there must be Maclaurens as well as Newtons in the service of science; and, indeed, we cannot, upon any department of Nature, bring to bear too great a variety of mental character. If, therefore, there were no other reason for instituting a school of practical chemistry than the prospect it affords of smoothing the path of future philosophers, it ought to be sufficient to call forth and sustain our efforts. But there are more immediate and pressing motives. The recent discoveries of chemistry, especially relating to organic matter, are like those of navigators who touch only the shores of new continents—they merely indicate unexplored regions, and point the way for future researches. That the instruments and methods of research recently devised for investigating organic productions should have been so little employed in this country—as you must admit they have been—can only be explained by the absence of efficient instruction. No one, in the present day, can oppose the progress of science on the ground of his not being able to discover any USE in it. For

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* *Lancet.*

while it would be difficult to point out any practical application of many beautiful and successful investigations in organic chemistry, instances have been so numerous of most recondite observations, finding a use in promoting the arts, and adding to the comforts of mankind, that such a plea can no longer avail as a subterfuge for ignorance. Who, for example, could have imagined that the experiment of Galvani on the frog, as he watched the jerking of its muscles on the contact of metallic plates, should have brought forth, as it has recently done, the numerous arts of galvano-plastics, electro-typing, electro-plating, &c., and have revolutionised some of the best established manufactures of Manchester and Sheffield. Who would stake his reputation on the prediction that the beautiful crystalline bodies, formed by the substitution of chlorine, bromine, iodine, &c., for hydrogen, in organic acids and alkaloids, by Laurent and Hoffmann, shall always remain mere objects of speculative curiosity, and find no use for the benefit of society?

Assuming, then, that you, gentlemen, are satisfied that the study of Nature and her laws should in every direction be promoted, and that if it be expedient to teach what is already known, together with the principles and methods of research, and to assist further inquirers, it should be done efficiently, and by the best means in our power, I would invite your attention to the practical school of our college.

I think it may be said with safety, that until this college was established, no chemist of any reputation was engaged in this country solely in teaching the science practically. The lectures of professors of colleges and private schools are doubtless admirable means for imparting a general knowledge of the science. Occupying usually an hour daily, it requires a full session of six months to teach the leading facts and theories of inorganic matter, allowing only a few compendious sketches for organic chemistry. The preparations for these lectures, together with other avocations, must necessarily have engaged the whole time of the professors and their assistants. It was impossible for them to establish practical schools. The Apothecaries' Company (with that desire to advance the education of the profession for which that body has been remarkable), three or four years ago, added to their curriculum practical chemistry, and the schools, in order to conform to this law, instituted classes, which, during an hour daily, practiced the preparation of substances, and the application of tests. A few laboratories were also opened for working students—at a fee of ten pounds per month—the most frequented of these numbered only six students; but on inquiring of every institution in London, I could not discover one where the whole time and attention of a qualified professor was given to the student, or where practical teaching was pursued systematically.

If an alteration has taken place—if practical schools of chemistry exist, and professors are appointed to devote their whole time and energies to teaching—if the fees now demanded are so moderate as to fill the new labaratories with students, it is to the enlightened noblemen and gentlemen who have formed this college that this service to science is to be attributed.

Since this college has been completed, too, the Pharmaceutical Society has opened a laboratory for teaching the art in a scientific manner. I mention this, merely to observe, that while it renders unnecessary a department of the college for teaching pharmacy, its objects are totally distinct and different from our own. It is to pure chemistry, and to its application to practical arts, unrepresented by active societies, that this institution is devoted.

The method of study adopted at Giessen having been successful, so many chemists of reputation in this country having studied there, and being accustomed to proclaim this fact in demanding public confidence, it became a matter of primary importance to ascertain wherein the peculiarities of that school consisted. I am able very distinctly to assign the causes of the pre-eminence of Giessen as a school of chemistry.

In the first place, the reputation of Professor Liebig would attract students, and the liberality of the Hessian government enables the professor to charge fees sufficiently moderate to come within the means of young men from the manufacturing, trading, and professional classes, and to employ qualified assistants to take the burthen of teaching. Secondly, a system of study is pursued, which embraces an initiation into all the manipulations required in the practice of the science, keeps the student's attention constantly on the alert, sharpens his faculties of observation, obliges him to become acquainted with the principles and theories of chemistry, in order to draw correct inferences from the phenomena he creates, and, in one word, acts as a rigid mental discipline, as well as a technical introduction to all the facts at present known. The chemical student, there, must possess the preliminary qualification of a speculative acquaintance with the science. He then begins a course of experiments with his own hands, which produce the actions or manifestations which are characteristic of every known substance, at least, of which all are of ordinary occurrence. The plan is contained in the little work, entitled, "Outlines of the Course of Qualitative Analysis followed in the Giessen Laboratory," which has lately been presented to the English public.

The student takes potassa, soda, ammonia, baryta, magnesia, lime, alumina, and all the metals and their oxides; and with each he mixes the re-agents, which have, by the consent of chemists, been established as infallibly manifesting the peculiar re-actions, and thus detecting with certainty their presence or absence in any given compound. In these experiments he has the constant assistance of a qualified teacher, to guard him against the use of too little or too great proportions of water, spirit, or other solvents—too great or too little heat—to show him the best manner of manipulating; and to assist him in interpreting the results. These results are certain appearances, mostly addressed to the eye; and consequently when spoken of as colors more or less vivid—precipitates more or less distinct, &c.,—form subjects, beyond any other in the whole range of human knowledge requiring the aid of an experienced teacher to define. The experiments thus to be made amount to many hundreds. When the student becomes familiar with all the phenomena produced by his re-agents with these bodies, and is able to assign the cause—to give the rationale of the re-actions—he begins the analysis of unknown substances—that is, of substances unknown to himself, but which are prepared by the professor, and arranged in order, so as to lead the student, by slow and safe steps, from simple to complex cases. This they call going through the bottles. About twenty liquids, containing each only one base, are first taken; then, as many with one acid and one base; subsequently, two or more bases, two or more acids, and so on, until the more complex cases are reached; and in the end, all the inorganic bases, and a number of acids in admixture, are analysed, to try the students skill and memory.

So important is this method of study felt to be, that even many men, with a high reputation for scientific knowledge, as soon as they have looked closely at its effects, have at Giessen submitted to go through the drudgery of this systematic course.

When perfectly familiar with qualitative analysis—that is, when possessed of the power, imparted in this course, of detecting the presence or absence of every element in any possible form of mixture or compound—the student is taught, by a similar system, to separate and estimate the quantities of every individual constituent; this is quantitative analysis; afterwards, the preparation of organic matter, and its analysis by combustion, becomes a comparatively easy step.

The constant presence, the incessant stimulus, and immediate aid, of a qualified instructor, is the corner-stone of the system. It has a parallel only in the custom, at our English universities, of working with a private tutor when the student aspires to honors in classics or mathematics. Every difficulty, as it is met with, is explained; every fact for which the student feels at a loss is imparted; and his daily labor is definitely marked by the point reached in his onward progress.

In a university education, the information obtained is generally held to be inferior in importance to the discipline of the mind, the attainment of habits of continued attention, and the exercise of the reasoning powers and memory. The deep study of classics and mathematics teaches men *how to learn*, as well as furnishes them with the instruments available on all occasions and on all subjects; and the facility with which the mixed sciences are acquired by those who have passed successfully through our universities is a matter of common remark.

The study of chemistry, in like manner, when pursued according to the system which I have described, becomes an ORGANON—an instrumental means of attaining an intellectual habit and qualification for the observation and interpretation of Nature. He who has thus become an expert analyst, has obtained a power capable of application in any direction for advancing his own knowledge, for practising the useful arts and manufactures, and for extending the boundaries of science.

It is this system which is pursued in the laboratory of the Royal College of Chemistry, and its effects are already begining to be manifested. A member of our council, himself accustomed to teach another science, applicable to one of the noblest practical arts, works daily in the laboratory. A few days ago, he expressed to me his admiration of the earnestness, constancy, and zeal, displayed by every student, without exception, and his conviction that the system of teaching adopted, in comparison with others, is the only method of acquiring a satisfactory knowledge of chemistry.

It is obviously too early for us to have any very striking investigations to adduce in proof of the value of our school. I may be allowed, however, to refer to a paper read by one of the students at the last meeting of the Chemical Society, on a purely scientific subject—namely, the phosphates of organic bases, as a contribution to science which would do no discredit to a veteran. Mr. Nicholson, in that paper, detailed a number of analyses, made with remarkable skill and accuracy. Of course, it is chiefly to practice in analyses that the attention of the more advanced students is now, after only eight months' study, confined. Our Professor mentioned to me a trial he made, lately, of three of the students, by giving them for practice the analysis of gunpowder. The numbers which they respectively presented to him were—

1. Nitrate of potass, 79·69 ·· Carbon, 13·86 ·· Sulphur, 9·45 ·· Moisture, 0·65
2. Nitrate of potass, 76·91 ·· Carbon, 13·17 ·· Sulphur, 9·27 ·· Moisture, 0·65
3. Nitrate of potass, 76·70 ·· Carbon, 13·28 ·· Sulphur, 9·47 ·· Moisture, 0·65

I select this merely as an illustration of the way in which the students occupy themselves; numberless analyses of various substances are made in this way for practice every day.

In a report presented to the Council, at the close of the first session, the Professor says, that several students will be perfectly qualified to commence, in the next session, the analysis of soils, ashes of plants, and mineral waters; and so far as the present session has elapsed, there has been no relaxation of their labors to frustrate that anticipation.

But you may very reasonably ask, gentlemen, to what purposes can the knowledge and skill thus obtained be applied, which shall compensate the supporters of the college for their labors and money. and which shall avail to the students themselves in their progress in life? To both parts of this question we can return a satisfactory answer. The advancement of science itself, the improvement of arts, the origination of new sources of industry, the illumination of the obscurer parts of physiology and pathology, the increase of our means of remedying diseases, and preserving health, may all be fairly anticipated to flow from a practical school of chemistry. A numerous body of men, qualified in the manner I have described, and scattered throughout this country, cannot fail to produce visible and palpable effects. There is a natural tendency in such knowledge as is definite to propagate and expand. Possessing a clear insight into the laws and phenomena of Nature, combined with manual dexterity, the young chemist will find everywhere around him materials unexamined, and examples of manufacturing industry leading to wealth and rank. Did these ever fail to animate men to exertion?

But we need not dwell on vague anticipations of the future. We hope, under your fostering care, something will be done in our laboratory, to honor the founders of the college, and to commend it to the patronage of a larger number of subscribers, to invoke the aid of other societies, and to secure the favor of the Government. It is proposed, as soon as the new laboratories are erected, to commence investigations for scientific and practical purposes. The Council have endeavored to obtain the co-operation of the Royal Agricultural Society in the pursuit of researches which may benefit the land owner and farmer. The education of chemists capable of analyzing soils, manures, and produce, and of giving safe advice to the agriculturist, for economy, the increase of produce, and to save him from the frauds now so openly practised upon him, would seem sufficient to ensure support from that source. But there are subjects of inquiry which are of immediate and paramount importance for the successful cultivation of land, of such a nature as to require a special laboratory, concerted efforts, and money to pursue; and we trust that the Royal Agricultural Society will, ere long, lend its assistance towards the institution of these inquiries.

There are great advantages in such a laboratory as we possess for carrying out investigations; the presence of numbers engaged in the same work, and of the professors to assist, by advice and suggestions, leads often to success where individuals, by their own solitary exertions, fail. Subjects, too, are supplied and help afforded, the value of which must be felt to be appreciated. It by no means, as I conceive, lessens the merit of the individual who thus seeks assistance; on the contrary, it is most honorable, and conducive, both to private and public benefit. We have already indications of fruits of this description. M. Warren de la Rue has worked out, in our poorly provided temporary laboratory, an investigation on cochineal, which, when published, will add to his already well-earned reputation as a chemist, and it has also furnished him with a new material available for the improvement of his beautiful and interesting art.

It could not fail to be a subject of remark amongst the students, that the chemist is obliged to have recourse to Bohemian glass for his combustion tubes. One of our friends remarked, that he could induce English makers to fabricate such glass if they knew the exact proportion of its ingredients. Upon this hint Mr. Rowney undertook the task of analyzing some specimens of Bohemian glass. Mr. Rowney's accuracy imparts value to the results he has obtained, and it seems very likely that from this point will emanate such improvements in the manufacture of glass as will preclude the necessity of importing it from Bohemia.

Gentlemen, these are trifles—this college is but in its embyro state, it has not even reached infancy; but it is difficult to repress our enthusiasm when we recollect what the science of chemistry has done and is now doing, and contemplate the boundless unexplored regions of Nature inviting our industry and offering us its sure rewards.

Look only at the present uses of gas. Our esteemed friend Mr. Lowe tells us that 1000 tons of coal are daily decomposed for the production of gas in the metropolis; that the iron pipes in London alone measure several hundred miles—the extent of the smaller being almost incalculable; that gasometers are now made as large as a house, and that every town and almost every village has its gas-works. Consider the trades created, the myriads of men employed in working the mechanism of this vast chemical process—and recollect that it is only in our own time that this branch of industry has been created. To chemistry, too, we are especially indebted for the present cheapness of this beautiful, indispensable light. The applications found for the other products of the decomposition of coal have enabled the manufacturers to reduce its price: it has become one of the most useful instruments of the laboratory, and it will now be employed, we trust, as a means of effecting further discoveries. Will any one affirm that it is impossible to obtain from coal a solid, dry, portable hydrocarbon, capable of being made a substitute for wax and tallow in the fabrication of candles, and so cheap as to supesede animal fat in lighting the humblest cottages?

The point, however, to which I wish to direct your especial attention, in reference to our anticipations of advancing the science, is organic chemistry. The apparatus for organic combustion, the great invention of Professor Liebig, is already almost daily in use in our laboratory. By its means, as you know, the analysis of matter belonging to the animal and vegetable kingdoms—the produce, in some form or other, of the vital force—is effected with greater ease and equal precision with the analysis of a soil or a mineral. The rapidity with which discoveries are being made, and the immense extent of the science, render it almost impossible, except for the professed chemist, to follow and appreciate its importance. Every now and then, however, some fact is made known, having an immediate application to practical purposes, and indicating how vast a field of inquiry exists in organic nature. Within the last few days, Professor Liebig has announced to us that a residue left in the manufacture of sulphate of quinine is the pure alkaloid itself, merely obscured by its form. The discovery of quinine by Pelletier, in 1820, revolutionized pharmacy. The active principles of the most nauseous drugs can now be isolated from the accompaniments of woody fibre and other inert matters, and exhibited in a concentrated form. This is a blessing which only those who have had to swallow bark in substance can fully understand. During the twenty-six years in which sulphate of quinine has been manufactured, a considerable portion, every year, of the pure alkaloid has been laid aside, and consequently a considerable amount has accumulated. Professor Liebig's discovery will not only bring this into use, but it will enable the practitioner of medicine to prescribe the pure alkaloid, and to combine it at pleasure with vegetable or other acids, and thus to obviate the objections which are found in practice to lie against the sulphate of quinine, placing at command quinine and a variety of its salts at a price which will not preclude the poorest from its benefit.

A great proportion of the vegetable kingdom still requires to be chemically investigated; of the materials which constitute the animal body, every one, without exception, awaits a new analysis and investigation. Not many months since, Professor Redtenbacher, of Prague, found that taurine, a constituent of bile, which acts a most important part in the animal ecomomy, contains sulphur to the extent of 26 per cent. This element had been previously overlooked, in consequence of what must be considered a defect in the method of analysis by combustion; in that process the oxygen of the analysed body being estimated by the amount of loss, the equivalent of sulphur being precisely double that of oxygen, its presence did not disturb the calculation of the results. Professor Gregory has remarked, that this observation of Redtenbacher will probably turn out, in its consequences, one of the most important yet made in animal chemistry; and it cannot be doubted, that although our views must be changed in many points, in consequence of it, our knowledge will be extended and rendered more precise and more capable of direct application to physiology and pathology. Already Professor Liebig has found reason to believe that Mulder's protein and oxides of protein exist only in imagination, as sulphur remains combined with the other elements after fibrin and albumen are subjected to Mulder's processes. To extend this inquiry into all the materials of the animal tissues, requires many skilful hands and much labor.

The principle upon which organic investigations are now pursued, is worthy of your serious attention; it is, to subject a substance to the action of heat, of oxidizing and reducing agents, and generally to every influence capable of changing or modifying its composition, to analyze the resulting compounds, to study their mutual relations, and thus to ascertain the true nature of the original matter. This principle was first adopted by Chevreul; its application by Dumas and Boullay to the compound ethers, Professor Liebig says, gave the first impulse to the extraordinary developement which chemistry has since then obtained. It has created, in fact, a new branch of the science, which it would be of great importance to distinguish by a distinct designation. We are familiar with the distinctions of analytical and synthetical chemistry. Analysis is the separation of a chemical compound into its component parts. Synthesis is the putting together the elements of compounds, and combining them into a chemical union. The formation of water, and of nitric acid, by directly combining their elements, as effected by Mr. Cavendish, are examples of synthesis. Very little has been done in this direction, beyond a few instances illustrative of the safety of depending upon analytical results, in certain cases, as philosophic facts. The only example I recollect of synthetic chemistry applicable to practical purposes is, however, a most brilliant one, made by Gmelin—namely, the formation of artificial ultra-marine, by mixing silica, alumina, soda, and sulphur, colorless bodies, to produce that beautiful blue pigment. There is no reason to believe that it is by way of synthesis that Nature produces the many complex substances, termed organic matter. The affinities of inorganic bodies are employed to group together the proper elements, and in the process of constructing the materials required in vegetable and animal organisms, intermediate compounds are formed, which, not being needed in a separate state, are again combined, and ultimately gave rise to the substances which are the designed results of the process. Proceeding in the path indicated by Chevreul, these internal forms of organic bodies are detached, re-combined, and undergo a variety of metamorphoses, by which the true constitution of the first is revealed, and new complex bodies created, as it were, as truly organic as any of those produced by the vital force. The theory of compound radicals is the general expression of the fact, that very simple forms, consisting of a few equivalents of two or three elements, may be assumed as the basis or rudimental centres of all the varieties of compounds constructed in living organisms. By breaking up the internal structure of these compounds, or by bringing to bear upon them other affinities, that is, by employing certain media in imitation of the way pursued by Nature, many natural productions can be produced artificially, and a great number can be artificially made, exactly analogous to the acids, neutral bodies, and alkaloids found in plants. Of the former kind, that is, of natural productions produced artificially, several have become of commercial importance: the manufacture of sugar from starch, by the mediation of sulphuric acid; of British gum, also from starch, which has materially affected the commerce of Senegal and the Gambia; and that of valerianic acid, are well-known examples. The process for effecting the first of these transformations will illustrate the action of media. Sugar and starch have the same composition, except that the former contains the elements of two atoms of water more than the latter. To add the water to the starch directly, so as to convert it into sugar, is at present beyond our power. But we know that the vital force, aided by heat, converts starch into sugar. This is the *rationale* of the process of malting. And a method has been known for some time, of effecting the same purpose; that is, of adding the elements of two atoms of water to the elements of starch, and thereby converting it into sugar. This is done by the long-continued boiling of starch water acidulated with sulphuric acid. The acid acts as a medium to effect the assumption of the water by the starch, and to transform it into sugar, the acid itself remaining unchanged. The same transformation may be effected on linen rags, and any form of ligneous fibre, thus converting them into sugar, Other natural substances which have been produced artificially possess only scientific interest, as urea, formic acid, butyric acid, &c. But it by no means follows that these are of no practical use. The formation of butyric acid from starch, threw an important light on the sources of animal fat. Natural urea is a curious and highly interesting substance. The restoration of nitrogenous matter which has served its purposes in the animal economy, to the atmosphere, whence it had been derived by vegetable organisms, requires it to be brought into the same form it had originally, in order to its diffusion through the whole mass of air around our planet—namely, into ammonia. But ammonia would injure the animal tissues. The contrivance by which this difficulty has been evaded, excited the admiration of M. Dumas. "Nature, ever provident," says he, "has caused the animal to secrete urea. Urea is a neutral and inert substance, which can pass through the delicate structure of the kidneys, ureters, and bladder, without irritating or inflaming them. By the addition of two atoms of water, it becomes converted into the carbonate of ammonia. In this form it travels from the earth to the atmosphere, from the atmosphere to the earth in rain and dew, and is thence again served upon, and elaborated by, plants, into the forms adopted for animal nourishment."

The manner in which Nature employs the affinities of inorganic bodies as media for the production or organic compounds, is in some cases understood. Thus, for instance, the potass taken up from the soil by plants has a great affinity for carbonic acid, and by virtue of that affinity fixes it from its gaseous state in the atmosphere. In the juice of the plant it meets with water in a nascent state, as it is separated from its combinations in the organism; this water, or rather its elements, combining with the carbonating potass, converts it into oxalate of potass;—or by a further accession of hydrogen, into citric, malic, tartaric, and other acids,—sugar, gum, starch, &c., oxygen being set free. Thus the affinity of potass for carbonic acid, &c., is the force which primarily originates the changes essential to these combinations. In like manner, but by processes not at present understood, phosphorus and sulphur, in all probability, mediate the production of materials for building up animal bodies, albumen, fibrin, &c.

Of the latter kind—that is, artificial organic compounds—several basic bodies or alkaloids have been formed by Laurent, Hoffman, and others, and a very interesting one from the oil of bran, by Professor Fownes, termed by him furfurine. These alkaloids combine with acids, and form series of crystalline salts, as distinct and well-defined as those of the natural organic alkaloids.

The Great Author of Nature seems to have provided laws regulating the affinities of the elements for the creation by our instrumentality of organic bodies, for which no use is found in nature. It is necessary, however, to be cautious in the latter assertion, because it has happened, in at least three or four instances, that such artificial compounds, first formed in the laboratory, have been afterwards discovered to exist in plants or animals.

In the course of the investigation of M. De la Rue, already alluded to, that gentleman discovered a new crystalline substance existing in cochineal, to the extent of about one-third per cent, which appears to be identical with a substance lately described by Liebig, and formed by him by acting on casein with caustic potass. Salicylic acid, and oxide of methyle, first produced by the decomposition of other organic substances, were afterwards found to coexist in the oil obtained by distilling the flowers of the gualtheria procumbens; and very recently, Professor Redtenbacher, by acting upon oleic acid with nitric acid, and submitting the result to distillation, obtained no less than nine acids in the distillate: one of these is a new acid; but, very curiously, it was discovered at the same time in his own laboratory by a student, in the plant called pelargonium roseum, from which, therefore, he has named the acid, pelargonic acid.

It is impossible, in contemplating these results, to avoid entertaining the belief that chemists are upon a track which will ultimately lead to the production, by artificial processes, of the natural alkaloids, morphine, strychnine, quinine, or such vegetable bodies as indigo, santaline, &c., and to the elucidation of the *modus operandi* of the vital forces in transforming the materials presented to the organs of nutrition of plants and animals.

As I have already observed, an especial designation seems demanded for this branch of chemistry, which is neither analytical nor synthetical. I will therefore propose, in the absence of a better term, to denominate the aggregate of the processes by the term *genesis*, and the subject, *genetical*, or *constructive* chemistry.

This branch of the science is occupying the attention of very many students on the Continent, and is undergoing a rapid development. From the masses of figures and calculations, now accumulating, representing analyses, we may anticipate many practical results, of vast importance, to emerge. I ought not to quit this subject, without alluding to the improvements in candle-making, resulting from Chevreul's labors.

For my own part, I entertain the most sanguine hopes that the diseases most fatal to mankind will be elucidated, and, perhaps, wholly prevented, by pursuing this path of chemical inquiry.

Whilst the science of organic chemistry was advancing on the Continent like a torrent, was this country to be permitted to remain the only one in Europe where the necessary qualification could not be obtained to enable men to assist in this great work? The example of Hesse Darmstadt, in founding and supporting a school at the bidding of Liebig, who has given his country new importance in Europe, has been followed by France, Prussia, Belgium, and even Russia and Austria, and nearly all the smaller German states. Is the accomplished chemist less likely to find profitable employment in the manufactures, arts, and professions of Great Britain, than in those countries? Much as we have to boast of in the way of popular lectures, it might occur to many to suspect the soundness of the knowledge thus obtained if the patent list were examined, and the extraordinary mistakes made, to the serious pecuniary loss of many persons, for want of scientific notions, considered. I may mention, as an illustration of this remark, the credit given by the public, and, I might add, by some distinguished individuals, to the profession, made not long since, that a fuel had been discovered which would burn and supply heat without leaving the ordinary products of combustion—that no carbonic acid was given to the atmosphere by burning this material. The belief entertained in this ridiculous fiction was sincere enough, as it cost the loss of several lives. Assuredly, the spread of chemists, taught after the manner we have adopted, will supply the public with better instruction.

It is a startling, but nevertheless certain fact, that in many of our manufactures, depending, wholly or in part, upon chemical principles, this country is fast falling behind those of the Continent. If the instances in which this is most evident may appear insignificant in comparison with the vast creations of our machinery, it should be remembered how easy a downward progress becomes. "A respect for every department of Nature," says Baron Humbolt, "is especially necessary in the present times, when the material wealth and increasing welfare of nations is so closely connected with the diligent use of natural productions and natural forces. The most superficial glance at the present condition of Europe assures us that any relaxation of effort would be followed by gradual diminution and ultimate annihilation of national prosperity. Nothing but energetic progress in chemistry, and other branches of natural science, will save any state from the evils of a decline. Man can make no use of Nature, can appropriate none of her powers, if he be not conversant with her laws, and the relations of number and measure existing amid her processes. Those nations which relax now in manufacturing activity, in the practical application of technical chemistry, must inevitably fall from any prosperity they may have attained; more especially as neighboring states, instinct with powers of youthful renovation, in which science and practice co-operate, enter upon the struggle, and press forward in the race."

It is wholly inconceivable, that the enlightened statesmen who guide the energies of this country, will, when once their attention is aroused to those truths, withhold their aid in establishing this college. Its special object is too great to be merged into courses of general education, or to be entrusted to individual exertion. The association and co-operation of but a few persons representing each science and art based upon chemistry, would, however, amply suffice to effect its purposes. Perhaps it is better to call forth voluntary efforts, than to institute schools of chemistry by authority, and to clog them with official restrictions.

Gentlemen, it only remains for me to say what grateful thanks are due from all persons interested in science and the arts, but especially from the gentlemen who enjoy the privilege of our course of instruction, to our Royal President, our noble Vice-presidents, and to the Council, for their generous assistance in founding this college. This they have afforded, under the conviction, that the institution is calculated to confer great and enduring benefits on all classes of their fellow-countrymen, and on all mankind; and that—although not obviously and conspicuously—yet, to the enlightened mind, the highest exercise of benevolence is to promote the extension of our knowledge and of our power over Nature.

To one gentleman, however, we owe an especial debt of gratitude, for his constant personal exertions, the devotion of his valuable time, and the unwearied employment of his influence, with the most noble and enlarged views, to promote the establishment of this college. I should be deprived of the greatest pleasure I derive from my own efforts in its behalf, if I could not publicly acknowledge that our success—which, I trust, is now unquestionable—will be mainly attributable to Sir James Clark.

5.—*Fermentation ascribed to the growth of Fungi and of Infusoria.*

The microscopical examination of vegetable and animal matter, in the act of fermentation or putrefaction, has lately given rise to the opinion, that these actions themselves, and the changes suffered by the bodies subjected to them, are produced in consequence of the development of fungi, or of microscopical animals, the germs or eggs of which are supposed to be diffused everywhere, in a manner inappreciable to our senses; they are supposed to be developed when they meet with a medium fitted to afford them nourishment.

Several philosophers have ascribed to this circumstance the fermentation of wort, and of the juice of the grape. They assert, that the decomposition of sugar into alcohol and carbonic acid is effected by the contact of particles of the sugar with the growing plants, which they view as the yeast, or ferment, without studying more closely the final causes of the decomposition of the sugar. It has been supposed that this view is opposed to the theory detailed in the preceding pages, which described contact as the cause of a peculiar activity or power.

In all chemical processes, and in all changes effected by chemical affinity, we observe that contact is essential for the exercise of the acting power. Hence, chemists describe affinity as a force distinct from other powers, because it acts only in immediate contact, or at inappreciable distances. Thus contact plays an important part in every case of combination or decomposition, for without contact these changes would not take place. In this sense, all substances effecting combination or decomposition are bodies acting by contact.

In the theory of fermentation alluded to, it was not asserted that the yeast or ferment could effect the decomposition of sugar at appreciable distances. In this respect, therefore, the two theories are not opposed to each other. They deviate, however, in this, that the one theory considers yeast as a body, the smallest particles of which are in a state of motion and transposition, and that, by virtue of this state, the particles of sugar in contact with it are thrown into the same state of change; while the other theory asserts, that the particles of yeast are little fungi, which are developed from germs or seeds falling into the fermenting liquid from the air; and that in this they grow at the expense of the substances containing nitrogen, which are thus converted into, and separated as, fungi. The particles of sugar in contact with the fungi are supposed to be converted into carbonic acid and alcohol, which, in other words, signifies, that the act of vegetation effects a disturbance in the chemical attractions of the elements of the sugar, in consequence of which they arrange themselves into new compounds.

Gay-Lussac showed by experiments that the juice of grapes expressed apart from air, under a bell-jar full of mercury, did not enter into putrefaction, although it did so in the course of a few hours when air was admitted. The same chemist also showed, that fermentation immediately commences on the introduction of oxygen gas, of which a quantity is absorbed equal only to the $\frac{1}{120}$th part of the volume of carbonic acid evolved during the fermentation. It scarcely can be supposed, that the germs of fungi exist in chlorate of potash or black oxide of manganese, out of which the oxygen was obtained; and hence it is difficult to ascribe to a growing vegetation the cause of the decomposition.

Gay-Lussac further showed, that the juice entered into fermentation on being connected with the wires of a galvanic battery, under circumstances, therefore, which quite excluded the introduction of every foreign body. Hence the view, that the fermentation of sugar is effected by contact with growing plants, must presuppose that living beings, plants for example, may be formed and developed without germs or seeds—a circumstance in direct contradiction to all observation regarding the growth of plants.

It is certain that sponges and fungi, growing in places from which light is quite excluded, follow laws of nutrition different from those governing green plants; and it cannot be doubted that their nourishment is derived from putrefying bodies, or from the products of their putrefaction, which pass directly into this kind of plants, and obtain an organized form by the vital powers residing within them. During their growth they constantly emit carbonic acid, increasing in weight at the same time, while all other plants, under similar circumstances, would decrease in weight. Hence it is possible, and indeed probable, that fungi may have the power of growing in fermenting and putrefying substances, in as far as the products arising from the putrefaction are adapted for their nourishment. When a quantity of fungi are exposed to the temperature of boiling water, their vitality and power of germinating become completely destroyed. If they be now kept at a proper temperature, an evolution of gas proceeds in the mass thus treated; they pass over into putrefaction, and, if air be admitted, into decay; and at last nothing remains except their inorganic elements. The putrefaction in this case cannot be viewed as the act of the formation of organic beings, but as the act of the passage of their elements into inorganic compounds.

Observations of another kind,—for example, that flesh and other animal bodies may be kept for several weeks without putrefying, if placed in a vessel containing air previously heated to redness,—have gone far to support the opinion that the process of putrefaction is effected by the growth of organic beings: but all such experiments are of very subordinate value in support of these conclusions. In some experiments instituted by the author, for the purpose of detecting quinine in the urine of a patient in the habit of taking this medicine, he obtained the remarkable result, that this urine kept for several weeks without passing into complete putrefaction, although the urea of urine, under ordinary circumstances, is often completely converted into carbonate of ammonia in the space of six or eight hours. In the present case, the urine effervesced only slightly with acids after fourteen days. This seemed to give sufficient foundation for the opinion that the quinine must be the cause of this delay in the putrefaction. But further experiments proved that common urine introduced when freshly drawn into perfectly pure vessels behaved in an exactly similar manner. When a little putrefying urine was added to the fresh urine, the putrefaction of the latter was accelerated in a high degree. Wood, in which urine had been retained, exerted this action in a very decided manner, and the white, or yellowish-white deposit from putrefying urine (which does not possess an organized form) effects the conversion of urea into carbonate of ammonia in the course of a few hours.

Fresh flesh remains for several weeks without experiencing appreciable change in a perfectly pure glass vessel, whether the latter contains common air, or air previously heated to redness; but, at the same time, it absorbs oxygen and emits carbonic acid, and passes into putrefaction, if the necessary quantity of water be present, the process not being prevented or retarded by the ignition of the air.

It cannot be supposed, that dung-flies, living upon animal excrements, are the cause of this putrefaction; neither can a similar conclusion be drawn in the case of mites and maggots found so abundantly in old cheese.

When we consider, that the intermediate products formed in the passage of animal and vegetable matters into inorganic compounds possess the power of supporting the life of certain animals and vegetables low in the scale of creation, then the only mystery is, in what manner the germs of the fungi, or the eggs of the infusoria, reach the place fitted for their development; for this being known, there is no difficulty since the discoveries of Ehrenberg, in conceiving this extraordinary increase. Now, as it is observed that the infusoria increase in size only to a certain point, it must hence be concluded that their nourishment, even if only from the point at which they are to grow, passes out of their bodies in the form of excrements, precisely as in the higher orders of animals. As is the case with all other excrements, these must possess, in an eminent degree, the property of passing into decay or putrefaction; and this condition must at all events be induced by contact with the original putrefying body. Hence the increase in numbers of the infusoria must induce and accelerate the process of putrefaction in the putrefying body itself. The ultimate products of decay and putrefaction are carbonic acid, ammonia, and water. In order to comprehend the chemical process by which this conversion is effected, it is of much interest to become acquainted with the intermediate compounds formed by the elements. But in regard to the process itself, it is, chemically speaking, quite indifferent whether the first, second or third product, before they assume the final state, be in the form of fungi, or of living animals (infusoria.) These plants and animals are not the causes of the conversion, for they suffer after death the same changes which finally occasion their complete disappearance.

The enormous layers of microscopic animals in the chalk, (the siliceous infusoria) do not contain any organic matter. The lime of their shells, and the silica of their bony coverings, were obtained from the water in which they were developed. If this water had been deficient in lime, or in silica, these animals could not have been produced; and if they had not found nourishment in the products of the putrefaction of former species (the remains of which are found in the *muschelkalk*,) they would not have been developed; and without the co-operation of both these causes, they could not have formed such extensive masses and layers as they actually do.

But these animals are not the causes of the formation of the chalk, or of the layers of flint, and as little are they the cause of the decay and putrefaction of those substances, which yielded to them their organic constituents. Without these animals there might not have been chalk, but there would have been marble, or another limestone; and the silica would have been deposited as siliceous schist, or as quartz, after the evaporation of the water. Hence it is only the form which is given to the layers by organic life; but the substance of these strata (chalk) is chemically in no respect different from crystallized calcareous spar: in fact, the same explanation of their origin might be made as that adopted in the case of the older limestone formations.

The conversion of the constituents of an elephant into aerial compounds is the same process, and is effected by the same causes as those occasioning the destruction of the carcase of the microscopical animals, which themselves obtained their elements from extinct species of other animals. The final products are identical in both cases.

There have been very wonderful and incomprehensible observations made on the behaviour and functions of certain microscopic animals From these observations, there seem to follow conclusions regarding the nutrition and growth of these creatures, quite at variance with all that we know of the process of nutrition of the higher classes of animals.

In a treatise on the composition of the salt-springs in Hesse-Cassel, Pfannkuch mentions a singular phenomenon, that the slimy mass which deposits in the tubs set to receive the brine percolating through the wells of the graduating-house, contains a gas which is found to be pure oxygen gas. The fresh brine obtained directly from the draw-well is quite clear, and contains 5 per cent. of salt with gypsum and sulphuretted hydrogen in such considerable quantity that it might be used as a sulphureous water. During the summer months, a slimy transparent mass forms in this brine, covering the bottom of the vessel containing it to the depth of one or two inches. This matter is everywhere filled with bubbles of gas, of a considerable size, often two or three inches broad; these rise to the surface, when the membrane inclosing them is torn with a stick. The quantity of these gas-bubbles is so great, that it would be easy to fill hundreds of bottles with them in a short time. They are so rich in oxygen gas, that a glowing match of wood introduced into the collected gas, bursts in flame, and continues to burn with brilliancy. On being analyzed, this gas is found to consist of 51 per cent. of oxygen, and 49 per cent. of nitrogen; but there can be little doubt that the gas originally consisted of pure oxygen, which became mixed with nitrogen of air by virtue of diffusion, just as it does when confined in an animal membrane. In fact, it is found, that when the water in the tubs is very low, the bubbles existing in the deposite appear to be pure air, owing to the celerity with which the diffusion has taken place. (Wöhler.)

Wöhler has subjected to microscopical examination the slimy membranous deposite, and has shown that it consists almost entirely of living and moving infusoria, principally species of Navicula and Gallionella, such as occur in the paper-like formations of Freiberg, and in the siliceous fossil strata of Franzensbad. The whole deposit possesses a slight greenish color, and is intersected with very fine colorless fibres of confervæ. After washing and drying the deposite, a residue like paper is obtained; and this, on being heated, gives distinct indications of ammonia, showing that it contains nitrogen. It yields also a mass resembling paper, which on incineration, being treated with muriatic acid, leaves behind siliceous skeletons, which preserve the shape of the animal so completely, that it appears as if the original deposite itself were submitted to examination. (Wöhler.)

These observations are of remarkable interest, for, as Wöhler asks—Whence comes the oxygen gas—from the confervæ or from the infusoria? The quantity of oxygen being so large, and the infusoria being in great preponderance, would lead to the conclusion that the former must be derived from these; and yet this is opposed to all analogy. The water comes out of a depth of 500 feet; and its sulphuretted hydrogen shows that it comes out of a layer of rocks containing putrefying animal matter, which, acting upon the sulphates, produces sulphuretted hydrogen: and in this water is formed, with the aid of solar light, a source of oxygen gas, to all appearances more abundant than we see in the case of green plants. Sir B. Thompson (better known as Count Rumford) published some experiments 56 years since, which are of such a remarkable nature, that we give them in the author's own words. Thompson found that silk, cotton, sheep's wool, eider-down, and other organic substances, evolve oxygen gas, when they are freed from air by washing, and then exposed to sun-light in a glass globe perfectly filled with water. After two or three days, the water assumed a greenish hue, and from that moment the evolution of gas commenced.

"One hundred and twenty grains of cotton, in a bell jar, along with 296 cubic inches of spring water, gave out, during the first four days, 2¾ C. I. of gas, containing hardly any oxygen. It was not till the sixth day, when the sun was very powerful, that the water suddenly became green, and gave out during the next six days, 44½ C. I. of oxygen nearly pure. On examining the water under the microscope, it was found to contain a multitude of very minute, nearly spherical animalcules. Wherever the water was green, these animalcules were found, insomuch that the green color seemed to be caused by them." After describing his numerous experiments, Count Rumford adds—

"The phenomena now described may, perhaps, admit of explanation, if we assume that the air produced in the water in the different experiments was derived from the green matter; and that the leaves, silk, cotton, &c., only facilitated its disengagement by furnishing a surface adapted to the collection and escape of the gas-bubbles.

"These phenomena may also be explained by an assumption favorable to the hypothesis of Priestley, namely, that the green matter consists of plants, which, adhering to the surface of the bodies placed in the water, there vegetate, and in consequence give rise to the gas."

"I would willingly adopt this opinion, were it not that a most careful and attentive examination of the green water by means of an excellent microscope, at the period when the oxygen was most abundantly disengaged, has convinced me, that at this period nothing to which the name of vegetable can be given is present. The coloring matter of the water is of an animal nature, and is nothing else than the accumulation of an infinite number of little moving animals." —*Philosophical Transactions of the Royal Society.* Vol. lxxvii., 1787.

In a very interesting memoir, by Messrs. August and Morren (*Transactions of the Academy of Brussels*, 1841,) it is shown that water with organic substances evolve "a gas" which contains 61 per cent. of oxygen; and they conclude their treatise in the following words:—"It follows from the preceding remarks, that the phenomenon of the evolution of oxygen gas is due to the *Chlamidomonas pulvisculus* (Ehrenberg,) and to several other green animals still lower in the scale."

The author took the opportunity of convincing himself of the accuracy of this long-observed fact, by means of some water out of a water-trough in his garden, the water being colored strongly green by different kinds of infusoria. This water was freed by means of a sieve from all particles of vegetable matter, and being placed in a jar, inverted in a porcelain vessel containing the same water, was exposed for several weeks to the action of solar light. During this time, a continued accumulation of gas took place in the upper part of this jar; after fourteen days ⅓ of the water in the jar had been pressed out of it, and the gas, which had taken its place, ignited a glowing match of wood, and in all respects behaved like pure oxygen gas. It must be here expressly stated, that the water, before being exposed to the action of solar light, was examined by one of Plœssel's best microscopes, without the detection of confervæ or of any kind of vegetable matter.*

Without venturing upon any opinion on the mode of nutrition of these animals, it is quite certain that water containing living infusoria becomes a source of oxygen gas when exposed to the action of light. It is also certain, that as soon as these animals can be detected in the water, the latter ceases to act injuriously to plants or animals; for it is impossible to assume that pure oxygen gas can be evolved from water containing any decaying or putrefying matters, for these possess the property of combining with oxygen. Now, it is obvious, if we add to such water any animal or vegetable matter in a state of decay,

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* One hundred cubic inches of water saturated with air contained, in the form of air, according to the experiments of Humboldt and Gay-Lussac, not above 1·6 cubic inches of oxygen gas.

that this, being in contact with oxygen, will resolve itself into the ultimate products of oxidation in a much shorter time than if infusoria were not present.

Thus we recognize in these animals, or perhaps only in certain classes of them, by means of the oxygen which in some way as yet incomprehensible accompanies their appearance, a most wise and wonderful provision for removing from water the substances hurtful to the higher classes of animals; and for substituting, in their stead, the food of plants (carbonic acid,) and the oxygen gas essential to the respiration of animals. They cannot be viewed as the causes of putrefaction, or of the generation of products injurious to animal and vegetable life; but they make their appearance in order to accelerate the conversion of putrefying organic matter into its ultimate products.

Many fungi grow without light, and in their growth and life are characterized by all the phenomena which characterize animal life; they destroy air by absorbing oxygen and evolving carbonic acid, and, in a chemical point of view, behave like animals without motion.

In opposition to this class of beings, which can scarcely be designated as plants, we have living creatures endowed with motion, and with the organs which characterize animals, and yet which behave in the light like green plants; for while they increase in size and number, they furnish sources of oxygen when its access, in the form of air, is excluded or prevented.—(*Liebig's Chemistry in its Applications to Agriculture and Physiology—London Edition of* 1847.)

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*A Clinical Lecture on Hypochondriasis, delivered at the York Medical School.* By THOMAS LAYCOCK, M. D. Physician to the York Dispensary, and Lecturer on the Theory and Practice of Medicine.

Policilinical or polyclinical—Abstract of a case of hypochondriasis—Diagnosis from indigestion and from melancholia—Physiological estimate of the symptoms—The reflex theories—Prochaska's—The "true spinal"—The "sensori-motor"—Objection to them—Reflexion a property of all organized matter, and of the whole nervous system—Application to hypochondriasis—Etiology—Quack hand-bills—Treatment—Ethics of quackery and of treatment of hypochondriasis—Prognosis.

We had on Saturday at the Dispensary one or two cases of hypochondriasis; two more have come to me since; and it has occurred to me that they present a favourable opportunity for a practical discourse on that affection and its treatment—and the treatment, I may add, of the patient: you will then be better able to study these cases. For I do not think it is sufficient to tell you that such are the symptoms, such the diagnostic characters, such the remedies to be adopted, in a case before us. Your relations to the man who is the object of your professional attention are ethical as well as curative; and the ethical relations, I may venture to affirm, rival the curative in importance.

Perhaps I ought not to call the kind of discourse I propose addressing to you from time to time, upon some case under my care at the Dispensary, a *clinical lecture;* for, in the first place, the term clinical ought to belong exclusively to a discourse given on leaving the bed-side of a patient in a hospital; and secondly, to a discourse having reference to the theory and practice of medicine as an art and science. But here we have no bed; the patient stands before us, is examined, prescribed for, advised, returns to his house and to his labour. You cannot restrain him from hurtful practices or irregularities, nor insure that his diet shall be suitable, that the remedies ordered shall be given to him at the times appointed, or even that the remedies shall be taken at all. Now the term clinical may be used in the sense of practical; it is, in fact, often so used; and if we adopt this meaning, then I think the term *poli*clinical, used very commonly on the continent in reference to Dispensary practice, will be applicable enough to the sort of discourse I propose to give you. But the prefix *poli* is not derived from πολυς, many, but from πολις, a city, as used in metro-*polis*, *poli*tical, &c. I mention this, because I have seen the word printed with *y* instead of *i*—pol*y*clinical; and this error has led persons to think it a very presuming word to be used.

In the case for consideration (for I select one of two,) you have a very common form of disease—a disease always difficult to cure, and almost invariably annoying or embarrassing to the practitioner. It is a form of disease which involves important ethical considerations—questions of moral conduct. The special points we shall have to consider, will be,—1, The disease itself and its management;—2, The ethics of gratuitous practice and of empirical practice—the misdeeds of quacks. I will read an abstract of the notes taken at our examination of the case.

The patient is a man aged 28, by trade a comb-maker, unmarried, of pallid cachectic complexion, bald across the head, looking older than he is, with a grave, anxious, and rather melancholy expression of countenance; thinner apparently than when his clothes were first fitted upon him. Gives the following account of himself—his *status presens* and his *anamnesis*,—or, in other words, of his present condition and past symptoms. He feels a large substance in the throat, near the sternum; it gives the sensation of rising of phlegm, and feels smaller after free expectoration. Just now (at the time of examination) it is more solid and larger than usual. Has a slight cough from the irritation in the throat. At the same time that he expels the phlegm palpitation comes on, and also a good deal of flatulence, and he feels fluttered. He has also wind rumbling through his bowels, and pain in the head, which is much relieved since I ordered him the cold douche two days ago, and he remarked that his head would be very bad without it. His appetite is good, his food apt to turn acid on his stomach, his bowels irregular, and his urine high-colored. He has been troubled with wind and water-brash, and palpitation, for about two years. Nine months ago he was under my care for the rising in the throat, when it rose to the top of his throat, he got much better, and the lump was reduced to the size of a pea. During the last few weeks he has been as bad as ever, and he went to Mrs. Lamb, a worm-doctress, paying her a guinea for advice and medicine; she gave him some medicine that boiled, and said she could cure him; she never felt his pulse; no worms have come away, and he is no better. Seven years ago he was first troubled with tape-worm, but he felt pretty well; took some medicine in gin for them; went to a quack doctor at Hull, who gave him oil of turpentine, and eighteen yards of tape-worm came away; two years ago went to a quack doctor at Sheffield, and got turpentine for the worms. He was made very ill, and none came away, and has seen none pass since. Ever since that time he has had the wind and rising; he feels very nervous and low-spirited, and muses a good deal; muses about the palpitation and rising: works by himself.

Looking at his tongue we find it clean except on the posterior part; this cleanness of the tongue does not correspond with the other symptoms, and, according, on inquiry we find the man has just dined. The taking of a meal always clears the tongue more or less. Examining his throat, we find no tumor or tumefaction at the point he indicates as the seat of the sensation that distresses him. He swallows perfectly well. His pulse is 90, and this is no doubt quickened by a little nervousness, for on stripping to let me examine the cardiac region, the pulse rises to 120. Percussion over the heart does not indicate any enlargement, the dull sound is circumscribed to the normal limits; the systolic stroke is short and jerking, with the sort of ring usually observed in nervous palpitation, but no other sound. No tumor, no tenderness in the epigastrium or hypogastrium.

Diagnosis.—Such are the symptoms. What is the disease under which this man labours? It is hypochondriasis. I knew already before he spoke, when he came to me two days ago; for he was an old patient; and I felt pretty sure he had paid a visit to Mrs. Lamb; for I had seen her bills distributed freely in the windows. Here is one: it describes the general symptoms of worms in a quotation (that heads the bill) from "The Cyclopædia of Practical Medicine, edited by John Forbes, M. D. F. R. S., Physician to the Chicester Infirmary, Alexander Tweedie, M. D.," &c. &c. Mrs. Lamb observes, "that she confines the bulk of her practice to the digestive function." She is therefore a specialist; and she modestly remarks, that the cases there detailed, "together with hundreds of cases published by her during her former residence in York, prove with what success Mrs. Lamb has studied this important branch of medicine. There are few diseases to which mankind are liable that may not be traced to a disordered stomach," &c.; and then she notes specially worms as a cause of indigestion, and gives cases and references, the dislodged tape-worms being to be seen at her residence—trophies of her skill.

Now what made me think that the patient had consulted Mrs. Lamb was my previous knowledge of his disease. As it is hypochondriasis he labours under, I thought if he read a bill (which he would not fail to do) he would take the bait. He did so, and Mrs. Lamb hooked his guinea; yet he is, I believe, really poor; at least he pleads poverty with me, and I know men of his craft don't get good wages. He borrowed the guinea, he says.

Now the general symptoms are those of gastric derangement; the melancholy musings rather belong to melancholia. Why, then, is the disease termed hypochondriasis? It is the combination and characteristics of the symptoms that determine the diagnosis. There is disorder of the nervous system concurrently with the indigestion, and of a peculiar kind, inducing nervous palpitation, the symptom of "rising," allied to globus hystericus, and often coincident with cardiac action, and a melancholy musing mood of mind, having reference to no external circumstances, as property, friends, crimes, a future state,—that would be melancholy, but to the state and condition of his body and his health, especially to the condition of his throat and heart. This melancholy musing on the bodily condition characterises the cerebral affection termed hypochondriasis. The nervous symptoms distinguish the case from indigestion.

Physiological estimate of the symptoms.—The physiology of the nervous system is a tangled web indeed. There are, however, two or three laws of action which I think you may regard as established, and may safely adopt in any estimate of phenomena arising out of the action of the nervous system. One of these is, that the nervous centres in the ganglia of the system, whether sympathetic, spinal, or encephalic, re-act on impressions derived from the surfaces of the body. Another is, that the changes induced in the ganglia by impressions from without, may be induced also by an inner action of the molecules excited directly by changes in the circulation,—changes in the distribution of the blood and in the composition of the blood. Another of these cases of action is, the propagation of changes in the ganglia originally excited from without to other ganglia, and exciting in them complex changes corresponding in character to those by which they were excited, and so on through the whole system of grey matter, from the smallest fibril to the whole hemispheres.

Now, when an impression is made on a nerve in connection with the ganglia contained within the vertebræ or spinal canal, those ganglia are excited into action, and that action may be propagated or passed upwards to the encephalic ganglia; re-action will then begin in them, and acts of mind result. But, if the connexion with the encephalic ganglia be cut off, as, for example, in cases of paraplegia, the re-action takes place in the ganglia below the break in the chain; for the impressions derived from without cannot pass upwards. The resulting motions are confined to the muscles, whose motor nerves are derived from these lower ganglia, and they constitute the socalled reflex acts, or rather the "purely" reflex acts; for there is no sensation. It is to be specially noted that these mechanical or automatic acts are not irregular convulsive movements merely: they exhibit *design*, just as if the individual willed them; but there is no volition, and no sensation. The acts take place in accordance with "laws written upon the nervous pulp," to use the expression of Prochaska; or, in other words, in accordance with a definite action of the grey matter of the ganglia. Dr. Marshall Hall, like Prochaska, maintains that these reflex acts are limited to a certain portion of the cerebro-spinal axis; namely, that portion extending from the tubercula quadrigemina to the cauda equina, and to a certain class of nerves different from those of sensation and voluntary motion, termed incident exitor and reflex motor nerves.—The term "incident," derived from Prochaska, seems to be used in a sense analogous to that used in optics, when we speak of an *incident* ray of light.—The impression *falls upon* the central ganglia along or through the *incident* excitor nerve, and is reflected along the reflex motor nerve.

Many persons think Dr. Hall invented these views and terms; but he did not altogether. Prochaska uses the term *reflexion*, and particularly cautions against any mathematical use of the term; for iatro-mathematics were common in his day, and algebraic equations were used freely in theories. He expressly observes that this reflection does not take place, according, to physical laws, where the angle of *incidence* is equal to the angle of reflection, &c., but according to laws writ on the *medullary pulp*; probably meaning thereby the grey matter.

Now many of the reflex acts are accompanied by sensation, and on this circumstance another class of movements has been made out with another anatomical department of the central axis. Dr. Hall's true spinal system is robbed of its upper and fairest portion to make up this "sensorimotor system," as it is called, the seat of the consensual acts—such as laughing, sighing, &c. Anatomically it includes the ganglia of special sense; first, the olfactory, the bulbus olfactorius; secondly, the corpora striata and thalami optici subservient to touch; then the tubercula quadrigemina or ganglia of vision; and, lastly, the ganglia of hearing and taste embedded in the medulla oblongata. These are the ganglionic centres of the consensual acts, just as the spinal ganglia are the centres of the reflex acts. The consensual acts are all excited by sensation; a sensation is the consciousness of an impression; this consciousness, of course, and not the impression, excites the consensual acts; how it is not explained, but not by exciting an act of will—quite the contrary. The consensual acts are all as involuntary as the reflex acts. The impressions made on the nerves of special sense (the incident excitor nerves of the sensori-motor system,) arrive at the sensory ganglia; then "what motions shall follow, what sensations are determined by the laws written in the nervous pulp," just as in the case of reflex acts: so that, in point of fact, the pith of the whole matter is just a reflex function attributed to these sensory ganglia, with "consciousness of the impression" superadded—a helpless spectator of the determination of the "laws written on the nervous pulp" by the impressions thereon. It is really nothing more than Dr. Hall's reflex acts combined with sensation, but hid under an ingenious medley of words. The sensory ganglia have also a reflex function, and the cerebral nerves are also incident excitor nerves, consciousness looking on the ganglionic changes as a helpless spectator, if they be "purely" sensorial—if no volition be excited by it. Prochaska had the same general ideas, but he did not minutely localize them. He united the "true spinal" and "sensori-motor" into one system, as distinct from the volitional. He distinguished between reflex acts with, and reflex acts without, sensation; he distinguished also the consensual acts, and argued that they were excited by impressions—not sensations—that consciousness of the impression accompanied them only; was coincident, but not causal. I believe he is right; I prefer his views to the more modern improvements; modern anatomy is, however, much better, and it well elucidates his physiology,—but Prochaska did not go far enough.

I happened once to publish an essay on the subject, but I extended the doctrines of Prochaska of a reflex function, from the "true spinal system" to these sensory ganglia, and also to the cerebral hemispheres. I maintained that this reflex function was a function common to the whole of the cerebro-spinal centres. The first statement of my views is in my "Treatise on the Nervous Diseases of Women;" the next in the 19th vol. of the British and Foreign Medical Review, (the essay I allude to,) and the next in vol. ii. of the Lancet for 1845, where you will find my correspondence with Mr. Combe. The fact is, that neither of the two systems, nor Prochaska's, is satisfactory; first, because they are conflicting; secondly, because they leave a vast number of residual phenomena unexplained and unarranged; thirdly, because they are opposed to the doctrine of unity of structure and function in all organisms.—And this objection I consider the most important of all. No theory of vital dynamics can leave out with propriety either the dynamics of vegetable life or of intellectual life. Now the power of reflex action, according to laws written on the organism, is a propriety common to *all living matter*, and its first manifestation is in the nutrition of organisms; but our reflex and sensorial theories leave this great and most comprehensive fact out of consideration altogether. I therefore recommend to you a higher generalization, and advise you to consider that, as regards the *nervous system*, all nerves, whether sympathetic, spinal, or cerebral, are incident excitor; that all central grey matter, whether encephalic, spinal, or sympathetic, has a reflex function; and that all the nerves proceeding from that grey matter, whether sympathetic or motor, are agents of this reflex function; of a function which determines not the consensual acts alone, nor the reflex acts only, but detremines also the phenomenon of mind, and *the nutrition of organs according to design*, in virtue of "laws written on the nervous pulp." I would not press this view upon you if it had not a great practical value in such forms of nervous disease as hypochondriasis, and the chronic forms of insanity dependent upon visceral disease. The reflex theory, so far as it is true, has a great practical value; but let me caution you against adopting the whole, for, as I think, that the "true spinal system" has no anatomical existence, I am likewise of opinion that in a few years it will become as forgotten as Bell's "respiratory system;" and take a place beside it in the limbo of science, amongst numerous other used-up theories now lying unregarded there.

To apply my views. In hypochondriasis we think there is a functional disturbance of some portion of the encephalon; and it seems probable that this functional disturbance is dependent, firstly, upon an incident excitor action derived through that portion of the sympathetic system, situate *upochondra*—beneath the cartilages; in other words, from the morbid visceral surfaces supplied with sympathetic nerves. Secondly, the functional disturbance may have a centric origin; that is to say, may be excited by mental operations, and this disturbance may have a morbid reflex action on the viscera supplied from the sympathetic system. In the one case the morbid changes are excited in the brain by the morbid viscera; in the other, the morbid condition of the viscera is excited by the morbid brain. But the two morbid states must exist together to constitute the disease termed hypochondriasis.

The visceral derangement in hypochondriasis presents differences according to the stage of the disorder. In the case before us the disease is in its first stage. There is functional disturbance only of the heart's action; after a twice-repeated examination with the stethoscope we can detect no signs of structural change. The change is in the innervation of the heart alone; the pulse was at 90 while we were inquiring into his *status præsens*, but the emotion consequent on the stethoscopic examination raised the pulse to 120.—This susceptibility of the heart is very often witnessed in hypochondriasis as the chief and most pressing symptom, and I think it is to be fairly assumed that it ultimately induces structural change, as hypertrophy. The heart, by being continually in a state of increased action, becomes over nourished, in the mode I have lately explained to you. The symptoms of indigestion detailed by the patient are highly characteristic; the borborygmi, the acidity, the inordinate excretion of mucus from the pharynx and œsophagus, are all every-day symptoms. The tongue is cleaner than is usual in these cases; but this was explained by the fact of the patient having just taken his dinner. It is very often scabrous or rough, with irregular red fissures in it, and covered with a dirty-looking fur.

There is a tendency to constipation in this case, but not so great as in the more advanced stages. Nor is there that disturbance of the renal function frequently met with. The nutrition of the body has evidently suffered from the gastric derangement; the pale sallow tint indicates defective depuration of the blood by the liver. The baldness is indicative of a tendency to cerebral irritation. You may often witness excessive and early baldness in persons laboring under morbid irritability of the brain. The hair is thin, dry, rough; and falls off from the region of the coronal and parietal sutures rather than from the crown.

The cerebral derangement is indicated by the sensation of globus, by the anxious expression of countenance, and by the circumstances detailed in the anamnesis, or history of the case. His patronage of Mrs. Lamb, the worm-doctress, and of the Sheffield quack, and his sitting musing, are significant of the hypochondriac. There was no real foundation for his idea that he had worms, as from his own account the last discharge of tænia took place about two years ago, and during the seven years that he occasionally passed portions of the animal his general health was little if at all affected. But Mrs. Lamb has some spicy bills out, suitable for a hypochondriac, and these had doubtless impressed him with the idea of worms.

The fancies of the hypochondriac are very varied, but I believe they have all a direct or indirect reference to the condition of the viscera, or to the functions of the nutritive nerves. I had a man to consult me lately who was strongly of opinion that his borborygmi and eructations, and pains under the ribs, and the multiform feelings he experienced, were all caused by a poor old woman, a neighbor of his; and he would watch her night and day, and if he saw her put her pipkin on the fire before going to bed—to warm her supper probably—he would shudder with rage and desperation at the infernal arts she was practising against his comfort: and the more desperate did her conduct appear, because she bought the pipkin at his own shop! I was obliged to humour the fancy, and make her out an amiable witch. Many fancies of mono-maniacs arise in the same way as these perverted notions; as, for example, when a man fancies that he is with child, or that he is actually consumed away, without stomach, liver, or intestines,—nothing but a void. The exact locality of the cerebral disease can only be guessed at; it is possibly in the phrenological organ of the "love of life." That it is in a portion of the cerebral hemispheres is manifest, for the *ideas* are perverted; and that it is excited by or in connexion with the incident excitor action of the sympathetic nerves may, I think, be fairly granted.

With regard to the etiology, I would observe, that in the case under consideration it is probable the disease first commenced in the *upochondra;* that the exciting cause of derangement in the digestive organs was the turpentine administered by the Sheffield quack, (although the tænia may also have had an effect,) and that this derangement has reacted upon a cerebrum already predisposed to diseased action, for he appears to be of the melancholic temperament. It very often happens, however, that mental causes excite the visceral disorder, such as excessive anxiety, continued tension of the mind in commercial or literary pursuits, or the depressing emotions, as grief. The recurrence of this attack at this season in our patient (and the increase of this class of patients) is in accordance with an opinion of the older physicians that the disease is most frequently observed in autumn. What may be the proximate cause of this autumnal recurrence is not manifest, but I would guess it to be some change in the blood. We have had a greater proportionate number of cases of chlorosis in both boys and young women, a greater proportion of gastrodynia, and other epigastric affections, and I am inclined to think that there is some common cause for all these affections. It may be that in this particular instance the perusal of the quack's handbill's has been the *exciting* cause, just as the perusal of medical works will induce an attack of hypochondriasis. If this be the case the visceral derangement is reflex. You may have real disease induced; that is to say, structural change in the organs in connexion with the sympathetic nerves, by the reflex action of the cerebral hemispheres. The individual excites abnormal action by continually directing his attention to an organ, most frequently the heart; at first the disturbance is functional only, afterwards it is structural. Medical students are particularly liable to this form of hypochondriasis.

Now, the method of treatment to be adopted in this case is first to relieve the gastric disorder. Once, on a previous attack, he took bismuth, and then the lump in his throat was reduced to "the size of a pea." The trisnitrate is an excellent remedy in cæliac neuralgia; it is prescribed for him again, and to relieve the acidity we combine a few grains of carbonate of soda; to act gently on the bowels we add two or three grains of rhubarb. He has already applied the cold douche to the head, and with so much relief that he "doesn't know how he could get on without it." Of course you heard my directions as to his diet and the mode of eating his food. If his case were more chronic and severe, a smart purgative would be useful, and small doses of steel and calomel, and perhaps a counter-irritant—an issue to the region of the heart: we cannot send him on his travels. The mental hygiene of hypochondriacal patients is very important. The mind should be occupied with a variety of occupations, and hope encouraged. Never say to a hypochondriacal patient—"Pooh! you ail nothing, 'tis all fancy, you're nervous." His sensations flatly contradict your assertion, and he immediately draws the conclusion that he is not safe in your hands; that, in fact, you will neglect him, and this aggravates his disease. Be sure that the man is really ill, that all pain or painful feeling is significant of disease somewhere, and sympathize with his sufferings. Hypochondriacs are often capricious, ungrateful, querulous; that they are so is a result of disease: never resent their acts; if they change their "doctor," be thankful that you have got rid of an annoyance; if they do not, do your duty firmly but affably. You would notice that I reproved our patient for paying a quack a guinea and coming to me in *formâ-pauperis.* I did that, not to resent his consultation of a quack, but his dishonorable treatment of myself. *I* had a right to the fee if he were able to pay one, for he had already, by his own acknowledgment, derived benefit from my previous services. We should always make as few paupers as we can; it is a duty which we owe to society as well as to ourselves. We should put down quackery, too, as much as we can; but I would advise you never to abuse common quacks, or their supporters. Diffuse sound knowledge; nothing else but that will stop quackery. In the meanwhile, laugh at the quacks, and the simple folk they plunder. Ridicule will do more than invective; in fact, they are not worthy the latter; I never would condescend to it. We have some amusing quacks at York, particularly on the market days. They travel about from town to town, and sell worm-cakes, corn-plasters, &c. The "Idle doctor" is a water-caster from a village in the West Riding; the cancer-doctors, bone-setters, and "wise men," are stationary, they do not "stand the market." I should recommend you to study these people and their methods. There is a worm-doctor, who "stands the market," that will amuse you much. He has a stall with bottles of worms ranged upon it, and there he sits with a spanish cloak, with a stern grave countenance, often reading in a book. Albert Smith might make a capital comic song out of him, for every now and then he makes an oration, and abuses the York people well, while he gets their money. The boys are badly behaved, he says, and the people won't believe his cures unless he produces worms as thick as a mop-handle!—"one thirty yards long" (and he exhibits a worm in a bottle) "won't satisfy them!" When you have witnessed the proceedings of these men for a while, and convinced yourselves of the gross ignorance of the people who support them, you will be pretty thoroughly convinced that quackery cannot be put down till the millennium, if then. At all events, never quarrel with your patients for consulting these quacks; laugh at the practice as a good joke, keep a very strict account of *your* professional services, and make no abatement in the charge. Be honorable and just to the quack abettor, but not generous. I believe these common quacks get their patients principally from the hypochondriac class.

The two who came yesterday had both been to the "Idle doctor"—the water-caster, and one had been also to Mrs. Lamb. The one from the country on Saturday had also been to the "Idle doctor;" so that the extirpation of quackery must depend partly at least upon the extirpation of hypochondriasis:—and who is so ardently hopeful as to expect that? Besides, I don't think we could deprive hypochondriacs of this amusement, or indeed ought to.

There are two classes of quacks to whom, however, you should show no quarter—the extorting and the murdering. It may be only a question of taste whether you should certify to the skill of a wandering corn-cutter—I would not do even that; but there can be no question about the obscene advertisers, some of whom, as I positively know, attempt to extort money by the threat of exposing their weak-minded victims. The obscene advertisements in some of our newspapers are disgusting, and I trust that in a few years no respectable family will countenance such papers; they are utterly unfit for the perusal of the female branches. As to quackery, then, let your patients please themselves; ridicule the humbug quacks, sternly denounce the extorting and murdering.

The *prognosis* is not favorable in cases of hypochondriasis if you can adopt none other than the ordinary plan of treatment. Medication never can do much in these cases; it can relieve the gastric derangement, and relieve or palliate the cerebral phenomena, but the curative treatment must be hygienic and moral. One of our cases on Saturday asked for a written prognosis to show his master (he was an agricultural laborer,) for if he was likely to get well soon, his cow would not be sold by the Board of Guardians. I, of course, would neither be accessory to the sale of the poor man's cow, and of his consequent removal to the workhouse, nor would I aggravate his existing disease by an unfavorable prognosis. Yet I told you the prognosis *was* unfavorable after he had gone, because, in fact, the curative means—change of air, of scene, of employment—could not be adopted, and because there was the fear of the Union Workhouse resting like an incubus on his nervous system. And this is the case with many examples of hypochondriasis amongst the poor; they are incurable by the limited means at your command, but not incurable absolutely.—*London Medical Gazette.*

2.—*Medical Evidence in a Case of Poisoning by Arsenic.*

We extract the following from the report of a trial (for poisoning) of a woman named Lennox, which took place at the Newcastle Assizes, on Friday last. While the evidence reflects credit upon the witnesses, it gives a fair outline of the course of examination adopted on these occasions. The woman was charged with poisoning her husband by arsenic. She was proved to have gone to one shop to buy poison under the pretence of destroying rats; and at another, where she succeeded in procuring an ounce, she stated that it was for destroying bugs. On the same day she was proved to have made a pudding for her husband, which he ate for his dinner. By a very remarkable coincidence, the man was soon afterwards attacked with symptoms of poisoning by arsenic, under which he sank; and by an equally singular coincidence, arsenic, as the medical evidence shows, was unequivocally detected in the body! The counsel for the prisoner suggested this was one of those "strange and unaccountable accidents" which every now and then occur; and he contended that there was no *conclusive proof that the deceased had died from the effects of arsenic at all!* We object both to his logic and his chemistry, but on these matters we leave our readers to decide for themselves. The woman was of course acquitted.

Dr. Fenwick.—Is a doctor of medicine at North Shields. Had seen deceased April 7th, 1846, for the first time. He complained of purging, which he had had for nine months. He did not complain of much pain. Continued under witness's care 17 days. He then said he had been well for nearly a week.

Never saw him alive again. It was a little after 7 o'clock on the 3d of July witness saw him. He was then dead. Assisted Dr. Coward at a *post-mortem* examination on the 7th. Examined the cavities of the head, chest and bowels. The appearance of the stomach, and the upper part of the small intestines, showed inflammation, chiefly in the stomach, and becoming less as they proceeded lower down. There was congestion of the lungs. There was disease of the heart and the vessels proceeding from it. The liver was rather diseased. The heart was greatly enlarged. The large vessel had a bony deposit. The other arteries were, generally speaking, in a diseased condition. Should say some irritant poison had been the cause of death. Applied some tests to the contents of the stomach which induced witness to believe that arsenic was present. Took away the stomach and duodenum, and the contents of each, separately. They were left in the care of Mr. Coward. Could not form an opinion of the time the poison had been taken before death. *Cross-examined.*—The liver was attached to the bowels. Purging may accompany inflammation of the bowels. Most commonly there is constipation. Inflammation of the stomach is very rare. Brown patches on the stomach are usually found in cases of poisoning by arsenic. The inflammation is usually localized. In other cases there is diffused or general inflammation. In a recent case one does not expect inflammation of the œsophagus. Did not in this case examine it. Violent pain usually accompanies death by arsenic. It may take place without any pain. In many cases there are convulsions ; not generally. In most cases the death is a severe one, unless coma comes on. The brain in this case presented no appearance of disease.

Mr. Henry Coward.—Is a surgeon at North Shields. Assisted Dr. Fenwick in opening the body of the deceased. Witness gave the stomach and its contents to Dr. Glover. Agrees generally with the testimony of Dr. Fenwick. *Cross-examined.*—The inflammation might be produced by natural causes, or it might be produced by an irritant poison. Gave the contents of the stomach to Dr. Glover in a stoppered bottle. Got the bottle from the surgery. They were washed out with distilled water.

Dr. Robert Mortimer Glover.—Is a doctor of medicine, and a lecturer on medical jurisprudence in the medical school of Newcastle-upon-Tyne. Received the stomach and the duodenum, and the contents, from Mr. Coward. The stomach was intensely inflamed, and there was the appearance of yellow powder. It was in very small quantity—mere specks. Took the contents of the stomach and boiled them with a little acetic acid. Filtered the liquid, and applied trial tests. Passed sulphuretted hydrogen through one portion of the fluid. Obtained a yellow precipitate. Added the ammoniaco-nitrate of silver to another portion, and obtained a yellow precipitate. Added the ammoniaco-sulphate of copper to another portion, and obtained a greenish color with a slight precipitate. The results of these experiments satisfied witness that arsenic was present. One of these precipitates was sulphuret of arsenic or orpiment ; the others would be arsenites of silver and copper. Took another portion of the filtered fluid, added a little muriatic acid, having previously tested it ; boiled it with small pieces of copper, which became black. Took the pieces of copper and dried them carefully, heated them in a test tube, and obtained a sublimate composed of a metallic-like film, and a white rim above the metallic film. Examined the ring with a microscrope, and discovered octohedral crystals with triangular faces ; these were crystals of arsenious acid. Took the stomach and intestines, and cut them in pieces ; boiled them in water and acetic acid, filtered the fluid, and tested it in a Marsh's apparatus. Obtained distinct arsenical stains. Produces them. They are a little fainter than at first ; the stains of antimony are black. These stains show that the fluid contained arsenic. Tried these experiments twice ; once with a new apparatus. There was no doubt of the presence of arsenic : it must have been some quantity. *Cross-examined.*—Will not say positively there was more than a grain ; thinks it probable there was more. In Marsh's test used zinc and sulphuric acid. Zinc contains arsenic ;

sulphuric acid does when it is made from pyrites; arsenic is a component part of glass. Would always wish to use Marsh's test in conjunction with others; would not wish to trust to it alone; it may be used so as to be free from any doubt. Water was used in boiling the contents ot the stomach; the filtered liquid was mixed with organic matter. The liquid tests, except sulphuretted hydrogen, are unsafe to trust to alone, where there is animal matter present to any considerable extent. Cadmium would produce the same appearance as arsenic when tested with sulphuretted hydrogen. Antimony gives a totally different precipitate. Does not think tin would give similar appearances as arsenic. Never tested it with that view. Orfila was once of opinion that arsenic existed in the bones. Believes he has since retracted that opinion. The stomach did not present any very characteristic mark of arsenical poisoning. It presented appearances which arsenic might well cause, but which are not peculiar to it.

In answer to questions from the JUDGE, the witness said he did not rely much on the trial tests. They were used in conjunction with others. The conclusive test is Reinsch's, producing the peculiar crystals of arsenic.

Re-examined.—The arsenic found did not proceed from the zinc—the sulphuric acid—or the glass. Witness tested the apparatus while it was working, and there was no arsenic present before the contents of the stomach were introduced.—*Ibid.*

3.—*The Sequels of Scarlet Fever and Measles.*—By J. A. HIGGENSTON, Esq.

(A paper read before the Brighton and Sussex Medico-Chirurgical Society, August 5th, 1847. Dr. KING, President, in the Chair.

As far back as the year 1843, I published in the pages of the GAZETTE some pathological remarks on scarlet fever, showing that this disease is, at a certain period of its course, an inflammatory one, requiring the aid of antiphlogistic measures. As my experience enlarged, my views became, of course, wider, and I was able to compare scarlet fever with other disorders, especially with measles: the conclusions of my mind upon which, I now submit to your approval and publication in the pages of your periodical, if you please.

It is not generally admitted, even if it be understood,—at least I have not hitherto found it distinctly stated in medical works,—that there is an essential difference between scarlet fever and measles exhibited in the particular mode in which the poison of either disease subsides of itself, or is eliminated from the constitution—the one subsiding or settling upon the liver, or being eliminated through it, and the other subsiding or settling upon the kidneys, or being eliminated through them. Both these diseases fall upon the skin in their primary manifestations; but, in their sequels, they are respectively opposite and different; for, in its issue, the measles attack the liver, while the scarlet fever attacks the renal emunctories. This is the proposition of the case, or the problem that I wish to propound or prove.

I apprehend that the strumous diàthesis will be found predominant in either predicament. But, where is struma not predominant, if not latent? The manifold cares and anxieties, or, in other words, the numerous causes of vital exhaustion which subsist in savage or civilized life, are more than sufficient to engender or aggravate struma, the most lamentable of human ailments, and the most lingering of incurable maladies. The first and the last stages of phthisis pulmonalis; the suppuration of a single cervical gland, and the destructive ulceration of the synovial membrane of the knee-joint; the precocious intellect of strumous childhood, and the premature old age of strumous manhood: are alike signs too well known to those before whom I am speaking, to render it necessary or becoming in me to dwell upon the peculiar character of health or disease which they indicate, or to describe the fatal goal towards which they tend and advance with incontrollable obstinacy, and very often with amazing velocity.

Whatever depresses the vital forces is the cause of struma—a cause whose habitation is favored by the congenital or acquired debility of the great nervous centres—the cerebro-spinal axis, which, in the discharge of its several functions, betrays a greater or less imperfection in their respective failures. In the brain itself, the governing organ of life, resides the origin of all chronic, if not of all active maladies. Blushing, the most common imperfection or virtue of the strumous habit, arises from debility of the brain, which, incapable of sustaining the shock of mental emotion, becomes, all of a sudden, powerless in its guard over the superficial or peripheral vessels of the cheeks, so that their capillaries, unexpectedly and ungovernably relaxed, admit the red blood to flow into them and to remain fixed there, for a time at least, in spite of every act of volition to the countrary. This is, in principle, an explanation of acute inflammation; there being, perhaps, no inflammation that does not begin in local or general debility; the received Hunterian dogma of *excess of arterial action* being an exceedingly debateable question. Not that this alters in the slightest degree the usual treatment, which rests upon *fact* and *experience*, and not at all upon the *probabilities* of this or that theory entertained by pathologists either of ancient or modern date.

But, to return to the subject of our present enquiry, namely, that of measles and scarlet fever, both of them being a poison eventually set free, the latter through the kidneys, and the former by the liver.

I appeal to those who have carefully attended to these two diseases, and request them to consider whether they have not observed diarrhœa follow upon measles, and renal affections succeed to the scarlatina? I submit the question to practitioners of large experience, or, if not to those of large practice, at least to those of still larger minds, who have well observed what little they may have been called upon to treat. For there is a wide difference between those who have seen a great deal carelessly, and those who have seen much less, but, at the same time, have studied what little they may have seen with care and attention.

Now the three great emunctories of the frame are the lungs, the liver, and the kidneys,—by the kidneys are eliminated the azotised or nitrogenous compounds in a fluid form,—by the liver the carbonaceous materials in a solid form,—and by the lungs the same carbonaceous materials in a gaseous or aerial form—namely, that of carbonic acid gas. Cruveilhier, in some very striking experiments, has shown, that, by introducing certain heterogeneous particles into the blood through the veins, one or other of these three great emunctories becomes deranged according to the kind of poison or obstruction that has been introduced. The first effect of these deleterious substances is on the veins themselves, that is to say, provided that these substances are arrested in their course before reaching the arterial branches or ramifications of the circulating tree, producing pus in the veins—phlebitis, in fact,—gangrene, and death. But if these heterogenoous particles pass forwards from the veins into the arteries, their ultimate effects fall upon the more vital organs far removed from the seat of the original mischief, giving rise to abscesses in the muscles, the joints, the eye, the liver, softening of the various tissues, inflammations in distant organs, depositions of pus, blood, lymph, and serous effusions. And what is very singular is, that Gaspard agrees with Cruveilheir in asserting that, in its course along the current of the circulation, the poison finds great difficulty in its transmission from the *arterial* to the *venous* capillaries; while Cruveilheir remarks that, in successful cases, the poison, when dilute or in small quantity, *is eliminated by intestinal or urinary excretion*, but that when large or concentrated it kills. Tiedemann and Magendie have, indeed, put the question almost entirely at rest by their experiments, which demonstrate that the liver and the lungs are the two great emunctories of the frame; and it is owing, perhaps, to this, says Dr. Ferguson, who has written very ably on this point of pathology, and from whose work on Puerperal Diseases the foregoing observations are chiefly borrowed—it is owing, perhaps, to this, that they (the lungs and the liver) are, of all the organs of the body, the seats of the greatest number of maladies. Surgeons know well enough the fever that follows upon amputation when the veins have been injured, or have become the focus of suppurative inflammation—a fever which is well known to them in its source as it is in its almost invariably fatal issue. And the accoucheur knows far better than I am able to describe to him, the formidable symptoms of puerperal peritonitis, its rapid progress, and its lamentable catastrophe—a catastrophe originating, as Cruveilhier has so picturesquely portrayed, in a pathological condition both of the womb in particular and of the constitution in general, precisely similar to that following upon the amputation of a limb, or the introduction of a morbific poison into the veins; deranged excretions, pus, &c., together with inflammation of the veins (phlebitis), disturbed circulation, typhus fever, delirium, purulent infiltrations, and death. In fact, the patient dies of poison. It is probable that some of the symptoms of the hectic infesting the last stages of phthisis pulmonalis may be attributable to the absorption of pus from the lungs, and that the colliquative diarrhœa which precedes the end may arise from the purulent matter continually welling up from the ulcerated lung, and as frequently swallowed as spat out by the patient; for the pus thus swallowed carries with it the germs of struma, and infects the intestinal glands with it; besides which, diarrhœa is, as is generally known, one of the modes by which pus is evacuated from the system.

I have been led to introduce these remarks in order to show that it is not a private opinion of my own, but that it is the conviction of some of the most approved pathologists in Europe, that the mass of circulating blood may be poisoned, and that the poison thus circulating is eliminated from the blood through the channel of one or other of the three great emunctories of the frame.

[i.] Thus, it will be found that, after the measles have passed by, and the eruption has effloresced and disappeared in the usual way, there supervenes a stage characterised by gastrohepatic symptoms, eruptions at the angles of the mouth, or sore eyes, (lipitudo, or chronic conjunctivitis of a scrofulous aspect). The liver is either directly or indirectly brought to a halt; there is black or pale, and eventually exceedingly yellow, bile. The countenance becomes flushed and excited, the sleep disturbed, the appetite diminished or lost, and the pulse and the heart's action accelerated. It may be there is bilious vomiting or diarrhœa, in which what is popularly called "*an overflow of bile*," shows itself. On the loss of appetite quickly follows failure of the strength, and with the hurry of the pulse is associated fever. Now, if the poison of measles be not (as I apprehend it ought to be) eliminated through the liver, or if the liver remain torpid and inactive even under the exhibition of its ordinary medical stimulants or reagents, it will follow that the fever runs into the type of what is usually known as infantile remittent fever, that is to say, a fever with irregular intermissions, chiefly affecting the mucohepatic surfaces, and continuing from one to six or eight weeks. During his anxious interval, there will be observed obstinate derangement of the entire alimentary canal, a very white tongue, costive or relaxed bowels, thirst, absolute loss of appetite, debility, delirium, emaciation, and death; or, on the contrary, instead of death, recovery from the lowest and most alarming stage of attenuation and decline. The most favorable crisis is vomiting—the vomiting of yellow or green bilious matters,—or diarrhœa, sudden and unexpected, by which the alimentary canal is discharged of its morbid contents, or the liver is emulged of its overloaded or poisonous congestion. But if this diarrhœa or vomiting do not take place, the fever proceeds, and may, after having lasted a certain number of days or weeks, subside by leaving the little sufferer as thin as a ghost and as pale as the waning moon. When the mother holds her darling in her lap, scarcely discernible in point of color from the sheet that envelopes its wasted limbs, she may begin to hope that the sharpness of death has passed, and that her offspring will be restored to life as well as to its wonted hue and noisy animation and strength.

[ii.] But the reverse of all this is the case in the sequels of scarlet-fever. Of the many cases that I have been unhappy enough to witness, I never saw one in which, when the kidneys had been decidedly affected, a perfect restoration to the former state of health was ever accomplished. They have all declined. The passing hopes of to-day have been blighted by the positive relapse of to-morrow; and though, in some instances, several years have ensued, yet the event has been always the same, and the starting-point of the illness has never failed to terminate in protracted hopes, disappointment, and death.

When we consider that the kidneys,—the great emunctories of those deleterious elements which, if retained within the round of the circulating blood, act as a specific poison on the brain,—are the organs primarily and chiefly affected, there can be no hesitation in declaring, that, if their natural or normal tone be not restored at once, death must, sooner or later, be the inevitable result. This is the simple fact. The inflammation of scarlet fever is, by one of the hitherto unexplained operations in disease, translated from the skin to the kidneys, which, when thus attacked, are no longer able to excrete the nitrogenous compounds from the blood, and, consequently, become the immediate instrument for interrupting and putting a stop to the functions of the arterial and cerebro-spinal systems together; for, if these nitrogenous or azotised materials be not excerned, but are retained and sent back on the brain, they operate as a direct poison, depress the vital forces, and eventually disable and annihilate the whole process of life. The truth is, when, upon discovering that after scarlet fever the urine contains no lithic acid, nor any of the lithates (the *lateritious deposit* of health), but that, on the contrary, it betrays the presence of the phosphates, or albumen, or blood (hæmaturia), we may be sure that the kidneys are in such a case already the seat of the diseased poison, virus, or action, that there is danger either immediate or remote, and that the final debility or decease of the patient may be predicted with the greatest certainty. I have not yet seen a confirmed case of this description ever recovered from. There is something so extremely delicate in the renal tissue, and, at the same time, something so essential to life in its perfect capacity, that any intermediate stage between its primitive perfection and its ultimate disorganisation is, in its various proportions, phases, and degrees, equally detrimental or destructive to the animal economy.

The result of renal disorder in consequence of scarlet fever, is profound debility,—a debility which, as it generally happens at an early or a premature age, it is very mournful to witness. At its commencement there is doubtless a stage of inflammation which can be met and successfully treated by the ordinary antiphlogistic remedies; only this stage is very short; and, if it be not foreseen in its onset, if its transit be not perceived, or if its existence have not been recognised,—then its after treatment is nugatory, at least only palliative. It is during the inflammatory stage that the brain is troubled with meningitis—a kind of inflammation by no means uncommon in connection with primary or secondary disorders of the kidney. At the first, while the urine is high-colored, scanty, and of a high specific gravity, the proper treatment will consist in calomel, salines, antimony, venesection, cupping, or leeches; but afterwards, when the acute stage, short as it is, has passed away, nothing will be beneficial, except the warmer purgatives, good food, the mineral acids, and removal to a more invigorating air. Recovery will, in a certain degree, reward the adoption of these measures, but perfect recovery never. The die has been cast: the lot of life is determined, and nothing remains but a broken constitution, scarcely worth the having.

The end of these cases is permanent debility. I have seen it at all ages—at 12, at 15, and at 45. One of the saddest results is, that, as the brain declines in power, stimulants are proportionally more and more requisite; for the sinuses of the encephalon are, from their anatomical construction, incapable of contraction, and, consequently, cannot accommodate themselves to the diminished stream of blood. Now, as the heart declines in power, the jet of arterial blood thrown up into the brain is depressed,—the same stream does not continue to reach the great nervous centres,—energy declines,—and, with the loss of cerebral energy, declines *pari passu* the tonic functions of the spinal cord and the kidneys. Thus, power is lost, and with the loss of power in general is also lost the particular energy to excern the lithates—those poisonous elements of the circulating mass of blood: in short, all is lost, except under the pressure of ardent spirits and the highest therapeutic tonics, which can, however, be sustained only for a limited length of time. The glands of the neck inflame, indurate, and run into suppuration: the pus is discharged, ulceration ensues, and sometimes lays bare an extensive surface: the limbs become œdematous,—the cellular or areolar tissue mortifies over those points of bone unavoidably pressed upon by the bedding,—the entire skin becomes white, dry, and furfuraceous, and anasarca is formed. The progress is soon told. The phosphatic diathesis is established. In protracted cases, renal calculi are formed and passed, life is prolonged, but the patient is cut off from the fellowship of life, and death calls him to his account by means of dropsy, paralysis, or coma.—*Ib.*

Brighton, August 6, 1847.

AMERICAN MEDICAL INTELLIGENCE.

1.—*Removal of Parotid.*

Dr. Pancoast, of Philadelphia, has recently extirpated the parotid from a woman 60 years of age. Ten years since, the disease commenced apparently as ordinary parotitis. Within the past year it increased rapidly, with distressing shooting pains about the face and forehead.

The tumour was on the right side of the face, nodulated and irregular in its external aspect, and appearing about half the size of a man's fist. It extended from a little above the zygoma, to a short space below the angle of the jaw—passing forward over the greater part of the masseter muscle, and backward under the ear, so as to elevate and press posteriorly the anterior border of the ear; it likewise nearly surrounded the auditory meatus, and also overlapped the insertion of the sterno-cleidomastoid. When grasped firmly, it was found but slightly moveable, deeply fixed and firm in its texture, except at its upper part, where there seemed a local point of softening.

None of the surrounding lymphatic glands seemed at all involved. The complexion of the patient was somewhat straw-colored, though she appeared vigorous for her age.

Operation.—The patient was placed on her left side, with the head and shoulders elevated, and her head well turned towards the left shoulder. The tumour was exposed by a single incision, shaped somewhat like the Italic *f* reversed: it was commenced above the top of the ear, and carried forward and downward to near the centre of the tumour, then in a direction sloping slightly backward to just below the lobe of the ear, when it was again directed forward, downward and nearly vertically, leaving a concavity in front, and terminating about an inch and a half below the base of the jaw, and somewhat within the inner edge of the sterno-mastoid. The dissection was then commenced by reverting the flaps so as to expose the tumour, and continued by separating the diseased mass first above, then posteriorly, next anteriorly, and lastly below.

The external carotid artery was now sought, with a view of placing a ligature upon it near its entrance into the tumour; this required a slight increase in length of the first incision, as from the size and attachments of the tumour, it was somewhat difficult to reach the vessel. It was isolated, however, with its vena comes, and the two were raised on the director, and a Physick's aneurismal needle armed with a ligature passed under them, along the groove in the director, and both secured in the loop. From this moment to near the conclusion of the operation, there was very trifling hæmorrhage. The vessels were now cut beyond the ligature, and while strong traction was made upon the tumour, Dr. P. detached it from its connexions to a still greater distance below. The patient complained much of the pain caused by the upward traction. The tumour was next loosened to a greater extent above, as well as posteriorly and anteriorly.

The central part of the tumour, deeply seated, was the last part detached; and a strong jet of blood, by retrogression from the internal maxillary artery as the final cuts were made, required that a ligature should be applied to the divided vessel. This ligature, with two on smaller bleeding vessels, and the one on the carotid artery, were all that were left at the conclusion of the operation.

A small piece of diseased structure being discovered after the thorough cleansing of the wound, near the bottom of the cavity, it was removed by the handle and blade of the scalpel. As far as was possible, the handle of the scalpel was used during the operation, but for the most part the attachments were so firm as to require the cutting edge.—*Med. Exam. for July*,—*From Wood's Retrospect.*

2.—*Vesico Vaginal-Fistula.*

Two successful operations for this distressing accident, are detailed by Dr. Pancoast; in one of which there was a complete destruction of a cross-section of the whole urethral structure, at its junction with the neck of the bladder; in the other there was an elongated orifice in the basfond of the bladder which would more than admit the end of the finger. "The principle of the operation performed in these cases, is to bring into opposition four raw surfaces, and it is necessary that the margins of the fistula should have considerable thickness;" and when not found in this state they are to be thickened, by repeated applications of lunar caustic, or, better still, of the actual cautery. Having exposed the fistula by means of a speculum, Dr. P. first splits the posterior margin of the fistula to the depth of half an inch, and then pares the edges of the other lip of the fistula into a wedge shape. The mucous membrane, both on the side of the bladder and of the vagina, is now dissected off, and "this is a very difficult but most important part of the process." Having checked the hæmorrhage, the wedge-like portion is inserted into the groove formed in the opposite edge of the fistula, "on the principle of the tongue and groove," and he then secures the parts in opposition, by means of his "plastic suture," allowing the stitches to remain "until they become loose." An elastic catheter is kept in the bladder, to prevent the accumulation of urine, and to prevent too intense inflammation a bladder of cold water applied for 36 hours to the vulva. The after-treatment consists in the application on the third or fourth day of a Sol. Nit. Arg. to the line of union, gradually increasing the strength.—*Ibid.*

3.—*Adulteration of Medicines.*

The following communications from the New York College of Pharmacy, which we find in the New York Journal of Medicine, together with the remarks by the editor of that Journal, are deserving of serious attention. That frauds are practiced in the preparation of medicines to an enormous extent, is not new to us. We have already commented upon the subject. The immediate consequences, as respect the treatment of disease, are of vast moment; and the remote evils in vitiating medical experience are of no inconsiderable importance. It is needless to amplify upon this view of the subject, for every medical man must at once perceive and appreciate its importance. We cannot see any other method by which the public and science are to be protected against these frauds, except by providing, under legal penalties, for the inspection of drugs by competent analysts. We have in previous remarks suggested this method. Our limits will not permit us to pursue the topic at this time.

"*Caution to Druggists.*—The Committee of Inspection of the College of Pharmacy, are instructed by the Board of Trustees to call the attention to Druggists to another dangerous fraud. A quantity of a base composition, under the name of Blue Pill is now in market, having been lately imported by, or consigned to and sold by Messrs. Cumming, Dodge & Co., of this city. It contains but little more than one-fifth the proper proportion of mercury, according to the examination of Professor Reid, of this college, made at our request, that we might have the corroborating testimony of the best analyst in the city. His certificate of its composition, which we append, shows an extent of methodical depravity in the manufacture, against which honest dealers will have to oppose extreme vigilance in the inspection of what they buy.

The article under notice is put up in rather large, white, flat-covered jars, containing one pound each; the joint covered with coarse pink-colored muslin; white label with nothing upon it but the British arms and the words "Blue Pill," in rather heavy letters in blue ink. The mass has tin foil laid over it, under the earthen cover.

From what we learn of its history, this spurious compound was made by William Bailey, of Wolverhampton, whose manufacture of similar blue pill, two years ago, was so faithfully exposed by the late Mr. Adamson. A transcript of the correspondence on that occasion may be found in the American Journal of Pharmacy, Vol. XI, (New Series,) p. 148. Mr. Adamson's letter still remains unanswered.

GEORGE D. COGGERSHALL,
JNO. H. CURRIE,
WM. HEGEMAN,
Committee of Inspection.

New York, August 9th, 1847.

New York Hospital. August 6th, 1847.

Dear Sir.—According to the request of the Inspection Committee of the College of Pharmacy, I have made an extended investigation into the composition of the Blue Pill furnished me, and have to report the following concerning this dangerous and heartless fraud.

Its composition by analysis is:

Mercury,	7·5
Earthy Clay,	27·0
Prussian Blue, used in coloring,	1·5
Sand in combination with the clay,	2·0
Soluble saccharine matters,	34·0
Insoluble organic matters,	12·0
Water,	16·0
	100

I could not see any thing differing in the state of combination of the mercury, from that generally found in blue pill.

The density of the pill is about the same as the genuine. This is accounted for by the large quantity of earthy matter, which, in combination with the water, gives the requisite specific gravity, and makes the deception most plausible.

The presence of so much earthy matter furnishes us with an easy means of trying it. Place 100 grains on a clean iron plate or shovel, and place the shovel over the fire until the pill is reduced to an ash. The genuine gives 2 per cent., or near it; this 29 per cent.

The per centage of mercury can be ascertained by a process proposed by me, and described in the Medical Journal of Pharmacy for 1844.

Your's respectfully,
(Signed) LAWRENCE REID.

Mr. Geo. D. Coggeshall, Chairman of the Committee of Inspection of the College of Pharmacy.

Remarks.—This is but a sample of the numerous impositions practised upon American physicians in the manufacture and sale of drugs. We have again and again adverted to the frauds constantly carried on in the manufacture of spurious medicines, and have invited druggists and others conversant with these impositions to expose them through the medium of our pages. We have received in reply two or three letters, which have been published in former numbers of this Journal. We solicit still further communications on this subject.

It may not be generally understood, that the importation of drugs and medicines into this country, is chiefly in the hands of commission merchants, mostly foreigners, (German and French,) who are not druggists by profession, and who know nothing of medicines, except to buy cheaply, and sell dearly. These men supply our wholesale dealers, who, for the most part, have nothing to do with the importation of the articles in which they deal, and who are not unfrequently imposed upon, as in the case of blue pill, as above stated. The commission dealers have agents, travelling and resident, abroad, who buy up the refuse drugs in all the principal European cities, and send them to this country, where they generally meet with a ready sale. We may mention, for example, *Rhubarb*, of which, we are credibly informed, there have been but two invoices of a good article (*Turkey*) brought into this market since December. Immense quantities, however, have been imported, of a worthless, worm-eaten article, called *Turkey*, invoiced from *two pence to eight pence sterling*, from *four to sixteen cents per pound*, which, we have reason to know, has been ground and sold to our retail druggists for genuine Turkey Rhubarb, worth four or five shillings a pound. The *Compound Extract of Colocynth*, which has been imported into this market for the last year, does not contain a particle of Colocynth, but is made up of an inferior sort of Aloes, with some other worthless ingredients. A great proportion of the compound extracts are adulterated in like manner. More than half of the narcotic and other extracts, as of *Belladonna*, *Conium*, *Hyoscyamus*, *Aconite*, *Rhatany*, etc., are entirely destitute of any active properties, as we know from our experience, and *Opium* is now rarely to be met with in a genuine form. The *Attar of Roses* is more frequently than otherwise adulterated with the oil of Rhodium, of which there is also an artificial compound prepared for this very purpose. Our *Volatile Oils* are adulterated more than half with sweet and other cheap oils. The *Hydrargyrum Ammoniatum*, U. S. P. *White Precipitate*, of *Bailey's* manufacture, (Wolverhampton,) is now as much adulterated as the sample of *Blue Pill* from the same house, analyzed by Professor Reid. This is an article of a chemical nature, which should, if prepared according to the Pharmacopœia, always be of the same quality; and yet we have its invoice price ranging from three to six shillings sterling per pound, *according to quality*. We have not ascertained whether it is mixed with *clay*, like the blue pill, *white lead*, *chalk*, or *gypsum*, but we have no doubt that one of these will be found to constitute more than 50 per cent. of it, whenever an analysis may be made. An article is now imported, under the name of *Colocynth Powder*, which is probably *Colocynthin*, mixed with some inert vegetable powder; this varies in our Custom-House invoices, from 5 to 14 shillings sterling per lb., and is often two-thirds adulterated, The *Extract of Rhubarb*, from 4 to 9 shillings sterling per pound, according to quality. The *Extract of Sarsaparilla*, as now imported, is a worthless imposition. *Quinine* is now imported in bulk instead of bottles. These latter are now generally manufactured here, together with the *labels*, according to the latest French patterns, usually the *Pelletier* stamp, we believe is preferred. The *Quinine* now generally in use all over this country, is atleast one-half *Salicine;* this latter being imported very extensively for this purpose, at an expense of less than one-third that of quinine. Some dealers, however, use flour or starch for the same purpose. We believe that it is owing to the adulteration of this article that such *large doses* are required, and *safely borne*, in the malarious diseases of the South and West. We have known practitioners in these regions occasionally get hold of a genuine article, and they very soon found that their patients, so far from requiring a *drachm*, or even half that quantity, found from five to fifteen grains sufficient. The house of *Teschdorf, Fischer & Co.*, of Hamburgh, sends us immense quantities of drugs of every description, especially of *Extracts*, as of "*Carduuss Benedictus*," "*Chelidonium*," "*Fumaria*," "*Gratiolus*," "*Lactuca Virosa*," "*Millefolia*," and "Graminis!" Where are these articles used? What are the medicinal properties of the *Extract of Grass?* The only use for the latter, we have very good reason to believe, is to mix with genuine extracts, for the purpose of dilution. The invoice price of these extracts varies from forty cents to $1 75 per pound.

Much of the *Nitrate of Silver*, so called, now on sale in our wholesale drug establishments, does not contain a particle of the metal; whether the substitution is prepared here or abroad, we do not know. Of the *Hydriodate of Potash* also a large proportion is utterly worthless, *Iodine* not entering into its composition; the article is extensively imported in this shape. In order to have an article on which they can depend, we would recommend physicians everywhere, to prepare their own Hyd. of Potash, which can be readily done as follows:—Heat slightly a mixture of 100 grains of Iodine, 2 drachms of water, 75 grains of carbonate of potash, with 30 grains of iron filings. The mass is dried to redness. The resulting red power is heated with water, then filter and evaporate to dryness; 109 parts of Iodine will thus furnish 135 parts of very white iodide of potassium, but slightly alkaline.

Thus we could go through with the whole catalogue of medicines in daily use by the physicians. It is now well known that there are establishments abroad for the express purpose of manufacturing spurious drugs for the American market, and it is high time that something was done to put a stop to it. As one important step towards reform, we hope that our wholesale dealers will hereafter import their own medicines, and not trust to a set of sharpers, who think more of money than they do of life and health. There is no propriety in leaving this branch of business in the hands of men who are not competent judges of the genuineness of the articles in which they deal. In the next place, we hope Congress will, at their next session, pass a law, forfeiting all spurious and adulterated drugs, and subjecting the owner or consignee to heavy penalties. We have appraisers now connected with the Custom-House, who are regularly educated physicians and chemists, and who are fully competent to detect these impositions whenever they may be practiced. At present, although the government is fully aware of these extensive frauds, it has no power whatever to prevent them; its *ad valorem* estimation may be *nothing*, or next to nothing, as in the case of the rhubarb, appraised *in the invoice* at two pence sterling per pound; but it has no right to exclude the article from our markets.—We need a stringent law, to prevent such practices in future. Again, physicians must purchase their medicines in the crude state, and not in powder; if they do, they will be imposed upon, nine times out of ten. They must make their own extracts, syrups, pills, and tinctures. They must resort more frequently to the use of our indigenous medicines, and never employ a foreign article where a domestic one will answer the purpose. When they do purchase, they should buy only of those wholesale dealers who import their own stock; and not take their articles from those who are unacquainted with the characters of genuine drugs. And lastly, they should deal only with those who sustain the reputation of being *honest men*, and whose consciences would not allow them to go on quietly in the daily practice of imposture and deception, involving the lives and health of their fellow-men. We hope the "New-York

College of Pharmacy" will pursue this subject, and expose a few more of the frauds now practised in the manufacture and sale of medicines. And although we are not personally acquainted with the Hon. Secretary of the Treasury, R. J. Walker, Esq., we have reason to believe that he will cheerfully co-operate in bringing about a reform in this matter, and thus put a check to the importation of spurious and adulterated articles, which not only detract largely from the public revenue, but prove destructive to the lives and health of our citizens, and often fatal to the reputation of the regular practitioner of medicine.

Buffalo Med. Journal.

4.—*Osteo-Sarcoma of the Lower Jaw. Removal of the body of the bone without external mutilation.* By J. Marion Sims, M. D., of Montgomery, Alabama.

Jack, a negro man, aged 68, the property of John M. Sanders, Esq., of Macon Co., Ala., was the subject of this operation. In 1843 he discovered a small tumour at the symphysis, just at the junction of the lip and gum. It was painless; grew very slowly; almost imperceptibly, till it gradually embraced nearly the entire body of the bone; the third molar tooth bounding its limits on the right, while it extended quite to the angle on the left side. As in all such cases, the teeth were somewhat loose and greatly displaced. The left side was more prominent than the right. The tumour was very hard at some points; quite soft and elastic at others. Just at the root of the central incisors was a small opening, constantly distilling a sero gelatinuous looking fluid. At a point near the last molar, on the left, was another opening, giving vent to pure pus, which had been discharging for a month or more. His breath had the peculiar disagreeable smell always attending extensive ulcerations of the mouth. His general health was good. At night he would get sick, and vomit in consequence of swallowing the secretions from the diseased bone: not so during the day, as he would then spit them out.

The mouth of the patient was of enormous size, to which alone I was indebted for the happy thought of removing the bone in the manner in which it was done. The very moment that I first saw him, it occurred to me to take the bone out through the mouth without any external incision.

The operation was performed on Tuesday, 5th of January, 1847, with the assistance of Drs. Boling, M'Lester, Jones, Spear, and Taylor.

The first step of the operation was to separate the diseased from the healthy bone, which was effected by the chain-saw, thus:—with a long narrow sharp-pointed bistoury, I made a puncture at the base of the jaw immediately under the place of the second molar, on the right side, running the knife flatwise into the cavity of the mouth, between the cheek and the bone, and as close to the latter as possible. One end of the chain-saw was now passed along the blade of the knife, or rather an eyed tube, armed with a ligature previously attached to the saw, and the knife withdrawn. It was again introduced through the same external opening, but was now carried into the cavity of the mouth by thrusting it up on the inside of the bone, between it and the tongue. Through this new channel the other end of the chain-saw was passed, and both ends drawn, *pari passu*, till the middle of the saw rested against the base of the bone, while the ends hung out at the left corner of the mouth. Thus I had the saw fitting snugly around the bone at the expense of a puncture not more than a quarter of an inch long.

The jaws were now opened to the fullest extent, and the left angle of the mouth widely retracted, while the saw was brought across the dorsum of the tongue, and the bone quickly severed by sawing from without inwards, and slightly upwards. As soon as the bone was divided, the saw was liberated by clipping the super-imposed gum with scissors. In like manner the bone was divided on the left side; but, as it had to be cut right at the angle, to get clear of diseased structure, the saw had to be passed from within outwards and downwards, its two ends hanging out at the external opening just under the angle, and thus the bone was easily divided from above, downwards and backwards. The next step in the operation was to dissect the lip, chin and cheeks from this loose diseased mass, turning the lip downward under the base of the bone. This required some little caution in putting the parts sufficiently on the stretch, and cutting close enough to the bone to avoid the facial arteries. After liberating the right side of the bone, getting it on the outside of the angle of the mouth, so that I could make a lever of it, the ease with which the operation was finished was a matter of great surprise to me. By rotating it inwards on the axis of the left side, it was easy to separate the attachments from the outer surface, and by reversing this movement it facilitated greatly the dissection from the muscles of the tongue and throat, while *traction* brought it so far out, that its separation from the soft parts at the angle gave no trouble at all. Just as the genio-hyo-glossi muscles were severed from their origin, the old man's head flew back, and he made a distressing sort of suffocative sob, when it was immediately discovered that the assistant was holding the ligature loosely, which had been previously passed through the frænum linguæ for the express purpose of preventing a retraction of the tongue. By seizing the ligature quickly, and putting it on the stretch, so as to draw the tongue forward, the cause of this sudden alarm was instantly arrested. The only artery requiring a ligature was the left facial, which was cut by a careless slip of the knife after it had been cautiously separated from its bed in contact with the bone.

The operation being completed, the cavity lately occupied by the diseased bone was filled with pledgets of lint, wet with creosote water, for the purpose of arresting the slight hemorrhage. The only dressing used was a bit of pasteboard, soaked in warm water, and moulded on to the chin of a healthy man, so as to form a perfect encasement of the whole inferior maxillary region, which being applied, was sustained *in sitû* by a proper bandage.

The day after the operation the old man was chewing tobacco. In two weeks he was out chopping wood; and in a month he went home perfectly well. His pulse was regularly 72 all the time, both before and after the operation.

There are several considerations to recommend this operation in preference to the usual one with its extensive incisions.

1st. There was no external mutilation; the slight punctures, through which the chain-saw was passed, healing by the first intention without the least mark or scar.

2d. As the third branch of the fifth pair of nerves was divided at the outset of the operation, its subsequent stages were comparatively free from pain; the stretching of the angles of the mouth being the most that was complained of.

3d. As no important blood-vessels are cut, no ligatures are required. (In this case it was my fault that the left facial artery was wounded.)

4th. There was no trouble with the after-treatment.

5th. It is just as easy of performance as the old operation.

This method of operating will be available under all circumstances, but is only proposed for the removal of the body of the bone, or any portion of it anterior to the angles. If the mouth should be small, and the diseased mass too large to be brought through entire, it will be very easy to divide it near the symphysis, or elsewhere, as the judgment of the surgeon may determine, and remove a piece at a time. Of course, where the jaw has to be disarticulated, the *curvilinear incision*, first suggested and executed by our distinguished countryman, Dr. Mott, will always be indispensable.—*American Journal.*

5.—*On the consequences of rising too soon from bed after Confinement, with two cases in illustration.* By Wm. M. McPheeters, M. D.

We have ever found it difficult to impress upon females the importance and absolute necessity of remaining for a sufficieut length of time after confinement in a horizontal position, and keeping perfectly quiet. Imprudence in getting up too soon, often entails upon the unfortunate patient, months, and even years of suffering, which might have been avoided had she listened to the advice of her physician, or to the suggestions of common sense. We are satisfied that the practice of getting out of bed too soon after confinement, is very general in our own community, and hence it is that such a very large proportion of our female population suffer with prolapsus, and procidentia uteri, as well as from other uterine affections, which subject them to the necessity of wearing pessaries, or to the use of those fashionable, but, in our opinion, very objectionable instruments, utero abdominal supporters. Sometimes this imprudence is attributable to the want of proper precaution on the part of medical advisers, but more frequently it is owing to the folly of patients themselves. On the third or fourth day after parturition, a patient who is "very smart," feels able to sit up in bed, or in an easy chair, and in spite of all that the physician can say, she will, in his absence, sit up for the purpose of changing her clothes, or get out of bed altogether, that it may be made up, and not unfrequently walk across the floor, by way of testing her strength. A moment's reflection must convince any one of the impropriety of such conduct. The enlarged and engorged condition of the womb, the great relaxation of the abdominal muscles, of the vagina, and of the broad and round ligaments, all tend, under the circumstances, when the body is brought into an erect posture, to force the uterus down into the vagina, and frequently through the vulva. Again, on the third or fourth day after delivery, it is the practice of most physicians to administer a dose of castor oil, or some other mild cathartic, for the purpose of securing an operation from the bowels, which are usually torpid up to this time. Under these circumstances, patients, especially those who "feel smart," instead of using a bed-pan, and evacuating their bowels in a horizontal position, will get up out of bed, and use the close stool, and thus bring about the evils of which we have just been speaking.

These remarks are called forth by two cases which recently occurred in our own practice, where our patients were guilty of the imprudencies here spoken of. In one of the cases, in which we attended in consultation with a medical friend, the labor was prolonged and difficult, and it was necessary to remove the child by means of instruments. It was a first confinement, and the woman had been some fifty hours in labor before we saw her. Her strength was well nigh exhausted, and it was with difficulty that she could be sustained during the operation, which, however, terminated favorably, and the patient was put to bed, with strict injunctions to keep perfectly quiet. On visiting her on the third morning after, we found that her bowels had been very much out of order during the night, that she had been up several times on the close stool, and had suffered very much with straining efforts. During our visit she complained greatly of pain, and uneasiness in the region of the vulva, and on examination we found the uterus highly engorged with blood, and of the size of one's fist, protruding entirely through the labia majora. The second case was that of a young athletic woman, also in labor with her first child, but who got along well. On the third day, the bowels not having been moved, a dose of castor oil was administered, and the patient, contrary to our express directions, got out of bed when it came to operate. The consequences were similar in kind, though not in degree to the case just mentioned.

In very many instances like imprudencies are not followed so immediately by bad consequences, but it is invariably the case that those who are guilty of such folly, are made to suffer for it, sooner or later. Often, too, the bandage, instead of being pinned tightly around the hips, for the purpose of supporting them, and being made to press from below, upwards, slips up, gets in a string, and acts as a ligature around the abdomen, pressing downwards, and consequently doing positive harm instead of good. Too much attention cannot be paid to the subject of bandaging. In the first instance it should be applied and properly adjusted by the physician himself, and he should instruct the nurse how to tighten and keep it in its proper position, and it should be worn long after the woman gets out of bed, and until the parts have resumed their natural tone and strength. The material, too, out of which the bandage is made, is worthy of consideration. Of all the articles in common use, we prefer the flannel, doubled, and of sufficient width to extend from the middle of the hips to the umbilicus; it possesses the advantages of being soft and somewhat elastic, and can be brought to fit the irregular surface around which it is intended to pass.

A patient, after giving birth to a child, however easy or natural a labor she may have had, should remain perfectly quiet on her back for at least two or three weeks, at the end of which time, provided everything goes on regularly, she may be allowed to sit up cautiously in bed, and gradually remain for a short time out of bed, in a sitting posture; but as a general rule, they should not be permitted to rise from their beds under three weeks, and frequently not so soon. This rule may seem a little stringent to those who have been in the habit of getting up at farthest on the *ninth day*, and often walking all over the room long before that period; but time would be saved by observing it, and patients would save themselves an immense amount of subsequent pain and unhappiness.

We are aware that our suggestions contain nothing new or original, but the subject is one of vast importance, and which is too much neglected—it is therefore necessary to add "line upon line, and precept upon precept." The health and comfort of the female sex is so intimately identified with our own happiness, that whatever affects them materially, concerns us; and we are satisfied, that if due attention were paid to their proper "getting up" after confinement, we would not see so many young and lovely wives suffering with uterine affections—pale and anœmic, and unable to take the least exercise, or even attend to their ordinary household affairs, without the greatest pain.—*St. Louis Med. and Surg. Journal.*

6.—*Oath of Hippocrates.*

[TRANSLATED.]

(The philosophic physician is equal to the gods.)

To all Physicians of all times.—I swear by Apollo, by Æsculapius, by Hygiæ, by Panacea, by all the gods and goddesses whom I here invoke as witnesses, to fulfil according to my best capacity and discernment, the oath which I pronounce and here subscribe.

I swear to consider my master as equal to my parents; I will unite my existence to his, and if he should ever require it, I will divide my effects with him. His sons shall be my brothers, and if they should wish to learn the art of healing, I will instruct them without any immediate salary, or any engagements for the future. Maxims, detailed explanations, in fine, all of my medical doctrines shall be transmitted by me to my sons, to those of my master, to pupils engaged by writing and sworn according to medical law, but to none others.

I will prescribe to the sick a proper regimen according to my ability and discernment. I will abstain from all things unjust and injurious. I will never produce abortion. I will preserve, as a physician and a gentleman the utmost purity of and sanctity of morals. I will never perform lithotomy, but leave that operation for those who make a profession of it.

Into whatever house I shall enter, going to carry aid to the sick, I will remain there a stranger to all iniquity, to all corruption, and to all criminal acts towards man or woman, bond or free. What I see or hear in private life whilst in the exercise of, or even out of the exercise of my profession, and which shall not be of a nature to be divulged, I will keep holy and inviolate.

If I fulfil faithfully this oath, and not violate it any manner, may I obtain a celebrity as a gentleman and a physician, and be glorified by all men in all ages; but if I transgress it and perjure myself, may the contrary befal me.—*Ib.*

APPOINTMENTS IN MEDICAL COLLEGES.

Jas. B. Rodgers, M. D. Professor of Chemistry in the Franklin Medical College, Philadelphia, has been appointed to the Chair of Chemistry in the University of Pennsylvania, vice Professor R. Hare—resigned. Dr. Hare has been appointed "Emeritus Professor of Chemistry."

Dr. Cabell, has declined the appointment of Professor of Surgery in the Richmond Medical College.

Jacob Randolph, M. D., has been appointment "Professor of Clinical Surgery" in the University of Pennsylvania.

Alonzo Clark, M. D., has been appointed "Lecturer on Physiology and Pathology" in the College of Physicians and Surgeons—New York.

Samuel Henry Dickson, M. D., late of Charleston, S. C., has been appointed "Professor of the Theory and Practice of Medicine" in the University of New York—vice Professor Revere, deceased.

Charles Bell Gibson, M. D., has been appointed Professor of Surgery the Richmond Medical College—vice Professor Augustus Warner, deceased.

Since the resignation of Dr. Warren, three new professors have been attached to the Massachusetts Medical College in Boston, and one to the University at Cambridge. The new professsors are Oliver W. Holmes, M. D., Professor of Anatomy and Physiology; John B. S. Jackson, M. D., Professor of Pathological Anatomy and Curator; and Jeffries Wyman, M. D., Hersey Professor of Anatomy at Cambridge.

In the Medical College of the State of South Carolina, Dr. Geddings, has been transferred to the Chair of Practice of Medicine, and Dr. Bellinger, appointed Professor of Surgery.

UNIVERSITY OF LOUISIANA.

The Medical Department of this promising institution is rapidly advancing to completion. Its dimensions are ample, the arrangements of the rooms admirable, and when completed, which will be in time for the lectures this winter, it will equal, if it does not surpass any similar institution in the country. It is astonishing to see with what rapidity this splendid structure has been raised; and much praise is therefore due the Professors for their enterprize in the matter. Students who desire to attend lectures the coming winter in this city, will find ample accommodations, and every facility offered them for acquiring a thorough and *practical* knowledge of their profession.

A. H.

NEW ORLEANS, NOVEMBER 1, 1847.

HEALTH OF THE CITY.

We shall continue hereafter, as heretofore, to make a few passing remarks upon the sanitary condition of our city. In our September number, it was stated that the Board of Health had declared the yellow fever to be epidemic. Events which have transpired since that announcement, have fully verified, we regret to say, the assertion of the Board.

The fever made its appearance about the 1st of July, and began to decline, in accordance with the laws of epidemics, about the latter part of September; and by the 1st of October, the deaths daily were about ten. Thus the epidemic, as such raged about six or seven weeks. It attacked many who had passed unaffected through the season of 1839 and '41; some who had been permanent residents for several years, fell victims to the disease. In some of its features, it differed from former epidemics.

We had fewer cases of black vomit; and many who were attacked with this usually fatal symptom, recovered. In many cases, the fever terminated in 24 hours; in others it raged for 56 or 72 hours. Nor was the issue of the case materially influenced by the duration of the fever. Since, many in whom the fever continued for three days, recovered as promptly as those in whom it ceased at the end of 24 hours. Throughout the disease, the head-symptoms were striking and obstinate; and many, very many, succumbed with all the symptoms of congestion of brain.

As it is not our object to go into a description of the disease, we shall confine ourselves to the statistics on the subject. After much labor and great care, we have compiled from the published reports of the Board of Health the following statement, which will speak for itself.

Interments in the city of New Orleans, from the 3rd of July, to the 18th October, 1847, inclusive.

For the week ending	10th July.	Total	138,	5	of yellow fever.
" " " "	17th "	"	143,	6	" "
" " " "	24th "	"	131,	16	" "
" " " "	31st "	"	177,	47	" "
" " " "	8th Aug.	"	263,	118	" "
" " " "	15th "	"	353,	197	" "
" " " "	22d "	"	432,	322	" "
" " " "	29th "	"	461,	328	" "
" " " "	5th Sept.	"	540,	435	" "
" " " "	12th "	"	491,	355	" "
" " " "	19th "	"	257,	169	" "
" " " "	26th "	"	181,	85	" "
" " " "	3rd Oct.	"	149,	61	" "
" " " "	10th "	"	126,	44	" "
From the 10th to the	18th "	"	148,	53	" "
Total.			3990	2241	of yellow fever

Interments in the city of Lafayette, from 26th of July, to 21st September, 1847, inclusive. Total, 793, of which 498 were of yellow fever. Thus making the total of deaths from all diseases during the time specified,—in both cities,—4,783, of which 2,739 were from yellow fever.

The above table will convey quite a correct idea of the state of health of our population from the 3d of July to the 18th of October, 1847.

From the foregoing table it will appear that the epidemic reached its acme about the 1st of September, and after that date it gradually declined. During the prevalence of the fever, it has been computed that between twenty and twenty-five thousand persons were attacked with the disease; this, however, is more a matter of conjecture than accurate calculation.

On the 18th of October, the Board of Health published the following statement:

Meeting of the Board of Health, October 18th, 1847.

The Board of Health feels authorized to make the announcement that the yellow fever, which has been prevailing for some months as an *epidemic*, has, for some time, ceased to exhibit this character, and as such has now disappeared. At the same time it is proper to state, that the *sporadic* cases, which have always been seen for one or two months after the disappearance of epidemic yellow fever, must still be expected to prevail.

Signed, W. Stone,
Chairman.

W. T. Brent,
Secretary pro tem.

As the yellow fever declines, our ordinary autumnal diseases begin to make their appearance. We have now under treatment two cases of typhoid fever in private practice; they are very obstinate, one 17 days standing, and not yet convalescent. Since the commencement of the fall season, the sky has been clear, the air cool and bracing, and but little rain to interrupt out-door business.

A. H.

HEALTH OF THE COUNTRY.

Jeanerette's, Parish St. Mary, La., October 15th, 1847.

Editors of N. O. Medical and Surgical Journal.

Gents:—This part of the parish is now healthy—some few cases of intermittents are occurring, but since the 1st of October, little sickness has existed. Through the prairies during the months of July, August, and September, much sickness occurred, mostly mild intermittents, and easily cured—a few doses of Quinine promptly arresting the paroxysms. The employment of Quinine has of late, much increased in the domestic practice of the inhabitants of the county, and fortunately has taken the place of excessive, and of repeated doses of cathartics and emetics.—Familiarity with the virtues of Quinine has sensibly diminished the practice of Physicians—fevers are arrested "in limine" and consequently our services are less frequently required, and we seldom meet with those aggravated cases of pseudo-typhoid and congestive fever, formerly so prevalent in this region. During the season I have met with but 3 or 4 cases of congestive fever, and those may have been attributed to injudicious treatment in the early stage.

Considerable sickness, I have heard, has existed recently in the upper part of the Parish of St. Martin, and some deaths have occurred.

Our Summer months were excessively rainy, the fall thus far has been mostly dry and pleasant. The crops are good, and promise an abundant yield.

The charbon in cattle and horses, prevailed in this vicinity in the early summer months, and destroyed great numbers. Some planters loosing nearly all their horses and mules. No cases occurred in the human subject. Little success attended any treatment. The most efficient remedy however seemed to be the application of the actual cautery at the onset of the disease.

Respectfully your ob'dt. servant,

J. B. D.

Woodville, (Miss.,) 14th October, 1847.

Gentlemen:—I am sorry my last report, for September, was too late for insertion. I send this, hoping it will be in time. We are all resting on our oars, nothing of any consequence occurring to interrupt our hours of reading, eating or sleeping. It is, and has been an unusually healthy season in this county. The following is the tableau of 3 physicians of this place, of the largest practice, embracing the time from the 15th August up to date.

Abortion 1; abscess, common, 4; mammary, 1; lumbar, 1; asthma, 3; bronchitis, acute, 1; do. chronic, 1; diarrhœa, acute, 12; chronic, 1; dysentery, 9; dentition 15, 1 fatal; epilepsy 1, fatal; enteritis, chronic, infantile, 6; adult, 2; fever intermit. 38; do. infant. remit. 25; do. remit. 45, 1 fatal; do. continued bilious, 6; do. catarrhal, 3; do. congestive, 9; 1 fatal; gonorrhœa. 8; hepatitis, acute, 1; do. chronic 1; hysteria, 3; jaundice, 4; labour, natural, 4; do. difficult, 1; mania-à-potu, 4, 1 fatal; menorrhagia, chronic, 1; menses, suppressed, attended with chorea sancti viti 1; morbus coxarius 1; neuralgia 3; pertussis, 4; phthisis, 2; pleuritis, acute, 2; placenta adherent 1; rheum. acute 4; syphilis, 3; ulcer, 3; vermes, 5; wound, lacerated, 3; do. gun-shot 1; fracture of clavicle, 1; do. of scapula, 1. I have omitted many minor cases of a casual nature, being of no importance.

I would beg room to mention a case of *calculus* of the bladder coming, under treatment in the past month, in a negro woman aged 32, mother of two children. She has suffered much for upwards of three years, and was thought by some to be incurable. The stone was of an oval shape attached to the fundus of the bladder by one extremity, while the other was elongated and impacted into the neck of the bladder, and through the urethra even to the external meatus, so as to be tangible and visible. The urethra was very much enlarged. Dr. R. L. B., the attending physician in presence of Dr. A. C. H., operated by Dr. Fergusson's mode, on the 29th September, dividing the urethra laterally on each side, outwards and downwards towards each ischium. The stone was seized and in tbe attempt to remove it, was crushed in the forceps. The fragments were removed and from the amount obtained, must have been as large as the largest sized hen egg. By accident the calculus was misplaced and lost, so that no analysis was made.

The incisions healed kindly, and the patient seems destined to enjoy good health and be useful again.

A. R. K.

MONTGOMERY, ALA., October 15th, 1847.

Editors of New Orleans Medical and Surgical Journal.

GENTLFMEN :—As heretofore I send you our list of cases for the two past months,—from August 9th, to October 10th, inclusive. Although, as you will perceive, the number of cases is much greater than during the previous two months, it has been considered quite healthy here.—This is owing to the fact, that the cases of fever,—by which the list is principally augmented,—have been generally, exceedingly mild, requiring but little time or medicine to effect a cure.

Abscess, 10; apoplexy, 1; abortion, 2; asthma, 1; after pains, (severe and protracted) 1; angina pectoris, 1; bronchitis, (acute) 1; convulsions, (infant.) 3; catarrh, 2; cholera-morbus, 6; cholera, (infant.) 3; cataract 1; colic, 13; contracted cicatrix, (of neck, from burn) 1; delirium tremens, 3; diarrhœa, (acute) 29; do. (chronic) 5; dyspepsia, 6; difficult dentition, 2; dysentery, 6; dysmenorrhœa, 1; enteritis, (acute) 4; do. (chronic) 1; epilepsy, 1; Eczema, 1; fistula, in ano 1; fracture, (simple of forearm) 1; do. (simple of radius) 1; do (simple, of thigh) 1; foreign body (fish bone) in rectum, 1; fever, typhoid, 2; do. (intermit. simple) 319; do. (remit. simple) 141; do. (remit. pernicious) 10; do. (remit. infant.) 11; do. yellow, (from Mobile, on boats) 2; do. (ephemeral) 1; gonorrhœa, 10; gastritis, 1; hemiplegia, 1; hysteria, 1; hemorrhoids, 1; Hyalitis, 1; hepatitis, (chronic) 1; hernia, (reduc.) 1; irritable bladder, 1; injury of head 1; jaundice, 5; incontinence of urine, 1; leucorrhœa, 3; menorrhagia, 4; meningitis 2; neuralgia, 12; necrocis, 2; purpura hæmorrhagica, 1; pterygium, 1; phymosis, 1; prolapsus uturi, 1; pneumonia 4; parotitis, 2; parturition, (natural) 1; do. (tedious) 1; phthisis pulmonal, 3; poisoning, (by sulphate of zinc, taken in mistake for sulphate of magnes.) 1; porrigo, 1; prurigo ani 1; opthalmia, (scrophalous) 1; do. (catarrhal) 3; otitis, 2; rubeola, 1; rheumatism, (acute) 1; retained placenta, 1; spinal irritation 4; spinitis, 1; syphilis, (primary) 2; do. (secondary) 2; sprain, 4; stricture of urethra, 2; tonsilitis, 1; tumor, (adipose) 1; urticaria, 1; ulcer of corea 1; do. of leg, 1; vertigo, 1; wounds (of corea) 2; do. lacerated, 1; do. punctured, 3; do. incised, 1; contused, 6; worms, 1; whitlow, 3. Making in all 710 cases. There were ten deaths—four from pernicious remittent, two from meningitis, two from acute enteritis, one from cholera infant. and one from yellow fever.

W. M. B.

PATTERSONVILLE, LA., October 19th, 1847.

Messrs. Editors :—The health of this portion of the Parish of St. Mary, since my communication in August last, has not been good. The prevailing sickness has been principally intermittent and remittent bilious fevers, generally of a mild, character, although occasionally, very aggravated.

The complications have been, abdominal, and cerebral congestions, and gastro-enteritis. But in all the cases, there was a ready yielding to judicious management. Notwithstanding the great amount of sickness, this region has never experienced less mortality. I think I have seen two well marked cases of cerebro-spinal meningitis; they both occurred on the same plantation, about four miles below this village, and within forty-eight hours of each other. They both yielded (but slowly,) to profuse blood-letting, cathartics, quinine, &c. Such cases being rare and interesting, I may at some future time, give a detailed report of the above two cases.

For the last few weeks, we have had some cool weather, particularly at nights and early in the morning, with tolerably high temperature during the middle of the days, since which the cases have declined in number, but become more violent in disposition. We are soon expecting that exemption, from sickness, which Southern Louisiana, usually enjoys after the appearance of "hoary frost."

Respectfully and sincerely,
R. H. D.

Covington, La., October 22nd, 1847.

In answer to your letter of the 18th, which I did not receive in time to acknowledge by return mail, I take the earliest opportunity of replying to.

Several families of Germans and Dutch, *who had resided in New Orleans for two or three years*, came over here the latter end of August to escape the fever in New Orleans, and took possession of some uninhabited houses. About the fifth day after their arrival one man was taken sick; from what I can learn, no physician saw him for two days;—he died the fourth day with black-vomit. In the meantime another man and his wife (also emigrants) were taken down and both died in a similar way. After a day or two, two of our citizens who had been with the sick, and attending on them, were also taken down with a similar attack; and after this the disease spread generally thro' the immediate neighborhood where it is thickly inhabited; from this it spread thro' the town, except that portion where I reside, which is separated by a small branch from the town. We have all kept well.

I may safely estimate the number of cases from 160 to 180 out of which, with other diseases, there have been eleven deaths, one of which was from consumption, and ten of fever.

I have seen eleven cases from New Orleans, who were taken sick either immediately on their arrival, or within a day or two after, some at Madisonville, some at Lewisburg, and some here; those cases have not varied in the slightest degree, from the patients I have attended here. I do not think it genuine yellow fever. The persons have generally been attacked severely; the fever has commonly been subdued in thirty-six hours, but has left the system very much exhausted. In my own practice in the place, I have lost only one patient—that from relapse. I have not bled in any instance, have only given Calomel to one person, and only cupped one—I have relied entirely on external applications, castor oil, injections and quinine.

There have been several instances of persons coming from the country to the town on business, several of whom, soon after have been attacked with fever, and generally have died mostly of black-vomit.

The fever has entirely subsided here for the last five or six days. In haste.

J. G.

REPORT

Of the Charity Hospital for the months of August and September.

1847.	ADMITTED.	DISCHARGED.	DIED.
August.	1656	980	494
September.	1105	1012	302

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1847.

By D. T. LILLIE, at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1847.	THERMOMETER. Max.	THERMOMETER. Min.	THERMOMETER. Range.	BAROMETER. Max.	BAROMETER. Min.	BAROMETER. Range.	COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — *Inches.*
August - 28	91.5	75.0	16.5	30.18	30.12	0.06	S.W.	3	3	3.900
Sept. - 4	88.5	72.0	16.5	30.15	29.94	0.21	S.W.	3	4	4.995
" - 11	89.5	68.5	21.0	30.08	30.02	0.06	N.W.	$2\frac{3}{4}$	3	1.830
" - 18	83.5	69.5	14.0	30.09	29.93	0.16	N.E.	$2\frac{1}{2}$	1	0.015
" - 25	83.0	68.5	14.5	30.11	29.94	0.17	N.E.	$2\frac{1}{2}$	2	1.380
Oct. - 2	84.5	69.0	15.5	30.13	30.03	0.10	S.E.	$2\frac{1}{2}$	2	0.770
" - 9	84.7	67.0	17.7	30.08	29.91	0.17	N.E.	$2\frac{1}{2}$	0	0.000
" - 16	79.0	57.5	22.5	30.13	29.96	0.17	N.W.	2	1	0.550
" - 23	81.5	52.0	29.5	30.13	29.96	0.17	S.E.	$2\frac{1}{2}$	1	0.850

Remarks.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

D. C. Spencer

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE

AND

THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—Galen.

NEW-ORLEANS CHARITY HOSPITAL.

JANUARY, 1848.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1848.

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New Orleans, May 1, 1847.

PUBLISHER'S NOTICE.

The present being the commencement of a New Year and a season which our country subscribers usually devote to making their collections; we take the liberty of suggesting to those of them who may have been postponing their payments, the propriety of their availing themselves of the present favorable opportunity and of remitting us the amount of their dues.

There are still a great many names on our list who are in arrears for the third volume and some even so far back as the second. To such we would particularly appeal and respectfully urge upon them the importance of our claims with the hope that they will be attended to.

We shall continue to publish as in the present number a list of payments on account of subscription, which will spare us the trouble and our subscribers the expense of a separate acknowledgment by mail.—To those who have thus liberally responded to our appeal we return our most hearty thanks as well as our best wishes for continued prosperity and success.

S. WOODALL, Publisher,
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TO READERS AND CORRESPONDENTS.

Communications have been received from Dr. Boling of Montgomery, and Dr. Lewis of Mobile. Dr. Lewis will reply to the Reviewer of his "Medical History of Alabama" in our next number.

We beg of our friends in the surrounding country to send us more original communications.

The following Books and Pamphlets have been received:

A Treatise on the Practice of Medicine. By George B. Wood, M. D. Professor of Materia Medica and Pharmacy in the University of Pennsylvania; One of the physicians to the Pennsylvania Hospital; One of the authors of the Dispensatory of the United States, &c. &c. In 2 volumes. pp. 791 and 840. Philadelphia. Grigg, Elliot & Co. 1847. (From the Publishers.)

The History, Diagnosis and Treatment of the Fevers of the United States. By Elisha Bartlett, M. D. Professor of Theory and Practice of Physic in Transylvania University; Author of an Essay on the Philosophy of Medical Science, &c. &c. Philadelphia. Lea & Blanchard. 1847. pp. 567.

Illustrations of Medical Botany: Consisting of Colored Figures of the Plants, affording the important articles of the Materia Medica and Descriptive Letterpress. By Joseph Carson, M. P. Professor of Materia Medica in the Philadelphia College of Pharmacy, &c. &c. No. 1. Vol. I. Philadelphia. Robert P. Smith. 1847.

Summary of the Transactions of the College of Physicians of Philadelphia. From June to November, 1847, *inclusive.* (From the Publishers.)

Introductory Address delivered to the Students of the Medical College of Ohio, Nov. 3, 1847. By John P. Harrison, M. D., Professor of the Theory and Practice of Medicine. Cincinnati, 1847. (From the Author.)

An Introductory Address delivered at the opening of the Session of 1847—48, *to the Students of the Memphis Medical College, Nov.* 1, 1847. By George R. Grant, M. D., Professor of the Theory and Practice of Medicine in the Memphis Medical College. (Published by the Class.)

Phrenological and Physiological Almanac for 1848. By L. N. Fowler. New-York: Fowler & Wells.

The Dental Register of the West. Published Quarterly, by order of the Mississippi Valley Association of Dental Surgeons. Editors: James Taylor, M. D., D. D. S., Cincinnati, and B. B. Brown, M. D., St. Louis.

The Medico-Chirurgical Review, London, for October, 1847. (InExchange).

The London Medical Gazette for October. (To the La. Med. College.)

We have received all our American Exchanges regularly, except the South-Western Medical Journal at Memphis.

JOSEPH COHN, PRINTER, 31 POYDRAS STREET.

REPORT OF PAYMENTS

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CONTENTS

OF

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

VOL. IV. NO. IV. — FOR JANUARY, 1848.

PART FIRST.

ORIGINAL COMMUNICATIONS.

PAGE

ART. I.—Researches on Meteorology. By BENNET DOWLER, M. D. 411

ART. II.—A Case of False Aneurism of the Prufunda Artery, with Disease of the Bone, for which a Ligature was placed on the Femoral Artery, and afterwards Amputation of the Limb. By THOMAS N. LOVE, M. D., Surgeon, 2d Regt. Mississippi Rifles. - - - - - 434

ART. III.—Lecture delivered introductory to the Course of Physiology and Pathology, in the University of Louisiana. By JOHN HARRISON, Professor of Physiology and Pathology. - - - - - - 439

ART. IV.—Address delivered before the Physico-Medical Society of New Orleans, November 27th, 1846. By R. M. GRAHAM, M. D.—Published by request. - - - - - - - - - 448

ART. V.—A Case of Ovarian enlargement successfully treated with Iodine and its preparations. By N. K. LESLIE, M. D., of Jackson, La. 457

PART SECOND.

REVIEWS AND NOTICES OF NEW WORKS.

ART. I.—The Medical History of Alabama. By P. H. LEWIS, M. D., of Mobile. - - - - - - - - - - - - 459

ART. II.—A System of Surgery. By J. M. CHELIUS, Doctor in Medicine and Surgery, Public Professor of General and Opthalmic Surgery, Director of the Chirurgical and Opthalmic Clinic in the University of Heidelberg, &c. &c. &c. Translated from the German, and accompanied with additional notes and observations. By JOHN F. SOUTH, late Professor of Surgery to the Royal College of Surgeons of England, and one of the Surgeons to St. Thomas Hospital. In three volumes. Philadelphia. Lea & Blanchard. 1847. - - - - - 491

PAGE

ART. III.—Lectures on the Principles and Practice of Physic, delivered at King's College, London. By THOMAS WATSON, M. D., &c. &c. &c. Third American, from the last London Edition, revised with additions. By D. FRANCIS CONDIE, M. D., Secretary to the College of Physicians, Author of a Treatise on Diseases of Children. Philadelphia. Lee & Blanchard. 1847. - - - - - - - - - - 495

PART THIRD.

EXCERPTA.

I.—A Course of Lectures on the Physical Phenomena of Living Bodies. Delivered in the university of Pisa. By Professor MATTEUCCI, F. R. S. Translated for THE LANCET, by S. J. Goodfellow, M. D., Lond., late Physician to the Cumberland Infirmary. - - - - - 496

PART FOURTH.

MEDICAL INTELLIGENCE.

FOREIGN.

ART. I.—On the Pathology and Treatment of Hysteria. By JAMES MILMAN COLEY, M. D., Physician to the Western Dispensary, and Senior Physician to the Royal Pimlico Dispensary and Lying-in Institution. - - - - - - - - - - - 528

ART. II.—Poisoning by Iodine. - - - - - - - 535

EDITORIAL.

Health of the City. - - - - - - - - - - 535

Yellow Fever. - - - - - - - - - - - 536

Mortality in the City of New Orleans for the year 1847. - - - 540

Health of the Country. - - - - - - - - - 542

Hospital Reports. - - - - - - - - - - 543

Dr. Sims on Trismus Nascentium. - - - - - - - 544

Morehead's Electro-Galvanic Machine. - - - - - - 547

Vaccination. - - - - - - - - - - - 547

Meteorological Table. By D. T. LILLIE. - - - - - - 547

Obituary. - - - - - - - - - - - 548

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

JANUARY, 1848.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*Researches on Meteorology.* By Bennet Dowler, M. D.

Meteorology by no means corresponds in a literal sense with its Greek origin—that is, μετεωρος, *sublimis; lofty, elevated above the earth; celestial* or atmospheric phenomena; for, "Hecla, pouring her flames through boundless wastes of snow"—the earthquake, the Gulf-stream—physical geography—hydrography and medical topography, are as much subjects of this science as the shooting stars—the thunder-bearing cloud—the hurricane—the hail—the rain—the cloud—water-spouts, those cataracts of the sky—the aurora borealis—the "solar walk and milky way"—"the sun from behind the moon, in dim eclipse"—the monstrous Spectre of the Brocken which looms on the summit of the Hartz Mountains, and which terrifies the good people of Hanover—or "the dewy cloud, and in the cloud a bow conspicuous, with seven listed colors gay."

In fact, Meteorology is becoming more *terrene*, and less celestial—less astrological. Our distinguished countryman, the late Dr. Noah Webster, in his elaborate, but dry work on Epidemics—a work in two volumes, published nearly half a century since, without logical analysis or lucidity of arrangement, but valuable for its historical data, has with unsurpassed research ransacked 'the unrelenting Past,'—collected the debris of departed centuries,—remarshalled the fearful portents,—enumerated the flaming comets,—recounted the planetary commotions, and pointed out the astral harbingers which appeared from time to time in the sidereal heavens, to warn an emperor of impending death,—to foreshadow an approaching earthquake.—or to usher in a mortal epidemic. These cometary visitors from the deeps of infinity, as yet, do not appear to shake pestilence from their locks upon our planet. This branch of knowledge may be called astrological meteorology or physical astrology, and may lead to ætiological discoveries, barren though the past has been in this respect.

The scope of the present paper is very limited. The enumeration and analysis, of the celestrial, terrestial, and subterranean phenomena and the general doctrines of meteorology, with the physiological, sanitary, ætiological and climatic illustrations and applications of the same, being subjects of vast extent, I have reserved for another occasion. Some of these subjects will necessarily be glanced at now, in an economical as well as in a medical point of view, together with the necessity for a thorough reformation in the manner of taking thermometrical observations, as the sequel will indicate.

It were easy to show, that meteorology furnishes the capitalist and the statesman with the natural elements, so to speak, of individual and national opulence. Whenever the science of Political Economy shall be based on nature—on climate, and not on theories deduced from false legislation, national prejudice, egotism and sophistry, then it will deserve to be called Meteorological, as well as Political Economy. Frenchmen will no longer make sugar of beets. Englishmen will find that the British Isles are too nigh the icy zone to raise—not three crops in a year, as in a genial climate—but even one with certainty,—that a good sun, plenty of caloric, a long summer, and a short winter, are not only necessary, but much cheaper than any other kind of capital to an agricultural population. This fact, as well as the truth of the Malthusian doctrine, namely that population, under certain circumstances, tends to increase faster than the means of subsistence, will be admitted in Great Britain as soon as other nations cease to be dependent on that country for manufactures. Agricultural Europe and America in high northern latitudes, spend half of their surplus capital in contending with long winters, or a low temperature, without any compensation whatever. The winter consumes the products of six month's labor as completely as the fire could,—all of which could be saved by *geniality*, that is *cheapness* of climate. Having said thus much, my Æsculapian brethern will allow me a little further liberty to digress.

In northern Asia and America, twelve millions of square miles sustain less than thirty-four millions of inhabitants, while a similar area in southern Asia contains four hundred millions. Extremes serve best for illustration. Greenland was settled more than a thousand years ago, and soon after christian civilization was introduced. For ten centuries the inhabitants have warred against the climate,—they had perished long ago but for the seal which they contrived to take. Twenty years since the Greenlanders numbered about seven thousand only, spread along the coast from 60° to 73° N. lat. Now, in South America, from the Amazon to the Rio de la Plata, many estates have from fifty to two hundred thousand cattle, without any labor to the proprietor except branding etc. In France the horned cattle fall short of ten millions, averaging only 290 for every 1000 people. All the capital in the United Kingdom cannot produce three crops of maize in Ireland, which an indolent Mexican may do without difficulty. In a great portion of America, horses, cattle etc. multiply with astonishing rapidity without labor or care, while, in the low temperature of high latitudes, nearly all would perish in a single winter unless provided for at geat expense.

Meteorology, independently of its medical utility, deserves the patronage of every government. It is gratifying to observe from time to time that the Congress of the United States has passed laws appropriating money for continuing the meteorological observations at the military posts, under the direction of the Surgeon General. The knowledge hence arising, from its tendency to accumulation, will be enhanced with the lapse of time. For a century and a half after the Anglo-Saxons settled in North America, this subject attracted but little attention—consequently the data do not now exist for illustrating one of the most interesting problems, namely the influences resulting from the clearing, draining, and cultivating the soil. A correct, continious meteorological history, from the first settlements in New England, Virginia, Carolina, Florida and Louisiana, is wanting to show the comparative humidity, temperature, and salubrity of the past and present.

A correct thermal chart is a national desideratum, especially in America; since it would afford the intelligent planter a guide, in advance of expensive and often ruinous experiments, in attempting to cultivate and acclimate plants in a temperature uncongenial to their natural requirements: the orange, palm, olive, vine, fig, banana,* coffee, tea, rice, wheat, maize, cotton, sugar-cane and many other valuable plants, which might be named as examples. The best coffee or wine cannot be produced, except in such places as have a certain and very limited maximum, minimum and mean temperature.

It is to be deplored that our planters neglect the science of meteorology, in which they have a pecuniary as well as a scientific interest. Planters enjoy opportunities for studying the thermal adaptations of many useful plants, denied to nearly all authors. There are doubtlessly zones for cotton, sugar and so forth, which a faithful thermometrical history of a few years would point out as superior to all other belts for quality, quantity, and economy.

Humboldt considers a mean annual temperature from 75° to 77° as superior to all others for producing the sugar-cane, though he says it may be reared where the mean falls below 66 or 68°. He says, whatever may be the latitude of a place, that good wine cannot be produced unless the mean annual temperature exceed 49°. 55, while the mean of winter must not fall below 32°, nor that of summer below 64°. 4. The range most favorable to the production of coffee, is, according to a late writer,† very limited in Cuba, having for its maximum 80° and for its minimum 75°. Fever, cholera and consumption, health and longevity, are not thus restricted by thermal lines, nor by humidity, nor disiccation.

* The Galveston News, (Nov. 1847), asserts that a portion of Texas equals Cuba in producing this most valuable plant, which transcends all others in the amount of its nutricious matter on a given area.

† Turnbull's Cuba.

It cannot be denied that atmospheric phenomena, particularly temperature and humidity, exert great influence upon the physiology of plants and animals, as well as upon the progress and termination of many maladies. Yellow fever, for example, is evidently a disease of the warm season and of the warm climates,—typhus chiefly of cold ones. Yet the hottest and the coolest years in almost any locality, stand nearly equal chances of being accompanied by their usual endemics and epidemics. The same is true of dry and wet seasons, at least in New Orleans. But how high soever may be the average temperature, how great soever may be its range, still this fever never assumes an epidemic form in winter. Hence, whenever one hot or cold season, or climate, is brought in comparison with another, even where the topographical and meteorological phenomena seemingly coincide or appear equivalent, it is altogether impossible in the present imperfect state of our knowledge upon this subject, to trace an invariable connection between these and endemics—between atmospheric and morbid actions. No one can affirm that such a connection is improbable, much less impossible, though it has hitherto eluded the observation of meteorologists. New, not lukewarm, careless and desultory researches are necessary, and promise the most important results in political economy, vital statistics, and the ætiology of disease. Synthesis and analysis—the artificial, in imitation of the natural conditions of phenomena, must be put into active operation. Chemistry and physiology are the two eyes of meteorology.

There is, indeed, much assumption, more prejudice, and but little trust-worthy experiment in relation to the agents or the meteorological changes, whether these be regarded as fore-runners, causes, coincidents or necessary conditions of epidemics.—Cholera, which has again awaked the fears of mankind, since a new invasion is now threatened from the East, will from its history serve to show how little is really known of its meteorology. This destroyer, a few years ago, raged amid Asian Jungles where enormous crocodilians drag their slow lengths along, and upon the confines of the icy zone where the gaunt wolf bays a Siberian moon. The destroying angel dipped his pestilential wings alike in the balmy air of temperate climates, and in the suffocating Sirocco from the great sand-ocean of Central Africa —in the drifting snows of northern Russia, in the humid breezes of Louisiana where the alligator crawls through vast swamps to seek his prey or reedy den—in the dry and cool winds which sweep over the high table lands of Mexico, or which descend from mountains whose lofty heads are white with the snows of countless centuries, and whose foundations rest in eternal summer. Cholera, therefore, prevailed in climates the most dissimilar. It may be impossible to discover the essential cause of such an epidemic, and such a discovery might leave the cause as it now is, beyond our control, but, if the origin, approach, or presence of these deleterious agents, or their harbingers, or the essential conditions of their appearance, could be opportunely tested, or even rendered probable, much advantage would result in the prevention, not to mention the cure of disease.

With our present imperfect means of exploration, how different do we find the diseases of places where the meteorology is similar,—

where, for instance, the annual, the winter and summer average temperatures are alike. About 50° is the yearly mean of Paris and of Fort Van Couver (in Oregon)—in both the winter mean is 39°—the summer 64° or 65°. The annual averages of Berlin, Augsburgh, Bergen, Dresden, Edinburgh, Aberdeen, Zurich and Utica, in New-York, are similar, as are those of Dublin and Boston : the latter corresponds in its summer mean with Baden, and in its winter with Berlin and Dresden. The summer average of London and St. Petersburgh, and the annual means of the following placees are nearly identical, that is Vera Cruz, Havana, Rio, Cairo, Canton, and Calcutta.

Hitherto, Meteorology seems to have becn regarded by many physicians as of great importance, chiefly, if not exclusively, in reference to the Italian hypothesis of Marsh-exhalation, or Malaria, promulgated by Lancisi in 1717. Theory is often stronger than fact, not with the multitude only, but with many of the wisest men.

Dr. Forry calls the shores of the lower Mississippi, (one of the most salubrious parts of the globe), " the valley of the shadow of death."—The effects of Malaria in the Southern States he enumerates thus,—" so deep and pervading are the effects of this subtle poison on the indigenous inhabitants of marshy districts, in warm climates, that the energies of the system are sapped, and premature decripitude induced ;—the mind becomes torpid and embecile, the moral sentiments debased, and the stature and symmetry of the body deteriorated."* " Boys," he says, are bowed down with age at sixteen years." Dr. Prichard, a learned ethnologist, has fallen into this very current error, in his Physical History of Man : "In approaching the equator we find the mortality increase, and the average duration of life consequently diminish. The warmer the climate, other circumstances being equal, so much the shorter the average duration of life."† Some of our Medical colleges announce courses of Lectures, not only explanatory of the *Genus Malaria*, but many of its species, as " *Necro, Eleo, Lumato, Idio-miasma, &c.*, —as if these were animals or plants, to be classed in like manner. It is very remarkable that the farther medical writers live from swamps, the more they seem to know about them. Hence, without the least scruple not a few authors undertake to define the movements of miasma with mathematical precision, to tell which side of a swamp is insalubrious, and how many feet miasma travels—how to arrest it by walls, trees, and water-courses—how high it rises—the elevation at which one must sleep to get above it—nothing of which can be known by the residents of the most swampy portions of this terraqueous planet. These quotations and allusions are given not with the view of showing their fallacy, as a complete refutation would require a volume.

That heat, humidity and vegeto-animal matter produce malaria, and thereby epidemics, will be neither affirmed, nor denied. Further investigations are certainly required. In the mean time, Marsh-miasma will answer as an Asylum for theorists when persecuted with antagonistic facts and explanations. Too much is taken for granted in meteorology, instead of " proving all things, and holding fast to that which is good."—

* Clim. U. S. 21. † i. 116, 117.

Humidity, for example, has been probably much overrated as a cause of disease. Professor Casper, of the University of Berlin, in his work on Medical Statistics, based on extensive data, and published in 1846, concludes that humidity, not dryness of the atmosphere is most favorable to life, while no state of the air is so prejudicial to health, as that of dryness with cold.* Humidity is injurious chiefly from the cold it induces, that is by conducting off or dissipating the animal heat.

Strange it is, that at the present time some of the German doctors, instead of sending their pulmonary patients to lands of perpetual summer, send them to Russia,†—perhaps next to the summits of Greenland's icy mountains,—or to hybernate in snow-houses near the pole, where the most dwarfish shrub dares not show its face,—a most quiet place, withal, for an invalid, since there is no noise except for a few days in summer, from the gratings of icè-bergs that have broke from their moorings, or from a glacier, ever and anon thundering from the mountains into the sea. Doctors like others blow hot and cold.

Thus many of the simplest questions remain undetermined—questions in which common sense, physics, and meteorological instruments aid, nay almost decide many dubious principles pertaining to the healing art, which have not been yet fully tested by the experimental and numeric methods. Can any one from numerous experimental comparisons say, that thousands of fever-victims now mouldering in their graves, might not have been saved by the skillful application of cold water, in addition to other means? Passing by the hot stages of typhus and scarlatina, it may be sufficient to observe, that yellow fever during the first 24 to 36 hours, in most cases, is marked by preternatural heat, which is nearly equally diffused over the entire body, sometimes rising to 109° in the axilla, and to 107° in the hand,—and yet this patient must have hot mustard baths, hot drinks, and be covered with non-conductors of heat, as blankets,—all, it may be, proper enough, nay absolutely necessary, when the circumstances are different—when the thermometer tells a different tale. Dr. T. Mayo, of London, in his clinical work, of the present year, advocating cold Affusions, particularly in Scarlatina Maligna, hurls a well merited anathema against his compatriots, (and he might have included the Anglo- and the Franco-Americans, not excepting New Orleans,) for their neglect of Dr. Currie's experimental teachings: "The neglect, which Dr. Currie's discovery has met with, is," says Dr. Mayo, "infinitely discreditable to the medical science of England."‡ Who applies the thermometer, as a pathological instrument, though more useful than the stethoscope? Is it worth while to fight about abstract theories, while the practical questions of cold or hot are left undecided?

As to the morbific effects of cold in nearly all high latitudes, there is but little difference of opinion. Cold is one of the greatest enemies of human life, particularly during infancy and still more in old age, wherein the heat-producing power is feeble. A single illustration taken from a most civilized and improved country, where, considering its remoteness from the tropic, the winters are extremely mild, will suffice. The English Registry of deaths from 1838 to 1841, gives the

* Brit. and For. Med. Rev. July, '47. † Ibid. ‡ Med. Chir. Rev., April, '47.

following as the mean mortality of the four seasons of each year: Winter 97,765; spring 89,141; autumn 83,639; summer 75,707.—Here the winter mortality exceeds that of summer more than twenty-two thousand. "Cold," says Dr. Reid, of London,* "conjoined with moisture has a powerful effect in lowering the nervous energy of the system, and, consequently, in suppressing the generation of that vital warmth which is necessary for the proper performance of the vital and animal functions, and rendering the body liable to low fevers, dropsies, catarrhs, chronic rheumatism, pulmonary consumption, dysenteric diarrhœa, palsy, congestion of glandular parts, as manifested in *scrofulous* swellings and scorbutic affections."—"At least two thirds of the complaints of children may be ascribed to cold."—"It may be laid down as an axiom that cold is the most common exciting cause of diseases, particularly of those of an inflammatory nature."†

Mr. McCulloch says in reference to persons aged sixty-five, that "warmth, temperance, tranquillity, may prolong their years to the end of a century; a rude breath of the atmosphere, a violent struggle will terminate their existence."‡

It is not intended in this paper to dwell upon the geniality and comparative salubrity of Louisiana, nor on the sanitary character of its capital—a city which is destined to become the commercial arch of the two Americas, and to be gemmed with the treasures of the West-Indies. Nor is it intended to show "a perverse and unbelieving generation," its error in pointing out New Orleans as the grand Aceldama upon the sanitary map of the world;—nor to indicate how wave after wave of an unacclimated population, breaking upon five miles of the Mississippi's alluvious and crescented plain, creates periodical increments and decrements, attractions and repulsions, centralizations and dispersions, and catastrophes like the upheavings of a volcano, unsettling the hygienic, vital, social and commercial elements of society; nor, yet, how many of these evils could and ought to be obviated.

I hasten from these generalities to the more restricted object of this paper, namely, a few remarks on Thermal lines—on the heat of the Globe—on thermometrical observations, &c., though I shall be compelled to omit, as too voluminous, even in the form of an analysis, data which have been long accumulating, and which will require additional labor to give them that *brevity* without which the most accurate observer can hardly hope to escape a punishment more awful than the lash of the critic—the punishment of not being read! interment! annihilation!

With respect to isothermal lines or belts of equal, annual mean temperature—isochemial, equal winter, and isotheral, equal summer lines, which the learned Humboldt first proclaimed to the world, it may be

* Philosophy of Death.

† Mr. Kendall, in his Army Correspondence of the New Orleans Picayune, dated in the city of Mexico, (October 1847), mentions the *dryness*, rather than the coolness of the air in that tropical capital, as most prejudicial to the health of the Anglo-American Army, causing rheumatisms, neuralgiàs, etc. The opinion that *dryness* of the air is unfavorable to health, is gaining ground.

‡ Statis. Brit. Emp.

said, without invalidating the truth of the leading doctrine, that, however beautiful and regular these curves may appear in maps, they are far from being at the present time ascertained and fixed. It may hereafter appear, that these lines when traced and deliniated by actual and extended observation in all climates, will be not as now laid down in maps, but far more irregular and complex, far surpassing the almost capricious variations of magnetism itself. Humidity, rain, soil, rivers, lakes, bays, seas, and most of all elevation, (all irregular in their distribution,) will in many places serve to converge these lines almost to a focus. Thus the intertropical mountain, with its base immersed in perpetual summer—its head crowned with eternal snow, presents to the eye at one view all the lines of temperature in close approximation, under the same latitude. Were every house converted into a thermometrical station, it would be impossible in a single year to deduce these devirous lines of equal temperature, forasmuch as one season gives in the same place an average different from that by which it was preceded, so that from the nature of the case a considerable period will be required before safe numerical analysis can be made. In connection with this subject, it may be proper to suggest a theoretical question which cannot now be proved, because thermometrical facts are not only too scanty, but too recent for its verification,—namely, whether there be not a thermal cycle—a calorific periodicity of increment and decrement, in which the mean annual heat of a place, though fluctuating, still augments or declines within certain limits and for a definite period. Magnetism furnishes an apt illustration, affiliated as it is with electricity,* caloric, and meteorology: The magnetic poles—the magnetic equator parallels and meridians do not exactly correspond to those called geographical. The variations of the Needle are, so to speak, both regular and irregular. The seasons—the succession of day and night, change agents, lines of action. Its curves resemble each other, without being identical. Topo graphical and meteorological as bodies of iron, and the aurora borealis contribute to augment these irregularities, yet over all these subvariations presides a mightier power, during the long magnetic century, from its maximum to its minimum declination. In 1576, at London, the needle pointed 11° 15′ East of North; in 81 years it had declined so as to point due North. It remained unchanged for five years. It then declined towards the West. In 153 years its maximum Western declination, amounting to 27° 18′, was completed, whereupon it began to retrace its steps, but in an irregular manner, equal times giving unequal velocities in its Eastern declination at the same and at different places. Here, then, is an example of a regular irregular series or cycle (perhaps ætiology may be in a similar category,) requiring the perpetual vigilance of science.—Although the maximum and minimum variations, dip, etc., will no doubt

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* Magnetic attraction and repulsion, in their phenomenal conduct, approximate the electrical action;—"in two magnets the North pole of each attracts the South pole, and repels the North pole of the other." Magnetism is comparatively a modern science. The locality of the magnetic pole was indicated, or perhaps discovered, by Captain Ross, in his Arctic Expedition, only a few years ago. Such are the great advances which have rewarded the unwearied labors of philosophers in this branch of—I might say—Meteorology!

continue to recur as long as our planet shall occupy its present position in the solar system. It does not hence follow—nay it is even improbable, that the data of one magnetic cycle, will exactly correspond with its fore-runner, or its successor. Many of the phenomena of nature are invariable, but not all. Facts grow old, or, at least, become in many particulars greatly modified, more especially such as relate to medicine, civilization, political economy, and vital statistics. Thus, in a practical point of view, some of the sciences, owing to remoteness of time, climate and changes among the data themselves, require to be eternally renewed.

Should equal thermal lines be established in all parts of the earth, still as remarked before, it will not follow, that equal or even similar lines of disease will be in every instance thereby indicated. One portion of the thermal belt may have cholera, another typhus, a third leprosy, a fourth elephantiasis, a fifth goître, a sixth yellow fever.

Having said something, and intending to say more, in dispraise of the present state of Meteorology, it is no more than justice to the moderns, by way of comparison, to glance at its former condition, in the century in which Dante, Petrarch, Boccacio, Chaucer, Froissart lived, and the Universities of Prague, Vienna, Heidelberg, Cologne, and Erford, and the Medical School of Montpelier flourished. A single example illustrative of the wisdom of our predecessors will enable us to make comparisons very flattering to our progress in the meteorology of medical ætiology. Near the middle of the Fourteenth century, the Black Plague or Black Death, which threatened to exterminate the human race, and, which in Europe alone hurried to the realms of the dead, twenty-five millions, or one fourth of the entire population, was according to the authors of that day, ushered in with the sublimest meteorological ceremonies. Above, around, below—the elements mustered their angry battalions against poor, frail man. Meteors shot athwart the vaulted skies. A pillar of fire hung over the Papal palace, at Rome. The earth quaked. The Zones and the Indian Ocean were infected,—the atmosphere corrupted.* In the midst of the general consternation, the august Faculty of Paris, then, as now, considered prodigiously learned, assembled. The doctors, after due deliberation, in a most solemn, official manifesto or medical bull, decided in the most positive manner, that the epidemic was "owing to the constellations, which combatted the rays of the sun, and the warmth of the heavenly fire which struggled violently with the waters of the sea, originating a vapor in the great Eastern sea of India, corrupted with fish, enveloping itself in fog. Should the same thing continue *not a man will be left alive*, except the grace of Christ preserve him. We are of opinion that the constellations, with the aid of Nature strive, by virtue of their divine might, to protect and heal the human race, and to this end, in union with the rays of the sun, acting through the power of fire, endeavor to break through the mist." The Faculty proceeded at the same time, to predict in the most oracular manner the future action of the constellations: "Accordingly, within the next *ten* days, and until the 17*th* of the ensuing month of July, this mist will be converted into a stinking deleterious rain, whereby the air

* Hecker, Epid.

will be much purified. Now, as soon as this rain announces itself, by thunder or hail, every one of you should protect himself from the air; and as well before as after the rain, kindle a large fire of vine-wood, green-laurel, worm-wood, chamomile, &c. Until the earth is again completely dry, and *three* days afterwards, no one ought to go about." The means recommended as precautions, are such as the following: "only *small* river fish should be used: rain water must be avoided in cooking, &c; chastity is well spoken of, but bathing above all things is condemned." The physicians and the learned generally, without hesitancy, ascribed the Black Plague to Astral action—the conjunction of Saturn, Jupiter and Mars, in the sign Aquarius, upon the 24th of March, 1345.

In his Decameron, Boccacio, gives a vivid description of this epidemic as it appeared in Florence, where from March to July, 1348, it destroyed more than one hundred thousand persons, "whereas, he remarks, before that calamity, the city was not supposed to have contained so many inhabitants. What magnificent dwellings, what noble palaces were then depopulated to the last person! what families extinct! what riches and vast possessions left, and no known heir to inherit! what numbers of both sexes in the prime and vigour of youth, whom in the morning neither Galen, Hippocrates, nor Æsculapius himself but would have declared in perfect health; after dining heartily with their friends here, have supped with their departed friends in the other world!

Boccacio was a firm contagionist: "It is wonderful, says he, what I am going to mention; which, had I not seen it with my own eyes, and were there not many witnesses to attest it besides myself, I should never venture to relate, however credibly I might have been informed about it: such, I say, was the quality of the pestilential matter, as to pass not only from man to man, but what is more strange, and has been often known, that any thing belonging to the infected, if touched by any other creature, would certainly infect, and even kill that creature in a short space of time: and one instance of this kind, I took particular notice of; namely, that the rags of a poor man just dead, being thrown into the street, and two hogs coming up at the same time, and rooting amongst them, and shaking them about in their mouths, in less than an hour turned round and died on the spot."*

The meteorology of the Earth or mass constituting the Globe, is a subject of great interest in a speculative as well as in a practical point of view, particularly as to the origin, propagation, supply, and waste of caloric. "The present temperature of the earth," says Mr. Bakewell, in his Geology, "appears to be dependent on two causes,—*the radiation of heat from the Sun, and internal fire.*"† It will be preferable for the purposes of illustration to notice this latter cause first.

The internal heat of the globe—or the increase of temperature from

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* This epidemic did not appear to lessen the tendency to refined sensualism in that age—if we may judge from the avidity with which this classical, but licentious work was read. Perhaps no single work of its magnitude was ever sold by an author at so high a price—nearly three thousand pounds—at a time when the relative value of money was much greater than at present.

† 358 Prof. Silliman's Edit.

the surface to the centre, in a constant ratio—is a most prevalent, and as I conceive, doubtful, nay false theory;—an allusion to which may not be improper in this place. Whether the increased temperature observed in deep wells, pits and mines, can be accounted for by atmospheric pressure, forcing caloric from the air as compression does water from a spunge,—whether the existence of certain thermal springs, and the ejection of melted matter by valcanoes, be owing to chemical actions, (a theory altogether probable,) are foreign to the present inquiry. Professor Bischof, of the University of Bonn, in a learned work* advocating the doctrine of the internal heat of the Globe, but proving quite the contrary, if I may judge, says, "Mairan first set up the hypothesis of the existence of fire in the interior of the earth;—at his instance the first observations were made in France, in 1749."† Bischof admits that the air of mines is no guide, and that from observations of this kind, "it is impossible to discover the law."‡ The thermal waters occasionally met with, and upon which he chiefly relies for proving this theory, are equally fallacious.

The water from a snow-clad mountain in descending or ascending through fissures, may be heated solely by chemical action, such as is witnessed in mixing an acid, or alcohol with water, not to name more striking instances where a metal Potassium, for example, takes fire upon coming in contact with water, burning with a brilliant flame and intense heat. Oxygen has been long and actively at work in changing the earth's crust. If the earth be composed of solid metals, or metalloid substances, (which is probable,) it is reasonable to suppose that their oxydation, &c., would produce not only hot springs, but volcanoes themselves.

As Mr. Bakewell's Geology is but little disfigured by exaggerated theories, it is remarkable, that he should rely in thermal springs as proving the internal heat of the Globe. His statement that "some hot springs have flowed without any known diminution of temperature, for nearly two thousand years," proves not what he wished to prove, but the contrary, namely, that there is no such a mass of melted matter near the surface, as the laws of conduction and radiation of heat clearly show. The *constant* temperature of a spring, not less than of the whole earth, for thousands of years, would be physically impossible upon this theory, and contrary to every known law of caloric.

The internal heat of the Globe is supposed to keep the whole mass in igneous fusion, except a thin pellicle or crust about 25 miles in thickness, less than $\frac{1}{300}$th of the earth's diameter,—thinner comparatively than an egg-shell, or a soldier's canteen, compared with its contents.—The heat, at a mile and a half, is estimated at 212°—the boiling point of water;—the central heat, would, agreeably to this augmenting ratio, amount to nearly half a million of degrees of Fah.—a hundred times hotter than melted gold. Now, if the boiling point be only a mile and a half below the surface, enormous quantities of steam would constantly escape from innumerable points and fissures, more especially in the mountain chains where the crust is known to be broken in all direc-

* Heat of the Globe, London, 1841. † Ib. 183. ‡ Ib. 186.

tions, vertically and horizontally ;—or, not finding vents, would cause earth-quakes or at least *water-quakes.*

The falsity of this theory,—its physical impossibility, is as I humbly conceive easily proved by experiment: Make a hollow globe, with a shell not quite the $\frac{1}{300}$th* part of its diameter, out of the principal metals and clays which constitute the earth's crust—fill this globe with melted iron, to metion no greater heat—suspend this miniature world in the air, as is the earth, and observe, whether the outside of the crust be in the least heated by the internal contact of the melted mass ;—observe whether in a day, or a year, or a century, not to mention thousands of centuries, the temperature shall remain constant without loss from radiation. Now if the earth were thus filled in its central portion, with melted matter, the heat would be speedily conducted to the surface of this crust, and would reach the poles first, as they are *nigher* the centre than the equator, by thirteen miles and a half. The eternal ice of these regions would dissolve—the glaciers melting at their bases would shoot down the mountains†—the ocean would become a boiling caldron, and every drop of water belonging to our planet would become elastic vapor.—A calorific equilibrium would at length be established between the centre and the circumference. In the meantime the Globe would contract greatly, compared with its size during its maximum temperature—a rapid condensation of vapor would take place—rains—not to mention deluges—would be abundant—the ocean's bed would fill up—the cold would constantly augment—frost would appear on the highest mountains, and the snow line would progressively descend their sides. The refrigeration would never cease, until as before mentioned an equilibrium should be established between the centre, surface, and surrounding media, agreeably to an universal law by which the radiation of heat is governed in a regular series, which may be calculated with exactitude both as to time and velocity. Thus the earth would continue to refrigerate.

The inhabitants would descend to the vallies and most depressed places, until overtaken by a congelation. The last man and the last woman having been petrified into everlasting frost, would be unmourned, and the earth would be transformed into a glittering cemetery of ice, as lifelsss, voiceless, and lonely, as a moon-beam straying through the chinks of a decaying tomb.

Prof. Bischof mentions a fact very little favorable to his theory of augmenting heat in descending from the earth's surface. In the town of Jakutzk in Siberia, under the 62° parallel of North latitude, a man has been engaged for years in sinking a well—in 1830 he penetrated

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* Let the crust of this globe be one inch thick—75 feet in circumference or about 25 feet in diameter—proportions agreeing with the assumed crust and melted contents of the earth.

† In his work on the Arctic Regions, Sir John Barrow shows that even a trifling noise will cause the glaciers to descend from the icy mountains headlong into the sea: "In their vicinity silence is necessary; the explosion of a gun scarcely ever fails to bring down masses of ice. Mr. Beechy mentions that on discharging a musket, a glacier half a mile distant fell with a thundering noise headlong into the sea—the first wave from which was 96 feet broad and wrecked their boat, throwing it on shore."—(Voy. 66. Lond. 1846.)

78 feet, and the next year 90, without reaching water, and without passing through the frozen soil. In the North-east of that country at a certain depth, even in the hot season, the ground is constantly frozen to an unknown depth.* Mr. Lyell quotes Professor Von Baer, of St. Petersburgh, as stating that the ground is now frozen permanently to the depth of 400 feet in Lat. 62° N., in Siberia, on the Western banks of the Lena, 600 miles from the Polar Sea.†

The influence of solar heat upon the soil, the depth to which it penetrates, the stratum of invariable temperature—in a word, the calorific changes in the earth's surface as well as in its atmosphere, involve an interesting problem. Suitable instruments are wanting to test the terrestrial temperature at a depth of 6, 12, 18, 24 and 36 inches. Although my own experiments on this subject are imperfect, yet I am inclined to think from them and from the physical character of Louisiana that the line of constant temperature sinks but a short distance below the surface. The soil is porous, favoring the percolation of water which abounds, and which at all times, but more especially in the season of the inundation, reaches near the surface—the winds are active, the sun ardent—all tending to favor evaporation, the great refrigerating process of Louisiana, in connection with the sea-breezes of its littoral.

In the hottest portion of this year (1847,) at a depth of six inches, in a grass lot, the maximum heat was 83°. in the afternoon;—at sunrise, in June, the highest 80°, the lowest 77°—the highest in July, 80°—the lowest 76½°—the highest, August, 80°—the lowest 78°, and for eleven days ending November 25th, the highest 68°, and the lowest 55°—a great terrestrial range, owing to two powerful causes,—rain after a long drought, and a sudden change of the air with Northerly wind, requiring fires—the first that were needed during autumn, with perhaps a slight exception towards the close of October, when the mercury descended to 52°.‡

In the vaults of the observatory at Paris, and in Brussels, with other similar places, at 91 to 92 feet below the surface, an invariable temperature of 52° ever reigns. A number of respectable authors maintain what could not have been readily anticipated—namely, that in the equatorial regions the influence of external temparature does not penetrate one foot, nay half that distance below the surface, where the line of invariable heat is found. This line constantly sinks deeper at every remove

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* Heat of the Globe, 96. † Geol. i. 151. Lond. 1841.

‡ About the middle of October frost was announced—the wish was farther to the fact—frost there was none—the mercury had only descended to 57°.—Even a month later, when Dr. Lindsay of this city returned from his plantation over the lake, nearly one degree North of New Orleans, no frost had yet occurred.

And now, in the last week of November—nearly seven weeks after the Board of Health announced the extinction of the *Epidemic*—no frost has arrived, to the great scandal of all faithful theorists; the miasma—the contagion, if any, was not frozen into substance. The summer heat was continued in autumn, and yet the city, overflowing with *non-acclimatees*, continued to improve in its sanitary condition. Strangers rushed into houses, where in some instances every person had died a few days before, without being infected. Think of this miasmatists, contagionists, quarantinists.

from the equator towards the poles. In Germany it is 60 feet deep.*—Humboldt says that on the continent of Europe, between the parallels of 48° and 52°, the stratum of invariable temperature occurs at from 55 to 60 feet deep; even at this depth the oscillations of the thermometer, in consequence of the influence of the seasons, scarcely amount to half a degree. In tropical climates, on the contrary, the stratum of invariable temperature is met with at no more than half a foot below the surface; and Boussingault reckons this an accurate way of determining the mean temperature of the air of a place. He gives the following table.†

STATIONS.	Thermom. foot under the surface.	Mean of the air.	Parisian feet above the sea.
Guayaquil	78. 8° ‡	78°	0000
Anserma neuvo	75.	75.	3231
Zupia	70. 5	70. 5	3770
Popayan	65.	65. 5	5564
Quito	59. 8	59. 8	8969

If the intertropical soil at the depth of six inches, according to some authorities, gives a constant temperature at all seasons, and, if that constant temperature be identical with the mean annual temperature of the air, it follows that meteorological labor will be greatly abridged. It is however extremely difficult to believe that the solar heat should not penetrate to a greater depth during the day—in other words, that the coolest part of the night and the hottest of the day, should produce no diurnal range, to say nothing of seasons, which, in some places vary considerably.—The quality of the soil, the humidity or dryness of the sub-soil, and the activity of the winds, must exercise a marked influence on the temperature of the surface. It is probable, as before remarked, that this line of constant temperature, is, for Louisiana, of very little depth compared with similar latitudes. Copious rains, ceaseless breezes, with abundant percolations of water from bayous, lagoons, swamps, lakes, and the great river,—some of which rise 8 or 10 feet above, and none of which fall much below the general level of the ground. These must under a most powerful sun, and ceaseless breezes contribute to neutralize or render latent the heat of the country by evaporation.

It is worthy of inquiry, to what extent stone pavements augment the vernal heat of Southern cities. It is probable that they absorb and radiate a vast amount of caloric, as well as prevent the refrigeration of the soil by hermetically sealing the surface, and thus arresting evaporation.

In the first week of August, (1847,) the average difference between a grass lot and a brick pavement, at similar depths, at 3 P. M., was 8°—an enormous difference—the latter being, of course the hottest.§

* Bishof. Heat of the Globe. 127. † Cosmos. I. 185. 444.

‡ I have converted the degrees from C. to Fah's. scale.

§ The method of taking the temperature of the earth usually adopted (as Dr. Forry's, clim. U. S. 80,) is worse than useless, I mean that of digging down and burying the thermometer three times daily. The ground should be perforated,—the thermometer, being introduced so as to exclude the air, should never be removed.

The meteorology of the Mississippi River, (my observations upon which I have not yet digested fully,) will possibly afford the shortest method of ascertaining, approximately at least, some important average results, or criteria for estimating the temperature of the atmospheric heat of Louisiana. The annual range of the river is remarkably uniform—its diurnal oscillations scarcely appreciable. Its annual maxima, give the same mean as the Gulf-Stream, nearly 86°—its annual minima, scarcely 40°—its range 46°. The mean temperature of two years ending in August, 1847, (by averaging its maxima and minima, not having time at present to be more exact,) is 63°.37.—According to Dr. Barton (now of the Anglo-American Army of Mexico, formerly a resident of New Orleans,) the general average temperature of this city, from 1833 to 1836, was 66°.93.*

The Orinoco running parallel with, and but a few degrees North of the equator, has probably nearly an uniform temperature during all seasons. Humboldt, who explored the upper portion of that river, in the spring of 1800, made a few casual observations on its temperature. He does not give either the diurnal nor the annual maxima, minima, nor the mean. The Apure, a tributary stream, gave 79° to 80°;—the Orinoco in the middle 82°.9—near the shore 84°.6;—670 miles from its mouth, 82°;—the rocks upon the shore 122°.4; at its upper cataracts, Lat. 5° 13′ 57″ N., 81°.7,—the air at night being 80° to 84°, and in the day 86°.† These scanty hydro-calorific facts are quite sufficient to show a marked difference between the Orinoco and the Mississippi, even when they approximate the nearest. In the current of the latter, at the breadth of the hand from shore, the running water is the same in temperature as every other part of the river, with scarcely any variation between the coolest and the hottest portions of the twenty-four hours, while the annual increments and decrements are regular, though the daily is nearly imperceptible. To the eye of science, if not to the eye of the traveller, the Mississippi must appear the greatest of all hydrographical sublimities,—from the volume of its waters, and the depth of its channel, etc.

Without dwelling now on the necesstiy of uniformity in nomenclature, and in classifying the phenomenal data of Meteorology, as in chemistry, physics, and natural history, it will be proper to allude to at least one of its most important instrumental processes, as being so defective as to require experimental researches *de nova.* Experience has convinced me that the method, or rather the lack of method in measuring and determining the meteorology of heat by the thermometer,‡ renders the observations generally made—(including my own for some years)—of comparatively little value for exact calculations, to say nothing of fractional

* Gibson's Directory, 1838.

† "Humboldt found the air on the Orinoco, at 2 P. M., 86°—coarse, movable granitic sand 140°.45—white, close grained, fine sand, 126°.5—granitic rocks, 117°.725. An hour after sun-set, the coarse sand was 89°.6—the granitic rock, 101¾°."

‡ The Thermometer (from φερμος, *heat*, and μητρον, *measure*—literally *heat-measurer*), was invented nearly two centuries cince by the Florentine Academicians, and which Fahrenheit and others subsequently improved.

decimations. Comparisons can never be accurately made, particularly during the maximum heat of the day in cities, until buildings suitable for observations shall be selected or erected, of similar form and materials agreeing in local circumstances; in radiating media, &c.—The elevation, proximity, and materials of the surrounding buildings, and the relation which they bear to the place where the thermometer is situated, must all be given or estimated, especially in cities where the absorption and radiation of caloric is great. The thermometer will give a different temperature at the same elevation above the soil or the sea, according to the height of the building in which it is placed. A house one story high, will absorb more solar heat during the day, and radiate more during the night than the corresponding or lower story of a house five stories high; the attic of the latter will during the hot season be much warmer than the ground story. The following illustrations will place this in a clear—not the strongest light—because reflected heat was *avoided*, not sought after. The observations on the river and ground were made in the sun,—the residue in the shade:

1847, June 14.	Air.	Office 1st Story.	Ground 6 inches deep.	River.	Attic or 3d Story.
Sun-rise.	75¾°	81°	79½°	80½°	84°
3 P. M.	85	—	83	81	105
Sun-set.	81	—	83	81	91
August 2.					
Sun-rise.	78	83	80	85¾	85
3 P. M.	88	90*	82	86	105

It wuold be tedious to recapitulate all the experiments made in houses, on the Levee, in the streets and in the public squares, before sun rise, at the minimum of the day, and, therefore, free from the direct influence of solar heat. These media give different, often materially different temperatures. (I give but one example—not the most striking—at the moment of writing this page—November 25th—sunrise—15 feet from the house, east, 38°;—15 feet, west, over a pavement, 42°;—in both places the instrument was alike exposed to the wind, but in the latter, two blocks of houses radiated heat; in the former one.)

The upper story of a high house absorbs not only more heat by day than it radiates by night, but much more than the lower story, for half of the year. The thermometer may give a result differing from 20° to 30° or more, in different stories of the same house, or in the open air, all the observations being equally in the *shade*. A large and high house never becomes heated to the ground floor like a low and small one, other circumstances being equal.

Shade is a material consideration in many points of view. A column of moving air, heated in the sun, on approaching a wall that has been exposed to the solar rays and sometimes heated to 150°, will have its temperature much elevated. A thermometer may be so placed, though actually in the shade, as to rise, perhaps, as high. The air thus heated by the sun, by the radiated heat of the wall and by reflected heat from other bodies, will on passing the line where the solar rays and the

* Picayune office, 91°—the Rotunda of the St. Charles Exchange, 90°, as reported.

shade meet, discharge its excess of temperature unless modified by reflection etc., until it reaches the remotest depth of the shade, in an uniform arithmetical ratio, at each successive moment, and for each increasing distance from the line where the direct and radiated heat is maximized. Perhaps, a thermometer might be so situated with respect to radiating media as to receive nearly as much heat as would ignite gun-powder, and yet be in the shade :* on the contrary, were a building elevated on pillars, so as to cover a vast area, the range of temperature under the centre would be but little, and would correspond to the area of the shade,—the hottest day of summer would be comparatively cool, and the coolest day of winter comparatively warm. The depth of the shade,—in other words the distance from the thermometer to the sun's rays on all sides, is a fundamental point, giving even after the sun dips below the horizon, or suffers temporary obscuration from clouds, a decided influence upon instrumental observations.

In a city during the hot season, houses of various sizes, elevations, colors,† and materials, possessing different calorific powers of absorption and radiation, constantly receive yet in different degrees, increments of solar heat by day, beyond what is radiated at night, until at a certain season of the year, these masses attain, but not always at the same time, their maximum temperature, after which the law of decrement sets in until each mass, but not in equal times, reaches its annual minimum.—The maximum, minimum, range, average, and general laws of temperature in these masses of masonry, and in aërial currents, do not coincide, nay differ materially.

In the Polar regions as the sun moves in a circle around the horizon, the shadows pointing to all points of the compass, it would be desirable that the thermometrical observatory should have a circular gallery, so that observations might be always made at the side opposite or remote from the sun,—in fact this rule applies to all the zones, to an extent corresponding to their winter and summer solstices, so as always to insure an equal depth of shade or an equal distance from the solar rays, how different soever may be the length of the day. Captain Ross, in his Arctic Expedition, saw the sun continuously from June 7th to August 24th,—1,872 hours. From the equator to pole, the reflex action of caloric (which conforms to physical laws, and is within the pale of mathematical calculation,) should be avoided, as causing an elemental perturbation or local change in the atmosphere, and consequently in the data, whether noted in the sun or shade. Take an example from the most boreal climate :—"Captain Scoresby, in his account of the arctic regions, observes, that when the sun's rays fall upon the snow-clad surface of the ice or land, they are in a great measure *reflected*, without producing *any material elevation of temperature ;* but when they impinge on the *black* exterior of a ship, the pitch on *one side* occasionally becomes

* Dr. Chalmers, of South Carolina, many years ago remarked that the atmosphere of Charleston was always 10° to 15° hotter than that of the country. (Bancroft Yel. Fev. 192.)

English authors mention that the mean temperature of London is 1°.8 higher than the country in its vicinity.

† See Franklin's experiments on the calorific or absorptive power of colors.

fluid, while *ice* is rapidly generated on the *other*." "The radiation of the sun, says Mr. Beechy, is in the Arctic regions, in *sheltered* situations so powerful during two hours on either side of noon, that frequently the thermometer upon the ice in the offing rose to 58°, 62°, 67°; and once at midnight to 73°, although in the shade at the same time it was only at 36°."*

Upon the 23d of July, 1845, perhaps the hottest day ever known in New Orleans, the temperature arose (the house but little suited for observation) to 100o ten feet deep in the shade—the true heat, fairly taken, was probably 96°;—yet many radiating bodies gave from 150° to 152 in the sun from contact. Now had these radiating bodies been properly arranged—had they been changed into concave reflectors, the rays of heat might have been converged upon the bulb of a thermometer in the shade so as to have reached in all probability nearly to the boiling point. Hence we read of a temperature in India, Africa, and other places, as rising in the shade to 125°—130° and more. In California, Dr. Coulter noted 140° in the desert near the mouth of the Colorado, more than two degrees North of the latitude of New Orleans;—the climate of which, according to Captain Wilkes, is colder in *summer* than *winter* owing to the North-west winds.† It is the free, not the stagnant, local air, saturated with radiated heat, to which the thermometer should be exposed, in measuring the heat of the shade, and the same rule applies to the taking of the temperature in the sun. Of the hot climate of Egypt, it is said, that in the night, and every where in the shade, the air is singularly cold.‡

If it be true that the souls of the mighty dead know no peace until they receive the full meed of praise due upon earth, the discoverer of the freezing and boiling points of the thermometer, is, to this day, doomed to wander a discontented ghost. He deserved immortality. Fahrenheit's improvement, which consisted in adopting quicksilver instead of the spirit of wine, was a great one, indeed; for whosoever trusts in a spirit-thermometer, wofully deceives himself, as I can say from experimental comparisons. No one should put any dependence in even a mercurial thermometer, until he has personally *tested both the freezing and the boiling points* of the same. Not more perhaps than one in ten, will be found correct, at least for New Orleans—the freezing point being nearly always too high.

The place where the thermometer is manufactured, that is, the elevation at which the scale is graded, is a fundamental point. For whether the freezing point be the same in all climates and at all altitudes, or not, certain it is, that the boiling point is materially different. General Scott boils his tea-kettle, in Mexico, with about fourteen degrees of heat less, than General Taylor needs for that purpose on the Rio Grande.—The London and New Orleans boiling point is 17° higher than that of Bogota and Quito; 7°.5 higher than that at Jasper's House—8°.5 higher than that at camp d'Orgal, and 14° above that of the Punch Bowl,—all three on the Rocky Mountain route to Oregon.§ Lieuten-

* Sir John Barrow's Voy. Arc. Reg. 69, Lond. 1846.
† U. S. Explor. Ep. V. 155. ‡ Univ. Geog. Edin. 1844. § U. S. Explor. Exped. V. 155.

ant Wood, of England, found in 1841, that water boiled at the source of the River Oxus, in the Himalayan chain of Mountains, at 184°, or 28° lower than the boiling point of London and Paris.

Many circumstances combine to augment the intensity and irregular action of solar heat in cities, whether compared with each other, or with the surrounding country. The absence of grasses, shrubs, and shade-trees—the presence of pavements, the condensation of the alluvial soil, favor absorptions by day, and radiations by night, differing however, according to the locality. Wide streets differ from narrow ones, even where the houses are of the similar materials, colors, and altitudes; the former admit the rays to the pavement over a larger area, and during a longer portion of the day, while the upper stories of the houses forming the latter, intercept and absorb the greater portion of the rays, and, consequently prevent them from reaching the ground, where they produce their chief physiologico morbid impressions. Hence, also, the ratios of evaporation and refrigeration will be different. Some buildings absorb and radiate almost equally; others absorb but little, and reflect much. It is a curious fact, for which we have the authority of experimenters, that heat passes through certain bodies, especially such as are transparent without heating them. Professor Kaemtz says, that a piece of pure ice made in the form of a lens, will concentrate the solar heat so as to produce combustion, without being itself heated.

The temperature at a short distance above the earth, is, as all know, very cold, and, beyond the atmosphere, in celestial space, where no absorbing or opaque bodies exist to arrest the calorific rays, the cold is estimated at nearly two hundred degrees below the freezing point.—Terrestrial absorption of the calorific rays is greatly diminished by shade. Were half of the native forest trees which once flourished on the site of New Orleans, now standing, interspersed with the houses, its sanitary condition would probably be very different.

Heat is the great agent of development and transformation, not only in the inorganic, but in the animal and vegetable worlds—in the nebulosities precipitated from the deeps of infinite space to form new worlds, and in the microscopic battalions of infusoria which revel in the glittering dew drop. Calorific metamorphoses in physiology, rival those of physics—in pathology, those of chemistry. Professor Liebig remarks, that "the same sugar which in beet-root juice fermenting is resolved into alcohol and carbonic acid, yields upon an elevation of temperature, (no addition whatever being made to the fermenting juice,) mannite, lactic acid, gum, carbonic acid, and hydrogen gas."

Now, as many substances at precisely the same temperature, possess very unequal quantities of heat, that is insensible or specific heat, so may many diseases; and even sensible morbid heat, of the same temperature in scarlatina, yellow fever, and typhus, may be different in qualities, and even in sensible effects;—hence the phrases, sharp, biting heat, &c. Sir John Pringle declared that the touch of a typhus patient produced an uncommon ardor, leaving an unpleasant impression on his fingers.*

The influence of temperature upon the health of cities, is a problem

* Dis. Army, 259; also M. Dance. Clin. 376.

that I do not now intend to examine. I will only add, that sun-stroke, the most fatal of diseases, is a strong example of the injurious effects of solar exposure in hot weather, while exclusion from the sun during epidemics, is probably the means of that exemption from yellow fever, which the inmates of the New Orleans prison have always enjoyed.

As learned treatises have been published, periodicals established, and professorships endowed expressly to teach the science of meteorology, every thing relating to its elementary methods, is fundamentally important. In offering a few remarks in relation to defective methods, I am not insensible to the great merits of many learned men, whose patient researches have brought the science to its present advanced state; nor do I imagine, that there is any great virtue in finding fault. I should not have ventured any opinions in relation to one or two existing imperfections, had I not labored sometime in this department—had I not felt convinced that the defects pointed out deserve attention, and can be remedied. It is here, (I confess it), that the remark of Gœthe upon satirical poets, applies with an almost equal force—"When I have called the bad—*bad*, how much is gained by that? The man who would work aright must not deal in censure, must not trouble himself about what is *bad*, but show and do what is good."

No one can deny, with respect to the meteorology of heat, the importance and the mutual international benefits that would arise from the adoption of an uniformity of method—the same kind of buildings, (however improperly constructed), the same relative positions, exposures, depths of shade, hours of observation etc.,—otherwise the data of Paris, Berlin, Vienna or Rome cannot be correctly compared with those of New Orleans, Mexico or St. Petersburgh.

Having omitted to refer in the proper place to palæontology, the science of the fossil races of animals and plants—the ancient fauna and flora of our planet—the 'Medals of Creation,' the remains of a former world, so much relied on by geological and palæontological writers as proving or favoring the doctrine of the internal heat, it may be proper to remark, that this branch of knowledge proves at most, nothing more than that the polar regions once enjoyed a tropical climate, (as indicated by animals and plants), without accounting for the alteration which has since taken place. The hypothesis of the internal or subterranean heat, with the progressive and continued refrigeration* of the earth, does not account for the appearances of the tropical fossilized and congealed organic remains found in the polar regions, in any degree more satisfactorily, than the hypothetical doctrine of climatic cycles, already mentioned. Even the Auroræ Boreales appear to have cycles or periodical increments and decrements during many years, as well as a maximum and minimum strongly marked for each year. Prof. Kæmtz supposes the auroræ to be intimately connected with magnetism, as magnetism is with heat, and that the poles of cold and the magnetic poles coincide.† The historical evidence, such as it is,

* This refrigeration, however gradual, would have caused great and easily appreciated changes in our planet—contraction—diminished size—acceleration in the diurnal revolution—and diminished length of the days.

† Meteorol. 462.

without thermometical data,—would seem to show that climates are growing not colder, but warmer.*

It is supererogation for philosophers to labor upon far-fetched theories to explain the little that is known in relation to the increase of temperature in descending into the earth, as *pression* alone is sufficient to account for this phenomenon to a great extent—an explanation too, which every body can readily comprehend; for if a few feeble blows of the blacksmith's hammer, produces pression or condensation in a bar of iron, so as to cause it to be red-hot, surely the pressure of perpendicular strata or columns of metallic matter entering into the structure of the earth—columns several thousand miles in perpendicular length—must force heat out of the same as completely as the rock of Gibraltar would squeeze the juice out of an orange, and this too in a ratio corresponding to the depth.

In these matters we know but little and can 'reason but from what we know'—

> "One part, one little part, we dimly scan
> Through the dark medium of life's feverish dream."

The hypothesis of igneous fusion with subsequent refrigeration is less plausible, in explaining the former tropical conditions of the polar regions than that of planetary or celestial aberration. An erratic sun may have communicated its heat to the pole for ages, before disappearing comet-like. With respect to the earth itself, astronomers agree that it does not now pursue its former path through space, that its orbit was once more elliptical than at present, that its eccentricity is diminishing, and that it more and more approximates a perfect circle. When, therefore, its orbit was the most ellipsoidal, our planet must have been nearer the sun during a certain period, than at present, and consequently, the heat must have been augmented in the same ratio, so that the poles may have possessed a climate approximating that of the present tropics. Another hypothesis may be mentioned as explanatory of polar palæontology, namely, the austro-boreal axis of the earth may have approximated the perpendicular axis of the equator. The present inclination 23° 28′—if doubled or tripled would, to the same extent, augment the heat of the polar regions. Milton believed that the earth's axis differs now from its paradisiacal condition:

> ——He bade his angels turn askance
> The poles of Earth twice ten degrees and more
> From the sun's axle.——

It is due to the reader, and still more to the writer of this paper, to mention that so far as arrangement, and especially composition are concerned, time did not allow of proper attention to either, owing to the late period at which the polite request for a communication was re-

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* M. Gayarré, of this city, the distinguished historian of Louisiana, mentions (*Es. Hist. sur la Louisiane*, 1830), that in 1768 the Mississippi in this latitude was upon its shores covered with ice—an occurrence which has not taken place, so far as I have been informed, since that year.

ceived from the editorial department of this journal. It is true, I had the data in my possession—a mere skeleton, not yet articulated, so that the printer got a leg one day and an arm the next. If the reader will join the whole together as symetrically as is possible, I will endeavor to compensate him for his pains, especially if a miasmatist, by offering him a sanitary or meteorological boquet, borrowed from the early history of Louisiana, such as no State of the Republic, during its colonial condition, can rival—if the very modern epidemical blight, (51 years), which has fallen occasionally upon that part of the population transplanted from remote climes, be excepted.—Descendants of Penn and of the Pilgrims! dwellers upon the rocky hills of New England and upon the mountains of Virginia, attend!—In a work published in Paris in 1803, on Louisiana, edited by M. Duvallon, founded on observations by a resident of New Orleans, it is asserted that the mercury never ascended in summer higher than from 24° to 26°.—86° to 90½° Fah.—nor fell in winter lower than 2° below 0, 28°. 4 F. This writer though less fascinated with the climate than his cotemporaries, sums up thus: "I say then, and I repeat it, that the climate of Lower Louisiana, (*le climat de la Basse Louisiane*) is vastly more salubrious than it appears to be upon the first view."*

La Harpe, whose official duties centered chiefly at New Orleans soon after its foundations were laid, kept a minute journal for five years before his return to France, in 1724. In a memoir on the state of the colony at that period, he estimates the inhabitants of New Orleans and its environs at sixteen hundred. The province, which had been settled long before the city, contained then only thirteen hundred negroes; the air was mild and healthy. The people knew nothing of the epidemics which had desolated other parts of America. *New comers* were liable to attacks of a *light* kind of fever, (*une fièvre lente*), which was attended with debility, without proving *mortal.*

According to La Harpe, the tidewater region was beyond all dispute very healthy, which he seems to attribute to the dry, sandy soil of the sea-shore. "Il faut aussi convenir que les côtes de la mer dont le terrein sablonneux est moins humide sont très saines, et dès qu'on a franchi ces bas-fonds, en avançant dans les terres on y jouit d'une santé très constante."†

Lozières, in his second voyage to Louisiana from 1794 to 1798,‡ maintains that the people of New Orleans are healthy; he seems to think this is owing to their using the water of the Mississippi river, which he regards as excellent.§ Indeed, such was the extraordinary salubrity of New Orleans in early times, so unlike the Anglo-American cities of the North, that a number of authors attempted to account for it from the using of the Mississippi waters by the inhabitants. Whether these explanations be correct or not, is foreign to my purpose in quoting them. Nor is the extravagance or exaggeration of some statements—the following for example—a material question:—

In a work entitled Travels in Louisiana, from 1794 to 1798, the author says, that "New Orleans is a particularly enchanting abode, (*un séjour*

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* Vue etc. 83, 93, 98.

† Jour. Histor. 355–6.

‡ 2 vols. Paris, 1803.

§ I. 313.

enchanté;) its air is so salubrious, its soil so fertile, its position so delicious, one has the belief that he is in the midst of a flower-garden, (*qu'on la croirait du milieu d'un parterre.*) The city borders the Mississippi, whose shores are favored by nature, and whose pure and agreeable waters have, it is said, the property of contributing even to the *increase* of the human species—(*ses eaux pures et agréables ont, dit-on, la propriété de contribuer même a multiplier l'espèce humaine.**) During a journey from New Orleans, up the river to *Pointe-coupée*, in Louisiana, each day, he says, revealed a crowd of new beauties! What richness! What elegance! Here nature is arranged in all her charms! The air flings forth nothing but the most voluptuous perfumes! What the poets have said of the Elysian Fields, is not fabulous—all their divinest conceptions are in these enchanting places, realised."†

The celebrated Count Vergennes,‡ "in his memorial to the French government, in the early part of the American Revolution," declared,—"I repeat again what I have already said many times, that Louisiana, without contradiction, is, from the sweetness of its climate, and its happy situation, the most beautiful country in the universe."

Du Pratz,§ affirms that life in Louisiana is not only agreeably, but of long duration to such as avoid debauchery.¶

Now the question is not whether the banks of the lower Mississippi are the *veritable* Elysian Fields or not, but whether they are the special abodes of the Angel of Death—not, whether every plant or swamp is ever flinging its sweet odors to the breeze, but, whether it send forth miasma, as Dr. Forry has said, to deteriorate body and mind, to bloat the one, and dement the other, bringing on premature old age, making boys old at fifteen, and, according to Dr. Prichard, shortening the mean duration of life? The direct and implied statements of these and other writers concerning the sanitary history of Louisiana, during its colonial condition, show that the horrors ascribed to malaria could have had no existence, but in theories,—in malarial abstractions.

I wish to notify all grave readers that this paper is here concluded —and, therefore, the following anecdote is not to be read, as it is mere surplusage, and, withal, tinctured with levity. The Parish of La Fourche, the great sugar-growing Parish, South of New Orleans, forming the littoral of Louisiana, where more than half the soil, sinking to nearly the same level as the Gulf of Mexico, is a vast salt water prairie-marsh—La Fourche where the other half, the higher portion, is annually inundated with fresh water, during the periodical rise of the Mississippi, except along the River La Fourche, which being an arm of the Mississippi, breaks through the banks of the parent stream, 10 minutes North of New Orleans—La Fourche, where one in every 913 persons is over one hundred years of age—a proportion 250 times greater than the average of the 86 departments of France—La Lourche, at once the most swampy and healthy part of the world, or at least of Louisiana, is the *locale* of the following anecdote taken from the Ascension Herald, of 1838: "We frequently hear," sys the editor, "persons boast of the health of their several neighborhoods, in very extravagant terms. A friend of ours living in the Parish of Lafourche, insists that no person

* 17. † 22. ‡ Darby, La. § Hist. La. 3 vols. Paris, 1758. ¶ i. 141.

was ever sick in his neighborhood, and that very seldom any person dies. He says that when the vicinity where he lives was first settled, the immigrants were generally very young, and had lived there so long, without seeing any body die, they did not know what death was. They did not travel much or they might have been better informed in other places. He says that at last one man about 140 years old died, and that they could not imagine what was the matter with him, but kept him four days sitting in a chair, when some traveller passing, told them the old man was defunct, and then they buried him."

II.—*A Case of False Aneurism of the Profunda Artery, with Disease of the Bone, for which a Ligature was placed on the Femoral Artery, and afterwards Amputation of the Limb.* By Thomas N. Love, M. D., Surgeon, 2d Regt. Mississippi Rifles.

John Arandes, æt. 29, private of company (K.) 2nd Miss. Rifles, native of Germany, stage driver, small stature, sanguine temperament, wounded himself with a pocket knife, in cutting meat which hung upon the edge of a barrel, in the upper part of the middle third of the right thigh, nearly over the femoral artery but external to it about an inch, while our regiment was encamped at the battle-ground below New Orleans in January last. I am informed by Dr. Workman who was called to the case immediately, that the hæmorrhage was profuse for a few moments, and flowed in jets, but was soon arrested by compression. In a few days afterwards, while at sea, the wound broke loose and bled freely for a little while, but was again easily arrested by compression. The case at this time was under the care of Dr. Kinchloe. The wound healed kindly, and nothing more was heard of the case until the 5th of August, when the patient complained of a rheumatic pain in the hip-joint of the affected limb. Upon examination, I discovered a small tumour under the cicatrix of the old wound. It had no defined edges, and seemed to consist of a simple elevation of the part. Being so near over the femoral artery, my attention was at once aroused, and I enquired particularly of the patient into the history of the case. The patient informed me that this "knot" had been there ever since the wound healed; that it had not grown any; that it had never given him any pain; that he had all the time done his duty as a soldier; that he had marched without difficulty or pain; that he was never able to discover the least pulsation in it. I examined the tumour carefully, and could not discover the slightest pulsation. The temperature was not above that of the other limb. The skin presented a healthy natural appearance. He felt no pain on pressure; no alteration in the tumour was perceived on pressing the main artery. The tumour was not very hard nor uneven in its shape.

Although I felt some apprehension that this was an obscure case of false aneurism, yet the patient was treated for rheumatism of the hip-joint. He suffered for several days excruciating pain, which was only allayed by opiates. He apparently recovered and was able to walk about in two or three weeks. My attention was again called to the patient about the first of September. I found him now complaining of severe pain near the old cicatrix, and upon examination of the limb, discovered considerable swelling, particularly on the outside of the thigh, under the *vastus externus* muscle. It now presented the appearance of a large abscess, being painful upon pressure, redness and heat of skin. The swelling occurring in this region and still without pulsation, the case was no less obscure than before. Perfect quiet was enjoined with low diet, with the determination to wait a few days the progress of the case. It was not until this time that Dr. Kinchloe and myself had a consultation upon the case and I became in possession of the true nature of the wound and character of the hæmorrhage. We were both inclined to the belief that it was a false aneurism proceeding from either the femoral artery, or one of its branches. We removed the patient to our hospital that he might be watched closely, and treated more carefully.

In a few days this abscess-like tumour seemed to point at the cicatrix of the old wound. It was soft and evidently contained a fluid. The whole limb was now extensively swollen, and the patient suffering with some fever. We determined to open this tumour and let out the fluid, and if it proved to be a false aneurism, we could take up the artery afterwards. On lancing the tumour it discharged freely a thin disorganized blood with some pus. We were now convinced of the absolute necessity of placing a ligature upon the femoral artery. In the evening I called to see the patient, and found the tumour still oozing a thin bloody fluid, containing some purulent matter. The next day the wound assumed an unhealthy appearance, and the swelling was not in the least diminished, but if any change, an increase of swelling under the *vastus externus* muscle.

The state of the weather and the patient's condition, which had prevented an earlier operation, were no longer sufficient reasons for a delay, and accordingly the patient was placed upon the table, on the 10th of September, for the purpose of taking up the main artery. Another plan was proposed by some of the physicians present, to lay open the tumour and let out the coagulum, if it proved to be an aneurism, and take up the artery afterwards. Feeling, however, confident that we were not mistaken in this being a case of false aneurism, I proceeded at once to place a ligature upon the femoral artery, about an inch and a half below the origin of the profunda. I, then, after dressing the wound, plunged the knife deep into the tumour, and following the knife, several large coagula came out. By running my finger into the wound and turning it transversely to the external side of the tumour, a large sinus was discovered filled with coagula. This sinus, which lay behind the vastus externus, could only be reached through a small aperture under the *rectus femoris.* I proposed to lay open this tumour by a deep incision on the outside of the thigh and let out the coagula, but it was not agreed to, as the patient was somewhat fatigued by the operation. As no fresh hæmorrhage occurred on laying open the tumour, we had some hope that our object had been secured.

Late in the evening, I was suddenly called to see the patient on account of hæmorrhage. Upon examining, I found that the bleeding proceeded from the incision into the tumour. It had ceased before I arrived, and had bled only about half a pint, but was evidently arterial blood. It was now evident that we had not accomplished any good by placing the ligature below the origin of the profunda. A firm bandage was placed upon the tumour, and a tourniquet left upon the limb, to be tightened if bleeding should begin, with a careful nurse to watch the patient through the night.

Sept. 11th. Bleeding occurred once during the night, and about a pint of blood flowed before it was arrested. The bandages were removed, and the wound in the tumour examined, which looked badly. The temperature of the limb was below that of the other leg. Swelling greater than yesterday. The patient looks badly—pulse over 100, and weak. We determined to take up the femoral artery in the groin and by free incision let out the coagula. The day was very unfavorable, cold and cloudy, like winter time. Before we were prepared for the operation the patient had a severe chill, and we were therefore compelled for the present to direct our attention to his constitution. We gave him quinine and Dover's powder, and infusion of serpentaria.

Sept. 12th. The patient rested badly. Free hæmorrhage occurred once in the night, and once this morning—swelling much greater—pulse 120—tongue coated with a white fur.

As soon as convenient, the patient was placed upon the table for the purpose of placing a ligature upon the main artery. It was now a question of deep interest which would be the safer and better practice, this, or amputation at once. The limb was extensively swollen and the incision in the tumour dark and gangrenous, and the discharge from it very offensive—the constitution of the patient suffering very much. If we placed a ligature upon the artery and failed in saving the limb, it would then be too late to save his life by amputation. The chance, at this time, of saving his life by amputation, offered a reasonable hope of success, and it was therefore determined upon immediately. The instruments were sent for and I proceeded to the operation assisted by Drs. Kinchloe, Strother, McQueen, Workman of our Regt., and Asst. Surgeon Bell of the Va. Regt. The anterior and posterior flaps were prefered, that as much of the limb as possible might be preserved. The amputation was performed near the trochanter major. In making the posterior flaps, the large aneurismal sac was opened and a very large coagulum fell out. Several small aneurismal sacs were found in the region of the flaps which were dissected off. It was only necessary to take up the profunda artery. The bleeding soon ceased; the flaps brought together and the patient placed on his matrass. He bore the operation finely.

Sept. 13th. Patient rested well last night. Suffered some from sickness, owing to the amount of morphine and stimulants given during the operation. Discharges from the wound very offensive.

Sept. 14th. Patient rested well last night; some fever; pulse 100; tongue foul; bowels costive.—Prescribed Ol. Ricini. The wound looks well; the discharges from it still offensive.

Sept. 15th. Patient rested well last night—appetite better—no fever—tongue foul—pulse 95.—Ordered Port wine and Arrow root.

Sept. 16th. Rested well until midnight; since then has suffered from diarrhœa—his discharges thin, watery and very offensive.—Prescribed stimulating enema, followed by one of starch and laudanum; paregoric and pepper-mint every four hours. *Diet.*—Chicken, toast bread and a little port wine. The stump doing well—suppurating very finely—discharges from it not so offensive. The largest portion of the wound seems to have healed by the first intention. The dressings were all removed and reapplied.

Sept. 17th. Patient much better this morning—no fever—pulse 90—tongue improved—appetite better—only one passage last night—more consistent.—Prescribed Rhubarb 10 grs. Castile soap 16 grs. Opium 2 grs. in 8 pills; 2 night and morning. The stump looks well; flaps fit closely; suppuration healthy; stump not so painful as yesterday.

Sept. 18th. Patient doing very well with the exception of diarrhœa; is much reduced—no fever; appetite not so good—stump improving.

Sept. 19th. Patient suffering very much with diarrhœa; discharges very thin and white, and very offensive—no pain in the bowels—℞. Pulv. Kino 30 grs. in 3 papers, one every 4 hours, with 2 teaspoonfuls of paregoric, and at bed time, enema of starch and tinct. opii 50 gtt.

Sept. 20th. Still suffering with diarrhœa; discharges sometimes involuntary—very weak—no fever—no thirst—℞. Opium 4 grs. Act. Lead 8 grs. in papers; one every 3 hours. The stump still looks well—the suppuration healthy.

Sept. 21st. Diarrhœa still incontrollable—passages involuntary—patient much exhausted. Continue the prescription.

Sept. 22nd. Patient some better—he has had as many as a dozen passages during the night; last night however he had only two, and these more consistent. He rested well under the influence of opium. Give him nourishing diet—coffee; and a little Brandy toddy.

Sept. 23d. Patient much exhausted, emaciated, weak, pulse almost imperceptible. Slept well during the night—no appetite; nor has he had any for several days. Give Brandy toddy freely to day. The stump not doing well: the healing process arrested—the absorbents have carried away the cushion-like muscles, and the suppurating surfaces look flabby and pale. Should the patient recover, of which I have no hope, I fear that I shall have great difficulty in healing the stump owing to the great debility.

Sept. 24th. Patient died last night.

Autopsy. Dissection of the amputated limb showed that the wound in January had injured a branch of the *profunda* artery, and perhaps the bone. A spiculum of bone an inch long and half an inch in diameter was found growing from the inner side of the femur, under the cicatrix of the old wound. Several strong tendinous sacs were connected with this bony tumour. The principal one of which extended down the femur about 2¼ inches, and by its pressure had caused caries of the bone an inch wide, and one eighth of an inch deep. The large aneurismal sac on the outside of the thigh communicated with one of these small tendinous sacs. This large sac was irregular in its figure, supported by strong bands formed from the cellular tissue, resembling the *chordæ tendinæ* of the heart. The main sac communicated with a number of other pouches both above and below by means of small apertures. The whole limb was much swollen, the cellular tissue being filled with yellow serum.

Dissection after death exhibited extensive ulceration of the bowels, confined to the rectum, descending colon, and a portion of the transverse. The ulcers were numerous in the sigmoid flexure of the colon: many of them more than one fourth of an inch in diameter, surrounded by a dark purple ring. Others presented a dark purple spot, and by gently scraping the part with the handle of the scalpel, the slough was removed, leaving the well defined edges of the ulcer. In the transverse colon the incipient ulcer consisted of a red elevated pimple resembling a variolous pustule of the fourth day.

The ligature upon the femoral artery (which was applied on the 10th) had come away, leaving that vessel beautifully closed with an organized coagulum.

The one upon the *profunda* still remained. The coagulum in this vessel had formed strong adhesions to the internal coat of the artery.

Remarks.—This case illustrates the importance of the timely use of the knife, and the great importance of being certain from what artery the aneurism proceeds. Had a ligature been placed upon the *profunda* when I first discovered the case, the operation would probably have been successful, notwithstanding the disease of the bone, which was perhaps in a state of inflammation when the patient was referring his pain to the hip-joint. It is hardly probable that the result would have been favorable at the time the operation was performed, had I placed the ligature upon the *femoral* artery above, instead of below the profunda artery, owing to the diseased condition of the bone and the extensive swelling of the limb. I am satisfied that the amputation was justifiable, and when the extensive disease of the bone was revealed by dissection, I was only more firmly convinced of its propriety. But his constitution had suffered so much, and the lymphatic system was so completely charged with poisonous matter, that when the bowels began to throw it off, I was unable to save my patient; notwithstanding all my efforts to restore him.

Buena Vista, Mexico,
October, 1847.

(We are much obliged to Dr. Love for the above exceedingly interesting case, and would be glad to acknowledge many such favors from our Army Surgeons, but we presume that their arduous duties do not allow them opportunities to report. They doubtless have extensive and valuable stores from which we hope to draw largely at some future time. Dr. Love informs us that he attempted to send us this report sometime since, by the hands of Sergeant Lott, of the Miss. Rifles, but that the messenger was unfortunately slain by the Mexicans on the route.—EDRS.)

III.—*Lecture, delivered introductory to the Course of Physiology and Pathology, in the University of Louisiana.* By JOHN HARRISON, Professor of Physiology and Pathology.

The following correspondence will explain why the following Lecture appears in this Journal.

NEW ORLEANS, Nov. 18th, 1847.

SIR:—The undersigned Committee, on behalf of the Students, composing the Medical Class of the University of Louisiana, would respectfully solicit a Copy of the introductory Lecture delivered by you before the Class on Wednesday the 17th Nov. inst., for the purpose of Publication in the N. O. Medical and Surgical Journal.

The Committee would add their individual wish to that expressed by the Class, and indulge the hope that you will readily assent to their request, inasmuch as they believe it will contribute materially to increase their zeal in the pursuit of that important branch of Medical Science.

JAMES SHEIL.
J. DORRY.
McILHENNY.
JAMES R. RHODES.
STEPHEN SMITH.
D. A. COHEN.
J. BROOKS.
R. S. McCRACKEN,
Chairman of the Committee.

To JOHN HARRISON, M. D., *Professor of Physiology and Pathology, Med. Dep. of the University of Louisiana.*

NEW ORLEANS, Nov. 23d, 1847.

GENTLEMEN:—Your letter requesting a copy of my introductory lecture for publication in the N. O. Medical and Surgical Journal, has been received by me. For the compliment conveyed in the request, receive my thanks. The lecture is at your service, since, though no one can feel better than myself how imperfect it is, I know not that I have a right to withhold it from you.

Your friend
J. HARRISON.

To Messrs. McCRACKEN.
SHEIL.
McILHENNY.
RHODES.
SMITH.
COHEN.
BROOKES.
} *Committee of Medical Class.*

GENTLEMEN OF THE CLASS,

The branches of Medical Science allotted to this chair are Physiology and Pathology.

To give a full course on each of these branches in the time allotted to the lectures, is altogether out of the question. We will therefore pursue in the course now commencing an eclectic method. We will endeavour so to combine these different branches as to make them illustrative of each other. By passing over points that are unimportant, or which you may easily unravel for yourselves either by reflection, or by reference to books, we will try to compress into the course, the largest amount of information on important subjects. We will take up those subjects in detail, which contain within them the general principles of all medical science; and impressing those principles upon you and making you fully understand them; we hope thereby to throw light on the rest of your studies and enable you with far less labor and perplexity to master the other collateral branches of the profession you have chosen.

The word 'Physiology' is derived from two Greek words: φυσις, nature, and λογος, a discourse. In its widest sense it would therefore comprehend the philosophy of all nature—or, in other words, the philosophy of all things. But in this general sense it is now seldom used—the word has been restricted in its application to the philosophy of living beings. Physiology then, in the sense now generally received, is the 'Science of Life'—but as expressive of this, the word 'biology,' which has been preferred by Treviranus, would certainly be more proper and consistent with etymology.

Physiology is the science of *Life!* But what is Life? This is a question to which an answer is certainly not be attempted at the commencement of our studies. We must take a wide and accurate survey of many general phenomena; we must weigh, analyse and determine the value of many separate facts, ere we can even approach the discussion of this question. Passing over then for the present, the essence of their difference we will take the obvious fact, that on the earth we inhabit, there are evidently two classes of beings, which display widely different phenomena. To the most heedless and unobserving eye this is apparent, and not only the philosopher, but the most unlettered peasant has to some extent arranged and classified these beings. The first class we call 'dead or brute matter'—the inanimate creation. To the other, we give the name of living beings.—The science that treats of the first class of beings, that records and generalises the phenomena which they display—and by so doing establishes what are called general laws, is Natural Philosophy. The science that treats of living beings;—in which are recorded the phenomena peculiar to them, and the study of which is the investigation of these peculiar phenomena, is, as I have already told you, Physiology or Biology.

But here arises important matter for consideration. When men first turned their attention to physiology—the physical sciences, or the first class having been but imperfectly studied and of course either misunderstood or not comprehended at all, the discoveries made during the investigation of vital phenomena could in no way be accounted for, by the principles brought forward and accepted in explanation of phenomena that occurred in brute matter. Upon what principles, for instance, was it possible to explain the beating of the heart from the embryo state to death in old age? How was it possible to account for the healing of a wound or an ulcer? It was ascertained that the juices of the stomach dissolved vegetable and animal substances introduced into its cavity; —how explain this fact? Was it by fermentation or putrefaction? Investigation seemed to prove that neither of these processes took place during digestion. Was it then a chemical solution that took place? If so, why was not the stomach itself dissolved by the juices it secreted?—And to give weight to this latter argument it was discovered that, after death, the stomach *did* undergo digestion—it *was* dissolved by the fluids which it had secreted. Here then was positive proof to the earlier investigators, that living beings did not obey the same laws with brute matter;—the facts they saw were not explicable on the principles of physical science.—According to custom, then, hypotheses were invented to smooth away these difficulties. Reason and observation were at fault—the problems to these powers were insurmountable. But the mind of man is not likely to rest easy under a conviction of ignorance—it will cheat itself rather than make this submission; and, in fact, to stop when the light of observation fails us; when we feel that we are on the limits of truth, and that to take another step will be to dash into shadowy realms where all our toil and wanderings must be profitless, requires not only the sagacity of a philosophic mind, but an innate strength fortified by the warning and admonition of history. Finding then, vital phenomena physically inexplicable, what was the recourse? An obvious one—one which has never been wanting in all similar exigencies. If reason could not untie the knot, imagination was at hand to cut it. Hippocrates brought forward his φυσις or nature—Aristotle his 'moving principle' Stahl, his 'anima'; Van Helmont, his 'archœus'; Hoffman, his 'vis medicatrix naturæ'—Barthes, his 'vital principle' and so on. Many of these fantastic words, were the names of imaginary beings supposed to reside in the living substance and to control and direct the phenomena of life;—of course, these were mere conjectures, and even as such, each in its turn was overwhelmed and buried beneath the multitude of objections wherewith it was assailed. Bichat, protesting against all speculations into the ultimate causes of vital phenomena, took another course and seizing some inexplicable phenomena, he endowed the organs which exhibited them, with *properties:*—thus the contraction of a muscle is a phenomenon, and Bichat strangled the difficulty of explaining it, by saying, 'that the muscle contracted because it was endowed with the *property* of contractility, &c.

Physiologists of late days have consigned the properties of Bichat to the common tomb, in which repose the errors of Genius. Of his five 'vital properties,' two are recognized to be the creatures of his own brain and to be altogether supposititious and fanciful. The other three are phenomena, towards the explication of which, we gain nothing by endowing the organs which display them, with the property of displaying them.

It is strange, that not one of these older physiologists seems ever to have raised a question concerning the completeness and accuracy of the principles into which he was unable to resolve vital phenomena. None of them seems to have doubted that the principles of physical science were full, inexorably fixed, and complete—not one of them appears to have doubted, that in the attempt to account for vital phenomena by physical laws, the widest possible observation had been taken on both sides, and that no important element had been omitted in the calculation. Such however was not the fact. Since the days of Newton, the physical sciences have made rapid and unprecedented progress; and as they have advanced, so have they thrown light on the phenomena of life. In fact, we have now attained an eminence, whence we can see that all vital phenomena (with the exception of its pschycological department) are the manifestations of those general laws which govern every change in the condition of brute matter;—that the sole difference lies in the difference of circumstances under which they are manifested, and that if our knowledge concerning the detail of these phenomena is imperfect, it is because the physical sciences, on which the explication of them depend, are themselves imperfect.

The study of Physiology, then, will be the study of physics or natural philosophy viewed under circumstances which modify the results, and give us all those peculiarities which we designate by the epithet 'vital.' If, therefore, you come unprepared to this study, you will find the task an extremely difficult one; and in proportion as you have paid more attention to the sciences of Chemistry, Mechanics, Hydrodynamics etc., will you outstrip your competitors in the study of physiology.

In philosophical studies, the method taken to arrive at truth, is this. We observe a great number of phenomena, and according to the analogies remarked among them, we classify or arrange them together. And this arrangement—this selection of particular phenomena from the chaos of nature;—separating them from others to which they bear no relation, and the forming of them into groups, showing thereby the dependence of one fact upon another, is Science. In this manner have the several departments of natural philosophy, such as chemistry, optics etc. been divided off and arranged. In each of these departments we observe as far as practicable all possible phenomena. We increase our knowledge as much as lies in our power, by experiment. We observe wherein facts agree and disagree, and classifying the points of agreements we mount to general laws and ultimate facts, and by so doing, we find that these general laws are few in number and that the great diversity of natural phenomena arise entirely from the circumstances under which they appear. They are all merely different manifestations of the same general principles. To give you an illustration of this, let us take a fact from the history of astronomy. When Copernicus revived the theory of Pythagoras, he was obliged to admit of three motions in the earth—one around the sun,—another on its own axis, and thirdly, of its poles to and from those of the ecliptic. At that time it was impossible not to infer that each of these motions had a different cause. But after Newton had written on gravitation and had investigated and established the laws of motion, it was placed beyond doubt that these different motions of the earth were all dependent on one single cause, viz—an impulse given to it in a direction passing NOT through the centre of gravity.

You will frequently hear of the "laws of nature" and you must know what is meant by the phrase. The difference, between an ultimate fact and a law of nature, is this: A fact is, from its very nature, particular—it is one thing and nothing else—but a law is a generalization —an expression of many facts in one word. For instance—if we place a bottle under an air pump, exhaust it and place it in this condition over water, the fluid will rise in the vessel. Here is a phenomenon;— but if we continue our observation we shall find that the pressure or weight of the atmosphere has forced the water up into the bottle—we then ask what pushes the atmosphere downwards, and we find that to this question we can obtain no answer,—there is nothing beyond that we are aware of—we have then reached an ultimate fact.—It is an ultimate fact, that the air tends towards the centre of the earth—*why*, we don't know. But examining other substances we find that they too have the same tendency;—in fact, that all bodies, as far as we know, tend towards the centre of the earth.—Here, then, we have many ultimate facts;—and we express them all by saying that it is a law for all bodies to tend to the earth;—or in other words that gravitation is a law of nature.

After having established a general law like this, we may extend our observations and see how far circumstances modify the manifestation of it;—and by generalizing again we may establish more particular laws. Thus, in the case already cited—after having found out that all bodies tend to the earth, we may go further, and making a record and analysis of all the circumstances, we generalize them and establish the particular law, 'that bodies tend to the earth with a force directly proportional to their mass and inversely as the squares of the distance.' Here you see there is a generalization of the two circumstances—bulk and distance. In the same manner have the laws of all other departments of science been established.

The laws of nature, then, being framed from a wide observation of phenomena, it is plain that we may descend again from these generalizations and apply them to particular phenomena; and in any hypothetical case, in which *all* the circumstances are mentioned, we may predict the results that will occur; and it is plain moreover that if we have taken all things into account in framing these laws, our predictions *must* be true; but if we have not taken all circumstances under review—if our generalizations have been partial, we must be frequently involved in error. Astronomy and those other sciences in which the principles of moving solids are involved, are almost perfect; because a complete and accurate record of the circumstances that modify the general law of gravity, have been made out and reduced to mathematical formulæ. But the science of Chemistry and Hydrodynamics are imperfect, because no such generalizations have been made. Put a hypothetical case to an astronomer, telling him the relative positions of the earth to the other planets and the sun, and he will predict most accurately the result; but ask a chemist what will be the results, when three or four elementary substances are mixed together for the first time, and it will be impossible for him to tell you; yet this he should do and would do, were the laws of his science accurately established.

In this severe method, i. e. by a close observation of facts and a subsequent generalization of them, have all the sciences, which have made much progress, been pursued. This is the Baconian method; and it is in fact the only truly philosophical method. All our knowledge must come in the first place from observation. Of nature and her phenomena we know nothing intuitively. We must make observations, and by reflection, generalization and abstraction, we form a science from the facts we gather. We can thus give to each phenomenon its particular cause, and to each cause its particular effect as we see them exhibited in nature. But, if leaving this method, we should adopt a contrary course—if taking some phenomenon in the gross, we should set about to imagine an explication of it, there can be no end to the labyrinth of error in which we involve ourselves. We must observe nature with a close and wary eye; we must return, again and again, to the same investigation;—we must assure ourselves that in any particular case we have observed *all* the phenomena and taken into the account all the circumstances connected therewith. Until we have done this, any theory that we may frame will be frail, and at the touch of time all our fine spun hypotheses will moulder away. It will be seen, that as accident or investigation develops new facts, that our theories are fanciful merely, or but the segment of some greater circle—of some more universal truth. Time, it is said, throws a mellow light over the works of the artist—the abrupt shades are fused into each other, commingled and softened; the gaudier tints are broken down, and the whole is touched and beautified with a grace unattainable by the efforts of man:—so, too, does Time operate with regard to human philosophy; —the bold and false is eaten away; the garish sinks into the chaste; and the tablet remains,—the transcript of truth and nature.

This spurious philosophy—this dealing in hypotheses and fanciful theory, has from the earliest times attached itself as if by prescription to the science of Physiology. Can it be believed that whilst these false gods were banished from all other temples, they should continue to infest, even, to our own days the most beautiful of all? It is true, nevertheless; and it is lamentably true. It is not long since that by a few well directed experiments, Magendie overthrew a theory which had been received from the earliest times, and which, with the exception of a few voices heard now and then in conjectural dissent, was unquestioned. And this theory had been established;—on what? On four imperfect and partial experiments performed by John Hunter. The doctrine of lymphatic absorption, which had been so long received, which had been so firmly believed in, which in fact was considered as one of those truths demonstrated and concerning which there could be no further debate;—a few well directed experiments sufficed to entomb. There cannot be a better illustration of the absolute necessity of discarding every thing in philosophy, that is not proved. Conjectures now and then may happen to alight on truth, but until we have interrogated nature by experiment, they must lie in abeyance and be considered as probable only. It must be plain, that we can know nothing of nature but what she reveals to us; and to spend time in conjecture is to waste it; for though we should imagine the very truth, the exact and whole truth, how, until we have put our thoughts to the test of experiment, could we be *certain* that we possessed it? It is plain that until the test is made, the conjecture, however good, is probable only. Facts we must receive from nature: we cannot make, nor conjecture, nor imagine them. It is left to the mind to arrange and classify them when received;—to discover hidden analogies—to generalize and abstract upon them; and in this way, mount from scattered and isolated phenomena to ultimate facts and all embracing laws.

Upon these ultimate facts or general laws does science rest; and accordingly as we can resolve phenomena into them, do we unfold the beauties of nature and advance in knowledge. The universe presents one wast series of cause and effect—a constant and perpetual change is going on around us and within us;—and nature, like the subterranean cities of Italy, is a hidden fabric which the intellect of man must uncover and reveal. Upwards to these ultimate facts we can trace the series of phenomena, but beyond them we cannot advance. Why, when one body strikes another, the first imparts its motion to the second; why, an acid will leave one metal to conjoin with another; why, when a body is left unsupported it will return to the earth, we know not;—they are ultimate facts which we must take as we find them—we do not know the efficient causes of anything. We can only record phenomena as they are invariably connected. "Hence," says Mr. Hume, "we may discover the reason, why no philosopher, who is rational and modest, has ever pretended to assign the ultimate cause of any natural operation, or to show distinctly the action of that power, which produces any single effect in the universe. It is confessed, that the utmost effort of human reason is, to reduce the principles productive of natural phenomena to a greater simplicity, and to resolve the many particular effects into a few general causes, by means of reasonings from analogy, experience and observation. But as to the causes of these general causes, we should in vain attempt their discovery; nor shall we ever be able to satisfy ourselves by any particular explication of them. These ultimate springs and principles are totally shut up from human curiosity and inquiry."

But as we have already said, the farther we can trace complicated phenomena to general laws, the more we advance in knowledge, and the more clearly do we understand the subject;—so that science would be perfect were it possible to resolve all phenomena into one single general law. The study of natural philosophy has demonstrated the applicability of a few general laws to many of the phenomena that occur in brute matter, and in proportion as each department of natural science has been thus unfolded and made to harmonize with the others, has the whole become more clear and intelligible. In truth, we feel instinctively that a science is imperfect in proportion to its number of inexplicable and unconnected phenomena. It is the instinct of our understanding to believe that some invisible link connects these apparently isolated facts. Nor can we rest content, when, studying a science, we find many phenomena which cannot be shown to depend on some more general law. We cannot receive them as ultimate facts—for these we know are few, simple and distinct. In an ultimate fact we feel convinced that we have taken all the possible circumstances under consideration. In the fall of a stone to the earth,—the earth, the stone left unsupported at a distance from it, and the subsequent conjunction of the two, make up the entire phenomenon. We have never discovered anything else;—either prior, intermediate or subsequent. *Why* should the stone return to the earth? Why should it not as well remain stationary in space occupying the spot where we have left it? Or, why should it not as readily fly upwards, or move to the right or to the left, or perform any other motion? *We know not;*—we find that it *does* return to the earth;—we have the fact—a simple, ultimate, inexplicable fact. But it is otherwise with regard to those phenomena of which we now speak.—

Here we feel convinced that all the circumstances of the phenomena are *not* taken under review—we feel assured there is something hidden from our observation;—something, which, if we had possession of, would, like a burst of sunrise through the fogs of morning, reveal the face of nature.

Illustrations of these remarks might be drawn from the science of Chemistry and particularly from the subject of Galvanism, which is a part of that science; but we may find examples as appropriate in our own peculiar province. There are many physiologists, even of the present day, who seem to think that nutrition, absorption, secretion, generation—the action of an organic muscle (as the heart) and the contraction of a voluntary muscle, are as yet inexplicable phenomena. But even admitting that they are, can we concur with Bichat and receive them as *ultimate* facts. We cannot:—we feel when contemplating them that something is concealed from us—some other facts are wanting which perhaps would reveal the mystery and reconcile them all:—in other words we feel that *all* the circumstances, that make up the gross phenomenon, are not in our possession. A muscle contracts—and then returns to its original condition,—the state of relaxation. If we pay attention to what passes in ourselves, we find that a desire of performing some particular motion has forerun the contraction. We will;—the muscle contracts: here are too distinct phenomena. Between them we see no connection, save that a nerve communicates with the muscle and brain;—the intellectual organ. But the muscle may be some feet distant from the brain; and we know, that if we cut the nerve, no contraction of the muscle will follow our volition. We are, therefore, forced to conclude that a change of some kind has taken place in the nerve, which change is invariably prior to the effect;—in other words, causes the contraction. Of the nature of these changes we are confessedly ignorant;—they perhaps occur among the ultimate atoms of matter, and if they do, we are likely to remain in ignorance.—Knowing not what those changes may consist in, it is plain, we do not know all the circumstances of the phenomenon;—it is not then an ultimate fact, but an inexplicable one.

And here let me tell you that in all those physiological phenomena which we have just enumerated, the train of particular facts is yet unrevealed to us. We are still ignorant of the precise sequence of changes that occur from the beginning to the end, in any one of these phenomena. But, we have accumulated so many facts of late both in physiology and physics; so many analogies have been observed among these vital phenomena, both with regard to themselves, and to the phenomena that occur in brute matter, that we are assured they all depend on the same general laws;—and this inference of our reasoning faculties is confirmed by the fact, that if we take the general laws of physics and apply them to physiology, we find that they are applicable to most of the facts and explain in a satisfactory way all those general actions of which we have spoken above. "In good philosophy," says d'Alembert, "every inference which is supported on facts or truths already known, is preferable to that which is only upheld by hypotheses, however ingenious they may be." Such being the case, it is evident that if we can explain vital phenomena on physical laws, known and established; it is better than to have recourse to the 'vital principle': for however respectable from its age this agent may be; and whatever influence it may have received from the great minds that have bowed to its authority; it is but an hypothesis at the best;—an unknown something brought in, in order that facts, which at the time were not explicable on the principles of physics, might be referred to some common point.

But there is another and important benefit gained by referring vital phenomena to physical laws. I have already told you that proportionally as the train of phenomena was laid open and all the particular facts shown to depend on a more general law and on each other, the whole subject of our study became more clear and intelligible. Now, it is plain that if in any case we can do this, we must know how far any one phenomenon differs from another; for if we do not, we most certainly cannot refer them to a more general law. Let us suppose, then, we have come to the conclusion, that nutrition, absorption, secretion, muscular contraction, &c., must be referred to the 'vital principle;'—is it possible for any one to say in what one of these processes differs from any other? It is not:—in truth, the very reference of these phenomena to the *vital principle* is a confession of ignorance;—it is saying that these functions are performed under the guidance of a certain something, of which we know nothing and the existence of which we are only cognizant of by the effects it produces. But on the contrary, if we succeed in referring these phenomena to the general laws of physics, we must have made up our minds as to what the difference among them consists in. The very fact that we can so refer them, presupposes that we can show wherein one differs from another.

Let me repeat then, that if on these occult points we are not in possession of all that is desirable;—if we cannot go into detail and tell you the precise series of cause and effect;—still we possess so many facts, both in physiology and in physics;—the *general* nature of the phenomena are so well explained by applying to them the general laws of physics; whilst on the other hand there are so many difficulties, obvious and insurmountable, to the hypothesis of a vital principle—that I cannot conceive how any one who examines the subject, can hesitate one moment which theory to prefer.

In these introductory remarks to our course, you may have observed, gentlemen, that I have said nothing concerning the importance of Physiology in regard to the practice of medicine.—The best method of convincing you on this point is to teach you what physiology is. I doubt not that you will soon perceive that all correct treatment and all the improvements we are to hope for in our profession, depend on the advancement of our knowledge concerning—1stly, the science of organization, or in other words Anatomy—2dly, of those actions going on in the organization, or in other words, Physiology,—in which word Pathology is included—for Pathology is the Physiology of the sick man—3dly, of the special influences which external agents exert on the organism. Medicine, in time, will be nothing more than this knowledge systematized and put in practice.

IV.—*Address delivered before the Physico-Medical Society of New Orleans, November 27th*, 1846. By R. M. Graham, M. D.—Published by request.

This is the 25th Anniversary of the Physico-Medical Society of New Orleans. It was ushered into existence under auspicious circumstances, and for a period of time its progress was characterized by the most triumphant success. Its usefulness was visible to all in the honorable and active emulation which it inspired amongst its members. In its temple, the ardent cultivator of our science was wont to offer up his daily sacrifice, with an assiduity, characteristic of the true love of Science. But like similar institutions, in order to be permanent in its duration, and useful in its results, it required the utmost harmony amongst its members. By this it is not meant that all should blindly obey the dictates of a self-constitued leader or clique; on the contrary, its very genius not only admitted, but elicited all the opinions of all its members, upon whatever question might be legimately brought before it for discussion. As long as this course was pursued, its march was onward and upward,—as soon as it deviated its decline was marked, and its downfall inevitable.

The harmony of which we speak, as essential to the success of all associations, is that personal decorum which should mark the conduct of every gentlemen, not less in his intercourse with the world at large, than with those with whom he is associated. Much, too, of the success of all societies depends upon the individual conduct of each member.

Certain events, gentlemen, have once or twice interrupted the harmony of this society, but, upon this the first Anniversary, that commemorates its resurrection, let us vow to each other most solemnly that it shall be sustained—that no personal feeling, or differences of opinion shall ever lessen our fidelity, or swerve us from our purpose to maintain the usefulness of the Physico-Medical Society. While this course was pursued, this society, in its early history, was prosperous, but so soon as it wavered and its members became disaffected, its downfall was certain.

To be useful therefore and respected, every member should be tolerant, and respect, if he does not receive, the opinions of all; and to carry to a successful issue the object of association, every individual member should divest himself of all prejudice upon all questions, and having no other object in view than the promotion of science, should emulate without malice those with whom he is associated.

This Hall is not an arena for the conflict of passion, or the display of personal malevolence, and it is the paramount duty of every member to religiously avoid anything that can mar its harmony or detract from its usefulness.

All societies which have a different object in view are based upon the idea that in "union there is strength" a fact we see illustrated every day in the rapid march of improvement in governments, and the arts and sciences.

The progress of Medical Science is undoubtedly one of the most momentous subjects that can engage the minds of men; momentous because that science which has so many direct and intimate relations with all the varied and complicated affairs of human life—so close a connection with all that pertains to the well-being of man and the development of humanity, cannot be otherwise than momentous. Health is man's first consideration; for without it he can do nothing. The most important operations of human life depend upon it. The labours of the statesman —the researches of the philosopher and the handiwork of the artist must all cease in the absence of health. Hence the preservation of health, and the prolongation of human life, become of the first importance, and the Science of Medicine the most important of all sciences. If such, then, is the importance of Medical science, the means of improvement become a subject of the gravest consideration. The history of medicine abundantly shows, that although the progress of medical science has been slow, the most happy results have followed every improvement— that human life has been prolonged, and human miseries largely diminished. To medicine, more than to any other science, are the world indebted; for by it, more than by any other one thing, have the evils, to which life is subject, been either alleviated or destroyed. Of all things life is the dearest; and hence the deep solicitude felt by the world at large for the progress of medicine? I propose on the present occasion, in the discharge of the duty this society has done me the honor to impose on me, to offer some observations *on the past and present state of Medical Science, and the means of improving its condition.* The present is undoubtedly the most brilliant epoch in the history of medical science, but although of all science that of medicine is probably the oldest, yet it is a lamentable fact, that it is also the least perfect of all sciences. With truth it may be said, that since the days of Galen, and even of Hippocrates, until the present age, little or no progress was made in it. The medical learning of one age consisted only of the senseless jargon and absurdities of the preceding; and from all the records now remaining, it cannot be discovered that medicine in the days of Galen, or even a much later period, in the time of Leonardo da Vinci, had advanced much beyond its condition in the time of Hippocrates. The Roman physicians only repeated the practice of the schools of Rhodes. Cnidos and Cos, and the "Cnidian sentences," and the works of Hippocrates, were the only text-books in the Medical Schools of Crotona, Syrene and Alexandria.

Plato is said to have known the circulation of the blood. "The heart," says he, "is the centre of the blood vessels, the spring of the blood, whence it flows rapidly round; blood is the pabulum of the flesh, in order to the nutriment of which the body is intersected by canals, like those of gardens, to convey the blood like water from a fountain to the remote parts "of the body." From this it must be inferred, that he had a tolerably correct idea of the circulation of the blood, though it is probable he possessed no information regarding the matter, founded upon actual experiment and anatomy. To show how little the ancients did know of the anatomy of the human body and of physiology, it is sufficient to remark, that until the time of Harvey the valves of the veins were entirely unknown 'and that it was taught by Galen and all his successors until Harvey's time, that the *liver* instead of the heart was the great centre of the vascular system, the veins conveying the blood from the liver to all the remote parts of the body. Hippocrates did not know that the nerves convey sensation, or that any of them are connected with the brain. Motion he supposed to be caused by all the tendinous white cords throughout the body without distinction. Of anatomy he knew next to nothing, and still less of physiology. It was a grave question among the anatomists of antiquity, and one which for a long time occasioned great disputes, whether the fluids we drink pass into the body through the trachea or œsophagus.

The "divine" Plato was a fine advocate of the œsophagus. Aristotle, whose knowledge of anatomy was much superior to that of his predecessors, believed that the brain was entirely unsupplied with blood—that the heart contained three ventricles, and that there were only eight ribs on a side. Galen, the most celebrated, and at the same time the most accurate and voluminous anatomist and physiologist of antiquity, is supposed never to have dissected a human body, and recommends in various parts of his works the dissection of asses, bears, goats and other animals. From this we are not surprised to learn, that he did not know the cause of the veins conveying a different kind of blood from that of the arteries. In surgery the ancients were more advanced. Hippocrates wrote ten treatises, which show that he was a skillful surgeon and that surgery was in a state of perfection in his day truly surprising. His works *De Tracticiis* and *De Articulis* exhibit great scientific skill in reducing fractures and luxations, and in his work *De Capitis Vulneribus* he gives minute directions about the line and mode of using the *trephine*. The *basso-relievos* of the ceilings and walls of the temples of Tentyra, Kornac, Luxor and other places sufficiently attest the skill of the Egyptians in the amputations of limbs, and the excavations at Pompeii afford us a large number of finely wrought surgical instruments, showing clearly that the surgery of the ancients was in a high state of perfection. Lithotomy was an operation well known to them, as is manifest from the works of Hippocrates. Ammonius, of Alexandria, about the time of Christ, performed the operation of lithotrity, and Celsus, in his work, gives a minute description of this operation. Bronchotomy was performed by Asclepiades before the time of Ammonius. Cælius Amelianus, who lived in the second century, spoke of the operation of paracentesis as a cure for ascites. All the major surgical operations were known to them and most of the minor ones. They produced works on many branches of surgery, some of which have continued to be consulted down to our own time. Haller quotes with admiration the works of Hippocrates, and in many respects perhaps these works have not been improved. In pathology the ancients were not as far behind the moderns, as in anatomy and physiology. Hippocrates wrote eight works on the subject and Galen three. The humeral pathology, which referred all maladies to the four cardinal humors, as they were called, namely, the blood, bile, mucus or phlegm, was that of both Hippocrates und Galen, and the same absurd theory has been advocated even in our own times. This humeral pathology for ages bound the medical world and impeded the progress of enlightened opinion. Few dared to avow sentiments or opinions in opposition to those inculcated by Galen, and fewer still were able to invent any thing better. In very recent times, manifold pathological theories have been started and for a time Love held sway over the minds of the profession. *And* we have the Cullenian, Brunonian and the Broussaian doctrines, the doctrines of the humoralists, the solidists, the organists, and the chemical and mechanical doctrines; the Brownonian and the Thomsonian, the Homœopathic and the Hydropathic, making altogether the most discordant and heterogeneous mass of theories and hypotheses that the world has ever seen. The doctrines of the ancient medical sects, the Methodici, the Hippocratici, the Empirici, the Pneumatici, Calcatici and the Essinguthatici, were not more absurd and unsatisfactory, and a thousand times more creditable to them. Much that has been advanced on pathology by the moderns, has consisted of pure conjecture and wild speculations, having no better or stronger claims to our consideration than the opinions of Hippocrates and Galen. Indeed, these ancient authors have not in many respects been improved, and are still justly regarded with some degree of veneration. In what are the doctrines of Cullen, Brown, Broussais, Thompson, Hahneman and others better than many of those of the ancients? In therapeutics it is doubtful whether there is as great a difference between the ancients and the moderns, as in the other departments of medicine. There have been very few modes of cure invented since the days of Hippocrates. From the manner in which Hippocrates speaks of bloodletting, the use of cupping instruments, enemata, issues, purgatives, emetics, external applications, such as ointments, plasters, liniments and curative means of every class, we must infer, that his knowledge of them was extensive, and his practice successful. The writings of Celsus exhibit rules for the use of the various curative means, differing in no essential particular from those generally adopted at the commencement of the present century. His description of the symptoms of fever, and the different varieties which it assumes from the circumstances under which it takes place, are correct and judicious. The same may be said of the practice of Aretæus and of much of that of Galen. There are absurdities now in medecine not surpassed by any thing that existed among the physicians of antiquity, ignorant as they were of anatomy, physiology and pathology. Hydropathy is nothing but an old Roman system revived; the Abracadabra of the ancients, was not more absurd than the infinitesimal doses of the Homœopathist, and both Hippocrates and Galen would have laughed to scorn the steam doctors of the present day. The writings of the ancients in therapeutics were very extensive, and many of them valuable. Until the present century, they ruled the medical world, and so little has been the improvement in medicine since the days of Galen and Celsus, that it is but little more than a hundred years since a school of pathology attributed all maladies to the influence of evil spirits, and pretended to cure them with charms, amulets and exorcisms. During the last century medicine has received all the improvement that it has received since the days of Galen.

Vesalius, of Brussels, about two hundred years ago, was the first to demonstrate the errors of Galen, until that time considered infallible, no one daring to oppose him; and how active the medical world have been during the last twenty-three centuries, is manifest from the fact that we did not know, until about one hundred and twenty-five years ago, that the blood circulated in our veins. The discovery of Harvey, the first fruit of the Baconian system of philosophy formed a new era in the history of medicine, and since that time the science has assumed an entirely new aspect. Anatomy is amongst the most perfect of all sciences, and every department of medical science has received new improvements.

The rise of Sydenham, the modern Hippocrates, gave a new impetus to therapeutics, and the appearance of Hunter forms a new epoch in the history of anatomy. Since the Hunterian period, a host of brilliant names have illustrated anatomical science, among whom we may name Pott, Gooch, Abernethy, the Bells, Physic, Dupuytren, Macartney, Larrey, the Coopers, Scarpa, Lawrence, Liston, Guthrie, Mayo, Brodie, Carmichael, Warren, Key, Travers, Dudley, Breschet, Tyrrell, Green, Dieffenbach, Civiale, Leroy, Barton, Cruveilhier, Bichat and a long list of others not less distinguished for the invaluable contributions they have made. In pathology and therapeutics might be given a list of equally illustrious names, and France may be said to be the country in which the new impulse to medical inquiry began. Since the work of Prost, in 1804, the work of observation and the collection of facts has been carried on by a multitude of inquiries. Broussais succeeded Prost in the work, and almost the entire domain of pathology and therapeutics has been scanned by such men as Corvisart, Lænnec, Bouillaud, Petit, Senec, Rostan, Rochaux, Lallemand, Andral, Chomel, Louis and others too numerous to mention. An equal number of English and American observers might be mentioned; and since the days of Hippocrates such a series of contemporaneous publications upon every subject and in every country, has not before appeared. So extensive, varied and valuable have been these labors, that there is hardly anything in pathology, or scarcely any disease upon which they have not thrown some new light. Many therapeutical processes have been greatly improved; diagnosis rendered more easy and certain, and many new means of diagnosis introduced. France has undoubtedly taken the lead in this effort to extend the bounds of Medical Science, but England beginning with Sydenham can array a brilliant list of medical observers. And our own America, too, during the last twenty years, has not been an idle recipient of French and English authors. Though we have no such distinguished names to offer as Louis and Lænnec, or any very brilliant discoveries to record, yet we have produced many valuable and important results. The writings of Caldwell, Rush, Miller, Jackson, Gerhard, Pennock, Hale of Boston, J. Harrison of New Orleans, Ware and a large number of others, have done honor to the medical profession in this country, and have contributed many important results. Still, medical science, advanced as it is, is nevertheless extremely defective. There is not yet one single branch of it complete. We cannot be said to have more than laid the foundation of the science, if indeed we can say as much. Few, if any of its laws, are definitely settled, and it will yet require many years of long and laborious research to give the science that precision, settled and positive character, that distinguishes many other sciences. The great laws of pathology, of ætiology and of therapeutics are yet to be more fully developed, and the entire natural history of diseases is not yet made out and written. Our observations are yet to be more extended, and diagnosis, the essential prerequisite of therapeutics, is far from being perfect. Many very important physiological questions are yet to be answered, and our knowledge of, perhaps, the most important part of man's physical constitution, the nervous system, is far from being complete. It is often asked, why is the progress of medical science so slow? The answer is easy; it is because there is no other science so difficult. It is the nature and character of the science itself, the almost infinite variety, complexity, and extent of its phenomena and relations that make it so difficult and slow in its progress.

This, of itself, is sufficient to keep it behind all other sciences. If we look at the phenomena and relations even in a single disease, we find them not only very extensive, but extremely complicated and difficult. We have not yet arrived at that point of perfection, in the diagnosis of diseases approaching at all to perfect certainty; and thus we have not yet made the first certain step in the cure of disease. Every uncertainty in diagnosis must contribute to the uncertainty of our therapeutics. If the first step is uncertain, the second, depending on it, must be more so. And even if our diagnosis were certain, before our therapeutic knowledge regarding a disease can be said to be complete, we must know all the various phases that a disease may assume from difference of constitution and locality; we must know all the modifications that these require in our therapeutics; and as to our therapeutical knowledge alone of a disease, we must know all the different effects and influences, which all natural substances and agencies in nature are capable of producing upon it, before we can be said to know how to proceed with perfect certainty in all cases. We must know what effect will be produced upon disease by all the different vegetable products of the earth, in all their different forms; the effect of all the different mineral productions in all their various chemical combinations; the changes produced by temperature, atmospheric pressure, various states of the atmosphere, as to its hygrometric condition and its electric state; we must know all the modifying effects of light, food, drink, exercise, condition of the mind, and in short, the effect upon disease and on every thing connected with it, of all the ponderable and imponderable agencies and influences in nature. Is it surprising, then, that a science so difficult should not progress rapidly? And do the world treat us well when they laugh at our failures, and accuse medicine of impotence and blindness? It is to be recollected, too, that even if we possessed all the knowledge required for perfect accuracy in medicine, we could not, from the very nature of things, do anything more than prolong life for a short period.

But, difficult as medical science is, it is quite certain, that causes other than its inherent difficulties have retarded its progress. These causes in previous ages of the world have been ignorance and superstition, and a false mode of philosophizing. In modern times, in our own day, causes different, but equally potent have been in operation, and are now active in retarding the science. Men have been directing all their energies rather to the building up of theories, than to the careful investigation and accumulation of facts. They have in almost every case generalized too rapidly from too few facts. They have founded upon a few prominent phenomena positive dogmatical theories, and taught them to the world as theories established, as systems deduced from all the greater phenomena of disease and medicines; when nothing is more certain, than that there is not one single department of medical science in which our observations are so complete as to furnish room for positive and final deduction. We will venture to say that all time spent upon the mere theories of the day, is time devoted to the retardation of medical science.

The last branch of my subject, to which I would invite your attention, regards the means of improving medical science.

In order to the advancement of the science of medicine, the first step is to discard all theories, as positive guides, and proceed to the work of investigation—to the careful collection of facts, even the minutest. But to do this successfully there should be more harmony in the medical world than there is. Like the philosophers engaged in the pure physical sciences, the medical world should be united into greater Associations for the advancement of medical science, which associations should be divided into sections embracing every department of medical science, and the members of this section should be distributed throughout every part of the civilized world. At the greater annual meetings of these associations, the observations of the various sections should be presented and permanently recorded. These records would, after a time, furnish something certain and substantial for the work of deduction and at the end of every ten or fifteen years, some real advance of medical science could be observed. As things are now, small isolated societies, (often at enmity with each other, and not at all agreed in anything but keeping separate) or single individuals with limited means of observation, do all that is done for the advancement of the science. It is thus not at all surprising that it progresses so "slowly." At the present moment it is a fact that the greater mass of the medical world, are not engaged at all in the work of observing and recording facts. How many eminent grey headed members of the profession pass away without leaving a line of what they have observed.

This is chiefly owing to the fact, that the medical world are not sufficiently united. There is not in it, as in the scientific world, any concentration of action—any harmony of movement—any periodical bringing together of facts—any annual publication of the facts thus brought together. —All is left for the most part to individual exertion and enterprise; and the result of this individual exertion in the form of medical books and essays, is subject to all the influences that individual weakness must ever be subject to. The independence of a large association can never be sought in the productions of an individual; but the influence of theory, or the character of public sectional feelings, will always more or less give a party character to such productions, which misleads and retards the labours of others.

It is contrary to the nature of things to suppose that science can make much progress when left to the labour of individuals alone. The progress of science depends upon observations, and this observation must be co-extensive with the objects to be observed. There must be observers every where, acting simultaneously and bringing together at stated periods the results of their observations. There must also be uniformity of plan of observation and this can only arise from association.—It is as true in medical science, as in political, that strength, progress and certainty and permanency of results, all depend on union. In the physical sciences men see this; but in the medical there is no great universal union bent on ransacking every department of the science, and in gathering facts from all parts of the world to serve us as food for deduction; and until this is the case no great progress will be made.

Thus far, in speaking of making observations we have supposed the existence of an abundance of observers. But is this really the case? Is it not a fact, that the greater progress of other sciences is due, in no small degree, to the great superiority of the observers, and to the great number of them? Most undoubtedly this is the case. The medical profession of the present day, is greatly lacking in those qualifications, so essential to progress. Those engaged in advancing other sciences are all of them men of science—men of profound attainments—well trained minds, classical scholars, and deeply versed in the pure mathematics.—They are thoroughly prepared to be investigators. But is it so in the medical profession? Is it not a fact that in most of our medical schools no preliminary education, beyond a common English one, is required? Is it not the constant practice of our medical schools to send forth into the profession, men utterly destitute of education? Is not the profession overrun with such men? And are these the ones to whom we are to look for the collection of medical facts and the advancement of the science of medicine?

These remarks are true not only of this country but of England.—English writers complain loudly of this same deterioration of the profession. And is it with such observers as these that we are expecting advancement? One might suppose, that, considering the wholesale manner in which medical schools are filling up the profession with uneducated men, they expected to supply by numbers the lack of qualification. As well might Astronomers expect to extend the field of their observations, and penetrate farther into space, by simply multiplying their telescopes without increasing the power of them. Increasing the number, without increasing the power, tends rather to strengthen old prejudice and theories, than to improve them. It is undoubtedly the best policy of the medical world to make fewer observers with greater powers of observation. How has the astronomer at last broke away from our little solar system, and extended the bounds of astronomical science to the distant nebulæ? By what means is he now revelling among ths stellar clusters, that former Astronomers regarded as quite out of their reach? Not by increasing the number of his telescopes, but by increasing the power of them.

Just so it is with the medical profession. If we wish to advance, we must increase the power of our instruments of observation,—multiplying them will not do. We must require that men be educated before they enter the profession. And I will state, too, what I mean by education. I do not mean a common English education. I mean a College and University education; a thorough classical, philosophical and mathematical education. Or plainer still, I mean that before one is allowed to enter the profession, he be required to have received the degree of A. B. at least from some College or University.

Until this is the invariable regulation let us not call our's a learned profession; for it is as far from the truth, as it would be to call the profession of a carpenter or black-smith a learned profession. If we wish to see the science of medicine advance, we must qualify its members to philosophise, to observe facts, and to know how to engage in legitimate deduction. Extremely difficult as the science of medicine really is, is it not the height of absurdity to expect progress from filling up the profession with unlettered men? It is not only the height of absurdity; it is more than that. It is a disgrace to the profession of medicine, and a positive injury to society at large. I speak plainly on this subject, for it is time to speak plainly. We all deprecate a quack. But what is a quack? Johnson, in his dictionary, defines him to be "a vain pretender to physic—a pretender to arts which he does not understand." If such be the true definition, then the majority of our medical graduates are quacks; for who will pretend to say, that a young man understands the science of medicine after having only two courses of lectures in a Medical College?

The thing is a monstrous absurdity. So long as the present system continues let us discontinue the use of "learned profession" and "quack" and say nothing about the progress of medical science.

If we would elevate the profession and cause the science to advance, we must have an educated profession. Our members must be learned men, with minds fitted, by classical learning, and a long course of philosophical studies for the work of observation and deduction. With such men we may look for progress; without them we may expect to sink lower in degradation, and to hear louder and longer the laugh of derision which is already raised against us.

We avail ourselves of this occasion to speak in just terms our high admiration for the Medical College of Louisiana, the learned faculty of which are all members of this society.

No medical school in this country has raised so high the standard for professional preferment or advancement as this College. And although it has struggled for years against a regularly organized opposition, no doubt can exist that it is firmly established. It now numbers about 160 students, young gentlemen from various parts of the South-west, and it may be said without any attempt at flattery, that they would compare, in all the requisites of mental endowment and education with any class in the United States.

In looking over this audience I see a large number of them present, and if I may be permitted to address but one word to them, it is with no other object than that of disinterested and friendly admonition. The influence that you will necessarily exert over our common profession is manifest to all of you. Upon you devolves a heavy responsibility. It will be in your power to do much either for or against the interests of science; to suppose for a moment that you would neglect to faithfully perform your duties to the profession you have adopted, would be to cast unmerited suspicion and reproach upon those, who have given evidence of their zeal in the cultivation of science; do not forget, that you owe a sacred duty, not only to those with whom you may be associated in social life, but to your *Alma Mater*: and let me now solicit you to unite with us in our exertions to extend the limits of our science, no matter where your lots may be cast.

To the members of this society I would say, let us persevere. Our's is an onorous responsibility. To us, in a measure, is entrusted the important charge of not only the best interests of medical science in this city, but also its well being and health. Whatever opinions this society may adopt must have a governing influence upon the practice in the city. Error and superstition, no matter where they be, or by whom sustained, must yield to the united opinions of the purest and best men who compose the medical profession in New Orleans.—No society ever had a better organization than this, and if its plans are carried out, the good that will be effected is incalculable.

The plan of monthly reports from standing committees upon the various branches of medicine, places each member in possession of a knowledge of all that is new or interesting, without the necessity of reading all the works that are daily issued.

A free interchange of opinion in medical conversation, and reports of prevailing diseases, with the best plan of treatment, is also a source of great benefit to the members. The Faculty of the Medical College have kindly tendered the Society the use of their well selected library, which is of great convenience for reference, so that the Society is in possession of all the elements necessary to progress.

Let us then be united and harmonious, and nothing can prevent our Society from at once taking a high and commanding position amongst its kindred institutions. Let each member contribute his mite to its stock of knowledge, and a few brief years will impress upon it the seal of immortality. Do not let us resemble those weak and feeble streamlets that wind their sickly course around every impediment that presents itself; no, let us rather resemble the mighty Ocean, the noblest emblem of majestic decision, which in its calmest hours still heaves its mighty rush of waters to the shore, filling the heavens night and day with the echoes of its sublime declarations of independence, and sporting and tossing on its bed with an imperial consciousness of strength that laughs at opposition.

V.—*A Case of Ovarian enlargement successfully treated with Iodine and its preparations.* By N. K. LESLIE, M. D., of Jackson, La.

In June, 1845, I was requested to see Mrs. D. . . who, about 14 days previous to my visit, had been delivered of her eighth child by an ignorant midwife. I found Mrs. —— to be a lady aged about 35, of a nervo-sanguineous temperament, and laboring under a severe attack of spasmodic colic, accompanied with excessive uterine irritation. Symptoms—skin cool and clammy, pulse feeble and frequent, tongue saburral, features somewhat contracted, abdominal muscles contracted, severe griping pains in abdomen, bearing-down pains severe, much restlessness and jactitation &c. These symptoms were soon relieved by proper treatment, but frequently recurring and her sufferings being intolerable, she was induced to communicate the existence of a large "abdominal tumor," as she termed it.

It was during a paroxysm of pain that I first examined this tumor. I found, occupying the inferior portion of the lumbar region, a tumor as large as the head of an infant, at that time perfectly hard, and fixed in one position. She however at the same time stated, that when she was free from pain, this tumor caused her no inconvenience, that it was soft and changeable in its position.

I was not able to detect any fluctuation.

History: About two years previous to date, soon after the birth of her seventh child, Mrs. D. detected in the right iliac region a small tumor, soft, moveable and destitute of pain, which gradually increased in size, till it occupied the greater part of the right iliac extending into the Hypogastric and as high up as the inferior portion of the lumbar region.

Mrs. —— stated that up to the date of her last confinement this tumor had never been at all painful, but that her mental suffering was severe, supposing, as she did, that this would certainly be the cause of her death. She also stated that the tumor would, when she was supine, spread out like a pancake and not appear thicker than a "saucer," that her monthly flows had been perfectly regular and her health in other respects good. Mrs. —— was of a scrofulous diathesis.—M. M. She was placed upon the use of a diet, light and easy of digestion, but nourishing; exercise recommended, and as she was peculiarly melancholic, the reading of interesting novels and good company were recommended.

She was recommended to use a pill.

℞ Mass. Hg. gr. iij.
Ext. Hyosciami gr. iss.
Creta Prepar. gr. iij.
M Ft. Pill No. j, one every night or every other night, as circumstances might indicate.

Lugol's solution of Iodine, strongest *S.* gtt. iij, in sweetened water, three times a day, locally an ointment.

℞ Iodine ʒ ss.
Iodide of Potass ʒ i.
Adeps ℥ i.
M Ft. Unguent.

A flannel roller was likewise applied to the abdomen.

Under this treatment health was restored in about two or three months.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*The Medical History of Alabama.* By P. H. LEWIS, M. D., of Mobile.

(*Continued.*)

Dr. Lewis next proceeds to the consideration of that variety of congestive fever in which the brain appears to be the organ principally affected. "There is a variety of fever termed by physicians cerebro-congestive, in which the brain is the organ principally implicated.* It is more common late in autumn, and so far as our observation extends is confined, with now and then an exception, to the blacks." One of our author's correspondents, Doctor Ames of Montgomery, has forwarded to him notes of five cases, of which he observes, that, "they not only convey an accurate and vivid picture of the disease, but are so perfect and complete as to be worthy of preservation as models." We regret that we can spare room for but one of these cases.

"A stout and rather corpulent negro man, about twenty-four years old, was taken with a mild intermittent fever of the tertian type, in July 1844. I learned that during the exacerbations of fever, he was sluggish and stupid, but as he was able to sit up, and walk about some during the apyrexia, very little attention was paid to him, and he took no medicine except some cathartic pills once. On the seventh day of

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* On a former occasion, we spoke of, and compared such accounts of congestive fever as we were able to procure, as it "has been described by medical men as prevailing in other sections of the State," with the form of disease spoken of by Dr. Boling as congestive fever, and as occurring in Montgomery and its vicinity, denied however to be such by Doctor Lewis. *The variety now under consideration*, has been described, and several illustrative cases given, by Doctor Wm. J. Johnson, of Fort-Gaines, Georgia, in an article "on the congestive fever of the Chattahoochie," published in the March No. for 1837, of the Southern Medical and Surgical Journal. As the Chattahoochie, to an extent, forms the dividing line between Alabama and Georgia, and as in all probability some of Doctor Johnson's cases occurred on the Alabama side of the river, we deem it proper, in connection with the present subject, to allude to the circumstance.

Allusion, to the remarks on the subject of congestive fever, by Doctor J. C. Harris, in the Western Journal of Medicine and Surgery for February 1847, was also at the same time omitted, because the paper of Doctor Harris was not before the public, at the time the history before us was presented to the Medical Society.

his illness I found him in the following condition. Dorsal decubitus, countenance natural, except that the eyes were a little sunken, breathing slow, heavy, irregular and stertorous, sweating, skin cold on the feet and hands, and cool elsewhere, pupils greatly dilated and but little sensible to light, pulse varying from 115 to 120, small and yielding to the least pressure, heart beating pretty firmly, but no throbbing of the carotid or temporal arteries, insensible to any common stimulant, but moans and struggles feebly when his nostrils are closed, and he inspires the fumes of aqua ammoniæ. He died on the next day."

We find the following general remarks, relative to this form of fever, quoted from the letter of Doctor Ames. "The coolness of the surface in these cases, is never the coldness of collapse, nor is there ever the profuse sweating, jactitation and general restlestness of the abdominal congestive remittent. Neither have I ever observed muttering delirium or picking at the bed-clothes. *Headache* is never spontaneously *spoken* of, when the disease is fully *developed*, although it is a common precursory symptom. The aspect of most of the cases is that of profound sleep, an apoplectic state, without the stertor or pulse of apoplexy."

"The anatomical characters of the cerebral congestive remittent fever, are little else than fullness of the bloodvessels of the brain, with occasionally a coagulum of blood or a liquid effusion in some part of this organ. I am inclined to think that whatever lesions may be found after death, these are all that belong properly to the disease." In one case, in which the examination was made, about three hours after death, "the pia-mater was a good deal congested; about the base, the bloodvessels were particularly full and dark; the brain showed an unusual large number of red points when cut; there was also about a drachm of serum in the lateral ventricles, and the vessels on the surface of the corpora striata, were very plainly defined."

In the special or particular sense in which Doctor Ames, as here quoted by our author, uses the term "anatomical characters," we cannot but doubt the propriety, or strict correctness, of considering as such, "a coagulum of blood or a liquid effusion," seeing that they only occur "occasionally," and are by no means a necessary result of the disease, nor yet necessary to its existence, nor to that of the peculiar symptoms characterizing it. The only one of the lesions named, we are disposed to think entitled to the term in this instance, is the fullness of the bloodvessels of the brain and its membranes; the others being but incidental; occasionally present only and not belonging "properly to the disease."

That this form of congestive fever, occurs more especially among the negroes in the South, is, we presume, owing to the fact, that they are the only class, who, in considerable numbers, as a general rule, are exposed to the direct rays of the sun, while engaged in any laborious occupation. This circumstance it is we think that concentrates so frequently morbid action upon the brain, whenever their febrile diseases, during the summer and autumn, have a tendency to assume a malignant character. We have reason to believe, however, that, taking into consideration the small proportion of white persons in the South, exposed to the same extent, and under similar circumstances to the direct rays of the sun, an equal of not a greater number are attacked with the various shades of the comatose or "cerebral congestive fever." We can at this moment call to mind four cases of this form of disease, which we have seen in white adults—and they were all of the laboring class. Two were ditchers, one a laborer on the rail-road, and the other a farmer, whose circumstances required that he should himself labor actively in the field; and I may observe, that they were all attacked, while they were engaged at their various employments. Indeed, that it has no preference for the negro, but occurs equally at least among the whites under equal exposure, is rendered probable by the excellent descriptions we have of it, under the various titles of soporose, comatose, apoplectic, lethargic, cephalic, and cerebral remittent and intermittent fever, by Popken, Walhoff, Alibert, Morton and many others, whose observations were derived from practice, in malarious localities, where negroes were scarcely to be found, and where, of course, the laboring classes were of the whites.

Doctor Lewis himself makes allusion to the post mortem appearances in eleven cases of congestive fever, examined in the Mobile Hospitals, but as he promises a more full detail of these, on some future occasion, we deem it better to pass them over for the present.

In regard to the liability of persons of different ages and sexes to congestive fever, Doctor Lewis observes: "We have already pointed out and sifted that testimony, which induces the belief, that with an equal exposure all ages and sexes of the white population are alike susceptible. As for ourselves, we have, notwithstanding much experience, met with no case, the subject of which was under twelve or over forty-five years of age; still, as the young and athletic are by habit and occupation more exposed than those in infancy and old age, and being bound to yield what is due to the observation and experience of others, we cannot demur to the conclusion. The negroes, however, who are much more exposed than the young men of the white population, are seldom attacked."

Doctor Lewis, it seems, propounded interrogatories to his "correspondents" relative to the question of the identity of intermittent and remittent with congestive fever. A few of the answers which were in favor of the affirmative, he presents before us, but disposes of them in the most summary manner. The Doctor here, of course, has it all his own way.

We give below an extract descriptive of an interesting form of disease, regarding which however we differ somewhat with our author, being rather disposed to view it as a form of congestive fever, in which the symptoms are modified by some cause, perhaps a degree of inflammation of the stomach, greater than is usually present in that disease.

"Occurring in the same localities, and at the same time with congestive fever, is an occasional case of violent acute affection, the seat of which would seem to be *wholly and entirely in the stomach.* Owing to the circumstances under which they appear, together with the sunken haggard appearance of the patient, they are usually classed with congestive fever, and treated accordingly. We have from time to time treated four patients laboring under this form of disease, every one of which presented in itself a case of *pure idiopathic gastritis.* * * * The following account of one of these cases will fairly represent the whole."

"Baxly, a large muscular man, arrived in the city yesterday morning immediately from the prairie region, admitted into the Hospital this morning; says he began feeling unwell on yesterday at an early hour, but did *not give up* until late at night. This morning (10th Sept.) his condition is nearly as follows:—skin bluish, damp and cool, pulse small, hard and sharp, great anxiety, restlessness and insatiable thirst, countenance haggard and troubled. Complete prostration of muscular power, the patient being unable to raise his hand, wishes his legs flexed upon the body, but is too much exhausted to retain them in that position. Complains of great burning at the stomach; the vomiting is incessant. As the disease progresses, (the notes go on to say) the pulse becomes variable and slightly intermittent, continuing small and hard, the tongue dry and very red, as also the whole internal surface of the mouth, the upper lip thinned and drawn up, the eyes injected, red and suffused, epigastrium sore to the least touch or pressure, every thing taken into the stomach is immediately rejected, swimming in an abundant discharge of grass green fluid, this matter is thrown forward on the neck and chest, the patient being too much exhausted to turn to either side. The breathing is apparently easy, but very quick, slight enlargement of the abdomen takes place, bowels constipated. Night of the third, speaks incoherently, tongue very dry and red, the only word he utters is water, water. The next morning (4th day) the coldness of the extremities increases, slight convulsive tremors shake the frame, and he died at noon. Examination two hours after death. Surface dark, pale or ash color (we cannot make out exactly what color the surface was, Rev.) and exsanguineous. The whole internal surface of the stomach presents a dark red color; all the coats, with the exception of the serous, vascular, red, and to all appearance thickened, the mucous coat softened, yielding under the least force; the balance of the digestive canal healthy,—*spleen enlarged and softened, liver engorged, and varying from a healthy to a dark purplish color. Membranes of the brain vascular and congested, the bloodvessels about the base much engorged and very dark; as much as three ounces of serous fluid effused into the cavity of the cranium.*"

We cannot but think, that our author has been somewhat careless here, in characterizing as "a case of pure idiopathic acute gastritis," or as a disease, "the seat of which would appear to be wholly and entirely in the stomach," a case in which the morbid appearances just named were discovered after death.

Doctor Lewis next takes up the subject of the "diseases of winter and spring." He commences.

"It has been observed by those scientific men who have directed their attention to the subject, that there is but little variation in the average amount of rain from year to year, and that an undue state of high thermometrical range during the summer, is usually followed by severity of cold during winter and spring.—Corresponding with these ordinances of nature we find that if the summer and autumn months have been comparatively exempt from disease, there is invariably an excess of winter and spring affections."—We confess that we are incapable of perceiving the *correspondence* between the proposition contained in the first part of the above quotation, and the statement made in the last. We will also remark, that so far as our own experience extends, it is directly at variance with the latter statement, and we have been in the habit of accounting for the greater amount of sickness, which we have generally observed during a winter and spring, succeeding a sickly autumn, by supposing that the debility still continuing after autumnal attacks, leaves the system more liable to those of winter; and also that in many instances, in those even in whom autumnal disease has not been developed, the predisposition at least to morbid action has been engendered by the prevailing miasm, and is excited into active operation, by exposure and the vicissitudes of winter. This latent predisposition would of course be extensive, in proportion to the extent and concentration of the producing miasm, and of this we may judge by the number of autumnal attacks. We are not alone in this opinion, for we have heard remarks of a corresponding character from a number of observing practitioners of Alabama.

It is the opinion of Doctor Lewis that the diseases of winter and spring have not partaken, to as great an extent as those of summer and autumn, of the "grave and malignant character," marking the diseases of the present epoch.

"It is to these two forms of disease, the thoracic and cutaneous, that we should properly confine our attention; for those more common to the North, as rheumatism, phthisis etc., are rare among us. * * * * When the months of autumn are closed by copious rains, it is generally noticed that thoracic diseases make their appearance; at first manifested by catarrh, influenza and incipient bronchitis; advancing onward till the lungs and pleura become seriously invaded. Those gentlemen with whom we have corresponded, unite in the opinion, that both pleurisy and pneumonia, either in a distinct or united form, are attended with symptoms of a low grade, strongly resembling a typhoid condition of the system, and that these symptoms attend the course of the disease. Independent of such pathognomonic signs as cool skin, depressed pulse and phlegmonous (?) tongue, we have confirmatory evidence of the true character of these diseases in the treatment adopted by physicians; for instead of the lancet, antimony and contra stimulants, they are forced to resort to those agents, that are known to sustain and nourish the depressed system. It is true that in some particular localities there is a tendency to active inflammation; but even here, in a large mass of cases, we find in the early stages, as well as during the oscillatory changes that ensue, a tendency to collapse."

We presume that our own knowledge in regard to the diseases of the State has been derived from an acquaintance only with those "localities" in which "there is a tendency to active inflammation;" for by a large portion of the medical men with whom we have had intercourse, the principal remedies used in the treatment of the inflammatory affections of the thoracic viscera, are mild mercurials, local, and occasionally general bleeding, blisters; and more especially Tartar Emetic in pretty large doses, and Quinine. Very rarely a case occurs, which in the advanced stages may be benefitted by the Bark in substance, Serpentaria etc.

"*Bilious or typhoid* pneumonia, as presented to our notice, may strictly speaking be separated into a distinct type." Accordingly Dr. Lewis speaks of a *bilious* and a *typhoid* pneumonia, and quotes from a paper of Dr. C. A. Woodruff addressed to the Medical Society on the subject. We will present a brief extract from the description of each form.

"So far as my observation has extended, bilious pneumonia is invariably ushered in by a chill or rigor of indefinite duration, succeeded by an intense heat of the skin extending over the whole surface of the body; and during the febrile excitement the pulse is full, strong and corded, with a slight remisssion at the interval of eighteen hours."

"Typhoid pneumonia is usually preceded by acute pain over the frontal, temporal, and occipital regions of the head, and sharp lancinating pain in the chest; the eyes of a dull leaden hue, watery, with great intolerance of light; the cheek usually suffused with a bright scarlet glow, sometimes extending over the forehead, chin and neck."

In regard to the prevalence of pleuritis, the observations of our author will be found, we think, to correspond with the experience of the profession generally, in South Alabama. He says: "Well marked and uncomplicated pleuritis is a rare form of disease, to be met with during this epoch, especially prevailing to any extent in any one settlement." All, we are sure, will also concur in opinion with him, that the colored population suffer more severely from these affections (diseases of the chest) than do the whites."

Doctor Lewis next takes up the subject of typhoid fever as occurring among the winter and spring diseases.

"It is a generally received opinion among medical men in various portions of the State, that typhoid fever, as described by Bartlett and others, has been an annual disease, although not prevailing as an epidemic, except from some local and strongly exciting causes. The first information that is presented to our notice of its appearance in a marked form, was in Dallas Co., in 1835. In this instance it was a sequence of typhoid pneumonia, that had prevailed during the previous winter, assuming at that time an irregular intermittent type."

"A planter in that county, for the purpose of procuring manure for some worn out lands, had exposed to the weather several hundred bushels of cotton seed, which during the latter part of December became completely saturated with water. The heat retained in so large a mass, soon set up very active decomposition. Some fifteen or twenty negro houses were situated in a circuit immediately around the spot where the seed were exposed. About the middle of January several cases of *pneumonia* were developed among the negroes, which continued occasionally to attack them till the month of March. The spring opening warm, *the disease* immediately *assumed a new type*, and continued to prevail till every negro *above the age of five years* had been seized. The attack in these cases was insidious; the disease forming very slowly; there was a slight *remission* every morning for five or six days, after which it became continued, with *clammy* skin, quick compressible pulse, diarrhœa, coma and sordes on the teeth. The disease ran its course in from fifteen to twenty-five days; average mortality about twenty per cent." Another instance of a similar character is also recorded by our author, in which there were sixty cases, all benefitted by the use of quinine.

"These and other similar instances, which we have no space to detail, are sufficient to induce the belief, that the decomposition of cotton seed by process of fermentation produces a gas, probably the ammoniacal (what will the ladies, so devoted to their hartshorn bottles, say to this. Rev.) which affects a majority of those, who may be subjected to its deleterious influence."

"Typhoid fever, as usually presented in this section of country, is variable in its character, in many cases attended with grave and malignant symptoms, owing in a great measure to locality; as for instance in our prairies and bottom lands; while on the other hand, there is more vascular excitement and inflammatory action in the hilly region of country."

"In the former instance, Doctor Hogan, in his letter, says: "The disease is usually ushered in by a chill, not unfrequently a double tertian; the fever being remittent, with partial perspiration, great enteric irritation terminating in special congestion, with a species of *paralasys agitans* or meningitis may supervene. There is more or less pulmonary congestion, that is preceded by cerebral disturbance, sometimes in the form of delirium, and at others it may be attended with stupor or coma. In brief, typhoid fever may invade by the brain, the lungs, or the bowels, and in the grave cases all these organs are apt to be involved, *and your patient may die from exhaustion in a physiological condition.*"

* * * "The symptoms that mark this disease, are, in the upland region, so very dissimilar in many respects from that of the low country, that we do not hesitate to adopt the true pathological signs (?) as furnished by Doctor Clark, a resident of Benton Co."

Notwithstanding the statement of Doctor Lewis, "that it is a generally received opinion among medical men, in various portions of the State, that typhoid fever as described by Bartlett and others, has been an annual disease," there are many physicians of our acquaintance, who are much inclined to doubt whether the genuine typhoid fever has ever appeared among us,—and are very positive in relation to this fact, as regards their own particular neighborhoods, for such time at least as they may have resided in them. Will these doubts be removed, by the lengthened quotations we have here made, after comparing them with the account of Typhoid fever, as given by Doctor Bartlett? The description however of the typhoid fever, "in the upland region,"—for which we regret that we cannot make room,—as given by Doctor Clark of Benton county, and quoted by Doctor Lewis, seems less wide of the mark; still, we are reminded of the fact, that this is the same gentleman, who,—as we have shown in a previous quotation,—says, that "it attacked *indiscriminately all ages*, without regard to sex or color."

All will admit, to a great extent at least, the truth of the following remark.

"Taking into view the rapid decline of severe autumnal fevers, together with the lessening mortality attending them, and the rapid increase of those diseases just pointed out, the winter and spring diseases may now be regarded as the most fearful maladies within the borders of the State. To the negro, who bears the heat of summer with impunity, those diseases which come on the chilly blast, and are nourished by cold and moisture, are peculiarly noxious and alarmingly fatal."

Allusion is next made to the exanthemeta.

"The exanthematous affections, like those of the thoracic viscera, are rapidly increasing, and if we are to judge by the effects generally produced by physical changes, the day is not far distant, when they will become the prominent disorders of the State, and the affections of summer and Autumn gradually yield to an improved cultivation."

We are somewhat at a loss, how to construe the above sentence.—Does Doctor Lewis really mean to say, that the exanthemeta are increasing in proportion as our autumnal diseases are disappearing—both being the result of "physical changes," and "an improved state of cultivation?" Or, merely, that without any proportionate increase, this class of diseases will rise into importance, in consequence of the partial disappearance of others, which have heretofore attracted our principal attention, and by which they have been, as it were, overshadowed?—However, we can conceive that even a proportionate increase in the number of cases of the contagious exanthemeta, would probably occur in a more closely settled, and as a general rule, therefore, more highly cultivated section; not in consequence of an improved state of cultivation, *per se*, but from the greater and more constant intercourse among the inhabitants, facilitating the spread of diseases of this character.

The next subject of investigation with our author is the

"*Summer and Autumnal fevers of Mobile.*"

Before entering upon their consideration separately, a few general remarks are introduced, from which we make the following interesting extracts. Speaking of the summer of 1819, in which the deaths are supposed to have amounted to 400, being nearly one third of the population of the place at that time,—Doctor Lewis observes: "During this calamitous season, it would appear that every character of endemic febrile poison was found in concentrated abundance upon the inhabitants, each one being active in its peculiar way, in doing the work of death. The whites invariably died on the fourth, fifth or sixth day, of black vomit, whilst the negroes and quadroons, after protracted suffering from bilious fever, resulting in frequent relapses, cold sweats and colliquative diarrhœa, shared the same fate. The equal prominence of bilious and yellow fever makes this a remarkable epidemic—the diseases however were respectively confined to different classes of persons."

"Since the year 1819 bilious fever of a grave and fatal character has disappeared, and the mortality from febrile affections has been confined to yellow fever."

"From an analasys of the epidemic of 1843, we will endeavor to present such facts and circumstances as will bring to view the several varieties of febrile diseases, and the striking phenomena they present as connected with a Mobile epidemic."

"The epidemic may be computed at about 80 days, say from the 20th of August to the 10th of November; the population of the city was at that time about 14,000, and the number of patients treated 1,350,—as follows:

Simple intermittents and remittents, - - - -	500
Intermittent and remittent yellow fever, - - -	100
Mild yellow fever of one paroxysm, - - - -	350
Grave cases of yellow fever, - - - - -	400
	1,350

Of the intermittent and remittent yellow fever 50 proved fatal; of the grave cases of yellow fever 190; making in all 240 deaths."

The Doctor next takes up, somewhat in detail, the different diseases above named, commencing with "*simple intermittent and remittent fever,*" under which head we find the following remarks relative to the difficulty of diagnosis; and the fact clearly set forth, that where the cases were not promptly arrested in their progress, black-vomit supervened.

"So difficult was it during the first paroxysm in many cases, to pronounce with any degree of certainty on the character of the disease, that the most prudent were known to avoid committal by their hesitation to express any positive diagnosis. After the first paroxysm passed off, the physician was still in doubt, in many cases, until the appearance of the second, after which he would not hesitate to pronounce it intermittent or remittent yellow fever, as the case or its specific symptoms might warrant.

"In these doubtful cases the judicious practitioner seldom failed to remark to the friends of the patient, that care should be observed, or it might run into black-vomit. This difficulty of diagnosis so frequently occurred as to make it obvious that the types of these intermittent and remittent fevers are so blended with mild yellow fever in the first paroxysms, that the distinction cannot be drawn."

* * * * * * * "These fevers usually yielded under proper treatment after the second paroxysm, but when the usual antiperiodic remedies were unavailing, there were strong reasons to suspect,—notwithstanding the apparent singleness of type,—that the morbid condition was one of a complicated nature, and perhaps after a third or fourth paroxysm ensues, the mask is thrown off, and the patient discovered to have passed into the collapse or black-vomit stage."

Next in order our author treats of

"*Intermittent and remittent yellow fever.*"

"Every summer and autumn intermittent and remittent fevers prevail in the suburbs, whilst those living in the middle part of the city seldom have this character of disease, except in sickly years, and then the cases are confined to those who are acclimated, while strangers unacclimated suffer in the more violent form of yellow fever." * * * * * * When medical men were treating what they supposed to be simple intermittent, and were not apprised of the real nature of the disease until they were startled by the appearance of black vomit, as was frequently the case, they consoled themselves by stating that a simple case of chill and fever had, *under atmospheric influence*, run into black-vomit."

Instead of considering these as cases of pure yellow fever, Doctor Lewis,—who advocates the doctrine of the separate and distinct character of yellow fever, from remittent fever,—believes that the patients were under the influence, at the same time of the poison of intermittent and remittent fever, and of yellow fever, and gives some symptoms by which he supposes the presence of the latter may be discovered. Relative to this he observes:

"Although the peculiar characteristics of chill and fever were very prominent, so much as to conceal from the observation of the incautious physician the real nature of the disease, yet there were signs, by which the hidden demon could be traced out. During the apyrexia, there were the peculiar pulse, some of the restlessness, and that peculiar appearance of the eye, or a glimmering of that unmistakable physiognomy, peculiar to the yellow fever, which once seen, can never be forgotten."

Many, we are disposed to think, will be inclined to take a more simple view of these cases, and to us, we confess, they seem to bear strongly in favor of the now waning doctrine,—of the identity,—except in degree,—of our other autumnal fevers and yellow fever; notwithstanding that they are presented by our author with quite a different impression and intention in regard to them. Although Doctor Lewis speaks of the effects produced by the poison of yellow fever in these cases, as distinct from those produced by that of our ordinary or milder autumnal fevers,—leaving the impression that they are concealed from the observation of the "incautious" only,—and gives the symptoms, by which he supposes the presence of this poison may be early detected, we doubt whether these, by others, will be considered reliable, or evincive of the presence of another poison, separate from that by which the more prominent symptoms are developed; and more especially are we inclined to this opinion, on reviewing the quotations which we have just made, showing the great difficulty of diagnosis; bearing in mind that the "*most prudent* were known to avoid committal"—and that "the types of these intermittent and remittent fevers are so connected with mild yellow fever, in the first paroxysms, *that a distinction cannot be drawn.*"

We next come to what Doctor Lewis terms—

"*Mild cases of epidemic Yellow fever.*"

"In these cases the pulse is more rapid, the skin hotter, and more pain and restlessness, than in those of a grave character. The peculiar physiognomy of the malignant cases cannot be traced in these. The phenomena of fever proper yield in 10 or 20 hours to a fine equally diffused perspiration, and the patient is enabled to take his nourishment, and return to his business in a few days. A moderate mercurial cathartic to excite the liver and act on the bowels gently, the warm bath, and stimulating ptisans are of service, but any EXCESS of medicines is too often injurious and *not requisite to a cure.*" Most certainly not.

"*Grave or Malignant Yellow fever.*"

The following is a brief summary of the symptoms. "Without the slightest warning, probably while asleep, the patient is seized with a chill or pain in the head, with cold creeping sensations. In a few minutes fever comes on, the skin is hot—pulse 110 or 120, pain in the head, back and limbs very severe. * * * * * In 8 or 10 hours the fever is modified, the skin becomes moist, pain in the head less, tongue slightly furred, pulse down to 90 or 100, full and bubbling, having lost the hardness or tension a short time previous."

This stage,—designated by Doctor Lewis the febrile,—lasts from 30 to 50 hours, and is succeeded by the "calm or passive stage;" the symptoms of which are: "the pulse sunk to the natural standard, the secretions diminished, evacuations scanty, ash-colored and inodorous, the eye muddy and yellow, the countenance gloomy, dejected or sottish." * * * * * * "This state continues for two or three days, when another, the "collapse" or critical stage, approaches. As this latter period in the disease advances, the pulse sinks down to 60—or 70, is full but gaseous and compressible, the skin continues moist, but not so warm. The patient speaks slowly, drawing out a syllable at a time, he is evidently grown weaker, hangs his head on the side of the bed, hugs the pillow closely. If he does not rally at this point he grows more restless, he sighs, and groans, the secretions are all stopped, the skin becomes cold, his features are sharper, the upper lip thin and trembling, black-vomit is thrown up and he soon dies."

The following fact must appear singular, to all who are not practically familiar with yellow fever, or have not seen cases of congestive fever, in which disease it will be recollected that the patient is capable of walking about, at a time when his system is under the influence of the most intense morbid action, and while the most profound pathological alterations are in rapid progress. Indeed, the circumstance might be named amongst others, as showing a close relationship between the two diseases.

"Now and then the physician meets with some patient who continues on his feet up to the black-vomit stage; in fact we have frequently seen them throw up this matter when walking in the street, or waiting in the office for medical aid. * * * * * * The patient from exhaustion, will lay down, but soon he is on his feet again, often getting up at midnight and going into the street, where he will walk for hours."

We transcribe entire Doctor Lewis's description of the physiognomy of yellow fever.

"The physiognomy of the disease is striking and peculiar. With many it throws a gloomy melancholy shade over the countenance; with others the brow is furrowed, the lip compressed, and they frown defiance on all around. At other times, it is so blended with a comic, lively expression of countenance, as to give the patient a peculiar variable and singular appearance. Once displayed, no effort of the patient can dispel it; he may rise from his bed, laugh and talk with his friends, become exhilerated with wine, or joyous in the anticipation of coming pleasures, but he cannot chase it away. There it sits enthroned upon the face like the shadow of a monster, smiling in contempt upon the efforts of the physician, mocking the assumed gaiety and levity of its victim. Nor is the cradle exempt from its visitations. Its gloomy, cheerless mantle is often placed on the infant brow, giving to it a sullen look not suited to its tender age."

From experiments instituted by Doctor Nott and himself, our author's opinion is settled in regard to the nature of black-vomit. He says of it, that,—"it is blood decomposed or changed in color, mixed with the secretions of the canal, just in proportion to the length of time the blood remains in the stomach, or comes in contact with the secretions."

In the experiments of Doctor Nott, "diluted muriatic acid so changed the blood as to make it resemble black-vomit,"—while it also appears from numerous tests by the same accurate observer, that the black vomit is highly acid.

In regard to the post-mortem appearances of yellow fever, Doctor Lewis informs us, that Doctor Nott, "in sixteen dissections, found the liver in 6 of a pale ginger-bread-color, dry and pliable, 2 olive, 2 normal, and 6 darker than natural and much engorged." He believes that the appearance of the liver described by Louis,—and believed by some as an anatomical characteristic of yellow fever,—to be dependent on considerable hæmorrhage, discharged either in the form of black-vomit or unchanged blood. He also states that it is occasionally found in those "who have died of long continued wasting swamp fevers."

From the post-mortem appearances, the Doctor recurs to a subject which we had supposed that he had dismissed some time since;—to wit, the different degrees of susceptibility to the action of the poison of yellow fever, of different classes of persons;—and tells us, that "in late epidemics a few mulattoes only have experienced attacks, and these exceedingly mild, seldom arriving at the black-vomit stage."

"What is called sporadic yellow fever, occurring in healthy years, is confined to strangers. During epidemics this class has also to bear the malignancy and force of the disease. Many of the acclimated, among whom are those who have had the disease in former years, experience mild ephemeral attacks. There can be no question that some persons have the disease, the second, and some the third or fourth time."

The Doctor gives several cases of congestive and remittent fever,—in which the patients either died of black-vomit, or presented some of the other phenomena of yellow fever,—and in which, accordingly, he supposes that the two poisons were at the same time present in the system, "each doing its work of destruction in its own peculiar way."

We have next a recapitulation, from which,—for the purpose of keeping fairly on the track of our author,—we will make a few brief extracts.

"In the coal, granitic and hilly portions of the State, subjected to great vicissitudes of weather, we find the fevers to be of the continued or remittent type." * * *

"In the prairie or middle part of the State, where the soil is peculiar, abounding in animal matter, we have dwelt at some length on the character of disease, which there strikes the eye." Here follows a *resumé* of the symptoms of congestive fever, but, as none are presented but such as have been already named, and those which Doctor Lewis considers "pathognomonic of the disease" are no better designated,—it is unnecessary for us again to present them before the reader.

"In the vicinity of the deep, humid vegetable morasses, marshes and swamps of the tertiary, or Southern portion of the State, * * * * the fevers are remittent, intermittent or continued, lasting from 4 to 15 days." * * * *

"We pass on to the city of Mobile, and there discover, peculiar to her trodden *animalized* streets, yet another character of disease." * * * * *

"Here are four varieties of acute disease, peculiar somewhat to certain terrestrial formations. Can any one possessed of candor and ordinary reasoning powers, examine into their history, and fail to see the striking characteristic differences. The enslaved and even tasked mind may reason that inasmuch as they are all fevers of the same season, appearing, blending, and mingling together, and often running into each other, specific distinctions cannot be made. To this kind of argumentation we may properly reply, that red, white, black and green are all colors; they may be so mingled and blended, running into each other by imperceptible degrees, as to produce various shades, and associate in the mind a most distinct and inseparable connection; yet, when they are displayed in their primitive natural character, how boldly and prominently do they contrast."

Now at the risk of being charged with want of "candor and ordinary reasoning powers," and of an "enslaved and overtasked mind,"—we must acknowledge our want of ability to perceive where or in what way Doctor Lewis has presented or pointed out, "the striking and characteristic differences," and that the facts stated, "that they are all fevers of the same season, appearing, blending, and mingling together, and often running into each other," will still appear to us reasons for considering them identical, until a stronger array of testimony than he has collected favoring an opposite conclusion, shall be brought forward.

There are but few, we think, who will not admit, that among the diseases "to which flesh is heir," there is a considerable number, of which any two may exist at the same time in the system, and perhaps modify somewhat the characters of each other; and also, that several colors may be blended together, modifying each other's shades, so as to produce an intermediate one;—but this latter it seems to us is merely an analogical argument in favor of the possible probability of such an occurrence as Doctor Lewis is advocating, and by no means proves the reality of its existence in any given case; and it would consequently seem that the "black, white, red and *green*" argument does not settle the point at issue, as the author must have imagined would be the case, from the confident manner in which it is presented; and, that to "this kind of argmentation we may properly reply," that the same disease may be presented under different degrees of morbid action, and, that of any one color, there may be numerous shades.

In continuation of his arguments in favor of the radical difference between congestive fever and remittent fever,—to which subject brief allusion is again made,—Doctor Lewis says: "Again, examine into the character of the mild endemic remittent yellow fever of 1844, and say if the connection which existed between the congestive fever and intermittent fever of 1835 and 1836, which has been candidly portrayed, was any closer than that which binds together intermittent and yellow fever."

If we understand the meaning or aim of this sentence, it has for its object the foundation of an argument in favor of the radical difference between remittent and intermittent and congestive fever, on the grounds that it has been shown, that there is not a greater difference between the phenomena of these two diseases, than there is between mild endemic yellow fever, and intermittent and remittent fever. But to make the argument of any value whatever,—admitting for the moment that Doctor Lewis has succeeded in proving this to be the case,—it would be necessary that no question or doubt should exist, as to the distinct and separate nature of intermittent and remittent, and yellow fever.

As to the latter question, we will say nothing at this time,—as each of the two we think should rest upon its own merits alone,—but in regard to the former, we must reiterate our belief, that Doctor Lewis has entirely failed in pointing out one single diagnostic symptom, one single particular, in which the two forms of disease differ, save in degree. We have not so far failed, nor shall we in the conclusion of our task, fail to present to the reader any thing in the paper, which may seem to have a bearing in favor of the Doctor's opinion, and if it shall be apparent to one even of our readers that his position is made out, or its correctness rendered even probable, by the enumeration of "the symptoms pathognomonic of congestive fever, or a connected chain of argument, we will cheerfully submit to be ranked among those obnoxious to the charge of an "enslaved and overtasked mind."

We proceed,—and, as we have under consideration still the subject of congestive fever,—pass over some interesting observations for the present, for the purpose of quoting the next paragraph relative to it. It is this: "On the other hand, history discloses the fact, that congestive fever, with all its perilous depressing phenomena, unlike that of any other, often displays, in a mild and harmless from, the same marked and distinctive symptoms—all going to prove, considering it in connection with bilious fever, that they are not different grades of violence merely." Our experience differs in regard to this matter, somewhat, from what Doctor Lewis says, that "history discloses," for we have never seen "congestive fever with all its perilous depressing phenomena," display "in *a mild and harmless form* the same marked distinctive symptoms," and further, we have never seen a single case of fever, which in our opinion deserved the term congestive, from which we did not consider the subject in considerable danger; and the perilous depressing phenomena we have always viewed as essential indications of the presence of this element, (congestion) in the pathology of a case. But admit for a moment, that "history discloses the fact," that the "marked distinctive symptoms" of congestive fever are sometimes displayed in a "mild and harmless form," and what would it prove? Doctor Lewis admits that intermittent and remittent fevers are identical, "differing only in degree." Do not these in many instances present certain shades of difference; and preserve them too, not only in the milder, but in the more violent grades?

Doctor Lewis, it will be recollected, advocates the doctrine that congestive fever, remittent fever, yellow fever, &c., are produced by distinct and separate causes; but in order to avoid the objection, which might be opposed to such a position, by the more marked cases of one of these diseases or forms of disease, occasionally presenting some of the prominent symptoms which are generally present in another—has recourse to the hypothesis, that the system is in such instances at the same time under the influence of more than one of the producing agents, and a disease of a mixed character the result. In support of this doctrine several instances like the following are adduced.

"Whilst residing in the interior, a young gentleman from Mobile came into the neighborhood and was seized with the congestive fever; a few hours before he died, on the 4th day of his illness, he ejected from the stomach coffee ground black-vomit in large quantities." Now, at a first glance, cases of this kind, we acknowledge, seem entitled to a certain degree of consideration, but still they cannot be looked upon as decisive or conclusive, seeing that in the more violent epidemics of our autumnal diseases, occurring in situations remote from any locality in which yellow fever is at the time in existence, in its more marked and generally recognized form, and in individuals who have visited no such place, cases of black-vomit occasionally occur; or, at least, a matter is occasionally thrown up from the stomach, corresponding so exactly with the descriptions given of that matter, that a difference cannot be detected; this symptom, in such instances, generally occuring too in connection with an unusual yellowness of the skin. Cases of this character have occasionally fallen within our own circle of observation.

As we have been governed almost entirely as to the order in which we have noticed the different matters contained in the essay, by the order in which they appear in it,—and this we offer in explanation of the desultory character of our notice,—we must now dismiss the subject of congestive fever, of which our author makes no further mention, if we except perhaps a few remarks on its treatment.

We again, with Doctor Lewis, resume the subject of yellow fever.—He assures the Medical Society of Alabama, to whom the essay is addressed, that, "after a careful study of the foregoing history of the yellow fever of Mobile, brief and imperfect as it is, no member of your society will doubt the distinct individuality of its character, and that the poison or agent of its production is not in common with other fevers, but also distinct and peculiar. The old argument founded in *ignorance* of a correct *knowledge* of all the facts, that yellow fever—is but a high grade of bilious fever, differing only in degree, is scarcely worthy of notice,—for bilious fever often prevails with a malignity unto death, without displaying any of the symptoms pathognomonic of the other.—Again, well marked cases of yellow fever are often lighter and more ephemeral than even the slightest cases of bilious fever."

Without expressing any opinion of our own at this time, relative to the important question involved in the above paragraph, we may venture to doubt the logical correctness of the process, by which our author arrives at the conclusion. The old opinion, that bilious and yellow fever are but different shades of one disease; which Doctor Lewis says is "founded in ignorance of a correct knowledge of all the facts," we know is still entertained by many judicious and careful observers, whose opportunities of arriving at a "correct knowledge of all the facts," have been of the very best known character, and on this account we would most certainly deem their opinion entitled to *notice*,—though it may ultimately prove to be erroneous. We have read the essay of Doctor Lewis with less attention than we intended, if he has any where informed us in it what are the pathognomonic symptoms of yellow fever; yet, as it will be observed above, his remark as to the unworthiness of the old opinion of notice, is concluded thus;—"for bilious fever often prevails with a malignity unto death, without displaying any of the symptoms pathognomonic of the other." As we have before had occasion several times to observe, Doctor Lewis considers bilious, or remittent and intermittent fever to be identical, differing only in degree. Now, does not intermittent fever at times prevail "with a malignity unto death," preserving certain shades of difference from the remittent form, even to the last hour? These shades of difference we will not call "pathognomonic symptoms," but are they not as well marked and peculiar, as any that can be named as invariably occurring in and distinguishing remittent from yellow fever? Are not the shades of difference between remittent and yellow fever, according to Doctor Lewis's own showing,—(and it is from this only that we speak at present)—as imperceptible, nay even more so, if possible, than between intermittent and remittent fever? "Again, (Doctor Lewis goes on to say),—well marked cases of yellow fever are often lighter and more ephemeral than even the slightest case of bilious fever." We would ask our author here,—as he speaks of these cases being "well marked," *how* were they *well marked*, and what were the *pathognomonic symptoms* by which he discovered that they were yellow fever, and not bilious fever? More relative to this, perhaps anon. But, are not cases of well marked remittent fever often as light and ephemeral, as cases of intermittent fever?

It is singular how much we are inclined, after having adopted any hypothesis, and embraced it with special favor, to press all facts and circumstances, connected with it even by the most remote analogy, into its service and support—and often, too, with the judgment and foresight so biassed and clouded by our inclinations, that to the disinterested and unprejudiced observer, the testimony in this spirit adduced is found to have an opposite bearing. We are led to this remark by the fact, that Doctor Lewis, having assumed the distinct and separate character of congestive fever, presents,—we presume as the basis of analogical reasons in support of this position,—the generally, of late, received opinion of the non-identity of yellow fever and remittent fever, and announces his belief in its truth,—though there is reason to doubt whether this opinion has been long entertained by him.* However, to a change or modification of opinion, it may be urged, that the Doctor has a perfect right, and that persistence in a favorite idea, after what may appear satisfactory testimony of its erroneousness is presented, argues more of dogged obstinacy, than of a spirit of philosophy;—but, what does seem to us most extraordinary, is, that while Doctor Lewis comes out the advocate of the distinctive character of yellow fever, from remittent fever, all,—I may almost venture to say,—of his observations and researches on the former disease in the essay before us, have a different tendency; are completely subversive, indeed, of the very foundation on which the arguments in its favor have been based. Although we have never come to any decided conclusion in our own mind, relative to this vexed question, we acknowledge that heretofore we have rather inclined with the popular current, in the affirmative; but since the perusal of the paper of Doctor Lewis, the belief that the opinion is based upon insufficient data, is strongly impressed upon us; and, at any rate, we know of no author, whose investigations go further than do his, to demonstrate the identity of the two diseases.

Let us for a moment examine into the facts and circumstances upon which the doctrine of their non-identity is principally based, and exam-

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* New Orleans Medical and Surgical Journal for July, 1844. Page 38.

ine to what extent, these,—consistently with the investigations of Doctor Lewis,—can be adduced in support of this opinion.

The first to be considered, and one of the principal grounds upon which an opinion may be based, in regard to the radical difference of any two diseases, is a marked and essential difference in their symptoms. But what does Doctor Lewis say on this subject? "So difficult was it during the first paroxysm in many cases to pronounce with any degree of certainty on the character of the disease, that the most prudent were known to avoid committal, by their hesitation to express any positive diagnosis." * * * * "In these doubtful cases, (speaking of the "simple intermittent and remittent fevers" of Mobile) the *judicious* practitioner seldom failed to remark to the friends of the patient, that care should be observed, or it might run into black-vomit." * * * * * * "These fevers usually yielded under proper treatment after the second paroxysm, but when the usual antiperiodic remedies were unavailing, there were strong reasons to suspect, notwithstanding the apparent singleness of type, that the morbid condition was one of a complicated nature."

We turn over a page or two, to bring a circumstance there mentioned by our author, in connection with the present subject.

"By passing into the dwellings you would find *children born of the same parents, living under the same roof, and nourished at the same table, attacked near about the same time, with a complaint which for some hours is characterized by the same phenomena. In the sequel one portion of them prove to be simple bilious, and the other, grave malignant yellow fever.*"

Now, what is the most rational and natural interpretation of these facts? That two distinct poisons were at work at the same time in subjects placed under such similar circumstances, productive of a series of symptoms, identical, in the different cases, up to a given time; or, that a single agent alone was in action, productive of symptoms of increasing gravity, in the progress of the disease, with shades, dependent on, however, or modified by the controlling effect of remedies used, the degree of constitutional resistance to the morbific cause, or individual susceptibility to its influence?

"When medical men were treating what they supposed to be *simple intermittent*, and were not apprised of the real character of the disease, until they were startled by the appearance of *black-vomit, as was frequently the case*, they consoled themselves by stating that a simple case of chill and fever had, under atmospheric influence, run into black-vomit."

Upon this, Doctor Lewis observes: "Now, what are the facts and conclusions to be drawn therefrom? Although the peculiar characteristics of chill and fever are very prominent, so much as to conceal from the observation of the *incautious* physician the real nature of the disease, yet there were signs by which the hidden demon could be traced out. During the apyrexia, there were the *peculiar* pulse, some of the restlessness, and that *peculiar* appearance of the eye, or a glimmering of that *unmistakable physiognomy peculiar to the yellow fever, which once seen, can never be forgotten*," and this, notwithstanding the difficulty of diagnosis,—acknowledged by Doctor Lewis,—was such, that "the *most prudent* were known to avoid committal,"—and the avowal subsequently made in treating of the "mild cases of endemic yellow fever"—that, "the *peculiar physiognomy* of the malignant cases cannot be traced in these."

Surely here are very insufficient data given, upon which to base a diagnosis. Is there a pulse *peculiar* to yellow fever?—and if so, what are its characteristics? Is it hard?—is it soft?—is it large and hard?—is it small and hard?—is it large and soft?—(this we presume is the "bubbling" pulse) is it small and soft?—or, with any of these characters,—is it quick or frequent, or slow? Is not any one of these, or any possible combination of these characters occasionally, found in the pulse in other diseases?—and is not the moderately full and soft pulse the one most frequently found in the remissions of remittent fever? Is there invariably present a *peculiar* appearance of the eye in yellow fever?—or, is it not a fact, that the description of the appearance of the eye in this disease by one author, is in exact correspondence with that of another, of the appearance of the same organ in remittent fever?—We would ask, too, in what particular does the "restlessness" occurring in the remissions of yellow fever, differ from the *restlessness*, occurring during the remissions of remittent fever, or in other diseases, that the one should give evidence of the presence of a "hidden demon," and the other not? Now it is most evident, we think, that the result of the case in these instances was the only real ground of diagnosis. If, after having advanced to a certain extent, the progress of the disease was arrested, short of the occurrence of black-vomit,—recovery following,—the case was set down as one of bilious fever; but if black-vomit supervened, or if nature's efforts or "the usual antiperiodic remedies were unavailing;" it was pronounced a case of yellow fever. No diagnostic symptoms then whatever, we think it will be admitted, are pointed out; for it is not pretended by any, that the yellowness of the skin and black-vomit are present even in all of the fatal cases of yellow fever; and Doctor Lewis himself says, that many cases terminated fatally in which this symptom, (black-vomit) was not present," and also," that in some who die as well as those who recover, the yellowness of skin is wanting."

Much stress has been laid upon the different degrees of susceptibility of different classes of persons to the two diseases, as a basis of distinction. We think that certain facts mentioned by Doctor Lewis, bearing upon this part of the question, by no means tend to support the view he has taken. It becomes necessary for us to repeat.

"During this calamitous season (the autumn of 1819) it would appear that every character of endemic febrile poison was poured in concentrated abundance upon the inhabitants, each one being active in its peculiar way in doing the work of death. The *whites invariably* died on the 4th, 5th or 6th day of black vomit, whilst *the negroes and quadroons*, after protracted suffering from *bilious fever*, resulting in frequent relapses, cold sweats and colliquative diarrhœa, shared the same fate. The equal prominence of bilious and yellow fever makes this a remarkable epidemic—*the diseases however were respectively confined to different classes of persons.*"

Now is it not equally as reasonable to suppose, that in this epidemic there was but one poisonous agent of disease at work, and that the difference in the effects, on different classes of persons resulting therefrom, was owing to the modifying influence of constitutional peculiarities, producing in the case of the negro a partial insusceptibility to the action of the poison, by which that peculiar pathological change, whatever it be, resulting in black vomit, was prevented, as that there were two distinct poisons in operation, one for the blacks, and another for the whites, and respectively confined to these different classes?—Whatever opinions may have been entertained, in regard to the insusceptibility of the negro to the action of the poison of yellow fever, none have ventured to assert that the white man cannot have bilious fever. It is not unreasonable then to suppose, that in this violent epidemic the poison was of so concentrated a character, as to produce those peculiar pathological changes, resulting in black vomit, "invariably" in those of the white population who were attacked, and that something peculiar in the constitution of the negro protected him from the effects of the poison in so far as these changes were involved; while it would be most irrational and absurd to assert, that an epidemic remittent fever could prevail extensively among the negroes of a place without, in any instance, the poison, if distinct, affecting the whites; admitted as it is universally, that the latter class of persons is more liable to this disease also than the former. It may not be irrelevant to state in connection with the present subject, too, that it accords with the experience of Doctor Lewis, that the remarkable exemption from the yellow fever, which this race (the negro) enjoys, extends in a great measure to all the malarious fevers of hot climates."*

"Every summer and autumn intermittent and remittent fevers prevail in these suburbs (those of Mobile), whilst those living in the middle part of the city, seldom have this character of disease, *except in sickly years, and then the cases are confined to those that are acclimated, while strangers unacclimated suffer in the more violent form of yellow fever*;" and, further on we find, that, during epidemics, many of the acclimated, among whom are those who have had the disease in former years, experience mild ephemeral attacks. If these statements of Doctor Lewis were allowed to speak for themselves, what would be their language? something like this we are induced to believe: that one poison only existed; that in healthy seasons, that is to say, when this poison is generated only to a moderate extent, and in a less concentrated degree, it produced intermittents and remittents only; and these confined to the principal foci of its production; but, that in "sickly years," that is to say, when a greater portion of the poison is generated, and in a more concentrated form, its ravages are extended to the middle parts of the city, producing there among the acclimated, including "those who have had the disease in former years," those whose susceptibility to its action has been diminished by long and gradual familiarity with it, intermittent and remittent fevers, and "mild ephemeral attacks of yellow fever, which, we think, according to the admission of Doctor Lewis himself, cannot be distinguished or separated from remittent fever, "while strangers unacclimated suffer, in the more violent form of yellow fever, from its action.

Doctor Lewis tells us, that "negroes rarely have the disease." * * *

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* New Orleans Medical Journal for March 1845. Page 417.

"In late epidemics a few mulattoes only have experienced attacks, and these exceedingly mild, seldom arriving at the black vomit stage." In regard to this fact, is it not a reasonable inference, that a constitutional peculiarity exists, bestowing upon the negro an entire immunity from that peculiar action of the poison resulting in black vomit, which is diminished by the admixture of races in the mulatto! The negro, it seems, is not entirely exempt from the disease, though Doctor Lewis gives no instance in which black vomit occurred in one of the race; while in the mulatto, it is admitted that the black vomit does occur; the disease seldom however arriving at this stage.

"We pass on to the city of Mobile, and there discover, peculiar to her trodden *animalized* streets, yet another character of disease." This text leads us naturally to the consideration of the question, in how far the non-identity of the two diseases or forms of disease, remittent and yellow fever, is sustained, by a marked difference or peculiarity, as is maintained by some to be the case, in the character of the localities in which the latter is wont to occur. Although the remark above quoted is intended, we presume, to convey the idea, that yellow fever is peculiar to the "trodden animalized streets" of a city—there are certain facts mentioned in the essay before us, the true interpretation of which, we think, would lead to a different conclusion, and others, not mentioned, in the present essay, but of which, it will be seen, that Doctor Lewis was aware, proving most unquestionably the erroneousness of such a doctrine.

It is not pretended by any, we believe, certainly not by Doctor Lewis, that remittent fever does not occur in those localities, which seem more especially favorable to the development of yellow fever, and even during the prevalance of the latter disease.

"In 1826 Mobile was *healthy* and the town of Montgomery *very sickly*. In the month of August, two gentlemen came from the *former* place, visited the *latter*, snd after remaining a few days, were seized with *remittent bilious fever*, the disease of the place, and died on the 7th day of illness, of *black vomit*."—This fact is presented by Doctor Lewis with a very different intention from that with which it is brought forward at present,—to wit,—to prove the existence of two separate poisons, in action on the system at the same time.

Now, it will be recollected, that at the time named Mobile was *healthy*. Yellow fever then of course was not prevalent there, and consequently its producing agent had not been generated. How then could the mere fact, of these two young men having been in Mobile produce any modifying effects on a disease contracted in Montgomery, different from what might have been produced by their prior presence in *any other healthy place?* We anticipate the answer to this. It may be said, that although the poison of yellow fever had not been generated to a sufficient extent to cause the development of that disease, still there might have been a sufficiency of it received by these two young men, to exert a modifying influence over a disease of a different character, when once excited. But, before admitting this explanation, it is but reasonable that we should ask, whether all, or any of the cases of bilious remittent fever, which occurred in Mobile during this season,—for we take it for granted that no summer, even the most healthy, since the settlement of the place, has ever passed in which more or less of this disease has not existed there,—terminated in black vomit, and if so, what were the diagnostic symptoms by which they were recognized as cases of bilious fever, and not yellow fever?

Moreover, as these two cases proved fatal during the presence of black vomit, an almost invariably fatal symptom of yellow fever, (admitting for the moment the non-identity of the poisons of yellow fever and bilious fever, and that they were both in operation on the system at the same time; remembering too the fact, that the latter is a much less fatal disease than the former); is it not reasonable to infer that the profound pathological changes preceding and resulting in this symptom, were those determining a fatal termination, and also that the symptoms resulting from the bilious fever poison would have been masked completely by those resulting from the deeper and more profound lesions of the yellow fever poison; in short, that the patients died of yellow fever?—Now, as Doctor Lewis merely tells us, that they were taken "with remittent bilious fever," and died of black vomit, without giving us the symptoms by which they were identified, as cases of bilious fever, it is proper that we should remember the facts, to which we have already alluded, mentioned by our author, showing the exceeding difficulty of diagnosis in certain cases between yellow fever and our other forms of autumnal disease.

But these cases, we think, are susceptible of a different interpretation. "*Mobile was healthy*, and the town of Montgomery *very sickly*." The gentlemen then, leaving a healthy, were suddenly thrown into a very polluted atmosphere, and not possessing that partial immunity from the effects of the poisonous agent of the prevalent disease, produced by acclimation, or gradual exposure to its operation, as would naturally be expected, suffered the full extent of its influence, both as regards the number and profundity of the changes resulting from its action.

We may in the present connection mention a fact, quoted by Doctor Lewis himself on another occasion,* showing the occurrence of yellow fever in a locality very different from the "trodden *animalized* streets" of a city, but in passing, will refer also to the instance of its occurrence in the "little village of Woodville," with which, we are sure, he is familiar. * * * "Père Dutertre, a historian who resided in the West-Indies in 1635 and who was a close observer, *describes with great fidelity* the disease that we now call yellow fever, and says: "*Those who were chiefly attacked were employed in clearing the land, in different islands, and were exposed to the poisonous vapours and exhalations.*"

As a distinguishing characteristic between remittent and yellow fever, others who have adopted the view of their separate nature, have advanced, as confirmatory evidence in favor of their opinion, that one attack of the latter confers a complete immunity from it ever after, or at least that the protection thus afforded, is about as certain as that afforded by one attack of small-pox against any subsequent recurrence of this disease, in the same individual, and that this is not the case in

* New Orleans Medical Journal for July 1844. Page 43.

regard to remittent fever; but, the experience of Doctor Lewis is entirely subversive of this proposition, for he distinctly says, that "there can be no question that some persons have the disease the second, and some *the third or fourth time.*"

In this mere partial immunity, established by Doctor Lewis, afforded by one attack of yellow fever, the observant practitioner, experienced in the treatment of remittent fever, will not fail to perceive a fact, strongly corroborative of the opinion of the identity of the two diseases, and that the former is the result of the same poison, in more concentrated action, and modified in its effects by attendant circumstances, as the latter; for, while it is well known that, though a patient may have several attacks of mild remittent fever, even in the same season, a recurrence of the disease very rarely succeeds a severe attack during the same year; in many not for several years, or until a renewed susceptibility is induced by a temporary residence in a non-malarious region, and in some never, though continuing to be exposed to the same causes and circumstances, productive of the disease in the first instance; and it will generally be found, too, that the immunity thus afforded will be proportionate to the violence of the previous attack. One attack of yellow fever, then, it may be urged, exercises not a complete protective influence, but one a shade more perfect only, than that afforded by such attacks of remittent fever, as merely fail to reach the grade called yellow fever.

But perhaps of all the arguments in favor of the non-identity of remittent and yellow fever, those based upon the supposition that each of the diseases was characterized by certain morbid phenomena discoverable after death, have been the most defective. The account of the supposed "anatomical characteristic" of yellow fever, given by Louis, after his examinations in Gibraltar in 1828, consisting in a certain degree of dryness of the tissue of the liver, attended with diminished depth of color, reducing it to a shade resembling that of sole-leather, mustard, straw etc., confirmed in the minds of many the correctness of the opinion of the distinct character of the disease; and the more recent observations of Doctors Stewardson, Stille and Swett, relative to certain appearances of the liver, found in the examination of some cases of remittent fever, altogether different from that described by Louis, and assumed by these gentlemen as an "anatomical characteristic" of the latter disease, have tended greatly to extend and strengthen this conclusion. The "anatomical characteristic" of remittent fever, as described by Doctor Stewardson, consisted in most instances, in a flabby state of the organ, with change of color; it being externally of a bronze or olive, or a mixture of these colors, internally of an olive color, with an entire extinction of the natural reddish brown, and "the two substances so blended as to be scarcely distinguishable."—Let us see how far the pathological researches given us by Doctor Lewis are in accordance with the opinion that the two diseases under consideration are marked respectively by a distinct and peculiar anatomical characteristic?

Speaking of the "remittent, or according to a strict definition, continued bilious fever, * * * * commonly known as swamp fever," the author says: "In the examination of four bodies in the Hospitals of

Mobile *the liver was atrophied in one, being also dry and brittle like those dying of yellow fever; and of a pale straw color.*" He gives us also the results of sixteen examinations after death, from yellow fever, made by Doctor J. C. Nott of Mobile. "He found the livers in six of a pale gingerbread color, dry and friable, *two olive*, two normal, and six darker than natural and much engorged. This dry, brittle, shoe-leather or straw colored liver, we have found in most of the cases where the hemorrhage, either in the form of unchanged blood, or black vomit, had been excessive. * * * * This lesion is considered by Louis as diagnostic of yellow fever. But as it is occasionally found in old drunkards, and those who have died of long continued wasting swamp fevers, and is confined (from our observations) to those cases of yellow fever which have such a termination, as we have designated, it cannot in Mobile, be received as a reliable or constant morbid appearance in the disease."

Thus, it seems, that we have not only occasionally present in remittent fever, the "anatomical characteristic" of yellow fever, but, that, so far as description extends, in two of the cases of yellow fever, quoted from Doctor Nott, there was present the "anatomical characteristic" of remittent fever. We allude to the two cases in which the liver was of an "*olive*" color, and regret that the appearance of the organ in other respects in these cases is not given.

The observation of Doctor Lewis, in regard to the coincidence of this appearance of the liver in yellow fever, with hæmorrhage or black vomit, we look upon, as of no little interest, and the explanation to which it leads, in all probability correct.

Whichever view may be the correct one, and whatever the ultimate decision in regard to the question of the distinct nature of yellow fever, we think it has been conclusively shown, that the researches and observations of Doctor Lewis tend to deprive it of all right to consideration as such, in so far as the opinion has been based upon any thing like a clearly marked distinction in symptoms, in locality, in the character of persons attacked, in immunity from second attacks, and in the post-mortem appearances; and these, it will not be denied, have formed the principal bases of argument in the affirmative. Nevertheless, in dismissing the subject of yellow fever, we must admit, that, though far from being conclusive, there is much ingenuity in the arguments, and the arrangement of certain facts presented by our author in favor of the occasional action of two separate poisons on the system at once, producing a disease of a blended character.

Leaving the diseases peculiar to the "trodden amimalized streets of Mobile," we will present verbatim the principal portion of the remarks of Doctor Lewis, relative to intermittent fever; some of which, we think, will be deemed both new and *peculiar*.

"The obstinate grade of fever, incident to the swamps and marshes of the tertiary already described, we believe to be identical with intermittent fever, differing only in degree. Aside from this and other disorders which have been the burthen of our narrative, we now wish to direct attention to the diseases of the mill-ponds, simple marshes, creeks and rivers of the poor unproductive portion of the country. In the vicinity of these, we find a disease which throughout all time has occupied a conspicuous place in the catalogue of endemics, *and is easily distinguished from all others.* It is intermittent fever, a definition of which is here unnecessary. In some portions of Alabama, in many parts of Florida, *and throughout Southern Georgia, it is the only endemic disease.* In many of these sections of country, especially along Flint river, the natives do not experience violent, well marked attacks; but the prominent ears, flat noses, tumid belly, large spleen, slender limbs and waxen skin, tell too plainly to be mistaken, of the powerful, though indirect effect of this *febrile agent* on the constitution. It is only under circumstances of this kind, where the only supposed cause is the decomposition of vegetable matter, that the disease is seen in its *simple genuine character; in that form, it is the most inveterate, obstinate, well marked epidemic, of which we have any knowledge. In many instances it may be interrupted or changed in type, but cannot be cured;* its victim may fly to Canada or London, but it will still be with him, ready at an unguarded moment to re-inflict its sting."

We pass next with Doctor Lewis to the

"*Therapeutics of the Third epoch.*"

From some interesting remarks, relative to the different views entertained by physicians from different sections of the United States on their first settlement in Alabama, and their subsequent modification, we extract the following:

"The advocates of these respective doctrines remained equally divided only for a short time; for soon they found the diseases of Alabama presented new features from those they were accustomed to witness in the older States, and want of success in practice compelled them to abandon original theories, and to direct their thoughts in a new channel of speculation."

There are but few now in Alabama, who doubt that the theory of disease propounded by Doctor Cook, and the practice based upon it, both of which were at one time so generally received in the state,—have been the cause of much mischief, though in all probability it may have been otherwise in the sections in which the experience on which the author based his views, was acquired.

Of Doctor Cook and his theory, our author uses the following language:

"In the far West a new theorist in the science had sprung into existence, and with pompous display promulgated his reseaches in pathological anatomy to the world; and his disciples, burning with ardent zeal, infused the doctrines of their master into every hamlet, throughout the populous valley of the Mississippi."

"Of all the doctrines of former or latter days that have elicited the attention of mankind, none ever found the same favor, (in Alabama we presume the author means. Rev.) with popular opinion, as did that which was peculiar to this sect. The theory and practice of Doctor Rush had attained a wide spread reputation, yet there was too much obscurity in his principles of practice for the common mind; but this doctrine, fresh from the Western Temple of Æsculapius, was within the comprehension of the most ordinary intellect, and the pathological theory of Doctor Cook became at once the medical doctrine of

Alabama. The ploughman, the mechanic and the politician of the village were ardent votaries at the shrine of this modern Proteus." (?)

"In 1835 a new impetus was given to this absurd doctrine in medicine, that had gradually began to decline in popular favor from want of success attendant on the practice, as is usually the result of all visionary schemes after having run their allotted time with the susceptible masses. This improvement in pathology and practice had its origin through the influence of a certain Doctor E., who was well known in South Alabama, where he made his appearance in the winter of 1834–35—a man of commanding figure, extremely tall, prominently marked features, and although his long hair was whitened by seventy winters, still he possessed all the energy and activity that belongs to the prime of life. Added to his imposing appearance, there was a boldness and fluency of speech, that sways the multitude, and having at one time been the student of Doctor Rush, and subsequently graduating at the far famed University of Edinburgh, he failed not to impress his theory and practice on most of those who came in contact with him, and the few who had the rashness to oppose his opinions soon withered beneath the popular blast of an ignorant multitude."

In appearing before the profession as the reviewer of "the Medical History of Alabama," and therefore pretending to some degree of acquaintance with the subject, perhaps we ought to blush, in the acknowledgement we are forced to make, that we have never before heard or read of this Doctor E., nor have we now the most remote conception of who he is, or was. Nor is this all. On enquiry of several of our friends, of whose zeal in the cause of the profession and accurate knowledge of every passing event of interest connected with it, we havet hought we had just cause to be proud, we find with them also the name of Doctor E. unknown, if ever heard, and his labors have passed into oblivion. This seems curious and difficult to reconcile with the conspicuous position assigned him in the "History," and the extent to which, according to Doctor Lewis, he influenced the medical doctrines and practice of Alabama. We can find but one at all plausible explanation; and that is, that "Doctor E." in his time had the good fortune to be the medical prodigy of some retired village, to which we must think his labors were confined, and where the wonder was,

"That one small head could carry all he knew,"—

a grave discourser, subtle in argument, who, meeting with our author in his earlier and more imaginative days, *loomed* then upon his fancy; that now, distance (of time) "lends enchantment to the view," and a picture somewhat magnified is the result. Indeed we think that the very style and manner in which Doctor Lewis introduces and speaks of him, would incline the reader to this belief. Thus,—from—we are not told where,—at the age of seventy years,—with something of Lara-like mysteriousness, "he made his *appearance*," at such a time; and the very genius of romance itself seems to have been invoked to inspire our author, and suggest the language, with which the personal appearance of the medical *Hero* is described.

In regard to the use of calomel, and the pre-eminence at one time assigned the liver in tropical diseases, we find the following:

"*The uninstructed mind* incapable of dispassionate reasoning, in fancy's fearful apprehension, was wont to behold at every sick bed-side, no less an adversary than the champion of death, presid.ng in the form of hepatic inflammations or congestion, in which all *their* attention was absorbed, and with which *they* entered into a rash encounter, with formidable weapons of potent agency, to master with a deadly blow the grim monster. * * * * The unfortunate liver was here made the citadel of attack against which enormous doses of *calomel* were discharged in rapid succession, until the undermined frame gave way to repeated bombardment. The voice of suffering humanity appealed in vain to the doughty chieftain who carried on the warfare, to cease for a time his operations, to raise the siege, and permit nature for a space to recover her balance of power, but without the remotest chance of pacification.* The calomel vial was again and again appealed to, to stimulate this dormant function, until bile on bile, black as the demon of darkness, was thrown forth, which in their ignorance of chemical action was mistaken for the presence of the enemy, and the exhibitions of forced excretions, instead of being received as a sign of subjection to the power of medicine, was heard as the voice of menace, and the warfare was continued with greater activity."

Now, although our opinion accords with that of Doctor Lewis, that much mischief has been the result of the doctrine assigning such pre-eminence to the liver in the pathology of southern diseases, and the free use of calomel, growing as it were, from it,—still we are aware that it is but opinion, and that there are many intelligent practitioners who entertain a different belief, and who yet may possibly be correct. From the facts, that so little is absolutely fixed and settled in medicine, and that the favorite theory and practice of one day, have so frequently been the subjects of ridicule for the next, we are of opinion that a lesson of modesty might be derived, and while condemning with unmeasured language the past, it would be well to recollect that another generation is to succeed, through whose ordeal the labors of the present have to pass. This sentiment is suggested by the seeming heedlessness of the language of Doctor Lewis, in regard to the once popular *hepatic* doctrine, and *mercurial* treatment, respecting the advocates of which we find such passages as: "the uninstructed mind incapable of dispassionate reasoning,"—"ignorance of chemical action,"—"misguided men who had turned aside from the true path of medical knowledge,"—as if indeed the author or any other had really pointed out any true path of medical knowledge, which had been received and acknowledged generally as such.—Surely the memory of our own venerated Rush, and of the recently departed Johnson, should have inspired (even in speaking of acknowledged and demonstrated errors, of which they may have been among the chief propogators), a more respectful consideration; and much more might this be claimed, when mere opinions, the one as unsubstantiated as the other, form the subject of discussion. The school-boy of to-morrow may have put at his tongue's end facts, of which the sage of to-day knows nought. Is

* See Western Journal of Medicine and Surgery for March 1847. Page 209. Rev.

the assumption of superior and perfect knowledge therefore becoming in the former, or the imputation of ignorance proper in its application to the latter? In a former quotation it is stated that the old opinion, that "yellow fever is but a high grade of bilious fever, is founded in *ignorance* of a correct knowledge of all the facts." And yet this has been and is the opinion of men, who possessed and possess excellent opportunities of making themselves acquainted with the facts bearing upon the question; and whose capacity for observation and deduction have been admitted, to say the least, to be respectable. And here we may again mention the name of Rush.

The author next,—"traveling out of the record," we think,—takes up the subject of Thompsonianism, to which,—though we could fondly wish otherwise,—we fear he inclines with something of partiality. He says of it, that, "however false in theory, the practice was much more appropriate to the type of disease of that period, than any other in vogue," and that it "achieved signal triumphs over the equally false theories ("de questibus non est disputandum." Rev.) and but too fatal practice of the unreflecting *routinist* of the regular profession." * * * * * "In many of those localities, where its triumphs were achieved, it still holds its place in popular favor."

We confess here again our ignorance in regard to statements made as facts by our author; for, as to the "triumphs achieved," by Thompsonianism, the avowal must be made that heretofore we have remained uninstructed, having had before us no evidence of their existence by any competent disinterested witness; and until Doctor Lewis voluntarily presented himself upon the stand, they were heralded only and proclaimed by,—to be sure very good, but—garrulous and silly old women, and sillier "busy-bodies," or *amateur practitioners* of the other sex. However, as to what may be the degree of intelligence of the people and their taste for Thompsonianism in "certain localities," we cannot answer; but so far as our own personal knowledge extends, the system, we know, has been received with little favor; and we gladly, therefore, embrace the opportunity, in so far at least, of removing so foul a bar-sinister from the State's escutcheon. In pursuance of this, we will wander a moment from the "Medical History of Alabama" to the Medical History of one "locality" in the State,—Montgomery—so far as connected with Thompsonianism. It is soon told.

About the year 1833 or 1834 the first of these "learned Thebans" settled in the place,—and though far superior in intelligence and education to the class generally to which he belonged, and even respectably connected in the State, to such a degree was popular indignation excited by the results of his practice, that he was actually burnt in effigy, and forced to retire in disgrace.

About 1842, another tried his fortune, not with precisely the same results, for getting nothing to do, he was permitted to depart unnoticed. A third—and so far the last—two or three years after, presented himself for "a share of public patronage." He departed, not under the most favorable auspices, nor in the most respectable manner, after a few months spent in idleness.

We now respectfully crave the reader's pardon for occupying his time at all, with this most hideous excrescence on medicine, and would not have done so, had not the position assigned it in the "Medical History of the State" demanded for it a passing notice. We therefore leave the disgusting and loathsome subject, and pass with pleasure to our author's "incidental notice of the general plan of practice agreed upon by most of our medical men in the treatment of some of the maladies described in the foregoing pages"—and first, of congestive fever.

"To equalize the circulation and call back the vital heat to the surface, is the first indication, and with this view, revulsives are applied extensively on the external surface, strong cataplasms of mustard are applied to the abdomen and extremities, and the more extensively the better."—In addition to the above, Doctor Lewis also suggests,—and the practice, we have no doubt, is a good one,—the use of dry cupping along the whole course of the spinal column.

"Calomel, opium, quinine and piperine are the favorite remedies for internal administration; they are used singly or variously combined, according to the condition of the patient, and the peculiar notions of practitioners. If vomiting and purging exist, calomel and opium (five grains to one) may be given every hour until quiet is restored."

After reading the violent tirade of Doctor Lewis against the "calomel vial," and the doctrines which led so strongly to its use, we were certainly not prepared for this. What; in congestive fever, *if vomiting and purging exist*, administer five grains of calomel every hour till quiet is restored? Now, it will be recollected, that Doctor Lewis estimated, that of the grave and more malignant cases, about one third prove fatal between the second and third day,"—but many, we are assured, do not terminate till a much later period. In a portion of the fatal cases at least, if not in all, it is well known that quiet is not restored, till death; the vomiting and purging, and general restlessness, continuing to the last hour. If then the patient live but 48 hours after the commencement of treatment,—quiet not being restored in that time,—he will have taken, by the termination of the case, no less than 240 grains of calomel. Surely the most ultra Cookite, could desire but little more than this; and if perchance he ventured upon the use of larger portions, he would scarcely dare, we think, to "*discharge them in such rapid succession*."—We do not wish to be considered among those, who join in the "hue and cry" against calomel; but five grain doses, repeated every hour for two or three days under the circumstances designated by our author, we look upon, to say the least, as little short of an abuse of this valuable and potent article.

In regard to venesection in congestive fever, we find the following singular and most strange paragraph.

„In but few cases do even the most "bold and bloody," use that "double-edged sword," the lancet—that *ignis fatuus* theory has often led the way to the chamber, but the murdered victim has as often risen up in judgement." * * * *

"Quinine is not given in congestive fever as a *stimulant* to promote reaction. On the contrary, it is a popular and well founded impression, that when given in adynamic diseases, in large doses, its effect is decidedly sedative, and consequently prejudicial. * * * * * It is therefore given in combination with stimulants." * * * * *

The author cites a case, given in a communication to him, from one of his correspondents,—Doctor Ames,—showing the impunity with which, under certain circumstances, large quantities of quinine may be administered. "I gave recently to a negro boy under twelve years of age about fifty grains within twelve hours, without producing any deafness or ringing in his ears." The case was one of cerebral congestive fever, in which disease, it is the opinion of Doctor Ames that quinine is "tolerated as tartar-emetic is tolerated in inflammatory pneumonia, and bleeding in encephalic inflammation."

The following remarks relative to certain facts,—sustaining, seemingly in a most incontrovertible manner, the doctrine of the sedative action of quinine,—are also given from the letter of Doctor Ames. "These relate to cases of debility; either diseases of this kind, or exhaustion from hæmorrhage or protracted diseases in which the depressing effects of bleeding or cathartics have been aided by long abstinence. In these, quinine is always injurious, quickening the pulse and augmenting the general debility."

Doctor Lewis makes some remarks relative to the early use of quinine, and mentions the names of some gentlemen who were in the habit of administering it in pretty decided doses, as far back as 1830. Several Southern physicians however might be named, who made free use of it several years anterior to this. But to whom in reality the credit is due, would at the present day be a most difficult question to decide. Among authors who practiced in malarious sections, shortly after the introduction of the Peruvian Bark into general use, we find *it* recommended in doses, which would be equivalent to the doses of quinine, as given by the judicious and guarded now; and in their venerable tomes also may be found precedents for the use of quinine, in recommendation of the former in full doses, in the same diseases, and under the same circumstances as those in which the latter is administered with such signal advantage at the present day. "It is questionable indeed, whether quinine is even now more highly esteemed, than was the crude bark in times past —or its real properties better understood, than were those of the latter, by a few, more than a century ago." Although a train of circumstances,—among which especially may be mentioned the exceeding popularity of the doctrines of certain authors, whose views were not favorable to the use of the Bark,—led for a time to its partial exclusion from practice; still it is but reasonable to suppose, that a number adhered to its use, and that the practice was transmitted down from one to another, till the discovery of the quinine, and that then the latter was adopted, by the last few of the succession, in proportionate doses, and for similar purposes, instead of the Bark.

The views of Doctor Lewis, we think, in regard to the action of quinine, will be found somewhat *peculiar*; but before remarking further on these, we will copy some remarks relative to the treatment of pneumonia.

"During the past two seasons of winter and spring, we have treated in the Marine Hospital 40 cases of pneumonia—the subjects were mostly active robust young men, belonging to the volunteer service.— We studied the character and tendency and effect of remedies on this disease, with no little interest. Twelve were of a remittent form—5 pleuro-pneumonia, 10 bilious, and 13 typhoid pneumonia. In all those of the remittent, and a few of the bilious type, quinine was used at different times; but was beneficial, so far as we could discover, in only three of the remittent, and very partial in these. In three of the bilious variety the pulse was corded and tense; general blood-letting was practiced in these. Cups were used in *every* case, but it was *seldom* that the strength of the patient would permit the abstraction of blood, even in this way. Tartar emetic was not tolerated in doses exceeding the eighth of a grain. Alteratives, such as calomel and Dover's powder and blue pill were occasionally given—expectorants were also used, and *purgatives* were necessary in most of the cases. The reliable remedies however were mustard cataplasms and poultices, after the manner advised by Doctor Baldwin of Montgomery, and the carbonate of ammonia. These were used more or less in every case, and throughout the whole course of the disease. In 6 cases of the typhoid or continued variety, we gave brandy in combination with ammonia, and with decided advantage. In many cases it is necessary to sustain and strengthen the patient in his exhausting efforts at expectoration,—otherwise the immense accumulation of pasty, dark colored matter, *within the cavity of the chest*, would have suffocated him. Stimulants, when judiciously given, instead of checking, materially aid expectoration, upon the ease and profuseness of which depend the life of the patient. This disease *runs its course in from eight to fourteen days, and it is dangerous to attempt to cut it short.** Three of the forty only proved fatal."

"*We are convinced beyond the shadow of a doubt, that had contra-stimulants* been freely used in the treatment of these cases, not one would have recovered; had quinine then been given with the best results, it should not on that account be classed among contra-stimulants —if so, brandy, opium and ammonia are entitled to the same distinction."

Let us, if possible,—or as near as possible,—present this in a logical shape, and see how it will look.

Doctor Lewis *thinks*, (for in reality his being "convinced" *can* amount to no more than this,) that contra-stimulants used freely would have killed all these patients. *If quinine had been* given with good results, *ergo*, quinine is a stimulant.

Now, if the opinion of Doctor Lewis in regard to what *would* have been the result of the administration of contra-stimulants in these cases, amounted to a demonstration of the fact; and then had quinine been used "with the best results," there would have been at least some appearance of legitimacy about his conclusions;—But, to make deductions from such *shadowy* premises as these, and expect them to be received? Why, as well might Doctor Lewis attempt to erect a towering, massive edifice upon moon-shine, and to persuade the world that its basis had the solidity of adamant.

Among those generally who believe that quinine is a contra-stimulant, its use we think would be considered inadmissible,—or admissible only with certain precautions and combinations,—even in "what are supposed to be inflammatory affections" of the thoracic viscera, in such a prostrate condition of the system, that "tartar emetic could not be

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* Here, and in several other instances which we neglected to designate, the italics are ours—Rev.

tolerated in doses exceeding the eighth of a grain;" and where the strength of the patient would not permit the abstraction of blood with cups. By the by, *why did* our author persist in cupping every one of these forty cases, seeing that it was "but seldom that the strength of the patient would permit the abstraction of blood even in this way."

* * * * "In the vicinity of the fever swamp there occasionally occurs, in plethoric healthy emigrants, cases of continued bilious fever attended with "violent pain in the head and back, hot skin, *and corded, tense pulse*." We have administered quinine in "sedative doses," in many of these cases, and it never failed to augment all the imflammatory febrile symptoms."

Was there ever anything so wayward and perverse? Truly Doctor Lewis has just cause to complain of the results in his hand, in some respects, of the use of the article under consideration. As if endowed with the most vivid instincts, coupled with a settled mischievous perversity towards him, and a determination to run counter to his inclinations at all points;—chameleon like, taking its hue from surrounding objects, and with Protean facility changing its character with changing circumstances, it "never failed to augment all the inflammatory febrile symptoms," in cases of a phlogistic character, where of course he would have wished it to act as a sedative; while, where the action of a stimulant might be desirable, "given in adynamic diseases, its effect is decidedly sedative." Will not the article have to be eventually excluded from all rational practice, or absolutely expelled from the materia medica, if it goes on behaving in this manner?

"Doctor W. M. Boling has maintained * * * * * * * that it (quinine) is contra-stimulant and highly serviceable in the treatment of *inflammatoıy affections*—inflammatory diseases, strictly so, are now rare among us." And yet Doctor Lewis has just told us, that "during the past two seasons of winter and spring"—in the course, it will be seen, of not more than six months,—he had treated in one small hospital no less than forty cases of pneumonia. But to proceed with our subject. "We have examined somewhat the notes of cases upon which Doctor Boling predicates his opinions, complicated as they are, these cases cannot be properly classed with the phlegmasiæ. * * * * * * * * * We may be wrong and Doctor Boling right; be this as it may he has too much liberality not to agree with us in the sentiment of the Poet,"

> "One man's word is *no man's* word,
> Truth demands that *all* be heard."

Well,—Doctor Lewis has been "heard," and "all" have decided;—"complicated as they are, these cases cannot be properly classed with the phlegmasiæ"—"I am sir Oracle"—

The subject, as regards the character of these cases, and as a consequence also, any influence from them, is of course no longer open for discussion, Doctor Lewis having set at rest and definitively settled the question. Were it otherwise, however, we would then feel free to remark, that we had not only "examined somewhat the notes of cases, &c.," but had scrutinized them most thoroughly, and that, though Doctor Boling does not pretend that they all properly belong to the phlegmasiæ,—(for he distinctly says in regard to them that "some of the cases will be recognised as remittent fever, complicated with local affections, while others will be recognized as local inflammations, on the consequent febrile excitement of which, a malarious influence is manifested by a periodical remittence,"*) still in every instance in which the separate caption claims for the case an inflammatory character, we find both the *rational symptoms and physical signs of inflammation present.* Still we will not say that they are "inflammatory diseases, strictly so," as this would be merely placing assertion in opposition to assertion. Seriously, Doctor Lewis now and then seems to have a summary way of settling matters concerning which he undertakes to treat: Another, in deciding a question like the present, involving points of at least some practical interest, would perhaps have given at the same time some of the reasons upon which he based his decision. Not so our author, however, for though, for ought we know, he may have had them "plenty as blackberries,"—he names not one.

We must now draw our notice of the paper before us to a close.—That we highly appreciate it, is apparent from the extent of our quotations and comments, and the solicitude therein manifested to bring it generally before the profession. We have, it is true, discovered a few seemingly objectionable points in it, a portion of which we have presented to the reader. More might be named, and more might be said upon them; but to the hypercritical, who may think we have been remiss in the performance of our assumed duty, we would suggest that it is an easier task in many instances to discover defects,—or what we may suppose to be defects,—in the composition of another, than to produce a labor more perfect. We can readily find an excuse for the presentation of the paper before the society in the shape it bears, seeing that the prize was to be awarded by a given time; but we must give expression to an opinion formed, even in the perusal of the first pages,—that Doctor Lewis has not only been guilty of great unjustice to himself, but has manifested a want of respect to his brethren of the South,—who more especially would be expected to feel an interest in his essay,—by thus forcing it prematurely, "deformed, unfinished" into the world, and presenting it

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* We find the following marginal note some few pages back, appended through mistake apparently to the observations quoted from the letter of Doctor Ames on cerebral-congestive fever,—in reference we presume to the series of cases here alluded to, which were published in the American Journal of Medical Sciences for July, 1844—and in the Western Journal of Medicine and Surgery, for September of the same year.

"Doctor Lewis takes this method of informing his friend Doctor Boling, that he (Doctor Lewis) would be as far from classing these diseases with the phlegmasiæ, as Doctor B. would the "unknown agent of yellow fever" with "remedies."* Now, the reader, who has accompanied us thus far, and has therefore discovered, what a way of his own has "Doctor Lewis in some things, we are disposed to think, will scarcely expect that Doctor Boling would be greatly surprised, even at the announcement that he (Doctor Lewis) would not class pneumonia, pleuro-pneumonia, bronchitis, &c., among the phlegmasiæ. As the Doctor has "changed all this," perhaps he will tell us, where in the nosological table he has placed diseases of this character.

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* American Journal of Medical Sciences, for July, 1844. Page 44.

before the profession, wanting shape, symmetry, and proportion. In conclusion; for the "Medical History," we most heartily wish, that,—corrected however in many respects,—it may form the nucleus of a more extended work on the diseases of the South, to which Doctor Lewis,—having in view a proper regard for his own reputation,—is so well capable of doing justice; and for the author himself, that he may live through many *ataxic epochs*, and "great longevity mark his pilgrimage on earth."

W. M. B.

II.—*A System of Surgery.* By J. M. Chelius, Doctor in Medicine and Surgery, Public Professor of General and Opthalmic Surgery, Director of the Chirurgical and Opthalmic Clinic in the University of Heidelberg, &c. &c. &c. Translated from the German, and accompanied with additional notes and observations. By John F. South, late Professor of Surgery to the Royal College of Surgeons of England, and one of the Surgeons to St. Thomas Hospital. In three volumes. Philadelphia. Lea & Blanchard. 1847.

This work may be regarded as embodying the principles and doctrines of the German Surgeons of the present day; and as the highest authority on the several and multifarious subjects of which it treats.. As an evidence of the high estimation in which it is held both at home and abroad, we may state that it has already reached its sixth edition, although the first was issued but yesterday.

It displays all that order,—careful arrangement and patient research for which our German contemporaries are so celebrated. To give some idea of the scope and character of this work, we shall enumerate the different divisions and subdivision adopted by the author.

Under the 1st division, he treats of *Inflammation in general—2nd, of some peculiar kinds of inflammation;*—as of *erysipelas;—burns; frost bite—boils and carbuncle.* 3d, *of inflammation in some special organs; as inflammation of the tonsils,—of the parotid gland,*—of *the breasts,*—of *the urethra,—of the testicle,*—of the *muscles of the loins,*—of the *nail-joints,*—of the *joints, of the synovial membranes—of the cartilages*—of the *joint-end of* the *bones*, as of the *hip-joint—shoulder-joint—knee-joint*, and so on.

Under the 3d devision he treats of those *diseases which consist in a disturbance of physical connexion.* 1st. *Fresh solutions of continuity—as wounds and fractures.* 2nd. *Old solutions, which do not suppurate,*—as *false joints,—hare lip,—cleft in the soft palate,—old rupture of the female perineum.* Of those which do *suppurate,*—as *ulcers*—1st, in *general*—2d, in *particular,*—as the atonic,—*scorbutic,—scrofulous,—gouty—impetiginious—venereal—bony ulcers* or caries.

2nd, *Fistulas*, as the *salivary—biliary, feœcal,* and *artificial anus—anal fistula—urinary fistula.*

3d. *Solutions* of *continuity by changed position of parts;* as *dislocations—ruptures—prolapses—distortions*—4th. *Solutions of continuity by* *unnatural distention*;—as in the *arteries*, *aneurisms*,—in the *veins varices*,—in the *capillary-vascular* system, *teleangiectasis*.

3d Division—*diseases dependent* on *the unnatural adhesion of parts*—1, *Anchrylosis* of the *joint-ends* of *bones*, 2, *growing together and narrowing of the* aperture of the nostrils; 3, unnatural adhesion of the tongue;—4, adhesion of the gums to the cheeks,—5, narrowing of the œsophagus,—6, closing and narrowing of the rectum,—7, growing together and narrowing of the prepuce—8, narrowing and closing of the urethra,—ditto of the vágina and mouth of the womb.

4th *Division—Foreign Bodies*—1st, *Foreign bodies introduced externally into our organism*—1, into the nose—2, into the mouth,—3, into the gullet and intestinal- canal—4, into the wind-pipe—2nd, *Foreign bodies formed in our organism by the retention of natural products*.

1st. Rentention in their proper cavities and receptacles,—1, as ranula,—2, retention of urine,—3, retention of the fœtus in utero,—or in the cavity of the belly.

2nd. Extravasations external to the proper cavities or receptacles—1—as blood-swellings on the heads of new born children,—2, collections of blood in joints.

3d. *Foreign bodies resulting from the accumulation of unnatural secreted fluids*,—1, as lymphatic swellings,—2, dropsy of joints,—3, dropsy of the bursa mucosa,—4, water in the head—spina bifida,—5, water in the chest and empheyema,—6, dropsy of the pericardium,—7, dropsy of the belly,—8, ditto of the ovary.

4th. *Foreign bodies produced from the concretion of secreted fluids.*

4th Division—*Diseases which consist in the degeneration of organic parts, or in the production of new parts*,—1, enlargement of the tongue,—2, bronchocele,—enlarged clitoris,—4, warts,—5, bunions,—6, horny growths,—7, bony ditto.—8, fungus of the dura-mater,—9, fatty swellings,—10, encysted swellings —11, cartilaginous bodies in joints,—12, sarcoma,—13, medullary fungus,—14, polyps,—15, cancer.

VI. Division—*Loss of organic parts*—1, *organic replacement of already lost parts*, especially of the face, according to the Tayliacozian and Indian methods—2, *Mechanical replacement*—application of artifical limbs and the like.

VII. Division—*Superfluity of organic parts.*

VIII. Division—*Display of the elementary management of Surgical operations.*

General Surgical operations,—Bleeding,—cupping—application of issues—introduction of setons—amputations—resections, &c.

The preceding arrangement is original in many respects; it is at the same time natural and full of suggestions to the reader—No one, but an erudite German Surgeon could have arranged his subject with such order and in such strict accordance with surgical science.

The work comprehends three large volumes, with most copious notes and additions by Mr. South. Like all writers, whether in surgery—or the practice of medicine, Mr. Chelius grapples first with inflammation, at once a perplexing and endless subject of discussion. He defines inflammation to be "that condition of an organized part in which the vital process and plasticity of the blood are unnaturally raised, and which is manifested by pain—redness—increased temperature and swelling."

By uttering the preceding doctrine, Mr. Chelius at one step enters upon debatable ground; since inflammation may and does exist,—we opine, without many of the phenomena enumerated in the preceding paragraph. After all, it is of little consequence whether we quarrel about this or that symptom—such and such a phenomenon, provided we agree as to the actual condition and alteration of the parts—the seat of this morbid process. Andral, with his usual originality and judgment, endeavered to banish the term inflammation from pathological technology, and substitute other terms more descriptive of the pathological changes which usually characterize certain morbid processes.

To him we are indebted for an analysis of the complex phenomena which ordinarily mark the progress of inflammatory action.

In fine, whatever doubts may be entertained in regard to the true condition of the parts in inflammation, *certes*, we thoroughly understand the different ways in which it may terminate. Further, we recognize its existence—we can combat its symptoms and often check its progress; and beyond this, we need not seek to go, provided we could restrain the speculative tendencies of our minds.

We propose to make some running comments upon *Inflammation of the joints*, at once an important and serious class of disease—Mr. Chelius has given us, in the work on hand, some excellent advice on the subject. He says that the various parts of which the joints consist, stand in the most intimate relation with each other; therefore, in disease of any one, all the other parts are gradually drawn into participation. We speak of the *ligaments* and *synovial membrane*, the *cartilages*, and the *spungy* ends of the *bones*. In each of these structures inflammation may be set up as the primary disease, which may be communicated to the others, and various organic changes be produced which are described as *articular* fungus, &c.

Inflammation of these structures may be produced from various causes—from external injury—from the effects of cold—from constitutional causes—as gout, rheumatism, syphilis, or the retrocession of certain cutaneous eruptions.

Diseases of the joints have received too little attention from American practitioners; peculiar and complex in structure, delicately organized and nicely arranged, the joints are subject to a great variety of affections, sometimes difficult to diagnose, and often ending in the destruction of the joints. How often, in proof of this, do we see "living and moving" monuments of either our indifference to or neglect of the diseases of the joints; constantly we encounter in the streets a poor cripple, crutch in hand, with an anchylosed-joint,—the result of some chronic disease, now too far advanced to be remedied.

Indeed, we know of no class of affections which are more likely to end disastrously, if mistaken or overlooked in their early stages. Sometimes the inflammation begins within and progresses without, and *vice versa;* it gradually extends from one structure to another, involving sooner or later the entire structure composing the joint.

As the ligaments and the capsular membranes are closely allied in their anatomical structure, so they, in like manner, are subject to the same morbid alterations. The synovial membrane partakes of the nature of a serous tissue—first it resembles that class of tissues as being a closed sack—secreting a serous fluid which serves to lubricate the articulating surfaces and favor the gliding of one body over that of another. The fibrous structure of the joints is also the seat of a peculiar kind of inflammation usually called articular rheumatism. It is as obstinate as painful and often assumes a chronic form, especially in the aged and infirm.

In acute inflammation of the ligaments of the articulations, we have usually severe pain, especially on the slightest motion or pressure; it is also attended with heat and a sense of fatigue, almost amounting to actual pain. Occasionally it is accompanied with considerable swelling, of an elastic feel, of a dark reddish hue, and intolerent of the touch.

In this state of the parts, the limb is usually flexed,—the pain extends to the flexor and extensor tendons,—attended with considerable febrile excitement—furred-tongue—head-ache and the usual concomitants of fever. If the inflammatory action be not arrested, it soon extends to the synovial sack, and finally to the cancellated structure of the heads of the bones themselves. Then the work of destruction sets in,—softening of the cartilages—thickening and other alterations of the synovial tissue—with disintegration of the epiphyses of the bones—a state of things generally irremediable.

It is in the early stage of the affection that the treatment, to be successful, must commence; to delay is to sacrifice the use of the joint—perhaps the health—even life of the patient. If the general health be good,—the constitution strong, active depletion from the parts inflamed must be steadily enforced, until the heat, tenderness and pain are greatly reduced or removed. Local depletion with considerable purgation in some cases, will speedily prepare the parts for other modes of treatment.

Nothing is so efficacious as a large fly-blister over the entire joint—it should be large enough to envelope both ends of the bones; and as soon as one blister heals, another should follow over the same spot—and so on until all soreness and pain on motion of the joint is removed.

If we have reason to believe that the joint-inflammation is allied to or connected with a scrofulous constitution—local depletion must be sparingly used; and blisters will prove, in a large number of cases, positively hurtful—by aggravating the pain and prolonging the inflammation.

A peculiar affection of the joints has been described by some authors: Sir Astley Cooper calls it *Gonorrhœal rheumatism*; it consists in an inflammation of the synovial tissue,—is attended with great pain and some swelling of the joint. It is concomitant with or succeeds a gonorrhœa—is usually accompanied with an obstinate opthalmia, not always the result of contact, as generally supposed.

The elder Cline was the first to call attention to this affection—Brodie and Lawrence have both described the disease. We have seen one case, much like those detailed by the above authors.

It is not improbable but that this affection is produced by the treatment usually adopted to cure a gonorrhœa—favored by certain constitutional peculiarities.

The treatment of this affection must be such that whilst, by local depletion and counter-irritation we endeavor to relieve the parts affected, we must at the same time not forget a proper constitutional treatment.

We may, for this purpose, use Zittman's decoction,—the hydriodate potassa, and the usual anti-syphilitic class of medicines.

It will be utterly impossible to even glance at the great variety of matter contained in the three large volumes before us. Much that it contains is already familiar to the *reading* American surgeons. The book is German both in matter and manner—none the worse for all that.—Take it as a system of surgery, it is quite practical and equal to any of the present day. Professor South, the English Translator, has added as much as the original text contains. His notes consists partly in extracts taken from the ablest authors of past and the present times, and partly in personal experience and observation.

We shall conclude this brief and imperfectly written notice, by advising those who wish to learn the theoretical and practical part of surgery, to buy and study Chelius. It treats of every variety of surgical disease, and in a style at once clear and concise.

A. H.

III.—*Lectures on the Principles and Practice of Physic, delivered at King's College, London.* By Thomas Watson, M. D., etc. etc. etc. *Third American, from the last London Edition, revised with additions.* By D. Francis Condie, M. D., *Secretary to the College of Physicians, Author of a Treatise on Diseases of Children.* Philadelphia. Lee & Blanchard. 1847.

We have received from the Publishers the *third* American edition of Watson's great work, on the Principles and Practice of Medicine. To say that it is the very best work on the subject now extant, is but to echo the sentiments of the medical press thoughout the country!

Until this work made its appearance, books on the practice of medicine were little else than a prosy detail of the symptoms of disease, backed by a half empirical and half rational practice.

Here, we have our knowledge of disease posted up to the latest dates; all the recent discoveries made in the different departments of our science are applied with a master's hand to the elucidation and cure of disease. As this work has already been noticed at some length in a former number of this Journal, we shall conclude our notice, by recommending every practitioner and student throughout our country, to read attentively Watson's *Lectures* on the "*Principles* and *Practice of Medicine.*"

A. H.

Part Third.

EXCERPTA.

1—*A Course of Lectures on the Physical Phenomena of Living Bodies.* Delivered in the university of Pisa. By Professor MATTEUCCI, F. R. S.

(Translated for THE LANCET, by S. J. Goodfellow, M. D., Lond., late Physician to the Cumberland Infirmary.)

LECTURE I.

GENTLEMEN:—I never before felt so diffident of my own powers as I do now, when I am about to perform the duty which has been imposed on me, of delivering a course of lectures upon the *Physical Phenomena of Living Bodies*. But while I feel the difficulty of such an undertaking, I also hope that my efforts may be rewarded by the advantages you may derive from them. In fact, this is perhaps the first time that such a course of lectures has been introduced into medical studies; there is no book which treats of this subject: the elementary principles are, indeed, scattered here and there, but they have never been considered in that light which is the most favorable to their development.

If, at the commencement of every course of instruction, the teacher endeavors to give an exact definition of the science under consideration, to show its limits and its objeet; in one word, to trace a plan or programme, certainly the necessity for these preliminaries was never more apparent than in the present instance.

Living bodies possess the general properties of all natural substances. The most extravagant *vitalist* has never dreamt of denying that living organized matter is extended, impenetrable, divisible, and porous. How can we believe that heat, electricity, light, and chemical affinity, act upon these bodies in a manner entirely different to that in which they are observed to act upon other natural bodies?

In some highly esteemed works on physiology, you will find tables of the difference, or rather of the opposing properties, supposed to exist between organized and inorganized bodies. I should find it a long and useless discussion to demonstrate that many of these pretended differences are of little or no value. Animals and vegetables increase by *intussusception*, minerals by juxtaposition; or, in other words, in the first, growth takes place by internal juxtaposition; in the latter, by external; and that because organized bodies conceal in their interior the dissolved elements of new formations, while, on the contrary, these elements are external to inorganized bodies.

During life there is a continual struggle between the physical and vital forces, and death is the triumph of the former over the latter. But is this sufficient to prove that vital and physical forces are essentially distinct and opposed in their mode of action? Would it be correct to say that the different parts which, united, form one arch, are endowed with a force opposed to that of gravity, merely because they do not fall?

Organized living bodies, like all others in nature, are, as I have said, extended, impenetrable, divisible, porous. Plunge them into water, or any other fluid, and they will imbibe, such, for instance, as pounded glass, porous bodies, and those formed of capillary tubes. This property of living bodies is of the greatest importance. In a great number of animals, life can be suspended for a long time with impunity, but on the application of water, which they have the power of imbibing, they retrn to active life, and reacquire its movements. Who is not acquainted with the beautiful experiments of our illustrious countryman, Spallanzani, upon the rotiferous animals? Look at a tendon, a membrane, which, as I show you, is hard and horny, appearing never to have formed a part of an organized body: yet if we plunge it into water, you will perceive that, according to its imbibing power, it will become moist, supple, elastic, and enabled to perform, in the living body, the functions which belong to it.

All living bodies, as well as all others in nature, are endowed with elasticity, For example: I can, at will, stretch or compress, more or less, a piece of intestine or artery. If I open this cock attached to the trachea, you perceive that the lung falls upon itself, whilst it swells and becomes dilated again, if I force air into it. You cannot, therefore, believe that these different organs can perform their respective functions, without the elasticity of the pulmonary parenchyma, and that of the intestine and artery. Let it be destroyed, and the functions will cease, or at least be altered.

Gravity acts upon the solid, liquid, and gaseous parts of living bodies, in the same manner as upon all others which exist or are found in Nature. We cannot explain the functions of respiration and absorption, if we do not consider the physical properties of the solids, liquids, and gases of the economy, and their conditions of equilibrium.

If you apply a sufficient degree of heat to an organized body, you will see the gas disengaged, the vapor of water escape, and carbon and hydrogen burn in the air, producing carbonic acid and water. If the first application of heat often hardens organic substances, and shrivels them, instead of dilating and liquefying them, as ordinarily happens to inorganic bodies, you certainly cannot attribute this difference to vital action, since life has been long extinct when these phenomena take place.

All these effects are caused by the particular structure and chemical properties of the elements of which these tissues are composed. In fact, organized bodies, submitted to the action of heat, first lose the water which they contain; and this effect begins in the part exposed to the greatest heat. The substance then rolls itself up in the form of a horn, like a sheet of paper moistened on one side, in such a manner that the longest side occupies the convexity of its new shape.

These organic bodies often contain albumen, which co-agulates by heat. Their elements separate in the gaseous state, and form more simple combinations, which are consequently more lasting.

The electricity of tension pervades organized bodies, and spreads through them with more or less facility, according to their different degrees of humidity. If the spark passes through them, it volatilizes, burns, and reduces them to ashes. If the electric current passes through the fluids of living bodies, it decomposes their salts—the acids are collected at one pole, the bases at the other. Albumen coagulates at the positive pole, where are yielded the oxygen, and a frothy liquid acid. Hydrogen goes to the negative pole, with a liquid alkali. Every one knows that rays of light, traversing the humors of the eye, deviate from a right line by diverging and converging, according to the different density of the humors, and the form of the parts which contain them, in the same manner as in an optical instrument. I would add, that the elements which compose living bodies always obey the general laws of affinity. A chemist can detect and separate them by the ordinary method of analysis. Treat them with chlorine, bromine, or iodine. and hydrogen will be the first element which is separated, and combines with these metalloides to form hydracids. All oxidizing action will convert organic matter into acids. Are we justified in concluding from this that all the phenomena presented by living bodies can be explained by the general properties which they exhibit in common with other natural substances, merely by the action of the great physical forces—heat, light, electricity, and attraction? Such a conclusion would be as far from the truth as that of those who deny these general properties to living bodies, and who regard them as altogether beyond the influence of physical agents.

Examine these phenomena of living bodies, which, if I may use the expression, are the most physical or chemical, and you will see a considerable difference in the mode of action of physical and chemical agents, when acting upon organized matter; a difference which is inexplicable in the present state of our knowledge of the laws which govern these forces. Do not even the phenomena of vision, which may be called entirely physical, present peculiarities which have not hitherto been explained? If the latest discoveries of science enable us to explain the distinctness of vision at all instances, and the absence of colored fringes, how can we explain by physical laws the perception of an object as single and in its natural position, from a double and reversed image? Why can we not say that hearing and voice are simply the effects of particular vibrations of the air, propagated through solids, according to the general laws of acoustics? Science can give no completely satisfactory answer to all these questions.

The chemical action of light which decomposes carbonic acid, carries the carbon, under the form of new combinations, into the interior of vegetables, disengages the oxygen, and produces such combinations as the most powerful chemical affinities are unable to effect, is certainly different from that which decomposes certain oxides and metallic chlorides, which can be effected by the weakest chemical action.

If you direct the electric current upon the nerves of any living animal, the singularity of the phenomena exhibited will convince you of the immense difference which exists in the effects of the great forces of nature, upon living and organized bodies and upon those which are inorganic and dead.

What is, then, the cause of these great differences in the mode of action of physical agents upon living bodies and upon other natural bodies? Here is a question of primary importance, to which the present state of science does not permit us to reply with certainty. Let us not, however, abandon the analogies which are offered to us by physics. A ray of light which passes obliquely through a piece of glass, or a body of water, leaves the right line; if, on the contrary,, it meets with a crystal of carbonate of lime, it is divided into two rays, each of which deviates in a different degree. The reason of the difference of these phenomena is the difference of physical structure which exists between glass and crystalized carbonate of lime, and perhaps also between the chemical nature of their molecules. But no doubt these modifications of the ray of light are more owing to the diversity of structure, or to the peculiarity of the molecules, than to the difference of chemical composition. We know that glass acts differently upon the rays of light, when it is more or less compressed on different sides, without any change in its chemical composition.

Who could confound an organized with an inorganic body? In these groups of closed vesicles, of different dimensions, united and disposed in an irregular manner, there is certainly *something* essentially different to a mass of polyhedral particles, united in a crystal. To say, with some microscopical observers, that organization is crystalization effected in a liquid by which the first crystals formed can be imbibed, is equivalent to admitting that the structure of a stalactite is the same with that of the parenchyma of the lung and the liver.

Molecules, composed of not less than three elements, in each of which a great number of elementary atoms enter, necessarily form chemical systems, the affinities of which differ from those possessed by molecules composed generally of two elements, and in which the number of elementary atoms is small; and if ordinary chemical action shows us that combinations become more feeble in proportion as the number of elementary atoms is increased, it explains the tendency of organized bodies to resolve themselves into more simple combinations; so, also, chemistry furnishes many examples of this tendency in some compound inorganic bodies, the composition of which bears a great analogy with some organic bodies; yet this must not induce us to admit that the laws of inorganic chemistry are sufficient to explain exactly the chemical phenomena of life. We must conclude from this that the organization and molecular structure of living bodies occasion great modifications in the action of different physical and chemical agents.

We must not omit to add, that every day increases the number of a particular class of chemical phenomena, the explanation of which cannot be given on the ordinary laws of affinity: I allude to actions of contact, or to those of a catalytical nature. In the greatest number of these actions, we remark that a body, ordinarily in very small quantity, causes, without undergoing any modification itself, some considerable transformations, either of chemical composition or physical properties, in other combinations. In this category of phenomena we find the different kinds of fermentation. We shall see that the number of these catalytic actions in living bodies is immense. We can produce them also in our laboratories; they are of the same nature as those which the black platina powder exerts upon a mixture of hydrogen and oxygen, and silver scattered on oxygenated water.

I should here notice a fact of some importance relative to our subject, and of which, by and by, I shall have to speak more at length. The cellule is certainly the elementary organ, or the molecule of organic bodies. We can now explain, by the aid alone of the phenomenon of endosmose, (occasioned entirely by the dominion of the physical forces,) the mechanism of life of this little cell, and render an account of the manner in which the materials of its nutrition can penetrate it, while others are eliminated. We constantly meet with a great series of physiological facts of which endosmose furnishes the explanation. We may also add, and are able fully to demonstrate it, that light, heat, and electricity are produced in the interior of living bodies, by the play of the same physico-chemical actions as those which take place in inorganic bodies, and are attended with the same results. But can we hope, by the aid of all these facts, and analogies, that we shall be permitted to arrive at the complete explanation of all the phenomena of living bodies? At the present time, at least, this is a vain hope. Open an animal, and examine its kidneys and liver, and then inquire by what physical force it can be explained, that the blood which is carried to an organ forms bile and urine. How can we, in having resource to the play of chemical affinities, even modified as much as we can imagine by the aid of the particular structure of organs, and even also by the actions of contact—I will not say comprehend—but even have a glimpse of the matter in which the different organs effect the separation and transformation of the constituent parts of the blood, in which all the organic elements are mixed, partly suspended, partly dissolved, and of which there must be continually a want to repair their lost parts? What can we say of the functions of nerves, and generation!

We must conclude from this,—

1. That living bodies have properties common to all bodies in nature, and that these properties exercise an influence in the production of phenomena which are peculiar to them, and, which cannot, therefore, be neglected or overlooked in their explanation.

2. That the great physical agents—heat, light, electricity, molecular attraction—act upon living as upon all other bodies in nature; and that their action must of necessity interfere in the production of functions peculiar to those bodies.

3. That these forces, when exerted upon organized matter, sometimes modify their general mode of action; and that this modification is owing to a difference of structure and chemical composition of organized bodies.

4. That there are, moreover, in living bodies, phenomena which are termed vital; that these are numerous, and of the highest importance; that, in the present state of science, we cannot understand how the physical agents, even when modified in their action by the organism, can interfere in their production. This is the reason why there exists a study, a science, which has for its object the physico-chemical phenomena of living bodies; like as there is an experimental physiology. The intimate and necessary connexion is found in the third class of facts which we have pointed out. The organization modifies the action of physical agents, and the study of these modifications requires the combination of physical and experimental physiology. Let us not forget that we have formed a fourth class of phenomena of living bodies, which we have termed vital. I have said vital phenomena, and not vital forces, and in fact the difference is truly vital.

If Newton had only or merely called that force which rules the wonderful system of the celestial mechanics attraction, or attractive force, his name would long since have fallen into oblivion; but in demonstrating that the attraction is exerted in a direct ratio to the masses, in an inverse ratio to the squares of the distance, and revealing the eternal, the unchangeable laws of this force, he has rendered his name immortal.

To speak of vital forces, to give their definition, to interpret phenomena by their assistance, and to be ignorant of the laws which govern these supposed forces, is to say nothing, or, that which is worse, it is to satisfy the mind to no purpose, to stop the search after truth. To say that the liver separates the elements of bile from the blood by means of the vital force, is to say no more than that the bile is formed in the liver. By this change of the word, we become conscious of a dangerous illusion.

I think I have satisfactorily established the end at which it seemed necessary to arrive in the study of the phenomena of living bodies, which brings us back to the last analysis in the examination of the physico-chemical phenomena of these bodies, of their organization, of the modifications which this organization exerts in the general action of physical agents, and lastly, the search after the laws of phenomena purely vital, which up to the present time are empirical.

I hope I have succeeded in determining what are the limits which we ought to impose upon ourselves in the vast extent of physiology, and what part of this we ought to study under the title of physico-chemical phenomena of living bodies. The general principles I have just unfolded ought to be sufficient to make you estimate their importance for understanding the function of living bodies. In these lectures, I propose to myself another end no less important; it is to introduce in the exposition of physiological facts, and in the investigation of their laws, that precision of language, that exactness of expression, that rigorous method, which are too often set aside in the study of physiology and medicine, and which have hitherto been almost exclusively the distinctive qualities of physical science. Every step that we take towards this object, however trivial it may seem at first, will certainly prove of great service to physiology. It will lead to a certain conquest, because it will be founded upon a separate acquaintance with the science of organization, the basis of which will be established and supported by the physical theories, which, as you know, are almost complete, and every proposition of which has been rigorously demonstrated.

LECTURE II.

Molecular Attraction.—Capillary Attraction.—Imbibition.

Every one knows that a living body can only continue to exist, by introducing new substances into its system. These substances, for the most part solid, are transformed and reduced to a liquid state, by means of certain functions of the organism; under this form, they penetrate into particular cavities, from which, after undergoing other transformations, they are afterwards expelled. We have seen, in the first lecture, that the porosity of the tissues of living bodies allows them to be imbued and penetrated by the liquids with which they are in contact. We could not, then, give a satisfactory account of the phenomena of absorption and exhalation, without regarding the influence of capillary attraction, imbibition, and endosmose—phenomena which we already know to be exercised by inorganic bodies.

The importance of studying these two functions is so great, that I intend to devote the whole of this lecture to the examination of the purely physical phenomena of capillary attraction and imbibition, in order to make you able to judge what part they play in the functions of absorption and exhalation.

To confine myself to a simple announcement of facts, I will introduce here, under the form of propositions, the principal conclusions from the observation of capillary phenomena.

1st. When a body is plunged into a liquid, the latter is either depressed or elevated, and, according to the one or the other, it presents, at the point of contact with the solid, a convex or a concave surface. In the first case, the body is said to be wet, as when glass is immersed in water; and in the second, it is not so, as when glass is placed in mercury.

2nd. When two bodies are placed in a liquid, the latter is either raised or depressed between them, according as they have been moistened or not by the liquid, and it is necessary, for this, that the bodies should be so near to each other, as that the two curved surfaces formed by the liquid should touch. The elevation or depression of the liquid, above or below its level, is in an inverse ratio to the distance between the two bodies.

3rd. If a glass tube, open at both ends, be plunged into a liquid, the latter rises or falls, and this effect is the more considerable, the smaller the diameter of the tube. If the elevation or depression which takes place in a cylindrical tube be compared with that between two slips of glass which are placed at a distance from each other, equal to the diameter of the tube, it will be seen that the elevation or depression in the tube is twice as great as that between the glasses. The liquid rises and adheres to the glass, or wets it; on the contrary, it is depressed in the tube, if the liquid is not capable of wetting it. In a tube of one millimetre* in diameter, water rises thirty millimetres, mercury is depressed thirteen millimetres.

It will readily be admitted, that capillary action must exercise great influence upon the functions of animal and vegetable tissues, if we reflect that the vacuities, the interstices, and capillary tubes of these tissues, are from $\frac{1}{100}$ to $\frac{1}{200}$ of a millimetre in diameter.

4th. The concave surface of the raised liquid, and the convexity of that which has been depressed, belong to an hemisphere, the diameter of which will be equal to that of the tube.

5th. A drop of water introduced into a conical tube of glass, plainly resorts to its narrowest portion; a drop of mercury, on the contrary, is carried to its largest part.

6th. The phenomena which have engaged our attention are entirely independent of the volume of the solid body which is plunged into the liquid, consequently the thickness of the walls of the capillary tube exercises no influence upon their action.

7th. These phenomena take place freely also in air at ordinary pressure or when condensed or rarefied, *in vacuo*, and in any gas that we may select.

8th. All bodies, of whatever nature, if they are capable of being moistened, furnish the same results.

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* A millimetre is one-twenty-sixth of an English inch.

9th. For with the same liquid, and in the same tube, the elevation or depression of the column diminishes according to the temperature of the liquid.

10th. The elevations and depressions are independent of the density of the liquid. Thus, if we represent by 100 the elevation of water in a tube, that of alcohol will be 40, that of the essence of lavender, 37, and that of a saturated solution of sea salt, 88.

11th. Two bodies floating upon a liquid within a certain distance, are attracted towards each other, and unite, provided that both are capable of being moistened, or that both are not so. They appear to be repelled if only one be moistened. It is according to this principle that we explain the tendency of light bodies, which float on water, to be attracted towards the walls of the vessels which contain them.

12th. Whatever be the height to which a liquid is raised, it never escapes at the upper aperture of the capillary tube. This is a necessary consequence of the result, which we have already stated. In fact, it will be sufficient to reflect that the surface of the liquid column in the tube is always concave, and this is why, if we add sufficent water in one of these arms of a capillary tube bent upon itself to make the surface of the column at first horizontal, and then convex, we find the surface of the liquid column in the other arm remaining always concave, and at a level more elevated than the first. Whenever the surface becomes *convex*, the force of capillary *depression* is exerted. You must not believe that the water which flows from a wick of cotton, saturated with this liquid, one end of which is held downwards, is occasioned by capillarity, because it is only necessary to suspend it horizontally, and the escape ceases.

I cannot enlarge upon these phenomena so as to give the theory, which falls entirely within the sphere of the highest mathematical analysis. The results of the observations which I have already cited are sufficient to prove that these phenomena depend upon that force which we term molecular attraction, or the force which is exerted among molecules, and ceases to act as soon as they are separated by the smallest distance.

To avoid all false application of capillary phenomena to the animal economy, we must not forget that a space completely filled with liquid is incapable of exerting any capillary action; that the action of a tube upon liquid is owing, less to the material of which the tube is composed than to the nature of the liquid with which its internal surface is moistened; and lastly, that it is never by the agency of capillary action that liquids escape at the superior opening of the tubes in which they are raised.

The phenomena of imbibition, of hygroscopicity, &c., are generally of the same nature as the preceding, and they depend upon the same force. A piece of sugar, a cotton wick, a tube of sand, cinders, or saw-dust, placed in contact with water, or with any other liquid which wets them, quickly draws the liquid through all their mass—that is to say, becomes saturated with them. This is also the case with certain tissues, cartilages, and tendons, which, if dried, and then plunged in water, regain, in a short time, all the properties which they possessed during life. This effect is produced by the water which they absorb. The same thing happens in the remarkable instance of the rotiferous animalcule, which is restored to life and motion when placed in contact with a drop of water. These phenomena of imbibition are also observed in the filtration of liquids. When a liquid holds in suspension some solid particles, we see them separate from it, and rest upon the filter, while the liquid soaks into its substance. When a drop of chocolate, or ink, falls upon cloth or filtering paper, it produces a dark spot, surrounded by a zone less deeply colored. The same effect is observed when the blood is diffused in the cellular tissue under the skin; the serum is carried to the margin of the stain, and separated from the coloring matter.

Among the phenomena of imbibition, we must first consider the force of adhesion between the liquid and the surfaces of the solid particles, afterwards the action of capillarity, properly so called, seeing that in sugar, or a mass of sand or ashes, and in organized tissues, there certainly exist extremely minute cavities, which ramify in their interior more or less tortuously.

The phenomena of imbibition merit a more attentive study than they have hitherto received. I will give you the results of some experiments which I have made upon this subject with Professor Cima. I should have wished to do it at greater length. Some glass tubes, about two centimetres (about four-fifths of an English inch) in diameter, were filled with white sand, which had been passed through a very fine sieve. The extremity, which was plunged in water, was closed by a piece of cloth tied round the tube. The precaution was first taken of drying the sand by means of a water-bath and then introducing it by the upper part of the tube, taking care not to shake it when full, in order that the mass of sand should not be unequally compressed. Six tubes thus prepared were plunged at the same time into six different liquids, of the temperature of 12° centigrade, (73° Fahr.) The action of imbibition continued to raise the liquids in the tubes for ten hours; it was rapid at first, but always proceeded more slowly, according as it approached the limits at which it ceased. Each tube was plunged about half a centimetre (quarter of an English inch) in the liquid, and care was taken to replace the fluid occasionally, so as to preserve a uniform depth in each vessel.

Subjoined is a table, showing the highest elevation attained by the different fluids. All the saline solutions were of the same density—10° of the aerometer of Baumé.

	Millimetres.
Solution of carbonate of soda	85
Solution of sulphate of copper	75
Serum	70
Solution of carbonate of ammonia	62
Distilled water	60
Solution of marine salt	58
White of egg, mixed with an equal quantity of water	35
Milk	55

This table shows how imbibition takes place in different degrees in various liquids. In thick solutions of gum, in boiled starch, or in oil, there is scarcely any imbibition. It is also very small in concentrated saline solutions, and in all those liquids which hold in suspension very minute solid particles. In the last case, imbibition produced a kind of filtration. This phenomenon of imbibition in solutions which contain very small solid molecules, suspended in the liquids, may be of great value for the purpose of appreciating the different properties of blood according to its density. In certain maladies, its density and viscidity are much diminished; and in these cases, serous infiltrations take place, such as are observed by the same causes from great losses of blood.

We shall presently find that alcohol, ether, water, and aqueous solutions, introduced into the stomach of living animals, disappear, but after different intervals of time; oil remains there a very long period.

It will, perhaps, be of some importance to compare alcohol at 36° Baumé, and distilled water, by means of tubes filled with sand, pounded glass, and saw-dust. Witness the elevations that I have obtained:—

	Tube with sand.	Tube of pounded glass.	Tube filled with saw-dust.
Alcohol,	85mm.	175mm.	125mm.
Water,	175mm.	182mm.	60mm.

In examining these results, it is very evident that the alcohol is raised less than the water in the sand and in the powdered glass. This agrees with what took place in the capillary tube.

I also plunged into water two tubes, the first of which contained double as much pounded glass as the second, with the following results:—

In the first tube the water was raised 170mm.; in the second, 107mm. It is not easy to give an explanation of the relative heights in the two tubes; and yet it is natural that the water should rise higher in the tube which contained the double quantity, if we reflect upon the greater extent of solid surface for attracting the water, and to the much smaller diameter of the capillary cavities.

This phenomenon of imbibition is constantly witnessed, and under a great number of circumstances, in animal and vegetable tissues. These, being abundantly furnished with minute spaces and capillary tubes, very readily imbibe and absorb the solutions with which they are in contact. This is what takes place in the cellular tissue, and in the parenchyma of the lungs: the opposite effect is observed in the epidermis.

I have also made it a subject of investigation, whether any difference in the phenomena of imbibition is produced by temperature. Two tubes prepared with sand were equally plunged into water: the temperature of one was 55° cent., of the other, 15° cent. The results obtained were as follows:—

	Elevation after 70 seconds.	Elevation after 11 minutes.
Tube at 55° cent.	10mm.	175mm.
Tube at 15° cent.	6mm.	12mm.

The influence of temperature upon the degree of imbibition is, it will be seen, very considerable. It is also known that in animals, absorption, either by the skin, or in the internal structures, is more active in proportion to the heat of the solution.

I am convinced that imbibition goes on with equal intensity when the surrounding air is saturated with moisture, and when it is dry. Another result, no less remarkable, is observed, that the imbibition of sand, ashes, and saw-dust, goes on as well under the exhausted receiver of an air-pump as in the open air. No difference was perceived in the height of a column of water at the end of ten minutes; the only peculiarity observed was, that imbibition proceeded more rapidly *in vacuo* than in air, in the first few seconds.

It may be asked, whether by the action of imbibition a liquid may rise to any height. It would appear, at first sight, that a column of sand, ashes, or any other powder, plunged at one end into a liquid, the level of which was always maintained, would carry the liquid to any height by the force of imbibition, till the whole column were saturated. In fact, if we consider separately the action of each of the layers which form the column, we may conceive that after the imbibition of the first layer in contact with the liquid, the particles of the layer immediately above it will take from the first part of its water, and that the first will again take from the fluid mass as much as it has lost. By repeating this reasoning for all the successive layers of the column, we may conclude that each of them takes the same quantity of fluid as if it acted alone; and thus if we suppose the level of the water constant, the column, however long it be, ought to be entirely saturated. But experience does not confirm this reasoning: the fluid rises rapidly at first—then the motion slackens, and after gaining a certain elevation, its stops. This effect cannot be attributed to the evaporation which takes place in the higher layers of the column; for water rises in a column of sand to exactly the same height in an atmosphere saturated with aqueous vapor as in dry air. I am not able to account for this limit of imbibition, except by admitting the existence of little canals reaching along the whole length of the column of powder, and, in consequence, capillary action will intervene, as well as the adhesion of the liquid to the surface of the grain of sand. *

It is impossible not to perceive that imbibition plays a great part in the movement of the juices of plants, and in the phenomena of the capillary circulation of the blood of animals. In another lecture, we shall see that living plants

* Might not the force of gravity of the imbibed fluid counterbalance, in this case, that of imbibition?—Translator.

and animals having some one part plunged into a saline solution, the presence of which is easily recognised by means of re-agents, are quickly penetrated by it throughout. It will be sufficient for me to mention the experiments of Hales, and those more recently made by Boucherie; the latter has seen a poplar, ninety-two feet high, absorb by the trunk, in six days, the enormous quantity of sixty-six imperial gallons of a solution of pyro-lignite of iron.

I will here relate the experiments made by Hales to measure what he calls the force of aspiration of powdered bodies, and stems of trees—phenomena in which imbibition plays an important part. This experimenter furnished himself with a large tube of glass, closed at the upper end, and filled with ashes or with minium, reduced to a fine powder. A cork was fitted in the open end, in the middle of which a narrow tube of glass was fixed, three or four feet in length. This second tube was filled with water, and quickly inverted over mercury. The mercury soon rose, and gained an elevation of several inches. In one experiment, Hales saw it rise seven inches, which is equal to a column of water of eight feet.

If the tube, full of ashes, is replaced by the stems of a tree, or, better still, if the branch of a tree is tied to a glass tube, filled with water, and inverted over mercury, the latter will rise, as in the preceeding experiments with the powders. Hales regarded this phenomenon as depending upon a force which he called the force of aspiration.

Here are some experiments which explain these facts in a simple and satisfactory manner. It is easy to show that the ascent of the mercury equally takes place in two tubes prepared like those of Hales, but yet differing from them, in having one tube filled with ashes, open at the top, and the other closed. And yet it must be observed, that this result will not be obtained if the column of ashes be short, or if this were less filled. I have also made the following observatious with a similar apparatus to that observed by Hales. I fluted a tube of lead with clay, to that of glass containing the powder; by means of this I could easily extract the air which was above the ashes. At the moment when the column of mercury began to rise I made a vacuum, and not only did the mercury not go down, but it even continued to rise. It is then, indubitable, that the ashes form a partition above the column of water, which exactly performs the office of the closed tube. In fact, Hale's apparatus is a barometer. In another experiment, at the moment when the mercury rose, I covered the whole with the receiver, and made a vacuum; at that instant the mercury fell again entirely. I have witnessed the same phenomena, by substituting, for the tube filled with ashes, a stem of a tree, having leaves attached to it. If I introduce the superior part of this stem into a ball from which I have withdrawn the air, the mercury continues to rise; but, on the contrary, it immediately falls again if I form a vacuum on the vessel containing the mercury. We must conclude from this, that that which Hales called the force of aspiration, is merely the action of a barometer, that whether the column of ashes, or the leaves and trunk of a tree, form the upper closed part of a barometer, the water penetrates the ashes of the vegetable tissue, by imbibition, and the atmospheric pressure gradually forces up the liquid.

We must, however, remark a very curious fact which takes place when we use the branch of the tree, as in all the other experiments of Hales, that the column of water continues to ascend, which would lead to the conclusion, that the vapor of water is exhaled by the leaves without these ceasing to act as the closed tube of a barometer. It would appear that Magnus obtained the same result in closing by a membrane the upper part of the tube; and as already stated, the water continued to ascend; but it is probable that the phenomenon becomes after a little time, less manifest, and ultimately ceases, in consequence of the disorganization which takes place as well in the membranes as in the leaves.

I will not leave this subject without relating some experiments made for the purpose of producing, by the simple play of the capillary forces, and of molecular attraction the effects of chemical affinity. If we reflect, that a liquid of whatever kind it be, is constantly raised to the same height in a capillary tube; that during the imbibition there is more or less evolution of heat, as the experiments of Pouillet prove; and what is more, that there is, according to Becquerel, a disengagement of electricity; and lastly, that capillary attraction is only exerted at very limited distances, and between the molecules of bodies,—we must admit that this force combines the principal characters of chemical affinity. We know the beautiful remark of Dobeirheiner, that when a mixture of water and alcohol is enclosed in a vessel, and exposed to the air, the water alone escapes. In this case the water is imbibed by the membrane more readily than the alcohol, and is dissipated by evaporation. Another fact, more conclusive, is that mentioned by Berzelius, that salt water, in its transit through a long tube of sand, flows away, more or less completely deprived of its salt. I have confirmed this experiment by means of a tube filled with sand, of about eight metres (twenty-six feet) in length, and I found that the density of the water, introduced at the superior office of the tube, was to that of the liquid flowing from it as 1:0.91; but I should add, that this difference of density is not constantly in this proportion, for after a certain time the saline solution is as dense at its exit from the tube as it was when introduced: which proves that the decomposition of the solution takes place during the first action of contact between it and the particles of sand. I obtained an opposite result by employing a solution of carbonate of soda, which I passed through a similar tube of three metres (ten feet) in length. The density of the liquid, after having traversed the layers of sand, was to that before its transit as 1.005: 1.

These phenomena are very important, from their extensive application to some of the functions of living bodies, which cannot be entirely explained by the influence of capillary and molecular attraction.

LECTURE III.

Endosmose.

Having occupied your time, hitherto with the phenomena of capillarity and imbibition, it becomes necessary for me, in order to enable you to apply the knowledge of these facts to the functions of exhalation and absorption in living bodies, to make you acquainted with another phenomenon, which, although appearing under exclusive physical characters, does not the less appertain to organized bodies, from the application which may be made of it to their functions. I allude to the phenomena of endosmose, discovered by Dutrochet, which, reduced to its simplest expression, is the reciprocal action which two fluids exert upon one another when separated by a membrane. Although the theory of this phenomenon, and the physical principle to which it is due, are as yet unknown, it is still of the highest importance.

I shall begin by showing the fundamental fact in all its simplicity. Here is a glass tube, having its inferior extremity closed by a piece of bladder extended in a rounded form. This instrument is called an *endosmometer*. If we pour into this tube an aqueous solution of gum or sugar, for example, and then place its closed extremity into pure water, we shall perceive the water continually entering the tube through the membrane, even in opposition to the pressure of the fluid. The fluid contained in the tube will also be raised so as even to flow over its superior opening; at the same time, some of the fluid in the tube, but necessarily less than the first, will transude through the membrane, and mix with the pure water. Dutrochet has termed the first of these phenomena *endosmosis* the second *exosmosis*.

Membranes are capable of producing endosmose, even when they are at the point of putrefaction, but after this process has begun, it no longer takes place, and the fluid which was raised in the tube, re-descends filtrates through the membrane.

Not only are membranes endowed with this property, but layers of slate, or better still baked clay, are capable of exerting the same phenomenon, though in a less degree. Calcareous and siliceous layers, on the contrary, have not this property, no endosmose taking place with them.

The nature of the fluid employed has a considerable influence upon the phenomenon. The endosmosis is the more evident as the density of the fluid in the tube exceeds that of the external fluid. It would appear that the intensity of the current is in proportion to the difference in density of the two liquids.—Nevertheless, alcohol, which is of less density than water, introduced into the tube, produces endosmosis upon water placed external to it.

Saline solutions, while traversing the membranes, occasion very energetic effects, which are, however, of short duration. Increase of temperature augments the velocity of the endosmic current. What is very curious in this phenomenon is, that the slightest trace of sulphuric or hydrosulphuric acid is sufficient to modify its production, even with those liquids which are the most active; and that the other acids, as the hydrochloric, or nitric, have not this effect.

All animal fluids produce endosmose with energy when in contact with water, except those usually found in the large intestine, which is perhaps owing to the hydrosulphuric acid which they contain. Dutrochet states that this gas is so destructive to endosmose that it completely ceases to manifest itself if a membrane be used which has been, even for a short time, in contact with it. He endeavored to measure the velocity with which, in virtue of endosmose, a fluid can penetrate from the exterior to the interior of a tube. The results are as follows:—If a tube of two millimetres in diameter be closed at its lower extremity by a membrane forty millimetres in diameter, a solution of sugar, the density of which is 1°,145, will rise thirty-four divisions in the space of an hour and a half, each division being two millimetres. In another experiment with a solution of sugar of the density of 1°,228, the ascent in the same space of time was fifty-three divisions. In a third experiment with a solution which had a density of 1°,083, the column rose 19½°, in the same space of time. The velocity of endosmose is, therefore, in direct proportion to the excess of density of the internal liquid over the water employed outside.

Dutrochet having taken solutions of different substances of the same density, compared them with water, from which they were separated by a bladder with the following results:—

Solution of gelatine	3
" gum	5.17
" sugar	11
" albumen	12

It appears by this table, that of all organic substances soluble in water, albumen produces endosmose with the greatest force.

Among the most curious facts discovered by Dutrochet, in studying endosmose, must be mentioned that of a change of direction in the current between certain acid solutions and water, according to their density and temperature; the solution of hydrochloric acid especially presents this phenomenon. Thus, with a solution of hydrochloric acid of the density of 1°,02, endosmose takes place from the water to the acid; while, at the density of 1°,015, the current is in the contrary direction—namely, from the acid to the water; but with this last solution, at a higher temperature than 20°, endosmose again takes place from the water to the acid.

It is important to know the force with which the liquid penetrates; or, in other words, the force of the current called endosmose. To this end Dutrochet thought of employing the apparatus which Hales, and afterwards Mirbel and Chevreul, used to measure the pressure of juices in vegetables. In this apparatus the force is estimated by the height of a column of mercury sustained by the liquid.

By studying endosmose under this point of view, Dutrochet has proved that, all other things being equal except the density of the interior liquid, the force of the current is proportional to the excess of density of the interior liquid over water. It is for this reason that the force, as well as the rapidity of endosmose, appears to be subject to the same law. Hence it results, if the law be correct in all cases, that syrup, at the density of 1,3, will produce a current capable of raising a column from 127 inches (three metres, forty-two centimetres) of mercury; or, what is the same, will be equal to the enormous pressure of four atmospheres and a half.

Dutrochet has endeavored to give an explanation of the phenomena of endosmose, as have also Poisson and Becquerel. Thus one has attributed it to the action of an electric current, developed by the contact of two different liquids exciting a movement of the water through the membrane, from the positive to the negative pole, as in the well-known experiment of Porret. But to render this hypothesis at all probable, it should first be proved that there is a development of electricity upon the contact of water with alcohol, a solution of sugar, &c., which has not yet been done. Poisson thought that the less dense fluid penetrates the capillary tubes of the membrane, and that this capillary thread, drawn down by the action of simple water, and upwards by the action of a denser liquid, must rise by reason of the excess of molecular (capillary ?) attraction. But this explanation is equally inadmissible when we reflect, that alcohol, which is less dense than water, produces endosmose; that the smallest quantity of sulphuretted hydrogen stops it; and that certain calcareous and siliceous stones, used in the same manner as membranes and plates of clay, do not produce the phenomenon. It must therefore be confessed, that we have no satisfactory theory of endosmose, though we know that the general conditions of the phenomena are, 1. That one or both of the fluids should have an affinity for the substance interposed; 2. That the two fluids should have an affinity for each other, and be therefore missible. If one of these conditions be wanting, endosmose does not take place. As far as experiments show, the direction of the current is neither determined by any difference in the density of the liquid, nor by that which possesses the greatest force in ascending capillary tubes.—The current is generally towards that liquid which has the greatest affinity for the substance interposed, and which it imbibes with the greatest rapidity.

What has been said of this phenomenon will be sufficient to convince you, that it is perhaps the most important of physical facts, as regards its application to the functions of living bodies. Microscopical observation has put it beyond doubt, that in all vegetable or animal tissues, and in those fluids which are generated by the changes in organized and living bodies, at a certain period of their formation, microscopic corpuscles are constantly found, which have been named cellular or elementary. These corpuscles consist of an excessively fine membrane of a spherical form, enclosing a liquid, upon the internal walls of which is found a small organized body, which is called the *nucleus* or *cytoblast*. The cells have their origin at first in a fluid, to which Schwaan has given the name of *cytoblastema*, and they end by becoming compressed and nearly blended together on the fluid becoming more or less dense. According to the different tissues, the elementary cells more or less approximate; the *cytoblastema*, or intercellular substance, is invariably the bond of union between the cells.—We shall recur more fully to this important subject, and have only glanced at it here, for the purpose of rendering the importance of the phenomenon of endosmose more evident. The life of the elementary cells certainly forms the most essential part in the development and preservation of the tissues of living bodies; and since these cells are found in favorable conditions for endosmose, there is no reason why it should not take place. A vesicle containing a fluid, and floating in another fluid, may imbibe the one that surrounds it, and part with that which it contained in a way analogous to endosmose. It should, however, be added, that but little has been done as yet in the application of these phenomena to physiology, of which it appears susceptible. It was necessary for this purpose to vary the fluids between which endosmose takes place, and choose the membranes in those conditions which offer analogies between this phenomenon and those which take place in the interior of living bodies.—This Professor Cima and I endeavored to do. I will now describe to you our experiments.

I shall divide the membranes which we submitted for experiment into three classes. In the first I shall place the skins of the frog, torpedo, and eel; in the second, the stomachs of a lamb, cat, and dog, and the gizzard of a fowl; and in the third, the urinary bladders of the ox and pig.

We will not pause to give a description of the apparatus, which differed in no respect from the endosmometers of Dutrochet. I may remark, however, that in these experiments we kept two endosmometers in action at the same time, the tubes of which, of equal bore, were both three millimetres in diameter, and had attached to them a scale divided into millimetres. In a glass vessel, sufficiently large to contain the two instruments, we placed a kind of support, upon which we firmly fixed a metallic plate pierced with a great number of little holes. Upon this plate we placed the two endosmometers; and to prevent the possibility of any change in their position, we fixed them by means of a large plate of lead pierced with two holes, having each a diameter equal to that of the neck of the instruments. In the course of the experiments, one endosmometer had the interposed membrane disposed inversely to that of the other—that is to say, supposing we were manipulating with skin we placed it in such a manner that in one case its external face should be directed towards the interior of the instrument, and in the other, on the contrary, towards its exterior.

All these experiments were conducted at a temperature of $+12^{\circ}$ to $+15^{\circ}$ centig. In the greatest number or cases they lasted two hours, and were repeated many times. We took care to employ for the two endosmometers destined for these comparative experiments two portions of membrane of equal thickness, both being taken from the same animal and in two symmetrical regions of its body, or of the organ employed. The fluids which we employed, besides spring water, were the following, of which we give, once for all, the density in degrees according to the ærometer of Baumé: sugared water, 19°; solution of white of egg, 4°; solution of gum Arabic, 5°; and alcohol, 34°.

These fluids were usually kept in the interior of the instrument, while the water was generally in the exterior.

Under some peculiar circumstances, which I shall allude to by and by, we changed the disposition of the fluids and instruments, and used a separate vessel for each endosmometer. We also made use of another instrument,* which may be thus described:—*B* and *C* are two cylindrical brass receivers, jointed together accurately by grinding their opposed surfaces. *B* has at *a* a plate of brass, pierced with holes, upon which the membrane that is to be experimented upon is placed. *C* has also a plate with holes, which, when the two cylinders *B C* are joined together, as in the figure, closely applies itself to the membrane, which is thus firmly fixed between the two plates. By this means the membrane cannot yield to the greater pressure which may be exerted upon it by a liquid more dense in the one portion of the cylinder than that contained in the other, *mn*, *op*, are two tubes of equal calibre. The first communicates with the receiver *B;* the other with the receiver *C*. When this instrument is used, we begin by introducing the denser liquid into *B*, and filling the tube *mn* to a certain height. *C* is filled by plunging it into a tubful of water; and then the two cylinders are joined together under water, and fixed by a screw, in order that the liquid in *C* shall not escape by the fissure between the joints. The instrument is then placed on a level, and the two liquids are made to stand at 0°, in the scale *S*. With this instrument we at once obtain the measures of

* The readers of this Journal will find a wood-cut of the instrument described at page 232, vol. 2nd of this Journal.

ascent and descent of the two liquids, which gives great precision and facility in experimenting with it, as the results are shown, as it were, in two ways.

I will now communicate the results obtained by employing the skins of the frog, torpedo, and eel, with the solutions before alluded to. In our first trials, we perceived very plainly the marked influence which the position of the membrane exerted upon the phenomena. It was this discovery that first led us to study in this point of view the urinary bladder and stomach of different animals.

With some care we obtained the skins uninjured, and deprived of all their subcutaneous cellular tissue. After having thus prepared them, and by cutting off the parts which, in the torpedo and eel, are pierced with the cutaneous appendages, we succeeded in obtaining membranes well adapted for this kind of experiments.

When employing the skin of the torpedo, we directed its external surface in one endosmometer towards the interior of the instrument, and in another towards its exterior; and on filling the two with a solution of gum Arabic, we observed that the solution in the first was raised thirty millimetres, and in the second, at one time, eighteen, and at another only six. In some instances, the fluid was raised twenty millimetres, and even more, in the first tube, while it was scarcely elevated at all in the second.

These differences were equally evident with syrup, which rose thirty or even eighty millimetres when the external surface of the skin was turned towards the instrument, where the liquid was contained; but which, in the inverse position of the membrane, did not, at the most, rise more than two. In one instance, where it rose eighty millimetres, in the first of the dispositions mentioned, it could only be raised twenty millimetres when the second was adopted.—With the solution of albumen, the elevation was twenty-six millimetres, when the external face of the skin was in contact with it, and thirteen only in the opposite direction.

The results obtained wirh the skin of the frog agree, in general, with those furnished by that of the torpedo. We remarked that the direction of the endosmotic current was constantly from the water to the solutions of sugar, gum and albumen. We found also that the membrane possessed the property of increasing or diminishing the velocity of the endosmose, according to its relative position to the two liquids. In placing the skin of the frog in the two endosmometers in the usual manner, we obtained an elevation of thirty-six millimetres when its external face was in contact with the syrup, but only twenty-four in the inverse disposition. In many cases, the first was exactly double the other.

There was an equally well-marked difference, and always in the same way, when using the solutions of albumen and gum. With the first solution, there was an elevation of twenty-four millimetres, and with the second, thirty-two, when the external surface of skin was in contact with them; but when, on the contrary, its external face was directed towards them, the solution of albumen rose only twelve millimetres, and the gum-water sixteen.

The difference which we previously observed in making use of the syrup, when we employed the skin of the frog and the torpedo, were equally present when using the skin of the eel: but what was singular in regard to this last was, that this difference did not become manifest during the first moments of the experiment. At the commencement, the ascent of the liquid was equal in the two instruments; but, in two hours, we found it thirty millimetres in the endosmometer, in which the external face of the skin was turned towards the syrup; and twenty millimetres in the other. With the solutions of albumen and gum, the differences were observed from the beginning of the experiment as ordinarily took place; and while, at the end of the experiment, we found that the former was raised eight millimetres when the external side of the skin was directed towards it, and the latter, twenty, we perceived, on the contrary, in the opposite position of the skin, that the first solution rose to four millimetres, and the second to seventeen.

The state of freshness appeared more necessary for the skins of the eel, than for those of the frog or torpedo, when we noted the difference in the elevation of the liquid in the two endosmometers. If the skin of the eel has been detached from the animal two or three days, the difference in height ceases in the two dispositions of the membrane; and the syrup and solutions of albumen and gum were raised in the same space and time, to an equal height in the two instruments.

In employing water and alcohol, Dutrochet obtained a current in the direction from the former to the latter. This was, therefore, an exception to all the other facts which he discovered, in which the current was directed from the least dense to the denser fluid. The influence of the direction of the membrane was very palpable in our experiments; but the disposition favorable for the current constantly directed from the water to the alcohol, was not the same for the three kinds of skin. Thus, in using that of the frog, the current from the water to the alcohol is more rapid when its direction is from its external to its internal surface. In different experiments, often repeated, we observed a rise of twenty, twenty-four, and forty millimetres, when the internal face of the skin was turned towards the alcohol, while in the contrary disposition, the corresponding elevations were only four, twelve, and twenty. In analogous circumstances, the disposition of the membrane being favorable, the elevation was twenty-eight millimetres; in the other, on the contrary, there was none.—With the skin of the eel, the contrary took place, the current being more rapid from the internal to the external surface; and while the alcohol contained in the instrument is raised to the height of twenty millimetres when it is in contact with the external face of the skin, it only ascended ten in the contrary case.

This difference of elevation always occurs in the same way, and is the same in the skin of the torpedo as in that of the eel. The elevation has been fifty millimetres in one instrument, and twenty in the other.

Some anomalies which we observed in our first experiments led us to study with more precision the circumstances of the phenomenon of endosmose through the skin of the torpedo between water and alcohol. We constantly found the difference above stated, when the skin of the torpedo had been recently prepared, and had not been used for other trials of the same kind; but it only persisted during the first hour of the experiment, or a little after: the elevations then followed a different law, and the ascent in the endosmometer, in which the external surface of skin was in contact with the water, seemed first to diminish, then to cease, and finally the direction of the current became changed.

Among the numerous experiments which we tried, we shall select the following, during which we noted the elevation, every hour. We shall mark the endosmometer in which the internal face of the membrane was in contact with the water A, and that in which it was directed towards the interior of the instrument B.

A.			Mil.	B.			Mil.
Height during the	1st	hour	23	Height during the	1st	hour	17.
" "	2nd	"	25	" "	2nd	"	3
" "	3d	"	25	" "	3d	"	0
" "	4th	"	25	lowered in the	4th	"	3

We may conclude then,—1st. That provided the skin of the torpedo be recent, endosmose takes place, as usual, from water to alcohol; with this difference, however, that while in the case where the internal face of the skin is in contact with the water, the elevation is as three; in the reverse case it is as two. 2nd. That while in the first disposition of the membrane, (A) the force of endosmose always continues some hours; in the second, (B,) this same force seems always to diminish, and, ultimately, to cease entirely. 3d. That after a certain time, the direction of the current changes, and takes place from the alcohol to the water, when the internal surface of the skin is turned towards the alcohol, while it remains constant in the contrary position of the skin. It occurred to us that we should attribute the singularities observed when using alcohol, to the chemical action which this liquid exerts upon the membrane, and to the consequent alteration of the structure itself.

The diminution of the intensity of the endosmose observed in the skin of the torpedo, but only in unfavorable positions of the membrane, is verified in that of the frog in every position; but it does not decrease in regular gradation, as we see by this table, in which A and B represent the same endosmometers as in the preceding table.

A.	Mil.	B.	Mil.
Height during the 1st hour	23	Height during the 1st hour	30
" " 2nd "	40	" " 2nd "	55
" " 3d "	12	" " 3d "	15
" " 4th "	22	" " 4th "	35
5th & 6th "	56	5th & 6th "	58

During the night the liquid overflowed out of the two endosmometers, but there was no inversion of the current, as took place in the skin of the torpedo. Nor was this phenomenon witnessed with the skin of the eel, even when the experiments were prolonged as much as five hours, but the elevations were irregular, as in the skin of the frog.

It was important to discover if the force of the endosmose varied according as the skin had been taken from different regions of the animal. The experiments which we tried with this view were not numerous. We can only say that the endosmotic current did not vary in any manner; that we tried some skin which, in the torpedo, covered the electrical organs, and some from the back, and we did not observe anything further, whether we employed skin taken from the belly or from the back of the frog.

We performed a long series of experiments in order to determine the respective force of the endosmose with different liquids through the three skins above mentioned. For this purpose, three endosmometers were simultaneously prepared; one with the skin of the torpedo, a second with that of the frog, and a third with that of the eel. In the three instruments the skins were arranged in such a way that the external surface was directed towards the interior of the instrument, which contained at one time a solution of sugar or albumen, at another gum-water or alcohol. The endosmometers were placed in a glass vessel filled with spring water. This disposition afforded us the opportunity of immediately recognizing the difference in height of these liquids through the three kinds of skin. This table shows the intensity of the endosmose of each of the liquids through the different skins:—

	Solution of sugar. Mil.	Solution of albumen. Mil.	Solution of gum. Mil.	Alcohol. Mil.
Skin of torpedo.....	100	30	120	35
Skin of frog...........	25	15	22	80
Skin of eel.............	15	8	6	55

It will thus be seen—1st, that with the skin of the torpedo the endosmotic current is strongest with the solutions of gum, sugar, and albumen; 2nd, that with the same liquid it is less strong through the skin of the eel than through that of the frog; 3d, that with the skin of the frog we have an endosmotic current from the water to the alcohol, stronger than that with the eel, and still more energetic than with the skin of the torpedo; 4th, that the stronger current, from the water to the alcohol, through the skin of the frog, persists even in the more unfavorable positions of the skin, with regard to the liquids, for the production of the phenomena; 5th, that the intensity of the endosmose in the same skin varies at each change of the liquid. It follows, then, that with the skin of the torpedo these liquids must be ranged in the following order, proceeding from that which gives the strongest current to that which gives the weakest :—

With the skin of the torpedo ; solution of gum, solution of sugar, alcohol, solution of albumen.

With the skin of the frog; alcohol, solution of sugar, of gum, and albumen.

With the skin of the eel; alcohol, solution of sugar, albumen, and gum.

These last results prove that the order in which Dutrochet arranged these liquids, according to the velocity of the endosmose which takes place between them and the water, must not be regarded as true in all cases; we shall see that it can only be considered as invariable in the single case of the urinary bladder, which this skilful experimenter made use of.

Reserving for another time the general conclusions to be drawn from what we have already shown, I shall now pass on to the observations which we made when using the membranes coming under our second category—namely, the stomach of the lamb, dog, and cat, and the gizzard of the fowl.

In all these experiments we always began by dissecting off, with the greatest care, all the muscular fibres of these organs, before applying them to the endosmometer, preserving only the mucous membrane. The greater number of our researches were made with stomachs taken from the animals immediately after death, and we were careful in noting the cases in which it was otherwise, as they occurred.

In using the stomach of the lamb, prepared as we have just stated, and the solution of sugar in the interior of the endosmometers, by placing the membrane with its internal or stomachal surface towards the instrument, the ascent of the liquid was 56 millimetres in one case, and 54 in another. In the reversed position of the membrane, it was 72 millimetres in the first, and 66 in the second. These two experiments lasted only an hour and a quarter; the endosmose was increased then by employing syrup, and by directing it from the internal to the external surface of the stomach. But when the solution of the white of egg was used, and placed in contact with the internal surface of the stomach, it was raised 23, 28, and 35 millimetres. In the contrary case it only mounted 11, 20, and 22 millimetres in the space of two hours, as usual.

On introducing a solution of gum Arabic, the elevation in the two contrary dispositions of the membrane was sometimes nearly, and at others exactly, the same, and only eight millimetres in each instrument. In one case, out of many experiments, when the internal surface of the membrane was in contact with the solution of gum, the ascent was twelve millimetres; and in another, in the contrary disposition of the membrane, it was eight. The intensity of the endosmose between the water and the gum solution was excessively feeble when it took place through the stomach of the lamb. It was necessary, therefore, to prolong the experiment more than usual to obtain any very palpable elevations. Those which I have just alluded to were obtained after carrying on the experiment more than four hours. Besides this, it should be remarked, that the endosmotic current through this membrane quickly ceased, when we employed these two liquids. It often happened, in fact, that the gum solution, after having attained a little elevation, did not rise higher after the lapse of two hours, and even more.

The favorable position for endosmose between water and syrup, which was remarked in the case of the stomach of the lamb, is not the same as with those of the dog and the cat. With the stomach of the cat, the height of the syrup in the tube was thirty of fifteen millimetres, according to whether the internal face of the membrane was towards the interior of the instrument, or in the opposite direction. With that of the dog, in the first case it was sixty-eight, and in the second eight millimetres.

With the stomach of the cat, the endosmose from the water to the gum solution is equally directed from the external to the internal surface of the organ. Thus, when the mucous surface is in contact with the gumwater, the elevation of the liquid attains to thirty-eight millimetres, while in the other position of the membrane it is not more than fourteen. This difference is only observed when the stomach is very fresh. If it belong to an animal which has been some time dead, we then perceive, in the first moments of the experiment, a slight elevation, and sometimes an equality in the two instruments. At one time it is greater, at another less, in the same endosmometer; but it quickly redescends. By changing the disposition of the liquids, by placing the gum solution outside, and the pure water within the instrument, this last descends. These phenomena are also produced when we employ the stomach of the dog.

We did not make any experiments with the stomach of the latter animal immediately after death, nor did we employ the solution of albumen as the internal liquid. Those, the results of which I shall now state, were made many hours after the death of the animal. The albuminous solution was raised to an equal height in the two instruments, in four different experiments. In one of these this elevation was twenty millimetres in an hour, and it did not vary during three hours longer in the endosmometer, when the internal surface of the stomach was turned from the interior of the instrument; while in the same interval of time it sunk to twenty-five millimetres in the other. In general, it rarely happened that the liquid column remained stationary in either of them. In the greatest number of cases (we still speak of the stomach of the dog that was not fresh) the liquid descended in the two instruments after it had attained a greater or less elevation; but the diminution in height is double, and even treble in the endosmometer which presents the external face of the membrane towards the solution of albumen. In changing the position of the liquids, by placing the solution of white of egg in the exterior of the endosmometers, and the water in their interior, we find that the internal liquid descends equally in both. These descents are occasioned by the cessation of the endosmose, in conseqnence of the alteration in the structure of the membrane some time after death; only the effect of the disposition of the two surfaces remains, to a certain degree, even in the altered membrane. In fact, we have remarked that the descent of the albuminous solution in the endosmometer is double, and even treble, when the external surface of the membrane is turned from the interior of the instrument.

With the mucous membrane of the gizzard of a fowl, using syrup and pure water, endosmose takes place from the external to the internal surface of the membrane, although generally the difference in elevation between the two endosmometers, is not great. Thus, when the inside of the membrane was directed towards the interior of the instrument, the elevation is forty-eight millimetres; while in the opposite direction it was forty-three millimetres. It is not unusual to see a certain elevation—for instance, seventeen or twenty millimetres—when the membrane is in the first position; while when it is in the second the liquid is immovable. It is worthy of remark, that the current from the water to the syrup, through the gizzard of a fowl, very quickly ceases. Generally within two hours the liquid column becomes stationary in both the tubes.

Endosmose between water and a solution of albumen, through this membrane, seems to go on equally, whatever may be the position of the surfaces with regard to the liquids. We have verified this result in a great number of cases. In a single instance we found the liquid ascend fifteen millimetres in the endosmometer, in which the internal surface of the membrane was turned towards the interior of the iustrument, while in the other it only rose five millimetres.

The same results are obtained in a solution of gum. In both the positions of the mucous membrane of a fowl, the elevation of the liquid is the same, even if the experiment be prolonged during the whole night. When, in some rare exceptions, a slight difference of, at most, one or two millimetres is perceived, it is always in that endosmometer in which the inside of the membrane is in contact with the gum.

To complete this account of the results obtained by employing the membranes belonging to our second category, it only remains for me to mention some phenomena observed in using alcohol for the interior liquid, by putting it in contact successively with each of the faces of these membranes.

With the stomachs of a lamb, cat and dog, the endosmose was constantly directed from the water to the alcohol, and it was most active from the internal to the external surface of the membrane. In fact, we have seen in the endosmometer in which the external face of the mucous membrane of the stomach of the lamb was turned towards the interior of the instrument which contained the alcohol, the elevation was eighty-eight millimetres and ten only in the opposite direction; and that, after this time, the liquid continued to rise forty millimetres more in the first endosmometer, but remained stationary, or even fell somewhat, in the second.

With the stomach of a cat, the alcohol ascended twenty-two millimetres in the tube during the ordinary space of time—namely. two hours, when the external face of the membrane was directed to the interior of the instrument; but in the opposite arrangement, the elevation was not more than two millimetres. In the first position of the membrane, it was sometimes even from twenty to twenty-four millimetres, and in the second it did not rise at all.

With the stomach of a dog, the rise of the alcohol was twenty-four millimetres when the mucous surface was in contact with the water, but was sixteen only when disposed in the opposite direction. Six hours after, the liquid rose forty more millimetres in the first case, and twenty-five only in the second. In another experiment, after the time stated, the elevations were, in the first, 130, and in the second, six millimetres.

With the stomachs which we have hitherto employed, the endosmose was more energetic from the internal to the external face of the membrane, and was always from the water to the alcohol, as with those which Dutrochet used. It is singular, that with the lining membrane of the gizzard of a fowl, endosmose took place, on the contrary, from the alcohol to the water, and even so in every relation of the membranes to the two liquids. This exceptional fact, which we at first attributed to default of integrity of the membranes employed has been proved by us in a great number of instances; at one time, by introducing, as usual, the alcohol in the interior of the instrument, when we saw the level of the alcohol fall; at another, by placing it at the exterior, and then the water rose constantly in the tube. The influence of the direction of the membrane is rendered equally plain in this case. I shall begin by giving the diminutions in height noted in the case where the alcohol was in the interior of the instrument. When the internal surface of the mucous membrane of the gizzard was turned towards the interior of the endosmometer, the diminution in height of the alcohol in the tube was twenty-four, twenty-eight, and even more, in the space of six hours, whilst it was only eleven and twelve in the opposite direction. In another experiment that I select from a great number, the pure water being placed in the interior of the instrument. the elevation was thirty-two millimetres in that where the external face of the membrane was turned from the interior of the endosmometer, and sixteen with the other, in the space of about three hours. The endosmose between the alcohol and the water was therefore most rapid from the internal to the external surface of the gizzard.

We will proceed, lastly, to the exposition of what we observed when employing, for the intermediate membrane, the mucous membrane of the urinary bladder of the ox, in the fresh state, and deprived of its muscular layers, as we had done with the stomachs. By employing this membrane, and introducing the syrup in the interior of the endosmometers, the liquids ascended in the tubes to eighty and even 113 millimetres in the usual space of two hours, when the internal surface of the membrane was in contact with the syrup; but only to sixty-three or seventy-two in the contrary direction. The velocity of the current was then greater, in this instance, from the external to the internal face of the membrane. The contrary was the case with the gum solution.

The elevation was eighteen, and sometimes even only seven millimetres, with this solution, when the internal face of the membrane was directed towards the interior of the instrument; while in the opposite direction it was fifty-two, and in some caces twenty millimetres.

With the gum Arabic solution, the liquid sometimes at first descended in the two tubes, and afterwards, after a certain time, ascended to nearly the same altitudes as we observed with the syrup. In one case the liquid descended in the two instruments seven millimetres during the first hour of the experiment; it afterwards began to rise, and in three hours after, the elevation was twelve millimetres in the endosmometer in which the internal face was in contact with the gum solution, and eight millimetres in the other instrument, where this face was in contact with the water.

With the solution of albumen and pure water, the endosmose did not take place through the mucous membrane at this time employed: the liquid fell in the two tubes, whether the solution was in the interior or in the exterior of the instrument. It should, however, be observed, that when the internal face of the membrane was in contact with the albumen solution, and was directed towards the outside of the instrument, the diminution in height was less than when in the reverse position, and that the contrary took place when this solution was in contact with the external surface of the membrane, and within the instruments.

Lastly, with the alcohol and pure water, the endosmose was from the latter to the former, as in most of the cases; but the elevation was at one time twenty-four, and at another fifty-nine millimetres, when the external surface of the membrane was in contact with the alcohol, and twenty-six or thirty-seven in the contrary direction.

Some differences, as evident as those observed when using fresh membranes, disappeared totally or nearly so by employing them when dried or altered by putrefaction more or less advanced. We will not vary much the experiments proper for determining the influence of the dried condition or putrefactive alteration of the membranes, but postponed this inquiry for other circumstances. It is certain, nevertheless, that by employing the ordinary fluids, and for the membranes interposed between these and water, the dried bladders of the pig and ox, softened before the experiment, so that we could attach them to the endosmometer, there is either no difference in the elevation of the liquids in the two tubes even after many hours, and whatever may be the disposition of the faces of the bladder; or else, if there be one, in all cases it is very little, and exists at one time in that instrument in which the internal surface of the bladder is towards its interior, and at another time in the other apparatus, where the disposition of the membrane was different. By employing the same bladders, after being some hours in water, we sometimes observed a certain regularity in the effects, as takes place with the dried bladders; but if we employed them in a very soft state, after keeping them a night in water, we could not perceive any elevation in the fluids of the endosmometers, or that it was equal in the two tubes. We could not, in some cases, explain the anomalies presented by the bladders in this state; in fact, we readily perceived in a softened bladder how its muscular fasciculi are swelled, and that this increased the longer it remained in water. These muscular fasciculi required also a certain thickness, approached one another, and returned to some extent to a state resembling those in the fresh bladder. But we saw many times that endosmose did not take place with bladders, gizzards, and stomachs in the fresh state, from which we had not previously removed the muscular layers. If the bladder be a little softened, the muscular fasciculi are, it is true, a little more dilated; but at all times there exists between them some interstices through which endosmose can certainly take place; but the inequality of these interstices, even in two symmetrical portions of the same bladder, must give rise to vague and uncertain results.

For the purpose of discovering the influence of putrefaction in the phenomena of endosmose, we only employed the gizzard of the fowl. A great uncertainty prevails in the results furnished by it in this state; at one time the fluid did not traverse it at all, and at another it attained an equal elevation in the two instruments. Whatever fluids we employed, or the disposition of the membrane, the endosmose was energetic, at one time in one way, at another in another; in short, there was sometimes a descent of the fluid in the two instruments. In speaking elsewhere of what we observed when using the skin and stomachal mucous membrane of certain animals, we remarked how the phenomenon of endosmose varied according as we employed these membranes immediately after the death of the animal, or else some hours after. All these facts show clearly the close relationship which exists between the phenomenon of endosmose, and the physiological condition of the membranes.

The phenomenon of endosmose, like all those which are carried on in organized tissues, does not present that constancy and regularity in its appearances, which are remarked in physical phenomenon carried on elsewhere. It is owing to this variable and accidental organic condition of fresh membranes, that we must certainly attribute the singular phenomenon which we witness in certain cases, with the same fluid, the same membrane, and the same relative disposition, sometimes that the fluid rises to eighty millimetres, for example, and at others, only to ten. And we must in the same way refer to a constant anatomico-physiological state, and in relation to the function of the same membrane, the constant difference of elevation which exists in the two different positions of the membrane, whatever difference there may be in other respects. It will be important to study the phenomenon with a view to ascertain the accidental circumstances which occasion this variety in the endosmose through fresh membranes, such, for example, as depriving an animal of nourishment, or the administration of certain substances before killing it, etc. Having in view these objects, we made one comparative experiment only, which led us to admit that the endosmose through the skin of the eel is more energetic when taken from an animal that had been kept some days out of water.

The novelty of these results, and their importance, must be my apology for stating them to you in their full extent. The following are the general conclusions which we deduced :—

1st.—The intermediate membrane exerts a very active part in the velocity of the endosmotic current, as well as in its direction.

2nd.—There is, in general, with each membrane, a certain position in which endosmose is most active. The cases are very rare in which, with a fresh membrane, the endosmose is the same, whatever be its disposition with regard to the two fluids.

3rd.—The direction most favorable to endosmose through skins, is generally from the internal to the external surface, with the exception of that of a frog, through which the endosmose between water and alcohol takes place more vigorously from the external to the internal surface.

4th.—The direction most favorable to endosmose through the stomach and urinary bladders is much more variable than through skin, according to the different fluids.

5th.—The phenomenon of endosmose is closely connected with the physiological state of the membranes.

6th.—With membranes dried, or altered by putrefaction, there is either no endosmose, or if there be any, there is no difference in different positions of the skin.

To give a full account of the results of our experiments, and of the conclusions we have deduced from them, it is necessary to consider *exosmose* in a manner differing from that in which it has hitherto been regarded. The increase of volume presented by the internal fluid, which is generally the more dense, is considered by Dutrochet as the result of the difference between the *strong current going in, and the weak one coming out.* According to this view, that fluid which receives from the other more than it gives back, should increase in proportion to the excess, or rather to the difference between the strong current and the weak one. All the facts that we have observed lead to the conclusion that the different membranes allow the water to pass more readily towards the fluid contained in the endosmometers, if their surfaces are directed after a certain manner, and according to the fluid contained in the instrument. But a great number of difficulties, which we shall abstain from mentioning, because they will present themselves to every one who has followed us in our relation of the facts we have observed, accompany this mode of considering the phenomena. We shall only remark, that with a solution of gum or of sugar in the interior of the endosmometer, endosmose alone cannot account for all the phenomena which are presented by the internal membrane of the stomach of the lamb, and by the mucous membrane of the urinary bladder of the ox, and that these phenomena are susceptible of a more easy and natural explanation, by admitting that by exosmose, the different membranes give to the different fluid a more or less easy passage towards the water, according as they are in contact with one or the other surface ; always supposing the constant passage of the water towards the denser fluid, in obedience to the general law of endosmose. It was necessary, however, to have recourse to experiments, to determine whether this be the true solution of the phenomena ; we were obliged, not only to prove the existence of exosmose, as had been done by M. Dutrochet, but also to measure it in the same way as endosmose.

In these researches, we preferred using the skins of frogs and eels, and, for the denser fluid, salt water, or, in some cases, a solution of sugar.

We began by preparing two endosmometers as usual, placing the skin in one with its internal surface towards the interior of the instrument, and in the other in the opposite manner. Equal quantities of salt water, of known density, were put into both endosmometers, and the instruments were plunged into separate glass vessels, containing distilled water equal in volume to the salt water. After some hours, the quantity of fluid contained in the endosmometers, as well as that of the distilled water remaining in the vessels, were measured, and it was also observed which of the two fluids was highest in the tubes. It was found that endosmose from water to salt water through these skins goes on most freely from the internal to the external surface. On examining the densities of the fluids contained in the two instruments, and of the water contained in the vessels, it was found that in the endosmometer in which the volume of salt water was most increased, its density had been better preserved than in the other ; and, *vice versa*, it was seen that in the vessel in which the distilled water had been more diminished, a smaller quantity of solution of salt was found to have been introduced by exosmose, than in the other vessel in which a smaller quantity of the water had disappeared.

In the following table are given the numbers furnished by two of the numerous experiments which led us to form these conclusions. The first column shows, in tenths of cubic centimetres, the volumes of liquid in the endosmometers after the experiment ; the second column the weight of a given volume of the fluid ; the third, the volume of distilled water found in the external vessels ; the fourth, the weight acquired during the experiment by a given volume of water in the vessels. The weight of an equal volume of salt water before the experiment was 17 gr., 350 ; that of an equal volume of distilled water was 16 gr., 025.

	I.	II.	III.	IV.
Frog's-skin	150 grs., 0	17 grs., 835	112 grs., 5	16 grs., 165
	149, 0	17, 680	113, 5	16, 405
Eel's-skin	222 grs., 5	17 grs., 145	200 grs., 0	16 grs., 170
	217, 5	47, 130	205, 0	16, 220

In some cases we precipitated the salt contained in the external vessels by nitrate of silver. The last column of the second table gives the quantity of chloride of silver thus contained.

	I.	II.	III.	IV.
Frog's-skin	172 grs., 0	17 grs., 190	160 grs., 0	0 grs., 190
	171, 0	17, 175	161, 0	0, 280

We obtained similar results with syrup and the skin of an eel; the weight of a given volume of syrup before the experiment was 18 grs., 180.

	I.	II.	III.	IV.
Eel's-skin	193 grs., 0	18 grs., 035	181 grs., 0	16 grs., 045
	191, 0	18, 010	183, 0	16, 050

These facts cannot be explained by supposing that the elevation and increased volume of the fluid in the two endosmometers are only the difference between the current of endosmose and that of exosmose. If it were so, the endosmometer in which the volume of salt water is most augmented, ought to contain a liquid less dense than the other, in which it has increased less. On the contrary, it is completely explained by admitting that the endosmotic current is equal, or nearly so, in the two positions of the membrane, and that the difference arises entirely from the current of exosmose, which is more feeble in the endosmometer in which the elevation is the greatest, and more active in that where the elevation is the least.

These results show the great importance of the action of the membrane interposed between the fluids; in fact, merely by its peculiar nature and physiological function we can explain more or less easily, the passage through it of different fluids of a certain density towards another which is less dense.

We acknowledge the necessity of further experiments to exhaust so important a subject as the phenomena of endosmose, which operates through the different lining membranes, and principally as regards the comparison between different species of animals, particularly between the carnivorous and herbivorous. Yet we can assert that the results we have obtained by this series of experiments, and our method of considering exosmose, lead to a more just application of the phenomenon of endosmose to the functions of organized bodies.

The exosmose of solution of sugar, albumen, and gum, towards water, is most active from the internal to the external surface of all the skins that we examined. It is precisely in the same direction that an abundant secretion of mucus passes through the skins of the torpedo, the eel, the frog, and other animals. Endosmose from the water to these solutions is less intense, from the external to the internal surface of the skin, than in the contrary direction. Consequently, even if it be not admitted that this direction of the mucous secretion, and this feeble absorption of the water in which these animals live—functions which, by a general law, must always bear a certain proportion to each other—are not entirely due to the phenomena we have discovered, it cannot, at least, be denied, that they are extremely favorable to it. Doubtless this function of the skin could not be carried on, or would be very imperfect, if, in animals which live entirely in water, this membrane acted by endosmose in a direction opposed to that we have discovered.

We dismiss, for the present, what takes place with water and alcohol through the skin of the frog. Alcohol is a liquid which has no analogy to those which are found in the bodies of animals; and the anomalies we observed when employing it as an endosmometric fluid, if they hold good with regard to human skin, belong rather to the science of therapeutics than to physiology.

The direction constantly observed to be most favorable to endosmose and exosmose through the skin is no longer found to be so when the mucous membrane of the stomach of different animals is employed. But every one knows that the function of the stomach is extremely complicated, and that all the substances conveyed into this organ are not absorbed there, or are so very unequally. Besides, we repeat again, this subject ought to be elucidated by further study. When it is found, for examble, that the directions most favorable to endosmose between water and syrup, is not the same in the stomach of a ruminant as in that of a carnivorous animal, it is clearly demonstrated by this fact, that the phenomenon of endosmose is intimately connected with those essential modifications by which the digestive functions of these two orders of animals are regulated.

I was anxious to relate in detail the experiments I made with Professor Cima on the subject of endosmose, being convinced of the great importance of this phenomenon in the functions of life. It is by endosmose that physiologists now explain the nutrition of the ovules of mammiferous animals in the ovaries, and how the sacs containing the germs of the cephalopodous mollusca (or spermatoferes) open as soon as they come in contact with water. The life of the cellule, which is certainly the elementary organ of all the vegetable and animal tissues, ought to include the action of endosmose. This shows how these phenomena ought to be studied, in order to make all the applications of which they are capable. I will not close without relating the recent experiments of Poiseuille, in order to explain hy endosmose the purgative effect of certain substances. He found that endosmose takes place through the animal tissues, from the serum to the Seidlitz water, sulphate of soda, and sea salt. This is precisely what happens when these medicines are used internally. The excrements contain an unusual quantity of albumen; in this case it must be admitted that endosmose has taken place from the serum of the blood to the saline water, in the intestine, through its capillary vessels. But to place beyond doubt the correctness of Poiseuille's application, it is necessary to show, that endosmose goes on when one of the fluids is in motion, and is continually renewed. This has been recently done by Dr. Bacchetti, who has shown that the rapidity of endosmose is considerably augmented when one of the fluids is continually flowing. This result also is in accordance with the principles of the theory of endosmose. The changes of the fluids constantly going on through the membrane certainly tend to check the action of endosmose; or, in other words, the condition of this phenomenon are better preserved the longer the fluids are kept without mixing. Poiseuille has also demonstrated, that endosmose ceases through a membrane after it has gone on for a certain time; but that this property may be restored to it after having used it with other fluids. The most remarkable fact of those discovered by Poiseuille is that of the influence of hydrochlorate of morphine. This substance, when added to saline solutions, greatly diminishes the endosmose from the serum to the solution, and at last changes the direction of the current. This fact has been confirmed by Dr. Bacchetti. How can this fact be entirely overlooked in explaining the action of morphine and preparations of opium as remedies for diarrhœa, and the constipation which they produce?

LECTURE IV.

Absorption of Animals and Vegetables.

The preceeding lectures upon the phenomena of capillarity, imbibition, and endosmose, were principally intented to prepare you for the study of absorption and exhalation. It is not my place to give you a long history of the researches made expressly for the purpose of deciding which of the different organic apparatus is specially endowed with these functions: in the different treatises on physiology, you will find them at one time assigned altogether to the veins, and at another exclusively to the lymphatics.

It is difficult to account for so much discussion when we consider the structure of the different tissues, and the necessity for absorption and exhalation in a large series of inferior animals which do not possess lymphatics.

Absorption, considered as a function of living animals, does not consist in the simple imbibition of fluid by a tissue, but also its transmission into the bloodvessels with which the tissue is in contact. It is into the blood that the absorbed matter must ultimately come: this is the completion of the phenomenon. We should, then, distinguish two things in the absorption—the passage of a substance through the interstices of some organized tissue, and then its introduction into the circulation.

It is easy to demonstrate the existence of the first part of this function. Here is a frog, the inferior extremities of which have been immersed for some hours in a solution of prussiate of potash. If we remove it from the liquid, wash it carefully with distilled water, and cut it in pieces, you will readily perceive that the solution has penetrated the whole body. Wherever we touch the viscera or the tissues with a glass rod dipped in a solution of chloride of iron, a blue stain, more or less deep, appears. I would also demonstrate in this way the reality of absorption, since it clearly shows the parts of which we said the function consists. A living frog, after having its lower extremities immersed for a short time in a solution of prussiate of potash, was killed; and on applying immediately the chloride of iron to the heart and lungs, the presence of the prussiate was plainly detected, while scarcely a trace of it could be discovered by the same means in the muscular mass of the legs and thighs.

In another experiment, precisely the same result was obtained: I immersed another frog, which had been dead some moments, in the same solution, and allowed it to remain the same time. When touched by the reagent, the heart and lungs exhibited no more of the prussiate than every other part of the frog. The solution was introduced into the body of the frog by simple imbibition; and this phenomenon, which took place equally in the living as in the dead frog, certainly cannot be considered as different from the imbibition which we have studied, and which belongs both to the organic and inorganic bodies, and is the consequence of their cellular or vascular structure, &c. But besides this, we find, in the heart and lungs of the living frog, a much more considerable quantity of the solution than in the other parts, although they may be nearer to the point of immersion. These viscera are the centre of the whole circulatory system; in them the great lines of bloodvessels either commence or terminate. The solution of prussiate of potash had therefore penetrated into the bloodvessels, become mixed with the blood, and so arrived at the heart and lungs.

It has long been a subject of discussion, whether the lymphatics alone possess the power of absorption, or whether this function belongs only to the veins; or, to speak more clearly, whether a body can be directly introduced into the bloodvessels by penetrating their walls; or whether, to reach them, it must pass through the lymphatics. Since there is no portion of an organized body that has not the property of imbibing water, saline solutions, and serum, it is clear that the first part of absorption can take place as well in the walls of the lymphatics as in those of the bloodvessels. Microscopic anatomy, by revealing the manner in which the bloodvessels and lymphatics terminate, has confirmed the preceeding conclusion. I am led here to cite the principal results of the observation of our countryman Panizza.

There is not a single fact in nature which demonstrates the existence of free extremities in the ramifications of the bloodvessels, which everywhere present a very close and continuous reticulated texture. The arterial network is always continued without interruption to the venous network, which in general predominates over the former. The lymphatic system also never terminates in independent extremities, but is always presented to us as a very fine and close trellis-work. Anatomy, agreeing with experiment, leads us to conclude that the first part of absorption can only be effected by the aid of porosities peculiar to the structure of organized bodies. In this way absorbed bodies become mixed with the blood, chyle, and lymph, and by their common movement are distributed over all the body. I shall regard it now as nearly superfluous to mention to you the experiments of Majendie, Segalas, and the latest of Panizza, by means of which they have shown that absorption not only can, but that it does, take place, principally through the agency of the bloodvessels alone. See the manner in which the last of these physiologist operated. He made an incision of ten inches in length in the belly of a horse, and drew out a fold of small intestine, in which several small veins had their origin, which. after a short course terminated in one large mesenteric trunk without any vein, proceeding from the glands, emptying itself into it.

This fold of intestine being cut off by a double ligature in such a manner that it could receive blood only by one artery, and that no blood could be returned to the heart except by the venous trunk, a hole was made in the fold for the purpose of introducing a brass tube, which was fixed by means of a thread, in such a manner that the substance introduced should not be brought in contact with the bleeding edge of the opening. This being done, a ligature was passed under the vein, which received the blood flowing back from the fold. The ligature was closed, and in order that the circulation should not be impeded, the vein was immediately opened to give exit to the blood returning from the intestine. Then, by means of a glass funnel and brass tube, a certain quantity of concentrated hydrocyanic acid was introduced into the portion of intestine, and the tube closed. On receiving the venous blood, we immediately found it charged with the hydrocyanic acid. Yet the animal did not give any symptoms of poisoning, although the nervous ramifications, and the lymphatic vessels were left untouched. In another experiment, instead of tying and opening the venous trunk, Panizza simply compressed it at the moment that he introduced the acid. There was no symptom of poisoning; but in a little time after the removal of the compression, the intoxication became manifest, and on the vein being opened, the acid was detected in the blood. Lastly, in a third experiment, Pannizza cut off quickly, and with care, all the lymphatics and nerves from a portion of intestine, and the hydrocyanic acid poured upon it was not slow in killing the animal, provided that the vein was left uninjured. Venous absorption is thus proved by the most careful experiments.

We find it stated in many works on physiology, that the presence of substances swallowed has been detected a few minutes after their introduction into the stomach, as a fact opposed to this opinion, that absorption takes place by means of bloodvessels. But this objection soon vanishes, when we reflect on the rapidity of the circulation of the blood.

On the other hand, that absorption can take place also by the lymphatic vessels, is a fact well known, and too evident to render it necessary to demonstrate. If we kill an animal two or three hours after a meal, take out the intestines, and examine the mesentery with attention, we shall see that the lacteals are full of a milky fluid, analogous to that which flows abundantly from the thoracic duct, which is the principal receptacle into which these vessels empty themselves. This fluid is the chyle, which by the act of digestion, is formed in the intestine, and absorbed from it by the lacteals. How many examples does pathological anatomy afford in which these vessels have been found full of pus from their proximity to an abscess! The lacteals and lymphatics, then, are endowed with the faculty of absorbing. In a word, absorption always take place under these conditions: 1, a vessel having organized walls; 2, an external fluid, which the tissues of the vessel are capable of imbibing; and 3, an internal fluid, which is miscible with the external fluid, and which circulates in the vessel more or less rapidly. Nothing, therefore, is more physical than a phenomenon so constituted. I will show you, by an experiment, the truth of this assertion.

Here is a considerable portion of a vein, taken from a large animal; it is fixed by one extremity to a tube, which terminates in an opening made near the bottom of a glass receiver; the other extremity is tied to a small, curved, glass tube. furnished with a cock. I fill the receiver with water, and consequently the vein also. I arrange them so that part of the venous trunk is immersed in water acidulated with sulphuric or hydrochloric acid. At first the water in the receiver shows no trace of the acid, but after a certain time it becomes evident. If, instead of waiting some time, and leaving the fluids at rest, I open the cock, and allow them to flow out, I soon discover signs of the acid in the fluid which escapes; but in the receiver it is not yet discoverable. What takes place with a portion of vein may also be seen by using an arterial trunk, or a tube of clay, pasteboard, or wood; and if conversely, the acid solution be contained in the vein, and some tincture of litmus be mixed with the water in the basin in which it is immersed, the same phenomenon will take place—that is to say, the acid will transude through the walls of the vein with a facility proportional to the rapidity with which the water flows out of the basin. The conditions of the phenomenon are always the same in two liquids capable of mixing, separated by a membrane which can imbibe both, and the movement of the internal or external liquid, as the case may be, which transmits, in a given direction, that which has passed through the membrane. If we were to suppose for a moment, that the circulation of the blood proceeded in an opposite direction to that which it really does, but without any variation in the structure and relative situation of the arteries and veins, we should not say, in that case, that the veins absorbed, but rather, that absorption took place by the arteries.* This is the very simple physical phenomenon of absorption. I wish also to explain to you the laws of this function, which have been discovered by experimental physiology, and you will easily perceive, that they are a necessary consequence of the principles which I have announced.

"1st.—Substances are absorbed more or less readily, according to their solubility, minute division, and disposition to combine with the organic juices, and to become constituent parts of the blood."

Notwithstanding the unscientific language in which this law is expressed, I wished to communicate it to you as it is found in the most esteemed modern works on physiology. This law is an evident consequence of the manner in which we have seen the phenomenon of absorption take place. It would be attended with great value if physiologists were to study accurately the different facilities with which organized tissues imbibe different liquids; this study would certainly produce very important results in therapeutics.

I will now show you some facts which may pave the way for these researches. Here are two rabbits: into the stomach of one I introduce some water, and some oil into that of the other. In two hours no trace of the liquid will be visible in the stomach of the first; but all the oil will be found in that of the second, and even for many hours after. If, instead of pure water, a mixture of water and alcohol be used, the absorption would be still more rapid. An acid or saline solution would also be absorbed, but less quickly than pure water.

"2nd.—The rapidity with which absorption takes place in different organs is principally determined by the number of their vessels, the flaccidity of their tissue, and the conducting quality of the parts which cover them."

I continue to repeat word for word what I find in works on physiology. It is evident that by flaccidity of tissue and *conducting quality of the parts which cover the organs*, nothing else can be understood than that the texture of the solid organs is more or less suitable for imbibition. *The greatest number of vessels* denotes nothing more than as affording the greatest number of points of contact between the body which is to be absorbed, and the liquid with which it is to be mixed and carried away. It is on account of this that the lungs, as

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* The meaning of Professor Mateucci is not very clear in this passage. If the Professor means that if the direction of the circulation were changed, it would be proper to say that the arteries alone absorbed, and that the veins did not, it appears to the translator inconsistent with the premises, and with the conditions of absorption, as expressed in the pages immediately preceding. In fact, taking into consideration only the conditions necessary for absorption, there appears no reason why the arteries and capillaries should not absorb also, but, from the difference in structure of their walls, with of course different facility.

we have seen, are the most favorable organs for absorption, and are the first to manifest the presence of the absorbed body. In fact, anatomy teaches us that the lungs, more than any other parts of the animal economy, possess a structure suitable for imbibition, and a very highly developed vascular system. The cellular tissue is also very permeable to liquids; but having fewer blood-vessels than the lungs, absorption takes place less rapidly in it. The skin, on the contrary, being covered by the epidermis, which is of a very compact texture, and without vessels, affords little facility for this function, but this becomes greater on removing the epidermis.

"3rd.—Absorption varies according to the quantity of liquid which exists in an organ; it is in an inverse ratio to the plethoric condition of the animal."

Bearing in mind the phenomenon of imbibition, it will be easy for you to comprehend this law of the function. A mass of sand already saturated with a liquid ceases to imbibe further; but, on the contrary, imbibes the more readily the further it is from this state of saturation. Dutrochet left a plant exposed to air until it had lost by evaporation about one-fifteenth of its weight, and afterwards, by plunging it in water, he found that in each of the four first hours of immersion, it absorbed twenty grains and lost eight; a little later it did not absorb more than nine grains, and lost the same quantity by exhalation. Edwards found that frogs absorbed the more rapidly according to the previous loss of weight by evaporation. Magendie reports that a dog, from which he had taken away a good deal of blood, died quickly from poisoning by strychnia, yet in another, into whose veins he had introduced a considerable quantity of water, poisoning did not take place.

4th.—Absorption varies within certain limits, in proportion to the temperature of the absorbing body, and that of the body absorbed.

Every one knows that hot drinks act more rapidly than cold ones. We have also seen that imbibition varies with the temperature. I told you that this variation could only take place within certain limits, inasmuch as beyond these, the structure of organized bodies becomes altered.

5th.—According to Fodèra, the electric current favors absorption.

If we were willing even to admit the experiments of this physiologists, it is not easy to account for it, the more especially, that, on applying the electric current in imbibition, this influence is not remarked. The single fact adduced by Porret, and which consisted in the transit of water from the positive to the negative pole, may in some way explain the results of Fodera.

6th.—Lastly, absorption varies according to the rapidity with which the circulation is carried on in the absorbing vessel.

It is unnecessary to state how this rapid circulation accelerates the transit of the absorbed body more or less quickly to a given distance. It is equally easy to comprehend that the molecules of the liquid contained in the vessel will be renewed the more frequently, as the actions of affinity which promote the absorption of the body into the interior of the vessel are the more energetic. It is probably owing to this that absorption takes place more slowly by the lacteals and lymphatics than by the veins.. This is why many colored substances, alcoholic liquids, and saline solutions, introduced into the stomach, are found in the blood, without our being able to discover them in the lacteals and thoracic duct. Frictions on the skin, the peristaltic movements of the intestinal canal, assist absorption in this way,—by favoring the movement of the liquids in the vessels.

The function of exhalation is generally effected by the same mechanism, and is governed by the same laws as those which we have been studying. Where the walls of a vessel possess the property of imbibing the contained liquid, a portion of it is constantly exhaled from them. The portion which escapes will vary according to the nature of the liquid—that is, according to the greater or less facility which the walls of the vessel possess for imbibing it. According as the walls of this vessel are more or less humid externally, so will the internal liquid escape with greater or less difficulty. The exhalation will increase if, on account of the greater mass of the contained liquid, the vessel has to support a very strong pressure. All these peculiarities of exhalation, which result from that which we regard simply as a physical phenomenon, and dependent on the same principles as absorption, are demonstrated by experimental physiology.

Edwards has proved that cutaneous exhalation is, in some cases, ten times more considerable in dry air than in moist, and that it is doubled in passing from 0′ to + 20°. Transpiration increases also, if, in the place of being in repose, the atmoshperic air be agitated around the body of the animal. These results, obtained by Edwards upon cutaneous exhalation, are very natural consequences of physical principles too well known to render it necessary for me to mention them here.

Some phenomena of absorption and exhalation from living bodies are accomplished by the transformation of the absorbed or exhaled body. The liquid which a membrane imbibes and exhales by its opposite surfaces is not identical with that which has been placed in contact with this absorbing membrane.—This happens in most of the cases of exhalation, and principally in the secretions.

We are far from expecting to find the explanation of the phenomenon of secretion in the actual state of physico-chemical knowledge. It must be confessed, that the secretions still form one of the most obscure objects of the animal economy. With regard to exhalation, we must not omit to mention, that a phenomenon analogous to that of filtration must intervene. A liquid holding insoluble particles in suspension is divided by filtration into two portions; the liquid part is imbibed by the filter and passes through it; the solid part remains upon the filter. Anatomists know that when veins or arteries are injected with a solution of gelatine coloured with vermillion in very fine powder, the gelatinous solution becomes colourless when it passes through the vascular walls.—Every contusion produces a stain, the centre of which is a bluish-black, and the periphery green surrounded by yellow. In this case, the clot of extravasated blood is separated from the serum, which is imbibed by the neighboring tissues.

Do not forget the fact which has been pointed out to you with regard to imbibition; salt water becomes fresh by passing through a bed of sand, but a solution of carbonate of soda, filtered in the same way, becomes more dense. Imbibition, capillarity, the simple play of molecular attractions, can overcome affinities; there is therefore no reason for entirely disdaining the ancient opinion that the secretory organs are merely filtering machines.

In another lecture we shall see how membranes, and all the organized tissues, are fitted for the passage of gaseous bodies. Fodéra first proved that sulphuretted hydrogen, enclosed in one part of the intestinal tube, spreads through the whole body of the animal, and produces death.

We must also say a few words upon absorption in vegetables. In these small glasses are a great number of plants, all plunged more or less into a very weak aqueous solution of acetate of iron: in some of these are haricot beans, in others, French beans. The leaves of some have been taken off, those of others have been cut off, and thus immersed by the stem only; some have been deprived of the extremity of the roots, of others the roots have withered before they were put into the liquid; and, finally, some have been placed in it after they had been completely dried up. If we use prussiate of potash, to determine whether the ferruginous solution has really ascended in the interior of the plant above the level of the liquid in which they are plunged we shall soon find that it has done so, and that it has been imbibed, by a part more or less raised above the liquid. It will be seen that in the living plant which retained its leaves and roots, the liquid has ascended above the level; in that which was withered, and which had regained its freshness in the aqueous solution, the absorption has been greater; and, lastly, that it is most abundant in those from which the roots have been removed. Whatever liquid be employed, it is always absorbed by the vegetable, except some acid, alkaline, or highly concentrated saline solutions, which alter and destroy the structure of the plant.

Almost all that is known upon this subject is contained in the celebrated work of Saussure, "Chemical Researches on Vegetation." This is an absract of the results at which he arrived: 1. The roots of plants absorb saline substances dissolved in water, but in a much smaller proportion than pure water. 2. The ablation of the roots, or altering them, or, generally, everything which weakens the vegetable force, favors the introduction of salts into the plant. 3. A plant does not absorb all the salts contained in a solution in the same proportion. This last result is confirmed by the fact that certain salts are constantly found in some plants. Professor Piria always found grains of manganese in the *lupinus albus*.

Let us now examine whether the absorption of nutritive juices, which takes place by means of the roots of a plant, and the movement of these juices in the plant, may be considered as simple phenomena of capillarity or imbibition.

At the beginning of spring, the sap rises from the roots to the leaves through the central part of the trunk, and during this time, a liquid, of different composition to the sap, called *the proper juice*, moves in the contrary direction, from the leave to the roots, through the cortical tissues. If a hole be made as far as the centre of the trunk of a growing plant, a large quantity of sap will flow from it, which is more dense in proportion as it is obtained higher up and nearer to the leaves. If, on the contrary, a ligature is fixed round the trunk, or if a circular layer of bark is taken off, the swelling, which will soon be seen forming above the bandage or the ring on the side next the leaves, will prove the existence of a descending current of proper juice. Hales has proved that the quantity of liquid which a growing plant absorbs, increases in proportion to the superficies of its leaves—a fact which he explains by attributing it to the evaporation carried on by their means.*

The double movement of the juices in the interior of vegetables is inexplicable by the forces of capillarity and imbibition alone. There is something more. Every one has seen, that on cutting the stem of a vine in spring, an enormous quantity of liquid flows from it. Hales applied to this section one end of a curved glass tube, which he left open at the opposite extremity, into which he poured mercury, and saw the liquid rise thirty-eight inches in the open part of the tube, above the level of the liquid on the other side of the curvature, which proves what pressure supports the mercury at the other extremity—a pressure which can only be attributed to the liquid forced up by the plant. This force of impulsion, the escape of the liquid from the plant by an incision, are facts incompatible with the effects of capillarity and imbibition. A liquid mounting in a capillary tube, cannot flow from the tube by the same force which raises it. Dutrochet demonstrated by a very simple experiment, that the force of impulsion which occasions the ascent of the juice of a plant, has its seat in the ultimate extremities of its roots. By making successive cuttings in the trunk of a vine, towards its roots, this distinguished physiologist saw the flow of sap continue, even from those parts sunk in the soil. One of the smallest radicular filaments placed in water, also permitted the escape of the sap. It is, then, in the spangioles that this force of impulsion resides. Dutrochet adds, that he discovered in the cells of the spangioles a liquid more dense than water, and coagulable by nitric acid. He therefore believes that he sees in spangiole or, rather, in its cells, filled with this fluid, heavier than the water with which they are surrounded, a group of endosmometers. The phenomenon of the ascent of the liquid in a plant is, then, an example of endosmose. I confess that I should like to see the identity of these phenomena more clearly shown than it is by the observations of Dutrochet. However it may be, the explanation of this author is, in the present state of science, the least improbable.

* This must depend also very much upon the structure of the leaves with regard to the number of stomata which it contains.—Trans.

How does the sap rise in a plant from which the roots have been cut off, and the lower extremity has been placed in water? The great height to which a liquid can ascend in the trunk of a tree is opposed to the explanation which we may give of the phenomenon, by considering it as the effect of imbibition or capillarity—phenomena which we know to be confined within much smaller limits than those which are presented by the trunks of plants.

Hales, having perceived that the quantity of sap which rose in a plant was proportional to the surface of its leaves, concluded that the liquid of the superficial cells of the leaves being evaporated, these, by means of capillarity, absorbed it from the inferior cells, and by these means the secretion went on by degrees, though the extremity was cut off. By drying some mercurial plants to different degrees, Dutrochet has proved that the absorption in plants thus dried does not increase in proportion to their dryness; in fact, one of these plants which had lost a third of its weight by evaporation, absorbed much less than another which had only lost one-tenth. Notwithstanding its greater dryness, the absorption was less, although the plant had not been dried so as to alter its texture. Evaporation or transpiration through the leaves is not, then, the cause of the ascent of the liquid in the trunk of a plant plunged in water, or, what is the same thing, it is not a vacuum in the superficial cells which occasions the ascent of sap. This last does not take place unless there be a certain quantity of water in the vegetable tissue, which perhaps acts by adhesion upon the fresh water which is to rise, as a sponge is more rapidly soaked with water when it is damp than when it is dry. Dutrochet also tried to dry a plant, to make it reabsorb the water lost, and to plunge it afresh in the water: he saw that the ascent did not take place unless the plant had regained the state of turgescence natural to it. This turgescence of the cells of the leaves is caused, according to Dutrochet, by the action of endosmose, by which the liquid would be transpired by the leaves in an active manner, and very different to that of a liquid evaporated in air. I may lastly state that Dutrochet has shown that the influence of light upon the ascent of the sap in vegetables is exercised in respiration, and in the fixation of oxygen in the vegetable tissues.

The phenomenon of the ascent of liquids in vegetables is, therefore, not owing to capillarity and imbibition alone: the cause is principally to be found in the roots, and partly in the leaves. It is probable that an action of endosmose takes place in the extremity of the roots; and it is not foreign tò suppose, aso, that a similar cause produces the movement of the chyle and lymph in the lymphatic aud lacteal vessels—a movement which we know is continued some time after death.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*On the Pathology and Treatment of Hysteria.* By JAMES MILMAN COLEY, M. D., Physician to the Western Dispensary, and Senior Physician to the Royal Pimlico, Dispensary and Lying-in Institution.

The frequent occurrence and uncertain duration of this disease, and its obstinate resistance to the remedies promiscuously employed for its relief, render it one of the most unfortunate afflictions to which females are liable. The term hysteria, which signifies some affection of the uterus, instead of pointing out the true pathology, or affording any explanation of the various symptoms of the malady, has been a constant source of erroneous practice; and the doctrine, which attributes all involuntary muscular movements or convulsions to simple irritation, without the intervention of vascular congestion or excitement, has still farther diverted modern inquirers from the actual source of this disease, and been a powerful means of encouraging empiricism. That the uterus has little if any special concern in producing hysteria, is proved by this disease being entirely absent during the progress of malignant and other organic affections of that organ, and by its occasional appearance in the male sex. And with respect to the fatal convulsions which sometimes occur during parturition, and which are supposed by some to be dependent entirely on uterine irritation, post-mortem examinations will discover in all such cases disorganization in one of the nervous centres, corresponding with the nature and severity of the attack; and all those cases of paralysis which are connected with utero-gestation, as amaurosis, hemiplegia, &c., which disappear after vascular compression in the cerebrum or cerebellum has been removed by the evacuation of the uterus, evidently proceed from obstructed circulation, and not from special sympathy, or imaginary nervous association with this organ.

Hysteria consists of a disturbance in the functions of oné or more of the three great nervous centres, namely, the brain, spinal marrow, and the abdominal ganglionic system of nerves, which disturbance is manifested by symptoms peculiar to the respective seats of disease. Thus, when the brain is affected, we find there is a suspension of the senses and of consciousness, or of the perception, arising from external stimuli, manifested by stupor, temporary loss of sight, hearing, smell, taste, and touch; and this condition of the brain may advance, and partake more or less of the nature of epilepsy or apoplexy; that is, it may be accompanied with convulsions, or stertorous breathing and coma, and terminate in death, like ordinary cases of those diseases, either by asphyxia, or compression, extravasation in the brain. When death occurs from paralysis of the pneumogastric nerves, or from asphyxia from any other cause, the right cavities of the heart are found distended with black blood, and the pulmonary capillaries in a state of congestion, while the left auricle and ventricle are empty. On the contrary, when the patient dies from congestion in the arterial capillaries of the brain, the left cavities of the heart and the principal arteries, which conduct the aortal circulation, are gorged with blood. An instance of fatal termination of cerebral hysteria in apoplexy occurred to a patient of my friend Mr. Ince, of Lower Grosvenor Place, a general practitioner possessing great discrimination and sound judgment. The patient was a young lady who had been subject to repeated attacks of hysteria, which at times resembled epilepsy. At length one of the paroxisms terminated with symptoms of apoplexy, which proved fatal. On examining the brain after death, Mr. Ince found a large coagulum of blood extravasated in one of the lateral ventricles. These, however, are rare and extreme cases. In ordinary hysterical affections arising from temporary obstruction in the cerebral circulation. the face and neck are flushed with a sudden efflorescence, which sometimes extends to the extremities; and in some females these flushings are the principal indications of the disease, when they occur in combination with depression of the mental energies, or slight intermissions in the functions of the pneumogastric nerves. Cerebral hysteria sometimes results from the transition of congestion, or subacute inflammation, from the mucus membrane of the bowels, or from some fibrous structure, to the brain.

Case I.—A lady, who had been in great distress respecting the loss of her husband, was attacked with dysentery. At the end of a week from the commencement of the attack, the dysentery and concomitant fever entirely disappeared, in consequence of the sudden supervention of hysteria, which declared itself by a slow interrupted pulse, absence of febrile heat, and by depression of spirits, accompanied with frequent fits of crying, followed by a copious discharge of lympid urine. This hysterical state continued three weeks, when it subsided as suddenly as it commenced, and left the patient in perfect health.

Case II.—Another lady, who had been suffering three weeks with inflammatory typhus, became suddenly afflicted with hysterical mania, produced by transition of disease from the tendinous expansion of the occipitofrontalis muscle to the serous membranes of the brain. She had been exposed to considerable anxiety before her malady commenced. The maniacal paroxysms were always terminated by fits of laughing and crying; and after continuing to recur for the space of a week, during which time the pulse was slow, and all symptoms of fever were absent, the patient became free from all disease.

The condition of the cerebral membrane in these cases appeared to be that of congestion. otherwise the pulse would have been excited, and local heat developed, as we find in cases of mania proceeding from subacute arachnitis.

When the pulmonary ar aortal circulation is interrupted by hysteria, we shall find the capillary vessels in the skin or subcutaneous cellular membrane endeavoring to relieve the temporary plethora, which may oppress the membranes of the brain, or of the air or alimentary passages, by producing the sudden efflorescence before alluded to on some parts of the external surface, or effusion of serum in the cellular membrane of the hands or feet. A similar temporary congestion or plethora in the minute vessels of the skin, and similar œdematous swellings, which are its results, occur during the violent paroxysms of spasm in the glottis, arising from interruption to the pulmonary circulation, and corresponding congestion in the lungs.* The hysterical cough is a modification of disordered function in the laryngeal branch of the pneumogastric nerve, and may be always traced to some mental emotion disturbing the circulation in the brain; and hysterical vomiting may also be referred to cerebral congestion acting upon the stomach throug the medium of the pneumogastric nerves.

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*See my Treaties on the Diseases of Children, Longman and Co., 1846, p. 248.

Case III.—April 22, 1846, I was requested in the middle of the night to visit a lady, who was attacked with vertigo, and unable to lie down in bed. She had also extreme agitation and restlessness, involuntary contraction of the flexor muscles of the upper extremities, and every now and then gave utterance to the most violent hysterical screams. Every time she attempted to lie down the symptoms were increased, and she complained of loss of feeling in the right arm and leg, attended with tingling in both hands, and fear of death. Her countenance was pale and contracted. As soon as she was able to swallow I prescribed a mustard emetic, which excited full vomiting, and at the end of two hours all the symptoms of hysteria disappeared. In this case I was induced to abstain from bleeding, on account of the delicate emaciated state of the patient, and the exsanguine appearance of the countenance.

The symptoms of histeria proceeding from temporary congestion of the membranes of the medulla spinalis are convulsions of the voluntary muscles, or remitting neuralgia, or morbid perception, or temporary paralysis, or enervation in the nerves of sensation. The latter symptoms are produced by vascular excitement or compression acting upon the sentient fibrils, and the former by a derangement produced in the same manner in the functions of the motor filaments of the spinal nerves; and the seat of the disease may be discovered by an attentive observation of the phenomena. The true hysterical affection of the spinal nerves is seldom if ever unconnected with a corresponding disturbance in the functions of the cerebrum, or of the great sympathetic. The exact condition of the capillary vessels in the membranes of the spinal marrow, during the hysterical paroxysms, is not known; but I have repeatedly discovered the existence of congestion or inflammation in the arachnoid and pia mater, in the vicinity of the pons Varolii, and effusion beneath those membranes after fatal convulsions in cases complicated with paralysis of the voluntary muscles; which facts tend to prove that the nerves of sensation and voluntary motion are excited by increased vascularity in their investing membranes, and paralyzed by the compression occasioned by interrupted circulation, effusion, or extravasation; and this appears to me to be a much more correct explanation of the cause of convulsion and paralysis than simple irritation of the nervous fibrils unconnected with the sanguineous circulation. Among the various symptoms arising from hysterical excitement of the metor fibrils of the spinal nerves may be mentioned the partial contractions of the intercostal and abdominal muscles, commonly described as cramps. These are generally disregarded by the medical attendant, and considered irremedial, the patient being left to endure protracted misery. The most common seats of these singular convulsions are the recti abdominis muscles. Hard, large, round, incompressible tumors are formed in them by the sudden involuntary contraction of the muscular fibres, presenting the appearance of solid indurations, and communicating to the superincumbent hand a rotary and sometimes a pulsating motion, resembling that of the fœtus in utero.

Case IV.—In 1827 I was consulted by a young unmarried lady, æt. 23, who had been a severe sufferer from this disease. On examining the abdomen during one of the hysterical paroxysms I discovered a hard round tumor, as large as a child's head, in the recti muscles, which communicated a rotary and strong pulsating motion. At the end of a few seconds this tumor, which was seated in the upper part of the abdomen, subsided, and was succeeded immediately afterwards by a similar swelling immediately in front of the hypogastric region, resembling the impregnated uterus at the sixth month of gestation. This round, hard, projecting, and circumscribed distension continued about half a minute, after which the abdomen was contracted in the most rigid manner; so that, after the tumor had disappeared, the whole front of that region became rigid, uneven, and retracted as close as possible to the subjacent viscera. This concave contraction of the abdominal parietes continued about half a minute, after which time the parts resumed their natural appearance.

Case V.—A lady, æt. 37, mother of two children, had been many years tormented with frequent attacks of hysteria in various forms. I was consulted in 1835, when I had an opportunity of examining the surface of the abdomen during a distressing paroxysm of what she called cramp. I found the abdominal muscles, especially the recti, drawn convulsively into hard tumors, which afforded to the hand a sensation exactly resembling that of the rolling or locomotion of a fœtus. She described it as if something were grasping her bowels.

In some of these cases the pulsations discovered in the abdominal region are apt to mislead the medical attendant into a belief of the existence of aneurism. Morgagni relates a case in which this mistake was made in a young lady, who was supposed to be laboring under aneurism in the abdominal aorta, until it was ascertained that there was no correspondence between the extraordinary pulsation in the abdomen and that in the left ventricle of the heart. I believe these pulsations to proceed from a convulsive motion in the abdominal muscles.

Hysterical disturbance in the abdominal ganglionic nerves is manifested by irregular action in some of the numerous organs, which they supply with vitality. Hence we may notice extraordinary pulsations in the heart, a disordered state of the peristaltic motion of the alimentary canal a derangement in the secreting function of the kidneys, &c. The pulsation of the heart is sometimes found to be intermittent; at other times this organ is troubled with a morbid irritability, which renders the contraction of the left auricle distinctly perceptible, and the pulse in the cardiac region twice as frequent as that at the wrist.

The rumbling noise, occasioned by disordered function of the fibrous coat of the bowels, so common in hysteria, whereby the gaseous contents of the colon are forcibly moved in different directions, is not peculiar to that disease; as it is met with in those cases of epilepsy which are connected with melancholy and dyspepsia; and the globus hystericus, or the sensation of a ball ascending the œsophagus and producing dysphagia and a sense of suffocation, is also neither peculiar to hysteria nor to women.

Case VI.—May 31, 1831, a gentleman, æt. 45, consulted me respecting a troublesome borborygmus, accompanied with extreme depression of spirits and a sense of suffocation, which was much increased when he attempted to take food, or even to swallow liquids. He complained also of palpitation of the heart, and the choking sensation was so distressing, that he refused to attempt to swallow any thing during two days. The tongue was coated with a thick white fur, and he had other symptoms of gastric derangement excited by intemperance.

In some cases temporary tumors within the abdomen are produced by a partial distension of the colon. In these instances the irregular muscular movements. described as proceeding from convulsive contraction of the abdominal muscles, are absent. In the sixth number of Guy's Hospital Reports, p. 257, one of these hysterical tumors is described as having been mistaken for a cyst; and the mistake was not discovered until an opening was made through the parietes of the abdomen, for the purpose of its being removed.

Treatment.—When hysteria is accompanied with epilepsy or temporary hemiplegia, or other symptoms of cerebral disease, and the patient is plethoric, leeches should be applied to one of the temples, or blood should be removed by cupping between the shoulders. This combination of epilepsy with hysteria is not uncommon in young women of full habit, when menstruation is delayed beyond the usual period of its appearance; and there is danger of the periodical recurrence of the attack as long as the temporary plethora is permitted to continue. In this condition, and in this form of hysteria, in which the cerebral symptoms predominate, the exhibition of steel should be strictly prohibited.—Unfortunately the custom of administering chalybeate medicines without medical advice, and without proper discrimation of the cases adapted for their use, is often followed by confirmed epilepsy, which sometimes continues through life.

Case VII.—A young lady was attacked with a most violent fit of epilepsy, which followed a paroxysm of hysteria. She was about 17 years of age, of full habit, and had a florid complexion. I found on inquiry that she had never menstruated, and had been taking for a considerable time large doses of steel. I directed twelve leeches to be immediately applied to the temples, a purging medicine to be given, and the steel to be entirely discontinued. The cerebral symptoms were by this timely depletion removed and the menstrual secretion soon followed.

In these cases of cerebral hysteria, partaking so decidedly of the nature of epilepsy, when local bleeding is found ineffectual, much benefit may be derived from the exhibition of digitalis, in the dose of two grains twice a day, and the counter-irritation of a blister applied to the nape of the neck. The digitalis in these cases acts beneficially, as in epilepsy, by retarding the cerebral circulation.

Case VIII.—May 28th, 1847.—A young lady, æt. 16, had been afflicted, almost daily, with hysteria in various forms during the preceding six months. She was of a lymphatic temperament, and inclined to plethora, and presented the appearance of a person predisposed to epilepsy. She was subject to attacks of sudden and vehement palpitation of the heart, during which the pulsations of that organ, perceptible to the hand placed over the cardiac region, were twice as rapid as the pulse at the wrist. She was also often seized with involuntary twitching of the muscles of the upper eyelids, temporary loss of sight, globus hystericus, fits of laughing, crying, and sobbing, and paralysis of the left leg, which continued some time after the paroxysm had subsided; the menstrual discharge was regular. The head has not been observed to be drawn to one side, but the patient has often made a moaning noise, and foamed at the mouth, like an epileptic person, during the fit. She passes a quantity of pale urine after the attack.

Mr. Ince, the medical gentleman who was attending the patient, and favored me with this history, requested me to visit her in consultation with him.

I prescribed twelve leeches to the temples, and the following medicines:—℞ Extr. Belladonnæ, gr. v.; Ferri Oxydi, ʒ iss.; Extr. Gentian. ℈j. M. ft. Pil. xxiv. cap. iv. octavis horis, C. cochl. ii. largis mist. seq.; ℞ Rhei Pulv. ℈j.; Tinct. Aurant. ʒ ii; Infus. Quass. ℥ vj. M.

31st.—Mr. Ince informed me that the patient was rapidly improving.

June 4th.—She has had no return of the fits, and the double pulsation in the heart has ceased. ℞ Extr. Belladon. gr. x.; Pulv. Lini gr. x. M. ft.; Pil. xii. cap. j. octavis horis cum cochl. ii. largis seq.; ℞ Ferri Oxydi, ʒ iss.; Tinct. Aurant. ʒ ii.; Rhei, ℈j.; Infus. Quass. ℥ vj. M.

10th.—Mr. Ince being engaged, I was requested to visit the patient in his absence. I found her suffering a severe attack of cerebral hysteria, which was preceded by rigors, and partook more of the nature of epilepsy than hysteria.—Neither the spinal nor the ganglionic nerves were affected, except the former in a slight degree, in consequence of which, occasional gentle cnnvulsions were produced in the muscles of the arms, which were moved as if by a galvanic shock; the flexor muscles of the forearm being at the same time firmly contracted. The patient lay in a state of stupor and insensibility during more than an hour, now and then foaming at the mouth. The forehead felt very hot, and the face was flushed, while the extremities were cold; the eyes were closed; there was no return of palpitation of the heart.

I prescribed the following medicines, and a repetition of the leeches, which I afterwards found were not applied, on account of an insurmountable objection which the patient had to loss of blood:—Digitalis Pulv. gr. xvj.; Saponis, ℈j. M. f. Pil. xvj.; cap. ii. bis die, c. cochl. ii. largis mist. seq.; ℞ Ammon. Sesquicarb. ʒ ss.; Tinct. Aurant. ʒ iii.; Inf. Quass. ℥ viiss. M.

Mr. Ince, when he afterwards saw the patient, and was informed that she had neglected to apply the leeches, recommended the application of a blister to the nape of the neck, which had formerly afforded considerable relief, and which he directed to be kept open.

24th.—The patient has not had hysteria in any form, except slight attacks of cerebral congestion resembling epilepsy. The medicines last prescribed have been regularly continued, but the pulse is not perceptibly under the influence of the digitalis.

September 4th.—Has omitted the digitalis for some time, and, I find, has seldom any attack, and when she has, it is trifling and transitory.

When the patient is feeble and emaciated, and the countenance pale and contracted, instead of being flushed, as in the variety just described, the most safe and efficient practice is to administer an emetic as soon as the patient has recovered the faculty of deglutition. The depression of the heart's action, and the general perspiration produced by the emetic, have the effect of restoring the balance of the circulation, and relieving the brain from the temporary compression produced by its congested vessels.

The convulsions in the voluntary muscles, which are the most troublesome and alarming symptoms to the attendants, are speedily and effectually removed by the exhibition of belladonna, which has also the effect of removing the disordered function of the nerves of sensation. As this successful mode of treating hysterical convulsions is, I believe, unknown to the profession, I will adduce the following cases, which have occurred in my practice at the public institutions which I attend, in corroboration of my recommendation of the remedy.—The proper dose of the extract of belladonna, which is the preparation I use, is half a grain for an adult, which should be repeated once in eight or twelve hours, according to the severity of the convulsions.

Case IX.—Mary Ann K., æt. 16, was admitted under my care a patient at the Western Dispensary, on March 4th last, with cerebral hysteria; she had never menstruated. As she had a pale and delicate appearance, I prescribed steel, and afterwards digitalis, the attacks presenting a great resemblance to those of epilepsy; but the frequency and violence of the convulsions in the extremities and in the abdominal muscles increased under treatment; so that it became necessary for several attendants to remain with her during every night until May 7, when I commenced the exhibition of extract of belladonna, which I prescribed in the dose of half a grain once in eight hours. When I saw the patient again on the 10th, I found the pupils dilated, and that she had had no return of hysteria after she had taken the first dose of the medicine. The belladonna was continued till the 20th, when it was omitted, no symptom of the disease having returned.

Case X.—Ann B., æt. 14, was admitted under my care a patient at the Western Dispensary, on May 17th last. She had hysteria more or less every day, requiring the continued attention of her family. It appeared in the form of general convulsions, preceded by globus hystericus and convulsive respiration. As this was an unusually severe and troublesome case, I commenced the exhibition of extract of belladonna in the dose of two-thirds of a grain once in eight hours. On the 21st the belladonna was discontinued, all symptoms of the disease having subsided the day following the first exhibition of that medicine.

Case XI.—Sarah B,, æt. 25, was admitted a patient at the Western Dispensary, under my care, on May 13th. She was married, and regularly menstruated. She had been suffering every day during the preceding month with hysterical globus and convulsions. I prescribed a pill, containing half a grain of extract of belladonna, three times a day. On the 20th, finding she had had no return of the disease, I ordered the medicine to be omitted.

Case XII.—Sus. C., æt. 14, was on May 18, admitted a patient at the Western Dispensary. This was a complicated case of cerebral and spinal hysteria, with which she had been afflicted daily during the last three years. The attack began with hysterical coma, which was succeeded by convulsions, frequently interrupted by laughing, crying, and sobbing. She had never menstruated, and had an anæmic aspect. On this account, I began the treatment with sulphate of iron, in conjunction with half a grain of extract of belladonna, twice a day.

She had no return of convulsions after the second dose of the belladonna, and the pupils in a few days became fully dilated from its use.

On the 14th of June, finding that the hysterical symptoms had subsided, and that the attacks were such as I have denominated cerebral, I directed the steel and belladonna to be omitted, and two grains of digitalis to be taken night and morning. At the end of fourteen days from this time the patient was discharged cured.

Case XIII.—Mary H., æt. 18. was admitted a patient at the Western Dispensary on May 27th. She had been attacked almost every day during a considerable time with hysteria, complicated with epilepsy, denoted by coma, convulsions, and foaming at the mouth, interrupted by frequent fits of crying and sobbing. I prescribed a pill containing half a grain of extract of belladonna, to be taken every night and morning. On the 7th of June, the patient was perfectly free from hysteria and epilepsy, and on the 17th she was discharged cured.

Case XIV.—Emma R., æt. 43, was admitted a patient at the Western Dispensary on June 21st. She complained of globus hystericus, and convulsive contractions in the abdominal muscles, which she called cramps. I prescribed half a grain of extract of belladonna once in twelve hours On the 24th, the hysterical globus and muscular cramps were removed, and on July the 19th the patient was discharged cured.

I am not prepared at present to explain satisfactorily the manner in which belladonna acts on the nerves of sensation and voluntary motion proceeding from the spinal column; but I apprehend that it is by producing partial enervation, or diminished supply of nervous fluid, and consequent temporary paralysis, as in the iris.

Hysteria proceeding from disordered function in the abdominal ganglionic nerves is frequently produced by constipation, which is permitted by some indolent females in the middle and upper classes of society to proceed to such an extent as to occasion an enormous accumulation in, and distension of, the colon. The consequences of this preternatural condition of the bowel are, obstruction to the return of venous blood from the intestines, and a general congestion in the abdominal viscera. The most effectual mode of removing this stagnant collection of fæces consists in the exhibition of pills composed of extract of jalap, compound extract of colocynth, and compound rhubarb-pill. which should be regularly repeated every second day, until the contents of the colon have been completely dislodged. In these cases saline purgatives will be found of no avail, as they fail to produce that contraction in the large intestines which is indispensable to insure their perfect evacuation.

When hysteria is combined with pyrosis, the cure of the former will often be found to succeed that of the latter, which is most successfully treated by trisnitrate of bismuth in conjunction with a bitter infusion.

A state of anæmia is another very common cause of this form of hysteria, as well as of clavus hystericus, and other varieties of neuralgia and of morbid sensibility in the nerves of sensation. This condition of the blood, with which there are found a deficiency of red globules, and a corresponding enervation in the vital organs, is that which induces hysteria in males; and, by depriving the uterine vessels of their natural energy, is the cause of that species of chlorosis in females which is curable by chalybeates. Whenever neuralgia is connected with anæmia, it will be found that the latter has preceded the former, and that the most successful treatment will consist in the exhibition of some preparation of steel, which, by restoring the healthy proportions of the elements of the blood, has the effect of removing that combination of anæmia and enervation which is essential to the generation of this kind of nerve-ache. In some cases this simultaneous deficiency of red globules in the blood, and of nervous energy in the vascular system, proceeds to such an extent as to produce passive congestion in the viscera of the thorax and abdomen, and consequent effusion of serum in the cellular membrane of the face and extremities.

47, Chester Square, Sept. 13, 1847.

II.—*Poisoning by Iodine.*

At the last annual meeting of the Suffolk branch of the Provincial Medical and Surgical Association, Mr. Smith detailed some very interesting cases of the bad effects resulting where iodine had been exhibited carelessly for a lengthened period, and he believed that this iodism was frequently the cause of very serious and fatal results. In these cases there was headache, dizziness, &c., which were relieved by expelling the iodine by emetics, and giving brisk aperients.—*Prov. Med. and Sur. Journal.*

NEW ORLEANS, JANUARY 1, 1848.

The dawn of a new year we deem a fit occasion to admonish our friends that if they *really wish* a Medical Journal from the *emporium* of the South, they must lend us more efficient aid than they have hitherto done. They must *write more, pay better* and afford us *a larger subscription list.* The press of engagements at this season has caused a little delay in issuing the present number of the Journal, for which we crave the indulgence of our readers, as it does not often happen.

HEALTH OF THE CITY.

Since our last date, our city has been as healthy as usual at the season. The warm weather continued late and we did not have frost at this place until the 26th November, when ice was to be seen. After the 1st of November the people poured in from all quarters and resumed their customary avocations; yet there was no increase of yellow fever. A few cases continued to occur till late in December, but these were chiefly among the lower class of the people and such as exposed themselves to the sun. The Epidemic seemed evidently to have exhausted itself, without the intervention of frost or any remarkable vicissitude in the weather. At the equinox we had rain and a pretty smart blow for *a single day*, but it then cleared off warm and continued so, with but slight deviation, for nearly two months. Who has not observed this manner of cessation occasionally to occur in the country, when the endemic fevers have prevailed to an extraordinary extent? And does it not constantly occur within the tropics, where frost is never seen? We have yet to learn the laws which govern *the rise. progress and decline* of summer and autumnal fevers. Frost certainly puts *a check* to them, but they do decline without it; and that it does not *extinguish* them, is verified by the fact that whenever and wherever they prevail, in temperate regions, cases may be seen until late in December. This is certainly true as regards the yellow fever of New-Orleans. Nothwithstanding the severity and general prevalence of the late epidemic in this city, many who remained here all the time escaped, whilst some had it who had escaped other epidemics, and others again had it *a second or third time.* All this depends upon the exposure to and operation of the *exciting causes.* During the prevalence of an epidemic, nearly all are more or less affected, but by prudence and care, many escape open attacks. But this is not the place to enlarge upon the late epidemic; let us rather confine our remarks to the state of health since we last went to press.

As usual, the months of November and December were healthy. Notwithstanding the sudden vicissitudes of the weather, being sometimes very cold and again very warm, the customary winter diseases have only recently begun to prevail to any extent. We now have *pneumonia, catarrh, scarlatina, measles, typhoid fever &c.*; also a few cases of *small pox.* If it be true that *typhoid or ship fever* will spread by *infection*, we should not be surprised to see it prevail to a great extent in our city during the passing winter and approaching spring. We have seen several cases in private practice and heard of others. An immense number of European immigrants afflicted with this disease have arrived here, and they are still pouring in. We shall be more fortunate than the northern cities, Montreal, Quebec, New-York and Philadelphia, if we escape its ravages. No sort of precautions are taken by our municipal authorities. The immigrants are admitted at once into the heart of the city and our large Charity Hospital is now almost monopolised by them. The main building is only caculated to entertain comfortably about 450 patients, whereas the number remaining on the first of January was 828. They are so densely crowded as to preclude all possibility of *comfort*, not to speak of *justice* to the sick. The consequences must be fatal to many poor creatures who with proper attention might be restored to health and the fulfillment of the fond hopes which caused them to relinquish their native land. Many of them are almost in a state of starvation when they arrive, and it may readily be conceived how unsuitable an asylum for recuperating their energies, is a crowded hospital with its polluted atmosphere.

We have had a good deal of rain, but nothing to compare with what has fallen in the western country above us. We understand there has been an extraordinary overflow in the Ohio and its tributaries. The Mississippi is at present high for the season and constantly rising.

YELLOW FEVER.

The late Epidemic was probably the most extensive that ever prevailed in this city. As to its severity and mortality, there may be difference of opinion. It is impossible to make a correct computation of the whole number of cases. Some have estimated it as high as *twenty* or *twenty five thousand*, but we are inclined to think either of these calculations above the truth. As to the mortality, the reports from the cemeteries, as well as they could be obtained by the Board of Health, only make out something upwards of 2300 from Yellow Fever; but this again is thought by many to be far short of the reality. It is much to be regretted that we cannot obtain greater precision in such important details. The fever raged as an epidemic about two months, and the greatest mortality from it was in September, when the number of deaths reported to the Board of Health, amounted to 1044. During the prepalence of yellow fever in this city, the most frightful and exaggerated reports circulated abroad, but we shall really never get at the truth, unless greater efforts be made than any hitherto exercised. A careful collection of all the important facts connected with the rise, progress and decline of the late epidemic, would form an exceedingly interesting memoir, and we have it in contemplation to undertake the task, but it must necessarily require a good deal of time and labour. On the other hand, it would seem to be almost useless to write any thing more on the subject of yellow fever. We really believe that the archives of the Profession already contain every thing that need be said on the subject; and yet the world is but little wiser in regard to it than it was half a century ago. Every debateable question which was then discussed, in relation to its *cause*, *nature*, *propagation* and *treatment*, still remains undecided; and, with the exception of a single point (*black vomit*,) there seems to be as great a diversity of opinion at this moment, as at any previous period. We believe the physicians of New-Orleans have completely settled the point, that *black vomit is a hemorrhage from the stomach*; but doubtless there are some who do not even admit this. Medical are very much like religious controversies: in either case, when men have formed and *expressed* opinions, they seem to shut their eyes against all farther light, and hold on to them with like pertinacity. Then why write any thing more upon a subject which has been so fully and so ably discussed as Yellow Fever? It would certainly be vain to do so, *with a view to enlighten or change any who have formed and expressed opinions*. But is it not our duty to transmit our observations to those who are to come after us? There is scarcely *any point* on which *some of us* are not probably right, and many wrong. With *our* assistonce, our successors may fortunately discover the truth on all points; but if *they* have to begin their investigations *de novo*, the same controversies must continue to arise and be endless. They are certainly entitled to the benefit of our observations; and some masterly mind, yet unborn, may analyze them thoroughly, winnow the grain from the chaff, and establish the truth by facts and logic.

We have ourselves seen yellow fever on *five different years*, to some extent in all its phases, and have used our best endeavours to study it with an unbiassed mind. We have finally been brought to conclusions, *by no means novel*, yet at variance with many possessing far more ability and who have had a much larger experience. When we proclaim our convictions, (if we ever venture to do so,) we shall be acquitted of the charge of *presumption*, if it appear they only corroborate some of the ablest authorities in the profession.

We will here simply state a few facts relating to the late Epidemic, which we think can be fully substantiated.

1. In the spring of the year, Intermittent fever prevailed to a great extent.

2. As the season advanced, frequent cases of mild Remittent Fever were to be seen among the Intermittents.

3. Soon after the first of July, severe Remittents became common; some of them terminating in hemorrhages and black vomit. *Yellow Fever* was now announced.

4. The Yellow Fever raged in Vera Cruz in May and June; the intercourse between this city and that, at the time, was very great; hundreds of discharged soldiers were returning from Vera Cruz to this place, some of them convalescents from Yellow Fever, but *hardly any labouring under it*.

5. On the 22d of June a man, recently from Vera Cruz, died of *black vomit*, at a hotel in Lafayette. He occupied a large room with some

ten or fifteen other persons. None of these are known to have been sick soon afterwards.

6. The next death from black vomit in Lafayette, occurred about the 18th July; four or five squares distant from the first.

7. The first death from black vomit in New-Orleans, was in an Irish ditcher, at work in the rear of the city, near the Charity Hospital. This occurred about the 1st of July.

8. The first death from the same that occurred at the Charity Hospital, was on the 6th of July.

9. The next ten deaths at the same hospital were from different parts of the city, showing no other connection between each other, except that a man and his wife were sick in the same house.

10. August came—deaths with black vomit were frequent and yellow fever was pronounced *Epidemic.*

11. At this time all the forms of summer fevers might be seen: *Intermittents, mild Remittents, severe Remittents, Congestive and Yellow.* Remittent bilious and Yellow Fever were predominent.

12. All the milder attacks of intermittent and remittent fever, if neglected or maltreated, assumed the appearance of what is called *Yellow fever*—especially if they terminated *fatally.*

13. On the other hand, plain cases of yellow fever, if not promptly cured, sometimes tapered off into intermittent fever, and then recovered. We saw one instance of this.

14. In the early stage of mild attacks of fever, no one would pronounce a *definite diagnosis:* if promptly relieved, it was *not yellow fever:* if it terminated *fatally*, all doubt was removed.

15. At this very time there were cases of what is called *Congestive fever*, which terminated fatally in the first or second chill.

16. September came—and the Epidemic raged with its greatest violence, affecting to a greater or less extent *all ages, sexes, castes and conditions, natives and foreigners.* Yet *all* were not sick: many escaped who had never had the fever, and some who had never spent a summer here before.

17. Many *creoles or natives* had it, both white and colored, though the disease was evidently milder with these and with unacclimated negroes.

18. A number of persons had the fever for the second and third time, and some had it who had escaped several previous epidemics. One of the worst cases we saw was an intelligent Irishman who said he had nearly died of it in 1825, and had lived here ever since, excepting 17 months spent in Mobile. He recovered, after having hemorrhage from the mouth and turning very yellow.

19. Recent settlers in the city, especially the poorer class, evidently suffered the most; above all, those who were much exposed to the sun.

20. The fever prevailed throughout the intire limits of the city and Lafayette, which lies on the river, immediately above; and to the greatest extent, wherever were to be found the greatest number of poor and unacclimated people. The extent and violence of the disease seemed be governed more by the last mentioned fact, than by any thing else pertaining to the different localities.

21. Many persons who left the city late in the summer, were attacked

on their journey; and some who came in late were attacked within a very short time after their arrival, especially if much exposed to the sun.

22. October came—and on its approach, what is callod *yellow fever* most rapidly declined; the monthly number of deaths having fallen from 1044 down to 198. The comparative number of Intermittents now began to increase again, and when *neglected or maltreated*, many of them ran into *yellow fever:* if promptly relieved, of course they were but *simple intermittents.*

23. During the month of October the weather was mild, fair and beautiful, hardly approaching *within* 20 *degrees of frost.*

24. November came—and with it our travelling citizens, together with the vast number of persons who annually come to New-Orleans to do business in the winter season. The papers had announced that the Epidemic had disappeared and every body returned to their homes and avocations. But yellow fever was not yet extinct: sporadic cases were to be seen in different parts of the city. The deaths from it reported for this month were 12. The weather continued so warm for the first half of the month as to render fires almost useless. The city was so rapidly filled as to give rise to some apprehension that the epidemic would be renewed; but such was not the case. It had run its career and was gradually dying away.

25. During this month European emigrants commenced pouring into our city, bringing with them their fatal companion *Typhoid or ship fever.* It was most curious to observe with what facility the subjects of this fever assumed the characteristic symptoms of yellow fever after entering the Mississippi river. Some who had escaped sickness during the long voyage by sea, were attacked soon after entering the city and going into hospital, in seven or eight days presented hemorrhage from the gums and yellowness of the skin. One or two cases terminated fatally with black-vomit. Through the politeness of Dr. Ker, we were shown several of these cases at the Marine Hospital.

26. On the 25th of November the weather suddenly turned very cold, and on the 26th and 27th there was ice in the gutters. On the 28th white frost.

27. The last death from yellow fever was reported in the weekly Report of the Board of Health for the 25th December. There were ten deaths from Yellow fever in this month.

28. Yellow fever prevailed this year at the following places, viz: Mobile, Galveston, Alexandria on Red river, Vicksburg and Rodney on the Mississippi river, all having constant communication with New Orleans. There were also some cases at Baton Rouge and Plaquemine; whereas Natchez, Grand Gulf, and we believe, St. Francisville escaped.

29. Quarantine was maintained at Natchez and Vicksburg. The former escaped and the latter had the fever. Whereas, of the intermediate towns between those places, Rodney, next to Natchez, was severely scourged, whilst Grand Gulf, next to Vicksburg—escaped. We have addressed a *circular* to physicians residing in each of these places, and hope to obtain from them some interesting intelligence relative to the matter.

Such are some of the prominent facts concerning the late epidemic, which we think can be fully substantiated by good evidence. There are many others of great importance which a minute history of it would display in their proper light. The whole would give rise to deductions which would probably vary according to the diversity of intellect by which they were examined.

F.

MORTALITY IN THE CITY OF NEW ORLEANS
FOR THE YEAR 1847.

The Secretary of the Board of Health, Dr. Hester, has furnished the following catalogue of interments and causes of death, taken from the books kept at the different cemetries by the Sextons. A perusal of it will show the want of improvement in the nomenclature of diseases amongst our physicians. We fear the Report of the National Medical Association on this subject has not met the eyes of many of them. The year was unusually sickly and the mortality has been very great; but it must be recollected all these deaths did not occur among the inhabitants of New Orleans. A very large number was furnished by soldiers going to and returning from the Mexican war, and perhaps as many by the immense influx of European Emigrants, many of whom barely reached here in time to be burried. Thus it is impossible to estimate the mortality proper to the city. We give the Report as it has been handed to us. It includes the interments from all the Hospitals.

List of Interments in the City of New Orleans from 19th Dec. 1846 *to the* 18*th Dec.* 1847, *being twelve months or* 52 *weeks.*

Abortion 1, Abscess 2, Accidental 6, Accouchement 3, Anasarca 3, Anemia 13, Aneurism 3, Angina Maligna 3, Angina Tonsillaris 1, Aorta, Aneurism of 1, Apoplexy 71, do. pulmonary 1, Arachnitis 1, Arthritis 1, Ascites 10, Asphyxia 4, Asthma 8, Atrophia 3, Bladder, inflammation of 3, do., paralysis of 1, Bowels, hemorrhage from 2, do., inflammation of 30, do., ulceration of 12, Brain, compression on 2, do., concussion of 5, do., congestion of 66, do., disease of 4, do., dropsy on 1, do., effusion on 5, do., softening of 5, Breast, abscess in 1, do., carcinoma of 1, Bronchitis 39, do. chronic 4, Burn 11, Cancer 1, Catarrh 41, do. chronic 8, do. pulmonary 10, Cerebral hemorrhage 3, Cerebritis 57, Chlorosis 2, Cholera 1, do. infantum 31, do. morbus 4, Cholerine 1, Chorea 1, Colic 10, Colitis 15, Congestion 7, do. pulmonary 2, Consumption 572, Contusion 8, Convulsions 168, Coxalgia 2, Cramp 7, Croup 48, Cynanche 6, do. Trachealis 6, Cystetis 1, Debility 196, Delirium 2, do. Tremens 75, Diptharitis 1, Dentition 96, Diarrhœa 114, do. acute 1, do. chronic 77, do. gastro-hepatitis 1, do. typhoid 5, Diaphragmatis 1, Disease chronic 17, do. inflammatory 5, Dothinenteris 1, Dropsy 79, Drowned 89, Dysentery 349, do. acute 7, do. chronic 90, do. malig. 2, do. tubercular 1, Eclampsia 1, Emphysema Pulmonary 1, Endocarditis 1, Encephalitis 14, Enteritis 69, do. acute 3, do. chronic 31, Entero-Cephalitis 3, Entero-Colitis 4, do. do. chronic 1, Entero-Gastritis 5, Epilepsy 8, Erysipelas 5, Face, cancer of 1, Fever 36, do. Adynamic 2, do. Ataxic 1, do. Bilious 15, do.

Bilious remittent 1, do. congestive 43, do. Idiopathic 1, do. Gastric 1, do. Hectic 2, do. Icterodes 6, do. Interm. 26, do. Interm. pernicious 13, do. Intestinal 1, do. Malignant 5, do. Malignant putrid 5, do. Nervous 27, do. Pernicious 22, do. Pernicious congestive 1, do. Puerperal 3, do. Putrid 4, do. Remit. 17, do. Scarlet 12, do. Scarlet malig. 3, do. Traumatic 1, do. Typhoid 226, do. Typhoid congest. 7, do. Typhus 152, do. Yellow 2306, Flaggelation excessive 1, Gangrene 14, do. Senilis 1, Gastritis 19, do. Acute 1, do. Chronic 12, Gastro-Duodenitis 7, Gastro-Encephalitis 3, Gastro-Enteritis 103, do. Acute 1, do. Chronic 15, Gastro-Enteralgia 1, Gastro-Hepatitis 11, do. Chronic 2, Gastro-Pneumonitis 1, Head, injury of 6, Heart, aneurism of 3, do., disease of 14, do., hypertrophy of 18, do., wound of 1, Hemorrhagia 7, Hemorrhage, gastro intestine from 1, do. Puerpera 1, Hepatic Abscess 2, do. Phthisis 2, Hepatitis 24, do. Acute 5, do. Chronic 6, Hepato-Gastritis 2, Hematemesis 2, Hernia 1, Humerus, fracture of 1, Hunger 1, Hydrocele 1, Hydrocephalus 20, do. Acute 1, do. Chronic 1, Hydropsia 13, Hydro-Pericarditis 1, Hydro-Thorax 7, Hypertrophy 4, Icterus 1, Inanition 1, Indigestion 2, Influenza 1, Intemperance 8, Intestines, perforation of 1, do., chronic inflammation of 1, do., rupture of 1, do., softening of 3, Jaundice 6, Laryngitis 8, do. œdematus 1, Leg, amputation of 2, do., fracture of 1, Leucophlegmasia 1, Liver, abscess in 7, do., cancer of 1, do., disease of 1, do., inflammation of 1, Lungs, apoplexy of 1, do., congestive of 4, do., gangrene of 5, do., hemorrhage from 3, do., inflammation of 14, Marasmus 46, Measles 38, Meningitis 45, do. Acute 1, do. Chronic 1, Mesenterica 2, Metorrhagia 2, Metro-Peritonitis 5, Myelitis 4, do. Chronic 1, Neck, abscess in 1, Œdema 1, Old age* 35, Paralysis 5, Parturition 1. Pericarditis 1, Peritonitis 14, do. Chronic 2, do. Puerperal 1, Purtussis 22, Phrenitis 4, Pleuritis 16, Pleurodynia 1, Pleuro-Pneumonia 16, do. Chronic 2, Pneumonia 64, do. Ataxic 1. do. Chronic 5, do. Intermittent 1, do. Typhoid 12, Poisoned 4, Puerperal Mania 1, Purpura Hemorrhagica 1, Rectum, cancer of 1, Rheumatism 8, do. inflammatory 1, Ribs, fracture of 1, Scald 5, Scorbutis 4, Scrotum, gangrene of 2, Scrofula 12, Sinile Catarrh 2, Skull, fracture of 18, Small Pox 27, Spasms 8, Spinal Arachnitis 1, Spine, fracture of 4, Spinal Marrow, softening of 1, Spina Bifida 1, Stomach, cancer of 3, Still Born 196, Stomach, congestion of 1, do., Cramp of 4, do., Schirrus of 2, Strangulation (hanging) 1, Stricture 1, Sun Stroke 6, Syphilis 3, Syncope 1, Tetanus 69, do. Idiopathic 2, Tabes Mesenterica 2, Tibia, fracture of 1, Traumatic tetanus 3, Trismus Nascentium 78, Tumor abdominal 1, Typhus Icterodes 1, Uncertain 577, Urethra, stricture of 1, Uterus, cancer of 2, do., disease of 1, do., ulceration of 2, Verminous affection 12, Vertrebra, fracture of 1, do., injury of 1, Womb, cancer of 3, Wound, contused 1, do., Gun-Shot 7, do. Incised 1, do. penetrating 9. Total 7499.

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* One, an African, 112 years old. Three of 100 years old each. One 90 years old.

Under 10 years of age,	1750
Over " " "	5304
Age not specified,	443
Whites,	6274
Coloured,	1220
Mortality in each month—	
From 19th December to 19th January,	408
" " February,	367
" " March,	292
" " April,	327
" " May,	427
" " June,	626
" " July,	566
" " August,	1292
" " September,	1771
" " October,	590
" " November,	396
" " December,	426

HEALTH OF THE COUNTRY.

Our attentive correspondents at Montgomery and Pattersonville will please accept our thanks for their letters, concerning the health of the country. A correspondent at Mobile has sent us an interesting communication concerning the late Epidemic at that place, which we shall reserve for our next number. So far as we have been able to learn, the lower portions of the country suffered generally from sickness during the last autumn, whilst the upper or hilly regions escaped. This in accordance with general observation. We do not hear of much sickness any where in the interior at present.

MONTGOMERY, ALA., December 15th, 1847.

GENTLEMEN:—For the two months from the 10th of October to the 9th of December inclusive, our case books present the following list:

Asthma, 1; Aptha, 2; Anus (prolapsus of) 1; " (fistula of) 1; " (fissure of) 1; abscess, 2; bronchitis (acute) 4; " (chronic) 1; Bubo-(sympathetic) 1; chorea, 1; croup, 1; cholera-morbus, 10; " infant. 1; colic, 7; cataract, 5; chilblain, 1; convulsions, infantile, 2; dropsy, 3; dyspepsia, 2; diarrhœa, (acute) 14; " (chronic) 2; difficult dentition, 1; dysentery, 3; delirium tremens, 4; dysmenorrhœa, 3; enteritis (acute) 3; epilepsy, 1; eczema, 2; epistaxis, 1; fracture (simple of thigh) 1; " (of radius) 2; " (of leg) 1; foreign body—in ear, 1; fever (intermittent) 104; " (remit.) 41; " " (pernicious) 3; " (infantile) 5; gonorrhœa, 3; gastritis (acute) 1; gastro-enteritis, 1; hemiplegia, 1; hemorrhoids, 1; hæmoptysis, 1; hernia, (strangulated inguinal) 1; hysteria, 3; leucorrhœa, 1; laryngitis, (acute) 1; intoxication, 1; influenza, 3; jaundice, 8; mamma, (inflammation of) 1; meningitis, 1; menorrhagia, 2; neuralgia, 16; otitis, 1; opthalmia, 4; orchitis, 3; paronychia, 1; phthisis pulmonalis, 1; pneumonia, (acute) 8; " (chronic) 1; parturition, (natural) 3; " (terious) 1; Phymosis, 1; rheumatism, (acute) 2; retroversion of uterus, 1; suppression of men ses, 1; stricture of urethra, 1; spinal irritation, 4; strabismus, 1; syphilis, (primary) 2; " (secondary) 2; " (tetiary) 1; sprain, 3; spleen, (chronic engorgement of) 2; spematorrhœa, 1; stomatitis, 1; tonsilitis, 5; trismus nascent, 1; ulcers, 6; urticaria, 1; worms, 1; wounds, (lacerated) 1; " (contused) 4; " (incised) 5; " (punctured) 2; " (gun-shot) 2.

There were, it will be seen, in all 361 cases—and 12 deaths: One from cholera infantum; One from Infantile convulsions; One from chronic Diarrhœa; One from Delirium Tremens; Two from acute Enteritis; One from acute gastritis; Two from acute Laryngitis; One from Meningitis, and two from acute pneumonia.

W. M. B.

PATTERSONVILLE, LA., December 17, 1847.

GENTLEMEN—Since my note to you of October 19th, the sickness in this section has pretty steadily declined. With the exception of a few chronic visceral diseases and some relapses of Intermittent Fever, the health of this community is now, and has been for some time, almost uninterruptedly good. I have had under treatment, since the winter set in, one case of Pneumonia, and also several surgical cases. In one, the scalp was extensively torn and injured by being thrown from a cart and struck with one of its wheels while rapidly revolving; another, in which the hand was caught with the cogs of the rollers of a sugar mill, and mashed and lacerated; and the third in which the hand and arm were entangled in the rollers of a sugar mill and crushed to pieces. In the second case I amputated the index and middle fingers with their matacarpal bones, and succeeded in saving the thumb and last two fingers—and in the third case, amputated the arm above the elbow, which also terminated favourably.

The weather is now very cold for this latitude; the thermometer has stood (early in the morning) as low as 28°. We have had considerable ice, and the sugar crops are cut considerably short.

Respectfully,

R. H. D.

HOSPITAL REPORTS.

CHARITY HOSPITAL.

This Institution has been crowded with patients from the beginning of the year to the end. The extraordinary number has been made up by Foreign Immigrants, United States soldiers and the epidemic of yellow fever. The following is the annual amount of admissions, discharges and deaths. The list of diseases will be furnished in our next number.

Annual Report for 1847.

MAIN BUILDING.

Admitted:	11,690.	Died:	2,037.
Discharged:	9,369.	Remaining on the 1st January, 1848.	828.

LUNATIC ASYLUM.

Admitted : . .	678	Died : . . .	85
Discharged : . .	541	Remaining on the 1st January, 1848. .	125

These are the largest figures ever seen on the books of the Charity Hospital. No hospital should be permitted to take in double the number of patients it can entertain comfortably, for it cannot be done with justice to the sick. The wards of the Charity are now literally crammed. A row of beds extends all round the walls and another in the middle of the floor. Many patients have to sit up all day and lie upon pallets at night. The whole house is infected with Typhoid fever, thus rendering it dangerous for patients labouring under other diseases to go there.—The house students, nurses and sisters of charity are suffering from the disease. Diarrhœa and dysentery are also common and there are seven or eight cases of small-pox in the adjoining Lunatic Asylum. But one thing is lacking to complete the catalogue of miseries—erysipilas will probable break out before long, and then the cup will be full. We deem it our duty to record these melancholy facts to make up the medical history of the day. When evils become glaring and outrageous, they enforce reform, and often not before.

Dr. Sims *on Trismus Nascentium.*

Dr. J. Marion Sims, of Montgomery, Alabama, having recently visited our city, was invited by the Physico-Medical Society to read a paper on the above subject. This he did at the Medical College, on the evening of the 29th December, before the members of the Society and of the Medical Class. It will be recollected that Dr. Sims published a paper on *Trismus Nascentium* in the *American Journal of the Medical Sciences* for April 1846, in which he maintained that the disease was caused by *a displacement of the os occipitis,* whereby compression was made upon the cerebellum, medulla oblongata and the important nerves originating from it. This displacement he believed proceeded from the careless habit of suffering young infants to lie too much upon their backs, and he suggested the simple remedy of placing them on their sides and letting them rest upon soft feather pillows. Since that time, Dr. S. has closely studied the subject, and although he has discovered the fallacy of some of the views which he then entertained, still he is satisfied that *the leading idea* is correct, and the object of this paper is to substantiate it, which he does by numerous, carefully observed facts and most plausible deductions. One of the errors which Dr. S. says he once entertained was, that the displacement was attributable to *imperfect* ossification of the occipital bone ; whereas he is now convinced that it is more likely to occur where ossification is *unusually advanced.* He says, that in intra-uterine life or before parturition, the os occipitis lies *beneath* the parietal bones, but that immediately after birth the occiput should bulge out and its superior edge rest *upon* the border of the parietal bones. Unless this takes place, a more or less dangerous compression upon the soft parts mentioned, will soon be produced. In most cases, if the proper position of the infant be attended to, nature will correct the evil; but it occasionally happens that surgical aid will be required to liberate the confined and misplaced bone, as in the case mentioned by Dr. Harrison in a previous number of our Journal. Dr. Sims has elevated the depressed occiput with an instrument something like an awl, upon one or two occasions, with success. He gave the details of some exceedingly interesting cases which he had saved by his method of treatment, and which strongly corroborate the views he entertains. Cases of *Trismus* present different degrees of severity, some terminating fatally in a few hours, and others continuing for several weeks; but under all its varied forms, Dr. S. thinks he has discovered one invariable diagnostic symptom, viz. *the inability to suck the breast.* This symptom he has *never seen wanting in a single case*, and it has often served to determine the existence of the disease, where the other symptoms left room for much doubt as to the true nature of the case.

A more extended experience has convinced Dr. Sims that the disease does not arise exclusively from a depression of the occiput; he has seen cases, where it arose from a depression of *the parietal bones.* The position of the child in this case should be different from the preceding. We confess our astonishment at the promptness with which, according to Dr. Sims, relief is afforded in many cases, simply by placing the child in the proper position. He has seen evident improvement in half an hour or less, and complete relief afforded in the course of a few hours. He says that nothing is necessary in cases of occipital depression, but to place the child upon its side, so that the head may rest fairly on the temporal bone. Not *partially inclined*, so that the weight of the head will rest on the parietal protuberance, as is too often done by mothers and others, who will assert that the child has been *constantly laid upon its side*, but *flat upon the side of the head*, when there will naturally be a slight inclination downwards and forwards. Dr. Sims asserts that the cradles in ordinary use among negroes and the poor are *abominable contrivances*—in fact, mere *troughs* in which it is *almost impossible* to lay an infant properly during the first days of its life. They are usually so deep and narrow, that if a child should be placed in one of them with its head sufficiently inclined forwards, it would be suffocated. The pillow should be of soft feathers, and beaten up so as to be thickest in the middle. In cases of parietal depression, the child should be kept almost erect on the back, or held over on the forehead. Dr. Sims recommends no medicine in the treatment of the complaint. All the distressing symptoms, such as insomnia, borborigmi, griping diarrhœa, tonic spasms etc., disappear as soon as the *punctum saliens*, the brain, is relieved. He thinks that all the recoveries from this usually fatal disease have been entirely *accidental*—in the management of them the child *happened* to be placed in the right position, to allow nature to rectify the evil. He is convinced of this in regard to the recommendation of Dr. Eberle, to apply *a blister to the nucha*, for then the child must necessarily be placed on the side. And as to a successful case mentioned by Dr. Stone in his lecture to the Medical Class, which followed the application of sweet oil all over the body, at the request of an old woman, he is equally convinced that the good result is more fairly attributable to the change of position accidentally made, than to any virtue of the oil. Dr. Sims spoke of certain infantile affections, which he calls *Trismoid*, because they resemble true *trismus nascentium* in many particulars, but lack the grand diagnostic symptom, the total inability to suck the breast. Dr. Sims alluded to a certain peculiarity of *Trismus*, which he was pleased to hear mentioned by Dr. Stone in his recent lecture to the class, viz. a tendency to observe something like a *hebdomidal periodicity*. He had witnessed this himself in several instances. If the disease did not terminate soon after the attack, the *crisis*, either favorable or not, was apt to occur on one of the succeeding 7th days.

As to the reputed frequency of the disease in Southern countries, Dr. S. contends that we are in want of farther and more careful observations, as well to settle *the existence of the fact*, as the *malign influence* of the climate. He thinks, it will be found on careful investigation, that its frequency depends more upon the improper management of children, than on the climate or anything else. By reference to "Curling on Tetanus," the best work extant on the subject, it will be seen that the disease has prevailed to a great extent among children in *a northern latitude*. As to the frequent occurrence of the disease in certain localities, on particular plantations in the South, for instance, Dr. S. thinks that more careful observation is demanded. He knows of two large plantations in the same neighborhood, on one of which the disease is very common, insomuch that within the last ten years, *fifty* negro children have been lost from it; whilst on the other it is *equally rare*. He is satisfied that in these instances, the different results depend on the different degree of care and attention paid by the owners to their negro children. As to the comparative frequency of this disease in our Northern and Southern States, whatever difference may exist, Dr. S. attributes to the character of the respective populations. He is inclined to think, however, that it is far more common at the North than is generally admitted. He believes that many of the deaths in early infancy attributed so vaguely in their bills of mortality to *convulsions, spasms, infantile complaint etc.*, are really caused by the disease under consideration. His paper will be published in the *American Journal of the Medical Sciences*, and we hope it will attract the special attention of northern physicians.

We have thus given an imperfect sketch of some of the leading views of Dr. Sims on this interesting subject. His paper was listened to with marked attention and called forth the encomiums of all who heard it. He professed to give nothing but a series of *facts, with his own deductions from them*, and so *striking* were his facts and so *reasonable* his deductions, that he made a decided impression upon the minds of many who had not thought so favorably of his first communication. He is a physician of some twelve years' experience, possessing fine talents and a happy delivery. He stands high in Alabama as an operative surgeon, and the case of resection of the lower jaw, published in a former number of our Journal, certainly affords good evidence of his merit. We look upon him as one of the most promising young physicians of the South, and sincerely hope he may be spared many years to prosecute the objects of his laudable ambition. F.

MOREHEAD'S ELECTRO-GALVANIC MAHCINE.

This is one of the most convenient apparatuses for the application of the valuable remedial agent Electro-Galvanism that has ever been invented. The remedy is one of the most useful we possess in many neuralgic and other affections, and should be at the command of every practising physician. It is contained in a small portable box and requires nothing but a solution of sulphate of copper, which can be prepared in a few minutes, to have it ready for use. Messrs. Guion & Co., corner of Canal and St. Charles Sts., keep them for sale at a moderate price and will fill all orders at the shortest notice. We would recommend our medical friends to purchase the article.

VACCINATION.

Dr. A. Mercier, 112 Customhouse St., requests us to say that he keeps a constant supply of fresh and genuine vaccine matter, and that he would be pleased to fill all orders from the interior. Dr. M. is one of the most respectable physicians of our city and can be fully relied on. We are very glad he has taken it in hand, as we have more or less of small-pox here every winter, and it is not less important that the community should avail themselves of this *invaluable protective* than to know where they may obtain *the genuine vaccine virus.*

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1847.

By D. T. LILLIE, at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1847.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — *Inches.*
	Max.	Min.	Range.	Max.	Min.	Range.				
Oct. - 30	70.5	56.0	14.5	30.38	30.00	0.38	N.W.	3	0	0.000
Nov. - 6	80.7	61.0	19.7	30.19	29.96	0.23	S.	$2\frac{3}{4}$	0	0.000
" - 13	83.0	62.5	20.5	30.32	29.96	0.36	S.E.	$3\frac{1}{2}$	1	0.180
" - 20	78.0	42.0	36.0	30.35	30.12	0.23	N.E.	3	1	1.600
" - 27	70.0	29.0	41.0	30.50	29.91	0.59	N.W.	$3\frac{1}{2}$	2	3.000
Dec. - 4	59.5	37.0	22.5	30.44	29.88	0.56	N.E.	$3\frac{1}{4}$	1	2.750
" - 11	76.0	43.0	33.0	30.35	29.97	0.38	S.E.	$2\frac{3}{4}$	3	4.375
" - 19	74.5	36.0	38.5	30.41	29.99	0.42	N.W.	$3\frac{1}{4}$	1	1.250
" - 25	61.5	32.0	29.5	30.36	29.97	0.39	N.W.	3	1	3.750
1848.–Jan. 1	71.0	38.0	33.0	30.36	30.11	0.25	S.E.	$3\frac{1}{4}$	2	2.881

Remarks.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

OBITUARY.

It is with the most poignant regret that we have to announce the loss of our late estimable *confrère* Dr. James B. Slade, of this city. In May last he joined the army in Mexico, as Surgeon to the 15th Regt. U. S. Infantry, and after sharing the dangers of the bloody battles immediately preceding the downfall of the Capitol, in which he acted a conspicuous part, and commanding *the gratitude and applause* of all the officers and men who came under his charge, he fell a victim to Typhoid Fever in the City of Mexico, on the 30th of November, in the 45th year of his age. He was a native of North Carolina, and practised his profession there a few years. In 1836 he emigrated to Madison County, Mississippi, where he did an extensive practice and attained a high standing. Wishing to find a field more congenial to his taste and ambition than could be afforded by a country location, he settled himself in this city in Dec. 1841. Whilst he lived here, his course was marked by such uniform dignity, urbanity and devotion to his profession, that he commanded the respect and esteem of all who knew him, and would doubtless have become one of the first practitioners of this city. In the Spring of 1846, when Louisiana was called upon to despatch Volunteers as quick as possible to the rescue of Gen. Taylor, on the Rio Grande, Dr. Slade was among the first to offer his services in a professional capacity. He was appointed Surgeon to the 2nd Regt. Louisiana Volunteers, under Col. Davis, with which he went out and returned with much reluctance after the brief term of three months. Having a natural fondness for military life, he eagerly embraced the first opportunity that offered to enter the army again. His real worth was soon discovered, as well by the officers and soldiers of the line, as by those of the Medical Staff. We heard the Surgeon General, on his recent return from the city of Mexico, pronounce him *one of the best of all the new appointments.* We have other good evidence that he was considered one of the most efficient surgeons in the army. In Major General Pillow's Report of the action at *Contreras*, Dr. Slade is signalized as having rendered most important services, not only by the bold and prompt discharge of his official duties, but by encouraging the men to do theirs. His health was at that time quite bad, but such was his ambition, energy and devotion to the service, that nothing short of severe illness could prevent him from the discharge of his duty. We heard Gen. Pierce, Col. Trousdale and other officers, recently from Mexico, speak of him in the highest terms and express deep regret at his loss. He had treated the wounds of the officers named. We understand he was sick with typhoid fever several weeks and was thought to be convalescent, when a sudden relapse took him off in a few days. He was laid in *a temporary resting place* in the City of Mexico with military honors, and his remains have been since forwarded to this city, but have not yet arrived. Dr. S. was one of the Vice-Presidents of the La. Medico-Chirurgical Society and a Surgeon elect to the Charity Hospital, when he left here. In the death of Dr. Slade, our country and the Medical Profession have alike sustained a heavy loss, for to the *one* he was a faithful servant, and to the *other* an enthusiastic devotee.

F.

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE

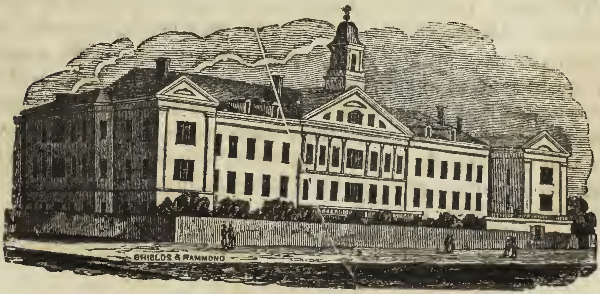
AND

THE COLLATERAL SCIENCES.

EDITED BY

J. HARRISON, M. D.
W. M. CARPENTER, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

MARCH, 1848.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1848.

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☞ The above constitute at present the only authorized Agents for this Journal. All monies or remittances may be paid or sent to either of the above as may be preferred—and to them only. We mention this fact for the benefit of our Subscribers many of whom have sent monies to other persons not authorized to receive the same and in many cases the amounts have failed to reach their destination. We therefore repeat that we will consider no payment as valid, should it fail to reach us, if addressed to any other persons than those mentioned above.

New Orleans, May 1, 1847.

JOSEPH COHN, PRINTER, 31 POYDRAS STREET.

PUBLISHER'S NOTICE.

We desire to express our acknowledgements to those of our subscribers who have so liberally responded to our call in the last number and remitted us the amount of their dues, and at the same time once more to urge upon those who still remain in our debt the importance of their attending to our claims, the want of which subjects us to much inconvenience. We repeat therefore that unless we soon receive from them some substantial proof of their interest in the continuation of the work—we shall (although with great reluctance) be obliged to suspend them from our list.

S. WOODALL,
49 Camp Street.

CONTENTS

OF

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

VOL. IV. NO. V. — FOR MARCH, 1848.

PART FIRST.

ORIGINAL COMMUNICATIONS.

PAGE

ART. I.—Practical Illustrations of Uterine Disease. By H. J. HOLMES, M. D., of Hinds Co., Mississippi. - - - - - - - - 549

ART. II.—Report of several Cases of Scarlet Fever. By F. DOWNER, M. D., of New Orleans. - - - - - - - - - 560

ART. III.—Yellow Fever concentrated with Bilious Fever—Reasons for believing it a disease sui generis—Its mode of Propagation—Remote Cause—Probable insect or animalcular origin, &c. By JOSIAH C. NOTT, M. D., Mobile, Alabama. - - - - - - - - 563

ART. IV.—Reply to Doctor W. M. Boling's review of Doctor Lewis' Medical History of Alabama, with some new facts and remarks, in relation to the diagnosis and identity of the fevers of the State. By. P. H. LEWIS, M. D., of Mobile. - - - - - - - - - - 601

PART SECOND.

REVIEWS AND NOTICES OF NEW WORKS.

ART. I.—On Poisons, in relation to Medical Jurisprudence and Medicine. By ALFRED S. TAYLOR, F. R. S., Lecturer on Medical Jurisprudence and Chemistry, in Guy's Hospital, and author of "Medical Jurisprudence." Edited with notes and additions by R. E. GRIFFITH, M. D., &c. Philadelphia. Lea & Blanchard. 1848. pp. 670. - - 641

ART. II.—Introductory Lectures. - - - - - - - - - 641

Contents.

PAGE

PART THIRD.

EXCERPTA.

I.—A Course of Lectures on the Physical Phenomena of Living Bodies. Delivered in the university of Pisa. By Professor Matteucci, F. R. S. Translated for The Lancet, by S. J. Goodfellow, M. D., Lond., late Physician to the Cumberland Infirmary - - - - - - - 643

PART FOURTH.

MEDICAL INTELLIGENCE.

FOREIGN.

Art. I.—Influence of Electricity in the Production of Diseases. By M. Pallas, Principal Physician in Algeria - - - - - - - 666

Art. II.—Painful Crepitation of the Tendons. By M. Velpeau - - 667

Art. III.—On Abscess of the Breast. By M. Velpeau - - - 667

Art. IV.—On the Nature of the Liquid secreted by the Mucous Membrane of the Intestines in Cholera. By M. Andral - - - - 668

Art. V.—On the Treatment of Typhoid or Entero-Mesenteric Fever, by the Black Sulphuret of Mercury. By M. Serres - - - - 669

Art. VI.—Treatment of Dropsy after Scarlatina. By Edward Charlton, M. D., Newcastle - - - - - - - - - - 670

Art. VII.—The Cause, Prevention and Treatment of the Typhus Fever. By I. Pidduck, M. D. (Lancet, August 14.) - - - - - 671

AMERICAN MEDICAL INTELLIGENCE.

Art. I.—Proceedings of the Medical Convention of the State of Alabama, held in Mobile, December, 1847 - - - - - - - - 674

EDITORIAL.

Withdrawal of Dr. Fenner - - - - - - - - - 681

Health of the City - - - - - - - - - 682

Health of the Country - - - - - - - - - 685

Hospital Reports - - - - - - - - - - 687

List of Interments in New Orleans - - - - - - - 687

Meteorological Table. By D. T. Lillie - - - - - - 688

TO READERS AND CORRESPONDENTS.

Communications have been received from Drs. Magruder, Fitzhugh, Boling, Banning, Ewing and from Joseph Tickell, Esqr.

Also the following Books for review:

Tracts on Generation; containing proofs that the periodic maturation and discharge of ova are in the mammalia and human female *independent of coition.* By T. L. G. Bischoff, M. D. New York, Samuel S. & Wm. Wood. 1847.

Memoir of George McClellan, M. D. By W. Darrach, M. D. Phil. 1847.

Introductory Lectures—by Professor Dickson—by Professor Holmes—by Professor Grant—by Professor Mutter—by Professor Mitchell, Philadelphia.

An Address on the Bonds of Professional Union, delivered at Batavia, Ohio. By John P. Harrison, M. D.

On Poisons, in relation to Medical Jurisprudence and Medicine. By Alfred S. Taylor, F. R. S.

Materia Medica and Therapeutics. By Martyn Paine, A. M., M. D., Professor in the University of New-York: Samuel S. & Wm. Wood, 1848.

Adulterations of various Substances used in Medicine and the Arts, with the means of detecting them. By Lewis C. Beck, M. D., Professor of Chemistry in Rutgers College, New-Jersey, and in the Albany Med. College, New-York: Samuel S. & Wm. Wood, 1846.

Common Sense on Chronic Diseases; or a rational Treatise in the Mechanical cause and cure of most chronic affections of the Truncal organs, &c. By Dr. E. P. Banning. From the author: New-York, 1848.

Our usual list of Exchanges have been received.

Also, "Report of the Eastern Asylum in the City of Williamsburg, Virginia."

"A Catalogue of the Medical Plants indigenous and exotic, growing in the State of New-York etc. By Charles A. Lee, M. D."

"Catalogue of the Officers and Students in the Western Reserve College, 1847–8."

"The New-Jersey Medical Reporter."

"The American Journal of Pharmacy, new series, vol. xiv—No. 1."

"Edinburg Phrenological Journal and Magazine etc."

"American Phrenological Journal."

"A Medico-Botanical Catalogue of the Plants and Ferns of St. John's, Berkly, South Carolina etc. By F. P. Porcher."

"The British and Foreign Medico-Chirurgical Review," No. 1. Jan. 1848."

LIST OF RECEIPTS

ON ACCOUNT OF SUBSCRIPTIONS, SINCE THE PUBLICATION OF THE LAST NUMBER.

Dr. Ash, W. C.	to Jan'y, 1849
" Bagley, A.	vol. 4
" Baldwin, B.	to Jan'y, 1849
" Beaumont, J.	to " 1849
" Burton, R. A.	to " 1849
" Baker, F. C.	vol. 4
" Batchelor, J. C.	" 4
" Cenas, A. (N. O.)	" 4
" Chester, Charles	to Jan'y, 1849
" Casson, John	to " 1849
" Crawford, C. R.	to " 1848
" Davis, E. K.	to " 1849
" Dupuy, T.	vol. 4
" Dockery, H.	vols 3 & 4
" Desha, John R.	to Jan'y, 1849
" Dancy & Murphy,	to " 1849
" Frierson, M.	vols. 2, 3, & 4
" Fox, (Fayette, Miss.,)	to Jan'y, 1849
" Finley, Wm.	vol. 4
" Farrell,	" 4
" Green, Allen	" 4
" Hornsby, J. L.	" 4
" Harris, F. F.	" 4
" Haywood & Taylor,	to Jan'y, 1848
" Jenkins, Wiley	vol. 4
" Logan, T. M.	" 4
" Logan, S. W.	" 4
Dr. Lewis, P. H.	to Jan'y, 1849
" Meteer, W. H.	to " 1849
" Mallett, Z. B.	to " 1848
" McMath,	vol. 4
" Mosely, G.	to Jan'y, 1848
" Allen, J. F.	vol. 4
" Nosworthy,	" 4
" Osborne, T. C.	" 4
" Pittman,	" 4
" Pickering, G. W.	" 4
" Palmer, Thos. M.	to Jan'y, 1849
" Parker,	to " 1849
" Ramsay, Prof.	vol. 4
" Skipwith, H.	" 4
" Shields, B.	" 4
" Sloane, W. K.	" 4
" Soher, Lewis	to May, 1848
" Stubinger, H.	to Jan'y, 1849
" Stevens, J. K.	vol. 4
" Sims, J. Marion,	" 4
" Starke, Horatio	Jan'y, 1849
" Taylor, E. A.	" 1849
" Vail, R. P.	vol. 3
" Wilkins, B. B.	to Jan'y, 1848
" Wilson, John J.	" 1849
" Wilson, J. T. J.	vol. 4

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

MARCH 1848.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*Practical Illustrations of Uterine Disease.* By H. J. Holmes, M. D., *of Hinds Co., Mississippi.*

Messrs. Editors.—Since my last communication upon diseases of the uterus, published in the January number of your Journal, for 1846, sixty-one cases have been placed under my care to be treated for diseases of the womb—viz : 32 of hard engorgement with ulceration, 17 of Dysmenorrhœa, 11 of menorrhagia, and one of amenorrhœa ; 38 of which have been permanently cured, 11 greatly relieved, 4 partially—and 9 still under treatment. Having been requested to report in your Journal some of the cases that have fallen under my care—I have selected a few which I trust will not prove uninteresting to your numerous readers.

Case 1.—Mrs. G. ; aged 42, of nervous temperament, the mother of nine children, came to my Infirmary at Spring Bridge, Miss., on the 4th day of December, 1847, having travelled from Vilasco Texas, via New Orleans, to Vicksburg, thence to Clinton by railroad, and by hack to my residence. Being very feeble and emaciated—the last day of her journey affected her very much, so as to bring on one of her usual hysterical paroxysms, which continued for two hours before reaction could be brought on by the use of warm brandy-toddy, sinapisms, hot bricks, bathing of feet, &c. The next day after her arrival I made an examination with the speculum, and by it, I detected a small ulcer around the os tincæ, embracing the canal and cavity, from which issued a yellow tenacious discharge—the uterus very white and shining. By the touch it was found to be very sensative and hard—producing an exquisite pain in the right groin, with nausea and difficult breathing. The 4th, 5th, 6th, and 7th dorsal vertebræ were very sensitive—the pressure of the fingers upon them would cause her to scream aloud, and afterwards a sense of suffocation and disposition to clear the throat continued for a time—pressure upon the lumbar vertebræ caused pain in the womb and groin. To obtain a correct history of the case for some years previous to her visiting me, I had to interrogate her daughter, a very intelligent young lady who had assisted her father and a favorite servant girl to nurse her, and from her I learned that she had had for several years regular and frequent hysterical paroxysms, which by the least noise could be produced at any moment, and would yield very readily to stimulants, sinapisms, &c.. leaving her very much prostrated after each attack. So great was her apprehension of death from these repeated attacks, that at no time would she retire to bed without having placed by her side a few blue pills and $\frac{1}{4}$ grain morphine—that she might reach and take whenever she felt the least approach of one of her attacks. I also learned that her nervous system had become so much impaired that the least noise from talking by others in the same room or by the rattling of a newspaper in the adjoining room would immediately produce one of her spells. To receive company, or to talk five minutes, or to sew, read, or five minutes walking, would subject her to a very severe attack; that she was a great burthen to herself and family can be very readily imagined by the reader from the constant and watchful attention she required both day and night. The nature and seat of her disease having been ascertained, and a new plan of treatment being about to be adopted, I suggested the idea that we would dispense with the constant use of the blue pill and morphine at night. This produced some degree of surprise, inasmuch as she had heretofore believed her very existence depended upon the use of both. By our treatment and the use of enemas, it required but a short time to convince her that we could not only dispense with them but that she continued to improve without the aid of her former panacea? The treatment was commenced the morning after her arrival, by drawing blood from the uterus and continued daily in proportion to her strength, for ten days. On the morning of the 16th of December, she informed me that she had rested badly, being very nervous and restless the night before and that she was very certain her period was about approaching. The following mixture was prepared and a tea-spoonful directed to be taken in water at intervals of three hours.

℞ Tinct. myrrh. ℥ ii.
" Guiacum, ℥ i.
" assafœtida, ℥ iss.
" Iodine, ʒ i.

This mixture was continued daily for two months, with a decided improvement in the tone of the stomach and with a very happy effect when nervous.

17th. Remarked that she had during the night slight symptoms of a paroxysm, which passed off by taking a small quantity of brandy, quinine and morphine—rested pretty well the balance of the night.

18th. She received the visits of several ladies, and having talked a few minutes, produced a severe paroxysm which continued in its worst form for nearly one hour. By the use of brandy, quinine and sinapisms, reaction came on slowly—being unable to articulate more than a word or two the balance of the day.

19th. Her period made its appearance—discharge very dark, thick and fœtid; small in quantity—intense pain in the right groin and hip—cold extremities and difficult breathing—deep moaning and frequent sighing—brandy, quinine and morphine given freely.

20th. Rested badly—catamenial discharge more free, with the same color and consistence. Slight dyspnea with oppression of the chest, pulse very feeble—very excitable and exceedingly nervous—brandy and quinine continued; took during the day $\frac{1}{4}$ grain morphine twice.

21st. Rested much better—still very nervous, period the same as yesterday, pulse more full, skin warm and natural, does not complain of pain, breathing easy, ordered quinine through the day, and use the assafœtida and myrrh. mixture.

22d. Rested well—very slight appearance of the catamenia—color brighter with less fœtor; slightly nervous; appetite pretty good, setting up in bed, very feeble; continue quinine and anti-spasmodic mixture.

23d. Period ceased—exceedingly nervous with oppression of the chest and dyspnea; ordered an occasional drink of brandy, quinine and $\frac{1}{8}$ grain morphine.

24th. Dressed and setting—a little nervous, appetite very good. Remarks to day that her period which has just passed off, was decidedly better than usual, the paroxysms and nervous excitement being more easily arrested and attended with less unpleasant feelings than any one she has had for many months past; that at home it was with great difficulty that life could be sustained by her attendants, and at each period she could scarcely survive the effects of them from time to time.

25th and 26th. Very comfortable.

27th. Commenced drawing blood from the uterus, caustic applied to the ulcer.

28th and 29th. Slight hemorrhage and sloughing from ulcer.

30th and 31st. Free discharge of yellow leucorrhœa.

January 1st. Drew blood and applied caustic Iodine to the neck of the uterus; complains of pain in the back and hip.

3d. Applied moxa to the 4th and 6th dorsal vertebræ—produced deep pain in the chest with difficult breathing, a slight sip of brandy affords relief.

4th. Yellow discharge—commenced the nitric acid wash, $\frac{1}{2}$ drachm to 28 oz. water—syringe full three times a day.

5th. The ulcer presents the same appearance, drew blood, passed the second and third size probe in the canal two inches, gave slight pain, symptoms of a paroxysm, a sip of brandy and quinine afforded relief in one hour.

6th. Applied caustic to the ulcer and canal, Iodine to the neck with the same effect.

7th. Great soreness of abdomen; pain in the back and groin, hemorrhage and sloughing succeeds.

8th. Complains of her old deep seated pain in right groin with numbness of right leg; apply a large moxa over the seat of pain.

9th. Remarked that the moxa had afforded great relief; apply moxa to the second lumbar vertebra.

10th. Applied caustic to ulcer and canal—gave instant pain in the back, right thigh and leg—nervous paroxysm succeeds dyspnea and deep moaning, unable to articulate; brandy, quinine and hot bricks affords relief after a time.

11th. Nervous through the night, slept but little; hemorrhage and sloughing—great soreness of the abdomen, apply a hot poultice with great relief.

12th. Hemorrhage continues with an occasional slough; slightly nervous, complains of soreness of abdomen, quite feeble, gave one of the following pills three times a day.

℞ Ferro cyanate Quinine, ℈ i.
Ext. Valerian. ℈ ss.
Make 20 pills.

13th. Slept pretty well, strength much better and less nervous, setting up and walks a little, appetite exceedingly good, continue the pills.

14th. Strength increasing—quite calm and composed; apply moxa to the 5th and 7th dorsal vertebræ, gave pain in the chest with dyspnea, disposition to cough; a sip of brandy affords relief.

15th. She is admonished of the approach of her periods by the symptoms which usually precede it; viz: her nervous system unusually excitable, loss of appetite, pain in the back and groin, which continues until the period sets in and during its continuance.

15th and 16th, same.

17th. Period commences with slight oppression of the chest and dyspnea; frequent sighing and moaning through the day, cold extremities, pain in the back, hip and groin, discharge dark, and resembles coffee grounds, fœtid and very thick, small in quantity, brandy toddy frequently; quinine and morphine twice through the day; in the evening quite calm and composed.

18th. Slept pretty well, discharge more profuse and better in color. The assafœtida, myrrh. and guiacum mixture seems to control the nervous paroxysm and makes her quite comfortable.

19th. Slept well, appetite very good, sitting up in bed, discharge much better in appearance, more free in quantity.

20th. Discharge ceased—last night very nervous, with dyspnea, complains of pain in back and groin, drew blood from uterus, expressed great relief, at bed time took $\frac{1}{4}$ grain morphine and 2 grains of quinine.

21st. Slept well, feels much better to day—delighted with the period she has past, being decidedly better than she could have supposed.

23d. Applied caustic and Iodine to the ulcer and neck of the uterus—pains in the back and groin.

24th. Slight hemorrhage.

25th. Ceased; apply moxa to the right groin—walks across the yard.

27th. Drew blood, passed the probe two inches along the canal, find above the cavity of the neck very much contracted, used sufficient force to penetrate with the smallest probe half an inch; complains of pain in the groin, deep moaning, in a few minutes unable to articulate, slight nervous paroxysm succeeds. Brandy-toddy to day freely, hot bricks to the feet, half hour afterwards much better, very nervous through the day; quinine and morphine given twice.

28th. Soreness of abdomen, pretty well in other respects.

29th. Passed the smallest probe into the cavity, complains of exquisite pain in the uterus and groin, severe nervous paroxysm succeeds ; brandy and quinine which arrest it in a short time.

30th. Doing pretty well.

31st. Ordered a strong drink of brandy half hour before being attended, passed second size probe nearly to the cavity, paroxysm succeeds.

Feb. 2d. Drew blood from the uterus, passed probe to the cavity; severe pain and spasm of the womb ; nervous paroxysm ; toddy and morphine ; hot bricks to the feet.

4th. Apply caustic to the ulcer and two inches of the canal.

5th. Hemorrhage and sloughing.

8th. One probing and treatment producing such intense pain and nervous paroxysms that she is requested to take freely of brandy before being attended ; passed 2nd and 3d size probes to the cavity, deep moaning and paroxysm.

10th. Passed 3d and 4th size probe with the same effect.

12th. Carried caustic with the porte-caustique to the cavity; intense pain in the back and groin ; deep moaning and unable to talk ; one of her worst paroxysms succeeds, which is finally arrested by the usual stimulants.

13th. Much soreness of the abdomen, warm fermentations and afterwards hot poultices ; considerable hemorrhage and sloughing.

14th. Slight hemorrhage, soreness nearly subsided.

15th. Premonitary symptoms of her periods ; through the day nervous, ordered one pill of the Ferro-cyanate quinine and valerian three times a day.

16th. Rested badly—still nervous ; continue the pills, expresses relief from them.

17th. Period sets in—discharge much lighter and thinner—more free in quantity ; complains of being nervous ; continue pills.

18th. Discharge sufficiently free and natural ; setting up in bed quite composed.

19th. Rested well last night—discharge natural and less free.

20th. Slept well—discharge ceased ; feels well sitting up and received company to day.

23d. Applied caustic to the ulcer, canal and cavity, very slight parox-

24th. Soreness of the abdomen—poultices, &c.

ysm ; complains of pain in the hip, back and groin.

26th. Moxa to the 4th and 6th dorsal vertebræ ; a little nervous.

28th. Repeat the caustic to the cavity, recovers from the effects of this much sooner than before ; slight hemorrhage only, with some pain.

March 1st. Apply two moxas to the lumbar vertebræ and one to the right groin.

3d. Complains of much less numbness of the leg and can walk decidedly better ; repeat caustic to the ulcer and cavity with less effects than usual.

5th. By touch, the uterus feels much smaller and softer.

8th. Visits and receives company ; can talk for two hours at a time, rests well every night, repeat the caustic ; complains of this days treatment of slight soreness of the abdomen only.

13th. Expecting her period ; drew blood and painted the neck of the uterus with Iodine ; apply moxa to the 5th and 7th dorsal vertebræ—she remarks to day that after the moxas are applied she feels a decided benefit from them, and insists upon their being applied regularly.

15th and 16th. A little nervous.

18th. Period sets in ; does not complain of pain in the back or groin, discharge sufficiently free and healthy in color, sets up and walks about during the period.

23d. Period ceased.

25th. Applied caustic to the cavity; complains of slight pain in the back with some soreness of the abdomen—slight hemorrhage.

28th. Is now assisting to make a dress ; can talk for any length of time, sets up two thirds of the day without lying down ; delighted at the prospect of being restored to health.

30th. Repeat caustic.

April 3d. Apply two moxas to the lumbar vertebræ and groin.

4th. Suspects period from her feelings ; a little nervous ; takes a little quinine and morphine, period sets in during the night.

5th. Rested pretty well—discharge sufficiently free and healthy, continues four days with but little inconvenience.

11th. Repeat caustic.

13th. Apply two moxas to dorsal vertebræ.

15th. Visits an acquaintance, distance ten miles ; returns next day.

17th. Repeat caustic for the last time, blood is drawn regularly every fourth day and Iodine to the neck of the uterus ; Iodine wash used daily up to the 4th of May, at which time her period came on without any premonitory symptoms, continuing sufficiently free and healthy for four days. The next period came on the fourth day of June, without any inconvenience whatever. It would be well to remark that when this lady came to my infirmary she could not allow herself to talk, or be talked to by others for any length of time. She was able to set up but for a short time only and could not walk without a great effort, or the assistance of some one ; having lost in a very great measure the use of her right leg and the effort to walk caused very great pain in the right groin, with a degree of certainty, should she make the effort, it would subject her to a very severe nervous paroxysm. I have endeavored to abridge the history and treatment of this case as much as possible for fear of wearying the reader ; I could not do justice to the case however without making it quite lengthy. The subjoined letters from Doctors Gautier and Hort will give evidence of the cure and a previous history of the case for twenty years past.

Brazoria, Texas, October 28th, 1847.

Dr. Holmes,

Dear Sir,—It gives me great pleasure to inform you of the restored health of Mrs. G. As I wrote you before, for many weeks after her return from Spring Ridge, she was reduced to a skeleton by a severe attack of obstinate diarrhea contracted on her journey home and which prevented me from appreciating the decided change which has taken place in her general health and particularly in those uterine affections which have embittered the longest part of her life. She comes home from your care, after trying a hundred physicians and resorting to watering places, and every variety of remedy within my reach and information for more than twenty years past—restored in health, renovated in constitution and bidding fair, I hope, to realize an healthful old age. It is useless for me to attempt any description of her maladies and sufferings previous to her being placed under your care. They assumed the Protean shape and were as many and as changeful as the days in the year. Direct local applications under your novel and successful treatment of the uterus, the main seat of her disease, could alone have saved her from paralysis and cancer. She now walks with ease, attending to her domestic avocations with less fatigue than she has experienced for twenty years and is entirely free from any uterine derangement. This state of things I never supposed possible and am gratefully obliged to you for the priceless service you have rendered me.

Medical men are becoming more and more convinced of the great preponderance of uterine over all other female diseases, and your mode of treatment and the success which has heretofore attended it, opens to you a vast field for the amelioration of human suffering. I hope your practice may be as lucrative as it is honorable to you.

Very respectfully,
PETER W. GAUTIER.

New Orleans, 27th Nov., 1847.

Dear Sir,—I have received your communication of the 21st inst. and am happy to give you whatever information I may possess of Mrs. G.'s case, although I am afraid that it will not amount to much. I had moved to Florida before her marriage took place and did not see her until nearly two years after her first confinement.

During that time, I must infer from Peter's letters, she was in a most critical situation, and from the despair expressed in some of the letters it is evident that her life was more than once despaired of. This long continued derangement of health was attributed by Peter to mismanagement in the first delivery—in short, injury done to the uterus by the attending midwife. I have no doubt but that this was the case, and that it was the commencement of the disease, which would probably have ended her life in a year or two, if you had not fortunately succeeded in making a complete cure of the case. Before I saw her in Florida the origin of the case must I presume have been forgotten, for she told me on our first interview that all the physicians who had visited her had endeavored to salivate her, but without success. The change of climate, of scene, diet and regimen soon restored her to health, at least in appearance. About twelve months after she arrived in Florida her eldest daughter was born; the labor was easy and delivery accomplished in the course of a few hours without the occurrence of any untoward event, although her mind was much affected with a presentiment of great trouble and even death. From that time until I removed from Florida in December 1828, I was induced to believe that time and the recuperative powers of nature had repaired the injury which the uterus had sustained on the occasion of the first delivery. But after a year or so I saw clearly from Peter's letters that her health had again become impaired. He was disposed to refer every thing to a torpid state of the liver; but from the description of symptoms, such as lassitude, depression of spirits, dull pains in the loins and lower part of the abdomen, I apprehended a return of the diseased condition of the uterus. I saw her again in 1837 at St. Joseph's when this opinion was confirmed; after their removal to Texas it seems that the disease slowly but gradually gained ground until she became almost as helpless as she was when she left Georgia to place herself under my care in Florida. During two years previous to her visit to you, I expressed my opinion of her case over and over again, and urged Peter to send her to you. I saw her in this city on her arrival from Texas, and again on her return from your domicil, and felt satisfied that you had effected a complete cure.

Now that she is relieved from the diarrhea I see no reason to doubt the anticipation of her husband, and sincerely hope that she will enjoy twenty years of uninterrupted health to compensate for previous protracted sufferings and disease. You would do well to publish the cases you speak of—they cannot fail to interest the readers of the Medical Journal, and facts of such importance to the suffering female should be promulgated in every possible way.

Your sincere friend
WM. P. HORT.

Dr. H. J. Holmes.

Case 2.—Mrs. B., aged 35, of nervous, bilious temperament, mother of three children, came to my Infirmary on the 20th Sept. 1844—had disease of the uterus for twelve years—complained of great weight of the uterus, tenderness of the abdomen, weak back—very feeble and emaciated—skin yellow, dry and husky—constant leuccorrhea with ardor urinæ—periods very irregular and attended with excruciating pain—remarks that she is accustomed to frequent hysterical paroxysms. The uterus presented a large and deep fissured ulcer—neck indurated and exceedingly sensitive to the touch—leuccorrhea yellow and profuse. This lady while under my care had constant spasms, which would continue from one to two hours from the treatment—the probing and use of the caustic was almost intolerable. Yet the situation in which she was placed, (being dependant upon her friends for a support,) urged her to bear the treatments with a degree of fortitude I have never witnessed before; at the end of three months she was entirely relieved of all of her unpleasant symptoms—spasm &c. The ulcer had healed, no appearance of leuccorrhea, uterus soft and pliable—with a very great improvement in her complexion—strength, vivacity of spirits &c. She returned home and commenced washing, ironing and attending to the duties of the house. This however produced a return of many of her former symptoms—spasms &c. In the summer of 1846 she returned again and remained with me four months. The uterus at her return presented some engorgement of the neck—mucous membrane of the whole vagina very red and inflamed, slight transparent discharge from the os tincæ, uterus free from ulceration. The caustic was again applied to the canal and cavity—Iodine to the neck—and blood drawn regularly from the uterus—blisters to the dorsal and lumber vertebræ until the irritation of the spine had entirely subsided. She returned home and now enjoys fine health, as will be seen from the following letter. She says—"I can now attend to my daily labor, such as cooking, washing &c., without feeling any return of my old disease whatever. My friends say I look ten years younger than when I first visited you. My health I consider perfectly restored."

CASE 3.—Mrs. A., aged 25, of sanguine nervous temperament, came to my Infirmary the 29th of April. I learned from this lady that she had had dysmenorrhea in its worst form for eight years past. For three years preceding her visit to me she had with each period severe spasms, which would commence in a few hours after her period set in and continue with intervals the whole period, during which time coagula varying in size from a pea to a large filbert would pass from the uterus. Eminent physicians had been consulted and various remedies prescribed with but little benefit. Seeing that but little hope presented itself in being relieved by treating her case upon general principles, she was induced by her family physician to consult me. The day after her arrival I made an examination with the speculum—a small ulcer about the size of a five cent piece was seen immediately around the os tincæ and could be seen extending up the canal a short distance—yellow leuccorrhea—the mucous membrane of the vagina and neck of the uterus highly injected and very red. By touch, the neck of the womb very hard, sensitive and elongated—had for years complained of heavy dragging sensation in walking and standing—tenderness of the abdomen—pain in the back—bowels very much inclined to constipation—periods very irregular, occurring at intervals of five, six and eight weeks. This case was treated regularly by drawing blood in proportion to her strength—Iodine to the neck. In using the probes I found the canal very much contracted and required a great length of time to overcome it, and at each effort it was attended with a good deal of pain with several spasms. Caustic was then applied to the ulcer, canal and cavity, until the ulcer was healed, leuccorrhea checked, the uterus soft and pliable. She was treated for three months and a half, during which time her periods came on regularly, healthy, sufficiently free and without the least premonitory symptom or any unpleasant feeling during their continuance.

Her husband writes me since her return home that on their arrival her friends scarcely knew her, so great was the change in her complexion, general appearance &c. Her periods continue to be regular, and free of pain, and during their continuance she attends regularly to her domestic matters. I would like to have given a full description of this case from my note book and would have done so were I not afraid I would have taxed the patience of the reader too much. I have simply given the general outlines of the treatment; in doing so, I have omitted many features of the case that would have made it more interesting.

CASE. 4.—Mrs. G., aged 19, of sanguine temperament, visited me on the 20th of January 1847, had been suffering for three years with dysmenorrhea, and for the last twelve months with pulmonary affection, during which time had the advice of the most eminent physicians of Kentucky with very little relief; so hopeless had she become by her protracted disease and the pulmonary symptoms having assumed such an alarming state, that she was advised to come South with a view of spending the winter. While in this neighborhood, she was advised to consult me. I found her with a most distressing cough, free expectoration of purulent matter, amounting in twenty-four hours to half a pint or more, pain in the chest, cadaverous appearance, face very much swollen, a dull sound over left lung, right more clear and natural—a deep and full inspiration produces pain in the chest. The entire dorsal vertebræ, with the exception of the four last, exceedingly sensitive and while making pressure, a sense of suffocation and disposition to cough is produced by it. Uvula natural in size and length—the uterus presented a small ulcer around the os tincæ—mucous membrane of the vagina and uterus very red and inflamed—luccorrhea free and yellow. By the touch the neck was found to be considerably elongated and very hard—could pass the second size probe two inches along the canal very easy and without pain, above this the contraction was very small. Blood was drawn from the uterus regularly; the canal dilated with the probes, and caustic used to the ulcer, canal and cavity, until the ulceration was removed and leuccorrhea checked. Iodine applied to the neck regularly. By this treatment her periods from being very scanty (scarcely a show), irregular and attended with a good deal of pain, became regular, healthy and sufficiently free and without pain—blisters were applied to the dorsal vertebræ four times and twice to the breast. Internally she drank daily a strong decoction of Cemicifuga Racemosa, four times a-day in wine glass doses. By this a slight nausea was kept up; preparations of Iron and Columbo were given to support her strength, and a nourishing diet allowed. After a time her cough and expectoration ceased altogether—the swelling of the face subsided—her complexion became clear and good. She gained twenty five pounds while under treatment, and when she left on the 29th March was as free from disease as at any time of her life. The following letter received since her return home will show positive proof of the efficacy of the treatment submitted to.

Oldham County, Kentucky, April 20th, 1847.

Dr. Holmes.

Dear Sir,—On our return home our friends were more than surprised to see Mrs. G. restored to health, although they had received frequent letters from us, asserting most positively that her health would be restored. Yet they could not believe otherwise than that we were flattering them with the vain hope of seeing her again in Kentucky; but such has been the good effects of your treatment, that not a symptom of her former disease has returned either of the lungs or womb.

Very respectfully,

A. L. GUAR.

Case 5.—Mrs. G., aged 35, of nervous temperament, the mother of six children, had been diseased for eight years. Visited her the 24th August 1843. This lady had had a most distressing cough with free expectoration of purulent matter for five years previous to her visit to my Infirmary. For many years she had consulted many eminent physicians of this State, who had time and again pronounced her's a genuine case of Phthisis pulmonalis. Besides her pulmonary disease she had had symptoms of uterine derangement, and was induced by some of my former patients to give me a trial as the only alternative. Not being very familiar with the various sounds of the lungs by the use of the stethoscope, I had to form my diagnosis from other circumstances. Although very much emaciated and expectorating freely of pus daily from the lungs, I inferred from the great tenderness of the entire dorsal vertebræ, the length of time she had been troubled with her cough and expectoration, had it have been, as supposed, a genuine case of consumption, she could not have survived the disease as long as she had. That there must be some other cause acting upon the lungs to produce this state of things, and if this could be ascertained and remedied, a favorable result might be expected. By making pressure upon the affected vertebræ her cough and a deep seated pain in the chest and a sense of suffocation would immediately ensue. The uvula was natural in size and length—the right tonsil very much enlarged and ulcerated—the left slightly swollen and hard. The uterus I found to be extensively ulcerated, with a large flow of yellow leuccorrhea—catamenia irregular, with a copious flow at each period. The treatment consisted in drawing blood regularly from the uterus by leeches in proportion to her strength—caustic applied to the ulcer and cavity at intervals of six and eight days—washes of Iodine and nitric acid well diluted alternately—four narrow blisters applied to the nitric dorsal vertebræ—three to the chest. Internally she took Sarsaparilla and Hydriodate Potassa—Tonics and a decoction of Cemicifuga racemosa regularly—the nitrate of silver and gargles were applied frequently to the tonsils—the right tonsil afterwards removed. This lady remained with me near four months, during which time her strength and flesh increased rapidly—cough and expectoration ceased entirely—catamenia became regular and healthy—ulcer healed and no symptom of leuccorrhea whatever. The following letter from her husband will show the great benefit from the treatment.

JACKSON, Oct. 10th, 1845.

DR. H. J. HOLMES.

Dear Sir,—On the 30th day of last month my lady was delivered of a fine son weighing eight pounds and a half. This is more than I ever could have expected, as she has not had a child for eight years before, owing to her general bad health and more particularly a disease of the womb, under which she suffered very much for six years previous to her calling on you for medical assistance. Her general health is still delicate, but very much improved, owing there is no doubt to the valuable and effectual cure which you made of the disease of the womb, and I take pleasure in saying that this is one of many instances of your cures of this kind which has come under my knowledge, and although it is rather a delicate matter I deem it my duty to authorize you to use this letter, if you think it will serve you to do so. Respectfully

J. S. G.

This lady is now enceinte with the second child.—My object in presenting these two last cases is to draw the attention of the profession to that stage of uterine diseases which, if not timely noticed and checked, must inevitably terminate in consumption and cause a premature death. It is now too much the fashion with our profession, when they find that lesion of the lungs is present, to condemn the patient to inevitable death, by adopting a mere palliative course of treatment, by which time is lost and the patient glides into that stage in which she is at first pronounced to be. We should look for other causes and be well convinced that the uterus is not in fault, before we pronounce an opinion that it is a genuine case of phthisis pulmonalis. The situation of these ladies certainly justified the opinion that they were utterly hopeless, and gave rise to many remarks that I was deceiving the lady and friends with the idea of a cure, when the symptoms were so well marked.

Spring Ridge, Miss., Jan. 15, 1848.

II.—*Report of several Cases of Scarlet Fever.* By F. Downer, M. D., of New Orleans.

Case 1.—Dec. 12th, called by appointment, at four o'clock, P. M. to prescribe in a chronic case, for a member of a large family. Before leaving, I was requested to look at their son, a boy of seven years, who for several days had been laboring under what appeared to be a bad cold, but on this day he seemed so much worse, they began to feel uneasy.

I found him with a high fever; great difficulty in swallowing; mind confused and wandering; tongue dry, and loaded with a thick dark coat; teeth covered with sordes; with a slight eruption on the face and neck, of a livid hue. I at once pronounced it to be malignant scarlet fever of a typhoid type. Ordered cold effusions to the head and face; a gargle of infusion of capsicum, alternated with one of sage tea, honey and alum. Ten grs. calomel, with four of ipecac, in four powders, one to be given every three hours till it operated; to be followed with flaxseed tea, or barley water, acidulated with lemon juice.

13th. Skin hot and dry; oppressed breathing; countenance haggard; great restlessness, with delirium. Owing to the unmanageableness of the patient, the directions had been but slightly fulfilled: a part of two of the powders had been given; and brought away two discharges of very dark offensive matter. Ordered one gr. tartar emetic to an ounce of water, a teaspoonful every half hour till it acted as an emetic; with volatile liniment to the throat. He vomited slightly, but so great was the difficulty of deglutition that but little more was done; delirium, with frequent convulsions during the night, and on the morning of the 14th he died from suffocation.

Case 2.—At daylight on the morning of the 17th, I was called to visit an older brother of the deceased, aged nine years. He had appeared well up to the day previous, when he was engaged the greater part of the day in shopping with his mother. Came home late, and very much fatigued, but ate an unusually hearty dinner of vegetable soup, meat and potatoes, which in an hour or two he threw up, and without making any complaint, soon after retired to bed. When I saw him, he was laboring under short, quick, and anxious breathing; pulse rapid, and vibratory; increased heat of the head, whilst that of the body was but little above the natural standard. There was uneasiness of the throat, with hoarseness and a slight enlargement of the tonsils; great intolerance of light, mind confused and wandering.

Gave an emetic of antimonial wine, which caused him to eject a large quantity of tenacious, ropy mucus, with decided relief. 8 o'clock. Ten grs. calomel with five of ipecac to be followed with flaxseed lemonade, cloths wrung out of cold vinegar and water to be constantly applied to the head.

12 o'clock. The bowels moved; fœces of a light clay color. and highly offensive. Warm sage tea, continue cold applications to the head.

At three o'clock. I was sent for in great haste, as the boy was believed to be dying. On arriving, found him almost in a state of collapse—pulse weaker and quicker; had had two discharges since I saw him at twelve o'clock, the last involuntary, and in the highest degree offensive. Ordered weak brandy toddy, a starch injection with laudanum, and despatched a messenger for my friend Dr. Harrison. The heat of the body being greatly diminished, with the pulse at the wrist quite imperceptible, we applied sinapisms to the abdomen, ancles and wrist; gave sub-carbonate of ammonia alternately with the brandy toddy.

At 6 o'clock we again met; he was now insensible to surrounding objects; great jactitation, constantly tossing his arms about, and moaning. These symptoms seeming to indicate a high degree of gastric inflammation, the idea of poison suggested itself to the mind of Dr. H.; but on pressure of the abdomen and over the region of the stomach, not the slightest evidence of tenderness or pain did he evince, nor had he from the commencement, as I had frequently examined these parts, and asked the question while he was able to answer. He now rapidly sank, till death closed the scene, during a convulsion, at 8 o'clock, it being but little over twelve hours since I was called to see him. An examination of the body was not permitted.

Case 3.—Dec. 23d. Was called at 7 o'clock, A. M., to see a daughter of five years; found her with slight fever; frequent, but not very full pulse; dry brown tongue; some swelling of the throat, with slight redness of the tonsils and difficulty of breathing; bowels constipated.—Prescribed senna and manna, flaxseed lemonade; vol. liniment to the throat, with a flaxseed poultice.

11 o'clock. The bowels had been moved once freely, fœces light and offensive with scybala and undigested food. Another discharge very copious, same color, without scybala or food. Ordered twelve leeches applied to the throat, and to take a teaspoonful of the following mixture every two hours—

Muriate ammonia, ʒ jjs.
Emetic tartar, 1 gr.
Ext. Glyc. ʒ j.
Aqua. Dist. ℥ iv.

7 o'clock. Leeches had drawn well; had had another passage of the

same kind; keep up the mixture, with the same drinks and frequent sponging of the head and face with vinegar and water.

24th. 8 o'clock, much better, would not take any more medicine, during the morning asked for tea and dry toast which was granted.

25th. Much improved, the rash well developed; tongue clean, with papilla very elevated, bowels costive; order senna and manna; to be kept quiet. From this time she improved rapidly.

While I regret that a post-mortem examination was not allowed in the second case, I am satisfied, on a review of all the symptoms, that this was a case of a regular congestive form, and that the head, and not the stomach, was the part most affected. The tendency to assume the typhoid type so early in the course of the disease, can only be accounted for, by the peculiar contagion to which he had been exposed; and this, as Dr. Good remarks, "under a depressed state of the living power, whatever be its cause, whether a want of cheerful warmth, cheerful passions, cheerful food, or cheerful and regular habits, typhus is often more likely to take place, than any other species of fever. But when febrile miasm, produced by a decomposition of effluvium from the living body, exists in a co-operation with these, it is almost impossible for an individual to escape; as the miasm thus generated has a specific power—a power beyond all other febrile causes whatever, of lowering still farther the vital energy as soon as it is received into the system, and thus of confirming the tendency to this peculiar type." In this instance, the boy suffered much during the last hours, and after the death of his little playmate; he had slept in the same bed with him, until within two days of the death of the latter;—was taken sick immediately after a long and fatiguing walk; the hearty dinner he ate, assisting still more to depress the exhausted vital powers. The weather for two weeks preceding, had been unseasonably warm and rainy; the thermometer, for about ten days, had ranged over seventy degrees, while the location was damp and low. All these causes acting on an existing predisposition; hence the sudden overpowering shock, under which the system gave way in so few hours, and while the medicine was apparently doing its offices kindly. Dr. Armstrong is the only author I know, who describes this form of scarlatina. He says, "the subjects of this modification are for the most part suddenly attacked." "Sometimes they at once sink as if overcome by a sudden shock, and lie in a state of confusion and oppression, without making much complaint." "The mind at first alarmed, confused or dejected, soon becomes disordered with delirium, or an indifference to surrounding objects, and a stupor succeeds under which patients finally expire.

In two other very severe cases of Scarlatina Anginosa which I treated a week before, both the parents suffered from severe sore throat, attended with febrile symptoms, but no efflorescence of the skin; an adult member of the first family labored under soreness and swelling of the fauces, unattended with febrile symptoms; thus showing the protean nature of the disease, and the degree of virulence it assumes according as it is met by a predisposition, or otherwise. As regards my experience, scarlet fever is most to be dreaded of any of the diseases incident to childhood; whether considered in its immediate consequences, or, as regards its results. As Dr. Francis observes, it is one for which we have no prophylactic; and in this climate delay in resorting to immediate remedial measures is so often fatal, that active treatment should be commenced from the first suspicion of the complex form of the disease; and then, alas! the bills of mortality tell with what success.

III.—*Yellow Fever contrasted with Bilious Fever—Reasons for believing it a disease sui generis—Its mode of Propagation—Remote Cause—Probable insect or animalcular origin, &c.* By Josiah C. Nott, M. D., Mobile, Alabama.

In the April number, 1845, of the American Journal I published an essay on the *Pathology* of Yellow Fever as presented to our notice in Mobile. I now propose to give the results of my observations on the peculiar habits, or what may be called the Natural History of this disease, and my reasons for supposing its specific cause to exist in some form of Insect Life. *Malaria*, which, according to the received doctrine of the day, is a gaseous or molecular emanation from the earth's surface, is, in my opinion, wholly inadequate to the explanation of the mode of propagation of this disease, and I am therefore induced to offer a different solution which is strongly supported by the phenomena attending it. The whole doctrine of Malaria is but an hypothesis, and if we can substitute another for it which is better sustained by reason and analogies, and which conflicts with no known law of nature, it is the part of sound philosophy to give it a preference, until a less objectionable one can be found.

There is no novelty in the doctrine of Insect or Animalcular origin of diseases. Many of the older writers, amongst whom are conspicuous Linnæus, Kircher and Nyander, have promulgated such an opinion, and it has been vaguely presented from time to time to the notice of the profession, but it is only since the publication of Ehrenberg's great work on Infusoria (1838) that its bearings can be fully appreciated. The medical periodicals of late years have made occasional allusions to the subject. Dr. Wood, of Philadelphia, Dr. Watson, of London, and others make honorable mention of it, but the most elaborate and ingenious article I have met with is that in Sir Henry Holland's "Medical Notes" "On the Hypothesis of Insect Life as a cause of Disease."

Sir Henry coyly approaches this *mundus invisibilis* as an "Hypothesis," and it is well that extreme caution should preside over our medical reasoning, and that undue weight should not be given to ingenious speculations; but when medical science in its onward course arrives at a point where an old "hypothesis" is inadequate and contradicted by established facts, another theory which is less exposed to these objections and well sustained by analogies may perhaps not improperly be dignified by some appellation a little stronger than that of Hypothesis.

As far as *doctrines* are concerned the history of Medicine is little more than a recital of successive delusions, and we have too much reason to know, that it takes almost as much time to uproot a false medical doctrine as a false religion, when it has once seized upon the public mind. From the time of Hippocrates to that of Lancisi the doctrine of Malaria had no existence, but at length the great revelation "*De noxiis* *palludium effluviis*," came, and the world marvelled and was converted. After a while, however, this great dogma began to be scrutinized more closely—doubts and difficulties sprang up and gathered strength as time rolled on—and finally some of the infidels, amongst whom is Dr. John Bell, of Philadelphia, (one of the best medical writers of our country,) have been bold enough to deny the very existence of Malaria in any shape, and have contended that Meteorologioal changes, radiating and absorbing qualities of soils and plants, dews, &c., are sufficient alone to explain the occurrence of those diseases commonly attributed to Malaria

Though this subject has for many centuries enlisted no small share of talent, learning and industry, yet has little been done towards dispelling the darkness which overshadows the morbific causes of fevers. A crude mass of facts has been collected, but so contradictory do they seem, that no attempt at systematizing them has yet succeeded—facts however are immutable—the contradictions are probably only apparent, and a careful investigation may show that the errors lie not in false facts, but in false hypotheses.

Malaria has been assumed to be a *Unit*, and *Identity* for all the fevers of hot climates follows as a corollary. Intermittents, Remittents, Congestive and Yellow Fevers are all thrown into this Grand Gulf of morbid Poison—*rari nantes in gurgite vasto*. Here arises a very grave question. If this doctrine of *identity* be wrong, it is clear that the whole history of fevers has been vitiated by false assumptions, all the logic based on false premises; and our chance now for arriving at truth is to go back and ascertain what *are* facts and make our deductions *de novo*. A field is here laid open far too wide for the limits of a Journal, and I must therefore confine myself to the illustration of one division. Though I shall be compelled from the nature of the case, by way of illustration, to allude, *en passant*, to other types, I beg it to be borne in mind that *Yellow* Fever is the subject before me. I regard this as a disease *sui generis*, and though I hope to do more, the establishment of this point alone would be a very important step in the Etiology of Fevers.

Macculloch, who may be regarded as the *ipse agmen* of the Malaria party, meets the question fully and fairly in the following proposition: "Whatever Malaria may be in its simple state, it is only as united to the atmosphere that we know it, and we must therefore view it as an æriform fluid, as far as the question of its propagation is concerned. It must be considered as the very atmosphere itself, when it exists; and *its propagation therefore must be primarily regulated by those laws which govern the motions of currents of air.*" Here one of the important issues is placed upon its true ground, and by this it should be fairly tested.

When then we have exhausted all the known laws which regulate the atmosphere, and which appertain to gases is general—when, too, we have exhausted all the known and supposable habitudes of molecular emanations of vapors and dews, and are still unable to account for the propagation or transmission from point to point of Yellow Fever, the idea of Malaria must so far be abandoned. It is not permitted to *assume* new laws, which are subversive of others that are known.

I propose now to show, from facts presented during the various Epidemics in Mobile, that the morbific cause of Yellow Fever is not amenable to any of the laws of gases, vapors, emanations, &c., but has an inherent power of propagation, independent of the motions of the atmosphere, and which accords in many respects with the peculiar habits and instincts of Insects.

I must here anticipate the main discussion by laying down a few leading facts in relation to the manner in which Yellow Fever *has been* propagated in Mobile on various occasions, as these facts form the groundwork of much which follows, and must be frequently alluded to.

The town of Mobile, which contains ab ut 15,000 inhabitants, is situated at the mouth of the Mobile River where it enters into the Bay. It stands on a plain composed of sand, here and there a little clay, with vegetable matter and shells. The whole formation is evidently alluvial, and from the numerous beds of unfossilized *grathodon* and other shells found in the vicinity, and other geological indications of comparatively recent change of level, there can be no doubt that the whole platform around the town has undergone a movement of upheaval at some epoch not very far removed from ours. These beds of shells are found of various elevations—some as much as 20 feet above the river, and are composed of shells which belong to species *now found in our waters*, in a perfect state of preservation. The soil in and around the town is very porous, the water from the heaviest rains disappearing in a few hours. The tides here do not rise more than from one to two feet, and ebb and flow but once in the 24 hours. There is a marsh on the north and another on the south which touch the suburbs. On the west the land gradually rises some 40 or 50 feet for five miles, when it breaks off abruptly into high pine lands.

Though in previous years, when the town was comparatively small, Epidemics were not uncommon, it is a remarkable fact that no Yellow Fever occurred for eight years previous to 1837, except sporadically. Since my removal to the city, (May, 1836,) there has been no year without sporadic cases, and not less than five Epidemics of greater or less magnitude have prevailed, viz: in 1837—'39—'42—'43—'44. I shall select, from the history of each, a few facts bearing on the points to be examined.

The first Epidemic I witnessed was that of 1837, which was announced by a single case on the 10th of September. Four more cases occurred about the 20th, and it is remarkable that all these cases occurred at points so remote from the shipping and so distant from each other as to preclude the idea of recent importation, or propagation by contagion. They seemed to arise, each from an independent focus. The next cases did not appear until about the 10th of October, or some twenty days after the last mentioned cases, when it commenced spreading rapidly in all directions as an Epidemic, and carried off about 350 persons before it was arrested by a "killing frost." There was nothing in the character of the weather to account for the slow progress during the first thirty days, and it assumed the Epidemic character a few days after a very heavy southern gale which caused the water of the river to overflow the low parts of the town on its margin.

The next Epidemic occurred in 1839, and commenced during the first days of August, where it should have been the least expected, viz: on the corner of Government and Hamilton streets, half a mile from the shipping, in a clean, well ventilated and fashionable part of the town. For a short time the disease spread slowly around this focus, but at length it burst forth in every direction with extraordinary violence, ravaging not only the town, but the environs for several miles. This was one of those great Epidemics, in which the disease, shaking off complications, assumes its true and undisguised character, and usurping the field, swallows up every thing else in the shape of Fever. Number of deaths 480. Almost all the seaports on the Gulf were visited by Yellow Fever this season in severe form. There was nothing peculiar in the weather, but on the contrary it had been pleasant, temperate and showery. There was no imaginable cause why the dormant germ of Yellow Fever should have been aroused to such extraordinary activity at so many distant points at the same time.

In 1842 we again see the disease, commencing the 29th of August, in Spanish Alley, a very filthy place near the docks, where it would naturally be expected. From this point it spread with surprising deliberation in a north westerly direction—travelling slowly from house to house, and taking more than a month to reach and extend along Dauphin street, which runs the whole length of the town, dividing it into two equal parts. Its course and progress could be traced step by step, and its ravages were confined to one half of the town, leaving the other almost untouched. Had frost kept off a few weeks longer, there is every reason to believe it would have continued its course and marched over the other half of the town. Another Epidemic appeared in 1843, commencing about the 19th of August in the opposite or northern extreme of the town, and pursuing a course the reverse of the preceding year, viz: south east—taking about the same length of time to extend itself over the northern, that it had over the southern half in 1842—leaving the southern part almost untouched. Number of deaths 240, and checked by a severe frost.

Such was the *general course* of the disease in the last two years, though there were some trivial irregularities. In each of these years persons by visiting the infected district contracted Yellow Fever and carried it home with them to other parts of the city, still the disease was not in any instance communicated or propagated by them. It travelled day by day for weeks, progressing from point to point like the army worm through the cotton region.

In 1844 Yellow Fever made its last appearance in Mobile (except sporadic cases,) but to so limited an extent as scarcely to deserve the name of Epidemic; still the facts are curious and important in connection with our subject. The first cases occurred about the 1st of August, and others continued to appear at irregular intervals for about two months, which were scattered here and there over the town in a very extraordinary manner. The number of deaths was but 40, and they could not have been more scattered, occurring on different squares with apparently as little connexion as would the same number of labor cases.

A review of the Epidemics just detailed, will reveal some curious and important *habits* of Yellow Fever, which have been strangely overlooked by writers on the subject; and so far from being peculiar to that disease in Mobile, its history in other places will show that they belong to it every where. The progress of the disease, in Philadelphia and New York particularly, has been marked by the same peculiarities. I beg leave to call attention especially to the Epidemics of 1842–3, as the mode of propagation in these years forms the basis of all my reasoning. In these years the disease started from a single focus at different extremes of the town, and after hanging about the point of origin for a short time, took up its march and progressed steadily and slowly for more than a month, until it overspread one half of the town, without being stopped by variations of weather.

How is this slow progress to be accounted for? Why did the disease, while the sea and the land breezes were sweeping the town daily in every direction, take a *month* to extend half a mile and then stop in the heart of the town? If the morbific cause exists in the form of *Malaria*, which "*we* only know as united to the atmosphere"—if it can be influenced by currents of air, or propagated by contagion, its course and conduct could not have been such as described. It was literally and truly a *migrating disease*, possessing an inherent power of reproduction and progression irreconcilable with any known laws of gases, emanations, vapors or dews. Even Liebig's theory of *fermentation*, which is the *latest fashion*, is equally insufficient, for a *fermenting* point of the air cannot stand still. Macculloch has discoursed largely about the fantastic motions of the atmosphere; he tells us of upward currents and downward currents—straight, curvilinear and irregular currents—the curious distributions of dews, &c., &c., but all falls short of the mark, however applicable such explanations *may be* to the propagation of *Intermittent* Fevers. Yellow Fever, in 1842 and '43, travelled from house to house for more than a month, as would a tax collector, and was just about as much influenced by the weather; for neither the fever nor the tax collector like to travel in rain, though they pay no regard to the direction of winds.

On the other hand, in the years 1837—'39 and '44, the disease started in succession from several or many foci, and diffusing itself gradually or rapidly in the different years, seemed to lose all connection with the points of departure or origin; cases occurred here and there in every direction. These facts may at first glance seem to contradict those before given; but a little reflection will satisfy the reader that they are all perfectly reconcilable by the Insect theory, and no other.

Before entering on the "Insect Hypothesis" in detail, it may be well to give a familiar illustration of it, based on facts well known to all classes in the cotton region. The perfect analogy between the habits of certain insects and Yellow Fever will thus be made apparent at once.

It is a law of nature that every plant affords sustenance to several parasitic insects, and the average number of each plant has been estimated at half a dozen. The cotton plant like others is attacked by *its* parasites, having their peculiar habits and instincts. One or several of these insects may appear the same season, and true to their instincts each attacks different parts or *organs* of the plant—as the leaves, bark, woody fibre, roots, pods or bolls, flowers, &c. Some years there may be an entire exemption from one of these insects, or to use a medical phrase, there may be a few *sporadic cases.* At another time a worm may appear at a single point, and from this focus will spread slowly over a portion of a field (as did the Yellow Fever in 1842 and '43) leaving the other portion almost untouched. In another year a worm comes like a great Epidemic, appearing at many points in rapid succession or simultaneously, and ravaging not only a single plantation but laying waste the cotton region for several hundred miles. Some of the insects appear on the hill-tops, others in the low places. Some attack the vigorous *plethoric* plants, others the delicate and feeble plants, &c. One planter, a very sensible, accurate observer, informed me that some very minute insect attacked his cotton field last summer in *concentric circles*, causing a very singular appearance; the alternate circles of healthy and diseased plants varying in elevation and resembling the waves of the ocean.

The history of the great *Army Worm* which destroyed the cotton crop of the last year is very curious and instructive. From the best information I can procure, this worm appeared in 1820, in 1840 and 1847, long and irregular intervals. A writer in the July No. of the New Orleans "Commercial Review" has, I think, demonstrated some instructive facts connected with its Natural History. He shows that there is no provision for its preservation during the winters of our climate, and that it must perish so soon as its food, which is the cotton plant alone, is exhausted. He states, also, that this worm commences in the extreme south, and progresses invariably in a north westerly direction. This worm belongs, like the silk worm, to the *Moth Tribe*, and there is a strong similarity in the habits of the two Insects. There is no natural provision *here* for the hybernation of *the* silk worm; it is a native of the tropical climate, and its generations follow each other in rapid succession. It feeds exclusively upon the Mulberry tree, and if the eggs were not preserved by artificial means they would all inevitably perish. When the warm weather of the spring comes on, the eggs require to be kept in a cool, dark place, to prevent them from hatching before their food is ready for them, viz: the leaf of the Mulberry. If the worm is born before the leaf puts forth, it perishes. The case of the *Army Worm* is perfectly analogous—from its known peculiarities it *must be a native of a tropical climate*, where the cotton plant is *perennial*. The time required from birth to full maturity, including all metamorphoses, is but ten days. We have numerous instances of the emigration of butterflies and other insects across water to a great distance, and it is very easy to conceive how the moth, which produces the army worm, might (breeding with the rapidity it does) find its way from Mexico or South America into the southern part of the United States and gradually overrun our cotton region. If, as the writer alluded to, asserts, the time of existence of the Army Worm is but ten days, and its food be exclusively the cotton plant, the conclusion is inevitable that it must come into existence and die in the spring long before their food is produced in our climate.

I have been a little minute in these details, as I shall have occasion to allude to them afterwards when speaking of the migrations and other habits of Yellow Fever and some kindred diseases.

Even this little sketch is sufficient to show some striking analogies between the habits of insects and those of certain Epidemic diseases. Some insects lie dormant for years, and then appear in several or innumerable points, and varying in number from a few up to countless myriads; others appear, but very variable in extent every year. The reasons for their long repose, their irregular and sudden resurrection, their varying numbers, the habitation and condition of their germs, during these different periods, are inexplicable difficulties which remind us strongly of the vagaries of Yellow Fever. The different insects, too, (like Epidemic diseases,) attack different *organs* of plants—at one time very circumscribed in their operations, attacking one or a few spots; and at another, bursting forth like a wide-spread Epidemic. There are no appreciable meteorological changes which can account for "each change of many-colored life."

Animal and vegetable decomposition are governed by laws which are more uniform, more palpable and easily comprehended—whenever animal or vegetable matter is subjected to the action of heat, air and moisture, decomposition rapidly ensues; and there is no summer in our climate during which a dead horse or a bale of hay will not rot in the open air and in a few days throw off plentifully its offensive effluvia. These effluvia too must abound *every* year, (though fevers are but *occasional*) and as Macculloch remarks they become incorporated with the atmosphere, and unlike the *materies morbi* of Yellow Fever, are compelled to obey its motions.

Though my argument is intended particularly to illustrate Yellow Fever, which I regard as a disease *sui generis*, still I may be permitted to remark that the present state of facts do not warrant us in assuming Identity for all the other forms of what are termed Marsh Fevers, viz. Intermittent, Remittent, Bilious and Congestive Fevers. The various and strongly contrasted types described in the United States—those of Flanders, of the different countries lying on the Mediterranean, and those of Africa and India, all of which have been described, may well excite serious doubts on this point. I am by no means sure that all these types may not be most rationally explained by attributing them to various insect species, but laying aside this hypothesis and assuming the *malarial*, it would be a strange anomaly in nature, should it be proven that but one morbific cause of fever is generated over the broad surface of our variously compounded globe.—Fever should have its *genus* and its *species*, like other things in nature.

Though chemistry has arrived at a wonderful perfection in analyzing and separating into their primitive elements the various mineral, vegetable and animal substances which surround us, yet the laboratory has not succeeded in bringing to light any gas or product of putrefaction which can produce in the human frame a train of symptoms resembling those of Periodic or Yellow Fever. Many of the products of the laboratory will disturb health or produce death, but they create symptoms of their own. There are many known facts which make it probable that a *multiplicity* of Malarial poisons exist. It is ascertained that different soils eliminate different gases, as Azote, Sulphuretted Hydrogen, Hydrochloric acid, Hydrogen, Carburetted Hydrogen, Carbonic acid gas &c., and yet we have no evidence that any of these have any agency in producing *Fevers*.*

Again we have a vast number of *Emanations*, which are known to exist, though beyond the reach of the chemist—for example the various forms of animal matter; as flesh, fish &c.—the infinite variety of plants, as well as soils, all give off peculiar emanations which are only detected by the sense of smell. There are also other emanations of which we should be wholly ignorant were it not for their effects on the human system. Some of these are mineral, some vegetable and others animal—such as those from Mercury, Lead, Arsenic &c.—those from the Mancinella tree, the Rhus Toxicodendron, the Upas &c.—those from the bodies of persons laboring under contagious diseases &c. &c., and there can be no question that the chapter on Emanations might be greatly extended could we trace all diseases to their causes. But we may well doubt from their *peculiar mode of propagation*, whether the *materies morbi* of "Marsh Fevers" exists in this form.

M. Chervin, in one of his last works "*De l'identité de nature des Fièvres d'origine paludéenne de differents types &c.*" has given the fullest and most labored argument I have met with in favor of the *Identity* of Yellow and Marsh Fevers; and as it is indispensible to my argument that Yellow Fever should be as far as possible isolated, I will introduce his *resumé* as a text for what I have to say on this point. These conclusions will be found at the close of his essay under the caption: "*Analogies entre les Fièvres Périodiques et la Fièvre Jaune.*"

1st. "Yellow Fever has never prevailed epidemically out of the Tropics except in summer or autumn, that is to say, in those seasons in which Intermittent and Remittent Fevers prevail."—Chervin.

This proposition may be admitted without hesitation, as it proves nothing, if true.

2nd. "Yellow Fever is never seen except in localities where Periodic Fevers may be developed."

If true, this proposition should deserve no more weight in settling the point in dispute, than the last. The fact that two diseases are always found in the same climates and localities no more proves *identity* for them, than similar circumstances would prove identity for two plants or two animals. But we have good authority for disputing the *fact* laid down. Yellow Fever *does* occur in localities where Intermittents are extremely rare or *wholly unknown*. Amongst other examples we may cite the Island of Barbadoes, which is thoroughly drained—almost every

* Important discoveries might be suggested by the phenomena which occur in sweetening a cup of tea as well as by the fall of an apple. When a lump of sugar is dropped into a cup of tea numerous bubbles of air, which had occupied its pores, are seen to rise to the surface; and in riding in the suburbs of Mobile a few weeks ago after very heavy rains I perceived large quantities of gas gurgling through the newly formed small ponds, which was displaced from the porous soil in the same way that the air was from the sugar. This gas is easily collected after heavy rains, and it would be a curious subject of inquiry to ascertain its composition in different soils and localities.

foot of it in a high state of cultivation and is according to the authorities *exempt* from Intermittent Fevers. Intermittents once prevailed here extensively, but have been exterminated by drainage and cultivation. The fact is notorious that invalids suffering from Periodic Fevers go from the surrounding Islands to Barbadoes to get cured of these diseases, and yet *Yellow* Fever prevails nowhere with more malignity than in Barbadoes.

3rd. When in the Equinoxial regions the Yellow Fever sweeps off the unacclimated population, Periodic Fevers prevail generally amongst the Creoles and old residents."

This opinion has often been advanced and although there is apparently some foundation for it, I think a false interpretation has been given to the facts on which it reposes. Periodic fevers do certainly often occur, to a very *limited extent* in those localities where Yellow fever is seen, and in the same season, but, as before stated, this proves nothing as to their identity. The *unacclimated* population of Mobile, for example, may be simultaneously attacked by the two diseases, whilst the *acclimated* residents, (who are exempt from Yellow fever,) may, if its morbific cause is present, be attacked by Periodic fever, against which there *can be no acclimation.* The extent however to which Intermittents prevail amongst the creoles during Yellow fever Epidemics has been greatly exaggerated by M. Chervin. In Barbadoes these Periodic fevers do not accompany Yellow fever *at all*, and in Charleston, Mobile, and New Orleans they are rare except in the outskirts bordering on the marshes. The physicians of these cities must all sustain me in the assertion that the creoles and other acclimated residents are healthy during Yellow fever Epidemics. Periodic fevers certainly do not "*sévissent généralement contre les créoles et les anciéns residens.*"

4th. "The meteorological phenomena, which exercise so marked an influence over the march of Yellow fever, exert an analogous influence over that of Periodic fevers."

The physicians of our Southern Seaports are certainly not prepared to receive this proposition as demonstrated, for according to our observations the *origin* of Yellow fever at different epochs is entirely independent of appreciable meteorological changes, though after it has once *started* it progresses more rapidly in dry than very wet weather. No one can pretend to predict the occurrence of Yellow fever the day before the first case appears. In Mobile we are often taken by surprise as is the case at the time I am writing (30th July, '47)—a case occurred in a Capt. Smith at the Mansion House, about the 18th of this month, though the weather has been remarkably temperate and the rains have been falling in torrents for a month—cases have occurred in New Orleans in June, under similar circumstances, and in both cities the disease is a month in advance of its usual time of appearance. The disease does not with any regularity therefore appear in the *hot years* as has been asserted, but just as often in the showery pleasant seasons; cases have been occurring every day or two in New Orleans, for the last 6 weeks in the midst of the unceasing rains, but the disease will not probably extend rapidly until the rains cease—if the cause is animalcular we can well imagine how rains may impede their march. But when this disease once gets under way it is unimpeded by winds or storms, and nothing short of a "killing frost" can arrest it—repeated light frosts may come, but the disease continues till arrested by the *freezing point.* If Yellow fever were caused by emanations from decomposing animal or vegetable matter, a single freezing night might *suspend* the elimination of these effluvia; but the influence must cease when the cause is withdrawn. We often have two or three weeks of weather after a killing frost, as warm as that which preceded it, and there is no reason why the decomposition should not resume its operations; but not so with the Yellow fever—like in insect life, when the ova are once hatched, the propagation of this disease goes on till arrested by a *killing frost*—and it can only be animated by another summer's sun, which calls from their slumbering places the various insect tribes.

5th. We know that the miasms which give birth to Periodic fevers may be transported by the winds; so is it with those which produce Yellow fever; *only in the latter case their deleterious action does not extend so far.*"

Even M. Chervin, then, is here forced to admit a distinction between these diseases—viz: that Yellow is *less influenced* by winds than Periodic fevers. But here a question surrounded by fearful difficulties opposes our progress—to what extent are these different diseases influenced by winds? The writers on *Malaria* present us a strange confusion of facts on this point and time does not permit us to enter as fully into its elucidation as we could wish. It seems to be a generally admitted fact that the morbific cause of Intermittent fever may be wafted several miles, *over land*, whether the *materies morbi* exists in a gaseous or animalcular form; but when we come to the propagation of Malaria *across water* we become lost in a most extraordinary labyrinth of contradictions!

Macculloch who has written the most elaborate and complete treatise in our language on Malaria, makes no allusion to the commonly received opinion of *absorption of malaria by water;* and not only gives well authenticated instances where it has been wafted to vessels 5 or 6 miles out at sea, but even goes so far as to attribute the spring Intermittents on the West coast of Great Britain, during the prevalence of Easterly winds, to Malaria which has been borne *across the sea from Holland.* He says that clouds, fogs, swarms of insects, the perfume of cinnamon groves and we might add clouds of dust may be transported by winds long distances; and if so, why may not the *materies morbi* of Intermittent fever? The doctrine of Malaria supposes a gaseous poison which rises from the earth and mingles with the atmosphere—it is clear therefore, that in the case alluded to, if there be no *local* cause of malaria on the coast of Great Britain, where these intermittents occur, the Malaria must come from Holland, as it cannot emanate from the sea. Thus, Macculloch argues, must stand the case, or the whole doctrine of Malaria be abandoned. These Intermittents come with the East wind and no other; and it cannot be maintained that an uncontaminated wind, from any point of the compass, can produce a specific disease. The East wind does not produce Intermittents in other parts of Great Britain or in other countries, and the Intermittents alluded to come only with the East wind.

Opposed to these opinions of McCulloch, which are maintained by a preponderance of authorities, we find a great many of equal respectability contending that *Malaria* cannot be propagated, or transported by winds across water, even a very short distance.

Sir John Pringle, Sir Gilbert Blane, Dr. Fergusson and others, give instances where the most pestiferous fevers could not be transported *a few yards* across water. Notorious examples of this kind have twice occurred in the English Fleet at Walcheren, one of which is related by Sir Gilbert Blane, though he gives an instance in another place of Intermittents attacking vessels several miles from land.

As an example I will select, from many, the following remarks of Sir James Johnson on the fever of the famous Walcheren Expedition.

"Nothing could more clearly *prove the limited range of marsh effluvium* than the contrast between the health of the Navy and that of the Army. Although the ships were distributed along the shores of Walcheren and Beveland, from Flushing to Batz, most of them within *a cable's length* of the banks, yet *no sickness occurred* except amongst such parts of the crews as were employed on shore and *remained there during the nights.* Most of the officers of the ships and many of the men were in the habit of making excursions through all parts of the islands by day with complete immunity from fever. The night was here, as in sultry climates, the period of danger." The entire width of the channel between Walcheren and Beveland is about 6000 feet. The history of fevers so abounds in similar facts that it would be needless to multiply them here.

How strongly do the two classes of facts, referred to by Macculloch and Johnson, contradict each other; how irreconcileable do they seem, and yet if human testimony be worth any thing, each is sustained by such a chain of testimony as to place it beyond question. As before remarked, the difficulty will probably be found to be in a false "hypothesis" and not in false facts. May not these contradictions be more rationally reconciled by *supposing a plurality of morbific causes to exist?* One rising into, and mingling with the atmosphere and obeying its motions, and another propagating itself by different laws? How else are we to explain the *facts* that Intermittents *are* transported to distant points and elevated to the height of a thousand or more feet; while Yellow Fever as certainly *creeps along upon or near the surface of the earth?* Yellow Fever is certainly very often, if not always, stopped in its progress by water, but is not impeded by rows of trees, houses, and other barriers, against Intermittents. There are numerous instances recorded of vessels lying near the wharfs of infected towns, or near other vessels, on board of which, Yellow Fever prevailed violently, without being contaminated. There are even perfectly authenticated instances where *one side or end* of a ship has suffered severely from this disease, whilst the other was entirely free from it! We can readily believe, that certain insects which are endowed with unaccountable instincts and habits, might attack a part of a ship, of a tree, of a wheat or cotton field; but we cannot imagine how a gas could be turned loose on one side of the cabin of a vessel and not extend to the other!!! Some new law of gases or emanations must be discovered by the Malaria party before they can explain this mystery.

It would appear that *Malaria* embarrasses its friends very much, not only by the irregularities of its journeyings over water, but by those over dry land. Dr. Robert Williams, the distinguished author of the work on "Morbid Poisons," says, "*different soils* also act as attracting or repelling causes which affect the transmission of the paludal poison." In addition to other similar facts, he tells us that Dr. Morton mentions the following instance in the neighborhood of Weymoth. The inhabitants of a dry district, immediately around, or on a level with the marsh, being nearly exempted from the fever which greatly prevails on the more distant hills. This same fact is also observed on the hills of Sussex." Dr. Williams adds instances of similar import which we might easily multiply. He gives no evidence of this *attraction* of Malaria by the plain, and if he were at all *practically* familiar with paludal Fevers in hot climates, he could not have advanced such an idea; for it is a well known fact in our Southern States that the morbific cause of Intermittent or Remittent Fevers will almost invariably pass *over* the low lands, *no matter what may be the character of the soil*, to attack the neighboring heights in preference. The low land rarely escapes entirely, but is less affected as a general rule than the overlooking hill.

Leaving the plain, Dr. Williams next seeks further illustration of his position on the high lands. "The different force" (he says) "by which the paludal poison is attracted by different surfaces, has been often observed in the West Indies. Fort Hildane, at Porto Maria, (Jamaica), occupies the extreme point of a promontory, which projects considerably from the main-land and divides the bay into two basin-like recesses. This promontory, which is one hundred and fifty feet above the level of the sea, and two hundred feet across, is so nearly *perpendicular* and so nearly alike in all its faces, that it has the appearance of an artificial structure, raised for the defence of the harbor. It is formed of pure carbonate of lime, and on looking at it merely as a dry mass of chalk, washed on three sides by the sea, we should imagine it to be one of the healthiest situations in the West Indies. Two streams, however, fall into the bay, one on each side of this headland at about a quarter of a mile distant. They move slowly, and their banks are covered with Mangrove, which it is presumed furnishes the more palpable cause of fever. But it is remarkable that the inhabitants of Porto Maria, which is situated on either side of one of these streams, do not appear to suffer from their position, while at Fort Hildane, the returns of the sick have shown it to be productive of a fever so deadly, that for some years past the Fort has not been garrisoned."

He relates another parallel instance, viz: at Port Spain, the Capital of Trinidad, where the town, on the marshy lowlands, is infinitely more healthy than "the covering heights, which rise out of one extremity of the marsh, and which are composed of the dryest and most healthy materials, or pure lime stone." * * * "No portion of their diversified surface, however elevated, sunken, or walled round, has been a security from the exhalations below."

Dr. Fergusson, in his oft quoted paper on Malaria, in vol. 9 of the "Transactions of the Royal Society of Edinburgh," adds his high authority to the support of these opinions. He says—"Another proof that from the attraction above mentioned, it (Malaria) *creeps along the* *ground*, so as to concentrate and collect on the sides of the adjacent hills, instead of floating directly upwards in the atmosphere, is the *remarkable fact that it is certainly absorbed by passing over a small surface of water*."

Fergusson testifies also, as do writers on diseases of hot climates generally, to the fact that Malaria is more concentrated near the ground, and consequently a much larger proportion of persons are attacked in a first than a second, and in a second than a third story; thus proving that Malaria, in *some* of its forms at least, keeps itself *near the ground.*

The "*altitudinal range*" of Malaria is a point of endless confusion; facts the most contradictory are presented to us in profusion by our highest authorities. All fevers, however different their habitudes and habitats, are thrown together, pell mell, and set down to one common morbific cause.

How are these contradictions to be reconciled? From the facts given, and many others which are easily produced, it would seem that Yellow Fever and Intermittent fevers are governed by very different laws in this respect. It must be admitted, for example, that if the *materies morbi* of Yellow Fever does not (as Fergusson says) "*creep along the ground*" like a worm, we have evidence of its existence only very *near the ground.* We have numerous examples in the West Indies, of its attacking ground floors of barracks, while the third story was almost exempt, and the truth of the fact is familiar to the profession in our Southern towns. But can the same admission be made for Intermittent Fever? There can be no doubt that ground floors, from their dampness, from various impure vapors and gases arising from the earth's surface, as also from imperfect ventilation, are unwholesome; but I am by no means satisfied that the specific poison which produces Intermittent, exists in greater force near the surface of the earth than higher up. A patient, debilitated and disordered by the causes just enumerated, might become more susceptible to the impression of this, as to other morbid poisons, and thus give support to the idea that the poison really exists here in greater force. But there is another explanation for the fact which is more satisfactory, viz: Intermittent is a disease of the country and not of the towns, and the houses out of town are almost always surrounded by trees which we are told obstruct the progress of Malaria. They are usually of a height to protect upper stories, while the winds blow without obstruction under the foliage, and may thus conduct Malaria to the lower floors. My own observation, in South Carolina and Alabama, which is by no means limited, satisfies me that lower stories have little advantage as regards *Intermittents*, though when *very near* the ground are insalubrious *in* and *out* of the region of periodic fevers. Macculloch and the other authors give us numerous instances, where Intermittents have selected *in preference upper stories*, while the *lower ones were left exempt.* The fact, too, is notorious that the summit of a hill, however precipitous, may be its sides, even a lime rock 150 or 200 feet high, as at Fort Hildane and Porto Maria, is more liable to Intermittents than the low land and marshes, which it overlooks. By what unknown power can a rock thus draw up this Malaria from a depth of 200 feet? The simple affinity of lime for moisture is inadequate to this effect, and still less when we come to elevations of 1000 or more feet.

Fergusson furnishes us with the following curious and instructive statement :

"In the Island of Antigua, the same results were confirmed in a striking manner. The autumn of 1816 became very sickly, and Yellow Fever broke out in all its low marshy quarters, while the milder *Remittent pervaded the island generally.* The British garrison of English Harbor soon felt the influence of that unwholesome place. They were distributed on a range of fortified hills that surround the dock-yard. The principal of these, Monk's Hill, at the bottom of the bay, rises perpendicular above the marshes to the height of 600 feet. The other garrisoned hill, which goes by the name of the Ridge, is about 100 feet lower, but instead of rising perpendicularly, it slopes backwards from the swamps of English Harbor. It was the duty of the white troops, in both these forts, to take the guards and duties of the dockyards amongst the marshes below, and so pestiferous was their atmosphere, that it often occurred to a well seasoned soldier, mounting the night guard in perfect health, to be seized with furious delirium while standing sentry, and when carried back to the barracks, on Monk's Hill, to expire in all the horrors of Black Vomit within less than thirty hours from the first attack ; but during all this, not a single case of Yellow Fever, nor fever of any kind, occurred to the inhabitants of Monk's Hill; that is to say, the garrison staff, the superior officers, the women, the drummers, &c., all in fact that were not obliged to *sleep* out of the garrison, or take the duties below, remained in perfect health. The result on the Ridge was not quite the same, but it was equally curious and instructive. The artillery soldiers (17 in number) never took any of the night guards, but they occupied a barrack about 300 feet above the marshes, not perpendicular above them, like Monk's Hill, but a little retired. Not a case of Yellow Fever or Black Vomit occurred amongst them, but every man, without a single exception, suffered an attack of the ordinary Remittent, of which one of them died ; and at the barrack on the top of the Ridge, at the height of 500 feet, and still farther retired from the marshes, there scarcely occurred any fever worthy of notice."

These and similar facts are brought forward by Fergusson and others to prove the identity of these different types of Fever—they say that the concentrated poison which produces Yellow fever below, in rising to the hill, becomes so diluted as to produce only the milder forms of paludal fever. This explanation is untenable, because it is a general law of intermittent and remittent fevers that they every where affect, more the hills than the lowland from which they emanate, not only in Yellow fever localities, but in those regions *where Yellow fever is unknown.* I have shown elsewhere that yellow fever is a disease of towns, and intermittents of the country, and that yellow fever occurs, (as at Barbadoes) where intermittents do not ; and that if *dilution* of the poison had anything to do with the matter, the winds blowing over points infected with yellow fever should strew intermittents before them.

Even Fergusson with all his talent, learning and industry, by taking a false position, has involved himself in the same inconsistencies as other writers on Malaria. In the very article I have been quoting from, he says: "In selecting situations for ports and barracks, it had been observed with surprise, that the border and even the centre of the marsh, proved a less dangerous quarter than the *neighboring heights* of the purest soil and healthiest temperature; and this has never been more strongly exemplified than in the instances I am going to relate." He then goes on to relate the instance already alluded to of Port Spain, Trinidad—where the heights overlook the town. He says—"no place however elevated, or sunk, or sheltered, or walled in, gives security against the exhalations from below, only it *has been distinctly ascertained, that these prevail with more or less malignity, exactly in proportion to the elevation of the dwelling.* The lower, consequently the *nearer the marsh,* the better. The tops of the ridges are uninhabitable—on the highest point, at an elevation of 400 feet, and farther removed from the marsh than the town itself, a large martello tower was built to defend the place. It possessed a fine temperature, but proved so dangerous that it was obliged to be abandoned; not even a creole mulatto Spaniard could sleep in it with impunity for a single night, after a course of dry weather."

In what an awful fog do these statements leave us! At one moment the poison gets *weaker* as it *goes* up; and the next, it becomes *concentrated* in the same ratio as you come down!

These strange and apparently whimsical habits of the morbific cause of Paludal Fevers would seem to corroborate the opinion that it is not governed by the motions of the atmosphere, and that it is more under the control of some *mysterious instinct.* In 1842 and '43, when yellow fever was progressing literally at a snail's pace, through the town which was constantly swept by the sea or land breezes, I confidently advised persons *out of the infected district*, to leave town, before the disease reached them, and seek safety in the country. Those who took the advice escaped, while those in their vicinity who remained, were reached and many attacked by the yellow fever.

Dr. Williams, in his chapter on "the Paludal Poison," after detailing a number of curious facts illustrative of the *Altitudinal* and *Horizontal range* of Malaria, says:—"The preceding facts are sufficient to show, if the altitude to which the paludal poison ascends, greatly varies in different places, its horizontal spread also varies according to the surface over which it passes, being strongly attracted by some bodies and altogether without affinity for others. In attempting to assign the law which may explain these varying, and often apparently opposite phenomena, there is no hypothesis so satisfactory as that which supposes the paludal poison to follow the same laws as those which govern the vapor or dew, by which it is held either in a state of solution or suspension, and which may be generalized as follows, &c."

He then branches off on the theory of dews—tells us of the radiating, reflecting, attracting and absorbing properties of different plants, soils, rocks, &c., accounts for the fevers on the hills at Porto Maria and Port Spain by the affinity of the *lime* rocks for moisture—explains the influence of water over malaria by its power of condensing the dews when it is colder than the air, and by its repelling the dews under the opposite circumstances by throwing off vapor from its surface. In short, Dr.

Williams and others have not only exhausted all the known laws of dews in sustaining their hypothesis, but have resorted to many forced constructions, and yet all falls far short of being satisfactory.

His recapitulation of the theory of dews is by no means well done, and I therefore prefer giving the established facts in my own way. The instructive little work of Griffiths on the " Chemistry of the four Seasons" presents us with some very clear illustrations of several of the points, which may be quoted with advantage. " It is an old saying that the *hills draw the wet*, or " *hills draw the clouds*," but they have no inherent or particular attraction in this respect; they *are only surfaces of the earth projecting above its common level.*" In this he is unquestionably correct,—there can be no " inherent or particular attraction" in a part of the same soil merely because it has been thrown up 50 or 500 feet above another portion—the difference of height is often too trivial for any appreciable Barometrical change which could influence the *elevation of vapors*; and the appearance of marsh fevers on elevations cannot be accounted for on the supposition of attraction of soils in moisture (as the lime hills spoken of), because it is notorious in our Southern States that the composition of the hill is immaterial;—wherever the morbific cause of intermittents is generated in the valley, the exposed hill which overlooks it, is attacked whether it be sand, clay, lime, rich dark vegetable mould, &c.; the color too, as white, black, red, gray, &c., makes little if any difference.

Lime certainly has a greater affinity for moisture than clay or sand; and yet so far from its being true that hills of this composition precipitate more moisture than the valley, the reverse will usually be found true. It is well known that plants, as grass, grains, cotton, trees, &c., and the rich *black* soils of the swamps are the best radiators of heat and consequently the best condensors of moisture. It is therefore in the luxuriant valleys, and not on the light colored hill, that we should expect, and really do find, the heavy dews.

" If, " says Mr. Griffith," the thermometer be placed on a grass-plat, it will very frequently indicate a temperature of 15 or 20 degrees colder, than one suspended over the grass at the height of 3 or 4 feet—thus proving that radiation is proceeding with extreme rapidity in the one case from the comparatively solid vegetable and soil, but not from the ambient air." Accordingly, the portions of air in immediate contact with the grass, become much colder than those far above, and are compelled to deposit dew; and if the air over a green locality remain tranquil for some hours, this phenomenon will solely ensue from the lower and colder portions."

" Small valleys and hollow ways, permit the air to remain undisturbed; and although they are apparently situations sheltered from cold, yet they are frequently more subject to reduction of temperature than higher situations, and accordingly, much to our surprise, we find delicate plants chilled or even frost bitten in hollows, whilst others suffer no injury upon the adjacent slopes."

How common is it in autumn when we look out upon a meadow or corn field, to see a dense white mist only a few feet in height directly incumbent on the surface, whilst the air is clear and bright above with the rays of the sun. Or we may even walk through the field with the lower part of the body enveloped in the mist, whilst the upper is free from its humidity.

There would then seem to be no known law of *dews*, or any other by which malaria can be transferred in a more concentrated form from the valley to a great elevation. It is evident from the facts given that if the poison be entangled with vapor, it should be most concentrated where the dews are heaviest, viz: on the dark, rich soil of the valley, covered with vegetation and not on the barren hill top. If, too, it be true that the malaria is entangled with dew, it is difficult to imagine how it should be so much more active at night—how does it rise up from the valley and ascend to the mountain summit 1000 or more feet (of which we have examples) at a time when the vapors not only cease to rise from the earth, but are rapidly *depositing* on the cold surface of the lowland?

It has been said that the malaria is active only at night-fall when the dews first descend and in the morning when they again are called up by the sun. But this is not true for several good reasons—1st. Malaria is often generated on a very small spot of ground, and the emanations which arise from this like the smoke from the chimneys must necessarily be borne off to the distance of several miles, and would not descend upon the overhanging hill as at Port Spain, or Porto Maria—2nd. Instances are common where persons have contracted fevers by going during the night to the infected district—as in the cases mentioned of the sentries at English Harbor—those travelling through the Pontrine marshes at night, &c. It is a familiar fact in Charleston, South Carolina, that it is hazardous for persons to leave the city and go into the neighboring marsh lands at night; and the danger is greatly increased if they allow themelves to fall asleep—instances might easily be multiplied. It is worthy of remark, too, that when the cool nights of autumn arrive, the morbific cause seems to attain its greatest virulence, though the valley from the increasing coldness of its surface, greatly increases in its power of condensing moisture, and it need hardly be said that a surface cannot condense and throw off aqueous vapor at the same time. So marked is this phenomenon, that we often see repeated light frosts in the valley and even thin sheets of ice on the ground, while the thermometer a few feet above in the air does not fall below 40 degrees of Fahrenheit—at the same time the hills are entirely exempt from the frosts and their vegetation remains untouched.

As far as we have any means of judging, all emanations are more concentrated as we approach the focus whence they originate, and if there is any analogy between these and Malaria, its greatest concentration should be in the valley. It is not on the hill, but in the marsh that our olfactories are offended by its putrid odors; so is it with putrid animal matter, and odors of every description. In the interior of South Carolina, away from the influence of the sea breeze, the nights are usually very calm and sultry—far more so than in the prairies of the West; and we should here expect (according to the theory we are combatting), that near the rivers and in small valleys where the fogs lie near and on the surface undisturbed during the autumn, the poison should produce its deadliest effects; but the hill here too suffers more than the valley. The idea that the hill suffers most, because the currents of air cause the fogs to *infringe on the hills* is alike untenable;

a wind rolling along this heavy mass of fog over a wide plain, and bearing it up to the top of a hill, could not certainly render it more concentrated.

It would certainly be quite as philosophical (as the Malarial theory) to suppose that some insect or animalcule, hatched in the lowlands, like the musquito, after passing through its metamorphoses, takes flight, and either from preference for a different atmosphere, or impelled by one of those extraordinary instincts which many are known to possess, wings its way to the hill top to fulfil its appointed destiny.

6th. "Experience has proved, that in cities where Yellow Fever prevails, the places which are low, pent up, and badly ventilated, such as narrow streets, alleys, cul de sacs, first and second stories, &c., are the places most dangerous to inhabit—observation has shown that it is the same with intermittent fevers."

I must here join issue again with M. Chervin, and feel confident that every physician of observation in these diseases, from Boston to New Orleans, will sustain me in a denial of this conclusion. All observation and all facts contradict it. Yellow Fever is a disease of the towns and Intermittents of the country. The City of Charleston stands in the midst of a very pestiferous region, where all the grades of periodic fevers prevail, but no Yellow Fever. Since the town was built a new local atmosphere has been engendered which has expelled the periodic fevers, and introduced the *new disease, Yellow Fever*. The experience of the whole world proves that whenever a large town is built Intermittent is expelled from the soil, though it may still hang about the outskirts. Even in Rome, which is surrounded by one of the most poisonous miasmatic regions in Europe, Sir James Johnson, in his "*Change of Air*," tells us, "The low, crowded, and abominably filthy quarter of the Jews, on the banks of the Tiber, near the foot of the Capitol, may probably owe its acknowledged freedom from the fatal Malaria, to its sheltered site and inconceivably dense population."

An *Epidemic* of periodic fevers, in a large town, is a thing unheard of. The bills of mortality of Charleston, Mobile, and New Orleans will show that when these cities escape Yellow Fever, they never have Epidemics of other fevers.

7th. "It is a well known fact that the miasms which produce periodic fevers, are infinitely more active in the night than the day. Those which give birth to Yellow Fever possess likewise extraordinary power of action when the sun is below the horizon."

The concession, were we to yield this point, would be unimportant in establishing the identity of these diseases. At all events, much that we have already said so far disproves the position as to make but a few more words necessary, in reference to it. Though fully aware of the opinions generally expressed to the contrary by writers, I am by no means sure that a difference does not here exist between the two diseases. The Campagna, the Maremma and the Pontines we are told may be traversed with impunity during the day, and many facts are given of the same nature in relation to Yellow Fever; but I am satisfied that persons often take Yellow Fever by coming into Mobile during the day for an hour or two, though the risk is certainly much greater at night.

A strangely absurd reason is frequently assigned for the increased activity of the morbific cause of Yellow Fever at night. It is said that the poison is volatilized by the heat of the sun during the day, and being entangled with aqueous vapor, is precipitated with the dews of evening in a condensed form. Not a particle of proof is given to sustain this assumption, which is contradicted by well known facts. In Charleston, for example, where Yellow Fever occurs, while periodic fevers prevail for many miles around, the facts would be reversed. The town and country are almost incessantly swept by the land and sea breezes, and at night the vapor from the marshes of the country should fall on the town, and produce "country fevers," while the Yellow Fever emanations would fall at some distant point in the country. The Yellow Fever sometimes is so circumscribed as to be confined to a single alley or very small portion of the city.

All the attempts heretofore made to account for the greater activity of the morbific cause of Yellow Fever at night have failed, and in my humble opinion the fact may be much better explained by a reference to habits of Insect Life. Many of the Infusoria, as well as insects proper, are rendered inactive by too much light, heat, or dryness. They remain quiet through the day, and do their work at night. This fact is too familiar to require illustration. The moth tribe, the night musquitoes, many of the Aphides, &c., are familiar examples.

8th. "In the Equinoxial regions, Yellow Fever attacks in general, almost exclusively, unacclimated persons; intermittent and remittent fevers attack also in preference (though M. Gerardin has advanced the contrary opinion in this assembly) those subjects who come from a healthy into marshy countries."*

9th. "Yellow Fever attacks particularly strong and vigorous men, who pass from northern to southern climates; it is the same with intermittents and remittents, as is abundantly proved by the successive occupations of Italy, Spain, the Ionian Isles, Morea, and Algiers, by our troops."

It is true that Yellow Fever attacks almost exclusively the unacclimated, and selects in preference the robust natives of cold regions, but I have strong reasons for believing that M. Gerardin is correct in denying the applicability of this rule to periodic fevers. Such persons, in consequence of their sanguineous temperaments, are more apt when attacked, to suffer severely than the natives of hot climates; but they are not more liable, if so much, as the latter to take these diseases. No population can ever be acclimated against periodic fevers, and the fact is equally certain that every attack of Intermittent increases the susceptibility to others. There is no place in the United States where the population can become acclimated against "marsh fevers." While the acclimated population of Charleston are living (as I have shown by the bills of mortality) in greater health than that of any large town in this country, the inhabitants of the surrounding country suffer from all the horrors of miasmatic fevers, and present all the physical signs of an enfeebled, degenerate race. Go to the Campagna, the Maremma, the Pontines—even to the "Eternal City itself—and ask for the de-

* The pamphlet from which I quote is a report of M. Chervin, read before the Acadamie Royale de medicine in 1842—page 120.

scendants of the proud Romans, who, two thousand years ago, held the world in bondage! The ghastly picture of the population drawn then by Cicero and Horace, has only become more hideous with time. The miserable inhabitants are now, even more than then, skulking from the fatal Malaria, and hiding themselves on the mountain tops and in the crevices of the rocks; and the jaundiced skin, the bloated abdomen and withered limbs point to the physical degeneracy of the race.

If there are apparent exceptions, in some parts of the world, to the above facts, it is probably only because different forms of fever have been unproperly attributed to one cause; but the face of the globe cannot show an exception, where genuine Intermittent fever prevails.

We may refer in illustration of this point to Fergusson and the other army and navy surgeons who have written on the diseases of the Peninsular War, and whose statements, in some particulars, seem to conflict with opinions advanced by me. They mention instances where the troops suffered much more than the natives, a result reasonably to have been expected, even if their susceptibilities were equal, when we take into consideration the exposure—sleeping on the ground, and various hardships which they endured. The facts given below are sufficient to excite strong doubts as to the *identity* of the diseases with which the troops and natives suffered. All the descriptions of those fevers I have seen are incomplete and unsatisfactory.

Mr. Fergusson, in his admirable paper on *Marsh Poison*, before alluded to, amongst other interesting particulars, give the following: The English troops, after the battle of Talevera, retreated into the "dry, sandy, rocky plains" of Estramadura, "at a time when the country was so dry and arid for want of rain that the Guadiana River itself and all the smaller streams had in fact *ceased to be streams*, and were no more than lines of detached pools in the courses that had formerly been rivers; and there they suffered from Remittent fevers of such destructive malignity, that the enemy and all Europe believed that the English host was extirpated; and the superstitious natives, *though sickly themselves*, unable to account for disease of such *uncommon type amongst the strangers*, declared they had all been poisoned by eating the mushrooms which spring up after the autumnal rains, about the time the Epidemic had attained its height. The aggravated cases of the disease differed little or nothing from the *worst Yellow Fever of the West Indies*."

The English army surgeons present us many other facts of similar import, and the well known habits of the fevers of our Southern States would lead to the conclusion that Fergusson, as in the Case of Antigua before spoken of, confounds distinct diseases. A camp disease of obscure origin was probably generated, different from the paludal fever of the natives, who were from long residence in the climate proof against this malady which "differed little or nothing from the worst *Yellow Fever* of the West Indies." The same astonishment was probably expressed by the Aborigines of the West Indies when the foreigners were first swept off before their eyes with Yellow Fever—a disease to them of "uncommon type."

Fergusson tells us, too, that the Estramadura fevers occurred in a season and locality, where the extreme dryness of the sandy soil and the paucity of vegetable matter precluded all idea of vegetable matter being a cause. Vegetable matter is *certainly* not a cause of Yellow Fever; and I do not see how any one who would travel through the alluvial country of Louisiana, where all the causes of Intermittents exist in the highest possible degree *without these diseases*, and then follow through the chain of facts in this paper, can have any *settled belief* in vegetable decomposition as a cause of Periodic Fevers. When I speak of "all the causes of Intermittents" I mean those usually assigned as rich soil, vegetable matter, stagnant matter, both wild and cultivated lands, hot climate &c.; all that the malarial "hypothesis" could possibly ask, is here in profusion, but *no fever;* while the "dry, rocky, sandy" desert of Estramadura is uninhabitable, at least to foreigners, and the natives are also sickly.

10th. "The individual who has contracted an Intermittent Fever in a marshy place, attenuates the effect of the poison and hastens his recovery by going to reside in a healthy locality; the same thing happens with the Yellow Fever, but in a less marked manner, because in this case the paludal "*intoxication*" is more rapid and is carried to a higher degree."

There is certainly no parallelism between the cases here; but on the contrary the most marked contrast. The poison of Intermittent Fever is so *adhesive* that the London writers tell us, that persons returning to that city (where Intermittent is *unknown*) with seeds of the disease contracted in the West Indies, will continue to *relapse for twenty years.* In spite, too, of travel, mineral waters and all other remedies, Periodic Fevers will often leave enlargement of the spleen, disorders of the liver, dyspepsia and other chronic affections to haunt the victim for life. How different is it with Yellow Fever? Like Roderick Dhu, it scorns all unfair advantage and nobly "tries the quarrel hilt to hilt," and when the "dubious strife" is over, if his antagonist has proved the victor, he may "falter thanks to heaven for life redeemed, and rise unmolested by the "*foeman's Clan.*"

There are rarely sequelæ to Yellow Fever, and strange as the assertion may seem to those unacquainted with this disease, I have seen more cases of dyspepsia cured by attacks of it than by all the doctors of my acquaintance.

The last part of the above quotation is equally erroneous;—there is no evidence that the poisoning in Yellow Fever "is more rapid and carried to a higher degree" than in other forms of what are termed "paludal fevers." The high grades of bilious and congestive fevers are quite as rapid—as unmanageable and fatal as the most malignant forms of Yellow Fever. In the interior of our South Western States, where Yellow Fever is unknown, these fatal forms abound—often causing death in a few hours.

11th. "Finally all the differential signs which are said to exist between Yellow and Remittent Bilious Fevers of hot climates are absolutely without foundation, such as the appearance of the eyes, nature and seat of the pain in the head, absence of remission, color of the skin, duration of the disease, morbid state of the stomach—nature of the matters vomited, immunity produced by a first attack, mode of treatment &c.

If a light remittent fever be compared with a very intense Yellow Fever, we shall without doubt see very marked differences in the symptoms of the two affections; but if we put beside a severe remittent a mild case of Yellow Fever, we shall see none; for as remarks Doctor Repey: "There is a point where these fevers are so confounded, that they really become one and the same disease," the same affection under different forms and various degrees."

When a writer starts in a wrong direction, the farther he goes, the farther does he wander from the path of truth. Such I fear has been the case with our "estimable ami" Monsieur Chervin (with whom, by the by, we had the pleasure of a personal acquaintance) and we must say of him as he said of Rochoux: "il a observé la fièvre jaune assez long temps pour la bien connoître, mais malheureusement il l'a vue avec une opinion préconçue"—and I cannot help thinking he would have come to very different conclusions had he, as I have done, sat down quietly in one place and studied Yellow Fever through all its grades and changes, instead of running incessantly from place to place for eight years in search of facts. It would at first glance seem a matter of surprise that one who has sacrificed so much time in the cause, and who has written so well on the point of Contagion, should have so erred on other points; but a moment's reflection should satisfy us that by the course he adopted he necessarily had to take the testimony of others (most of whom were *not observers*) instead of observing for himself.

We have no space here to follow out the line of demarcation between the two diseases by comparing their Pathology and Symptomatology, and must rely principally on the difference of habits &c. already treated of. Diagnosis, between two diseases, even the most opposite in their causes and nature, is often embarrassing; but how much more difficult is it to lay down conclusive diagnostic signs between diseases of the same genus, though different species. If a physician were called in the forming stage of a number of cases of Plague, Small Pox, Yellow Fever, some forms of Typhus, and other diseases arising from Morbid Poisons, as well as certain vegetable poisons, he would be much at a loss how to distinguish them for two or three days; and in some of those in which the characteristic signs are never developed (as Small Pox without eruption &c.), a diagnosis never could be made. It should not be wondered at then, that difficulty of diagnosis should sometimes occur between Bilious and Yellow Fevers, which belong to the same family, the same season and same locality.

Another strong reason for this difficulty of diagnosis is found in the fact that no two Epidemic or atmospheric diseases can possibly prevail together without becoming blended. When Yellow Fever prevails, as I have seen it, in a milder form than what we term Epidemic, it is invariably seen more or less blended with the Intermittents and Remittents of the environs—they are mingled in every possible grade. Andral, in speaking of the influence of Epidemics over other diseases, makes the following pertinent remarks.

"But on all these diseases, differing in their seat, it impresses a uniform modification; it brings them to an *identity* of nature, and consequently an identity of treatment. It is therefore much less important in therapeutics to know the seat even of a disease than the "*Epidemic Constitution*," under which it has taken birth; for it is on this constitution that the treatment should be based." He goes on to illustrate, by giving instances of the "Inflammatory Constitution," the Bilious Constitution, the Mucous or Catarrhal Constitution, the Putrid &c., during which a Pneumonia or other inflammatory disease would require "the most *opposite treatment.*"

I, on a former occasion, explained more fully the nature of those cases which are termed Intermittent and Remittent Yellow Fever. In 1844 many of these cases occurred in Mobile—in this year there were only 40 deaths from Yellow Fever and no *Epidemic Constitution* of the atmosphere was established—the two diseases struggling for mastery, with nearly equal force, were blended in every conceivable degree in different subjects—sometimes the Periodic and at others the true Yellow Fever type predominating—the periodic type preponderating particularly in the suburbs, near the marshes. The cases were sprinkled over the whole town without being confined to any particular focus.

No one at all familiar with the history of Epidemics could doubt this tendency of diseases to amalgamation; if there should perchance be a Sceptic, let him wade through the four volumes of Ozanam "des maladies Epidemiques," and the facts will bring him to the conclusion which reason points to.

Suppose a Rattlesnake or a Tarantula were to bite a patient laboring under Intermittent Fever, or he should swallow a large dose of vegetable poison—what would probably follow? The effects of the two poisons would be blended, and the stronger would predominate over the weaker—after the subsidence of the effect of the newly applied poison, if the patient survive, should we be surprised to see the Intermittent recur and resume its regular course? Ozanam tells us that when Small Pox is prevailing in the East, the plague will sometimes come and drive it from the field. After a certain time a few scattering cases of the dormant Small Pox reappear, and this is looked upon as a sure sign of disappearance of the Plague, and the Small Pox about to resume its course. Williams in his "*Morbid Poisons*" says—"The variolous poison is capable of coexisting with many other poisons; also of influencing their actions and being reciprocally influenced by them.—Dessessarz has seen Variolæ coexist with Scarlatina and with hooping cough; Cruikshanks, with Measles; Frank, with Psora; Dimsdale, with Syphilis; and Heberden, with Intermittent Fever, who adds in his commentaries a case of this latter complication lately occurred in St. Thomas's Hospital. A patient was admitted laboring under tertian fever, which was unusually intractable and resisted quinine. At length, however, the variolæ appeared and the fever subsided; but no sooner had the eruption run its course, than the intermittent again appeared and was now readily cured by the usual means. Ring even mentions a case of triple disease coexisting, or of the Small Pox, the Measles and the hooping cough, all of which ran their course together."—It is needless to multiply facts on this point as they may be found in the works of Williams, Ozanam and other writers on Epidemic diseases.

In short, what is our whole system of Therapeutics based on, but the modifying influence which one impression on the system exerts over another. Why do we give mercury to cure Syphilis—quinine to cure Intermittent, &c., unless to counteract the action of one poison by that of another impression. Why are we so cautious in selecting a proper time for administering opium and other drugs. In a word, it is evident that the skill of the physician depends entirely upon a proper selection of modifying agents and time for their administration.

The subject of *Morbid Poisons* is one of incomprehensible difficulties. Epidemic diseases, as Influenza, Measles, Scarlet Fever, Small-Pox, Hooping-cough, &c., often prevail so together, or follow each other in such a mysterious manner, that some writers, as Holland and others, have suggested a common morbific cause, variously modified by season, climate, meteorological changes, temperaments, &c. &c. This opinion has not gained much favor with the profession; but the *fact* stands, that diseases which are regarded as the most opposite in their causes, symptoms, pathology and duration, are sometimes strangely allied.

A singular instance has twice occurred in Mobile during the last few years—viz: an amalgamation of Measles and Scarlet Fever—I have seen in the same house (as have other physicians) a case of pure measles, another of pure Scarlet fever, and a third in which the symptoms of the two were so commingled as to render it impossible to say which predominated—these mixed cases commenced with all the symptoms of measles, as inflammation of the eyes, catarrhal symptoms, sneezing, distinct measly rash, &c.; and in a few days a putrid sore throat, and scarlet fever tongue would appear, and if the patient survived, all the sequelæ of Scarlet fever, as affections of the ears, extensive desquamation, dropsical effusions, &c. It is very remarkable that some of these cases were still farther complicated by distinct chicken pox, thus showing a co-existence of three diseases generally regarded as distinct.

When Yellow Fever shakes off its mild endemic form and assumes that of a great *Epidemic*, as it did here in 1839, it comes robed in majesty and power—all febrile diseases disappear before it, or are compelled to wear its livery—the peculiar characteristics of the disease stand out boldly, and with few exceptions, all difficulty of diagnosis vanishes—patients are stricken down by hundreds with attacks varying from the mildest to the most malignant and yet all wholly unlike periodic fevers—in the same family and house, one will be so lightly attacked as scarcely to lie down, while another is dying with all the horrors of black-vomit; and what is particularly worthy of note, the light cases pass off spontaneously in two or three days *without a dose of quinine*, and afford *protection against the disease in after years*.

We are led to conclude from the mass of evidence on this point, that yellow fever varies much as to type in different localities—in extremely hot climates for example, as in Asia and Africa, the excitement is more intense, and the brain is more uniformly and violently affected. It is well known that every morbid poison influences different individuals in very different degrees—a familiar illustration may be seen in the degrees of violence presented by Scarlet Fever, Small-pox, Typhus, &c., where persons have been equally exposed to the morbific causes. The same variety is seen amongst the cases of yellow fever. One will, as we have already said, have it very mildly, while another will be struck down speechless, as by appoplexy, and die in a few hours. Yet there is quite as much uniformity in the symptoms of yellow fever as in other diseases arising from morbid poisons. Leaving however what may be called anomalous cases, the disease is every where in its pure form, characterized as a *fever of one paroxysm.* The following is the ordinary type of the disease in Mobile. The subject while in perfect health is seized with a slight chilly sensation which occurs either during the day in the midst of his avocations, or he is awakened by it at night during a profound natural sleep—acceleration of pulse to 100, to 110 or 115 beats in a minute soon follows, accompanied by *moderate thirst*, and most excruciating pains in the head, back, and limbs—the acceleration of pulse and thirst are not at all in proportion to the violence of the pains and general anxiety—often the pulse during the fever does not exceed 90 and the skin is of natural temperature and perspiring all the while.—After about 40 hours the fever subsides and the patient is left in a state of calm, called by some a *remission*, during which there is frequently such a complete absence of all external signs of disease, that a physician unaccustomed to yellow fever, would not hesitate to pronounce the patient out of danger and convalescent. This calm lasts another 40 hours, and is followed by the stage of collapse in which there seems to be a sudden and almost complete exhaustion of the vital powers—during this last stage the patient usually requires stimulants, such as brandy, porter, &c., and if he does not sink with or without black-vomit, the disease *runs its course*, and by the 6th or 7th day, he enters fairly into convalescence. There is no fever after the first paroxysm, unless the lesion of some organ again calls the heart into action—a second fever is not a *necessary part of the disease.* Another striking peculiarity of yellow fever, too, is the entire absence of bilious vomiting after the paroxysm of fever has passed—if perchance you see a blue, green, or yellow tinge in the clear fluid vomited, you may hail it as the harbinger of safety—the prognosis is almost certain. Contrast these symptoms with those of bilious fever,—each in its distinct uncomplicated form, and where I would ask is the *identity* of which M. Chervin speaks?

In 1839 Yellow Fever in Mobile assumed its highest *Epidemic* form—it not only overwhelmed the town, but, gathering extraordinary strength (like the Cholera) it burst over its accustomed bounds and ravaged the habitations around for several miles. There was something, I presume, peculiarly favorable to the generation of its morbific cause this season, for it occurred in violent form in nearly all the towns on the Gulf of Mexico.

Admitting, as has been argued, that genuine Yellow Fever does occasionally present the Intermittent type, with a succession of paroxysms, the fact would deserve little weight in settling the question of identity. *Intermittence* is an unexplained pathological fact when connected with any disease. Many diseases, in opposition to their ordinary phenomena, may assume the intermittent type—as Neuralgia, Opthalmia, Paralysis, etc. Even Pleurisies, Pneumonias and other inflammatory disorders, in our latitude, frequently assume the bilious remitting form. What are termed Bilious Pneumonia and Bilious Pleurisy, are Phlegmasiæ proper, modified by the morbific cause (Malaria) of Periodic fever.

It was not my plan to argue the Insect origin of Periodic fevers in this paper, but the morbific causes of Fevers have been so long and so inseparably united in the minds of the profession that it is almost impossible to tear them asunder now.

All writers are agreed on the fact that a very imperfect barrier will obstruct the progress of *marsh miasmata*—a row of houses or of trees, etc., will often effectally protect dwellings from the access of this fatal poison. It is moreover asserted that these miasms are not only impeded, but *attracted* by trees, and this would seem to be the case from the well known fact that the danger is greater from sleeping in a cluster of trees, than in an open space.

I have been able, in my researches, to discover no facts of this kind in connection with Yellow Fever, and my personal observation repudiates them *in toto*. We never find Yellow fever as the Sportsman say "up a tree," but on the contrary, the *materies morbi*, whatever it be, creeps along the ground, regardless of winds, passing under and through houses, trees, etc., and knowing no impediment but a sheet of water.*

The Insect theory is perhaps as applicable to Periodic as Yellow Fever. We can well understand how Insects wafted by the winds (as happens with musquitoes, flying ants, many of the Aphides, etc.,) should haul up on the first tree, house or other object in their course, offering a resting place; but no one can imagine how a gas or emanation, entangled or not with aqueous vapor, while sweeping along on the wings of the wind, could be caught in this way; and we, on the contrary, often see fogs and clouds swept by winds *through the forest*. Another insuperable difficulty, too, is found in the fact that the dews are deposited as heavily on the one side as on the other of the protecting woods. I have very strongly impressed on my memory an instance of this kind: at my father's summer residence in South Carolina, our house stood upon a hill which gradually declined for half a mile till it terminated in the lowlands of the plantation; a row of trees, which were so scattered as but imperfectly to obstruct the view of the fields below, stood about midway between the latter and the residence; though the fact was inexplicable, this imperfect barrier *did* protect us, and our family lived there for fourteen summers, with uninterrupted health. The trees presented scarcely any impediment to the force of the winds, and *I never saw heavier dews than those on the rich grass plat around the house.* After my father's death, the old residence fell into the hands of my brother-in law, and the protecting row of trees having been cut down, it has become so subject to marsh fevers, that he has been compelled to abandon it.

If these emanations are *attracted* by and attached to trees, how do they get loose again and come down to attack persons in *lower stories?* They should remain on the trees until again evaporated by the morning's sun—these miasms must have some power *per se* of migrating, and clustering in trees, else these facts could not exist. It should be borne in mind, too, that the very writers who thus run their Malaria up trees, are those who tell us that its specific gravity is so great that it lies on the ground!!

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* It is a curious fact that from 1829 to 1837 there was no Epidemic of Yellow Fever in Mobile, and during this time the streets were beautifully shelled; since '37 we have had it five times, and the shelling was not continued. If the Insect theory be correct, could the lime be an impediment to their progress across streets?

CONTAGION.

If by this term we understand that a morbid poison generated in one living body may by contact, either mediate or immediate, reproduce an identical disease in another, then are we justified in denying that Yellow Fever is a contagious disease. But while without hesitation I take this position, I am equally strong in the conviction that there exists no conclusive evidence, that the germ or *materies morbi* may not be transported from one locality to another. There are many curious facts connected with this question which require a passing notice.

The Insect theory here again comes to our aid, and may explain difficulties which have much perplexed writers on contagion. The early history of Yellow fever is involved in great obscurity, and many of the very highest European authorities believe that this disease was imported originally into the old world, and that it still may be transported from one country to another. There is no time here for discussing this point, and I will only say that the mass of authority in favor of this opinion is such as to challenge our full respect; no reasonable man, in the present state of facts, can assert positively that Yellow Fever may not under peculiar circumstances be transported.

I have shown that Yellow Fever often commences in a point from which it gradually extends from house to house for several weeks—now, it is clear, that in this case there must be a local, though invisible cause—it cannot exist in the atmosphere, as it could not, if thus diffused, be confined to a point. Supposed the infected point and a few surrounding acres of ground were taken up in August and put down in the centre of New-York or Philadelphia, is it not probable that the disease would spread from this point as in Mobile? If so, why may not the morbific cause be carried and thrown out of a vessel with a cargo of damaged coffee, potatoes, grain, sugar, meat, etc.? The germ might here find a hiding place, though I have no idea that the gaseous emanations from these vegetable or animal substances could produce Yellow Fever. We have no reason to believe that such emanations, differing so widely in themselves, can produce *one specific* disease.

We have evidence around us almost constantly that the germs of Insects lie dormant for indefinite periods and are then suddenly called into activity and propagated with inconceivable rapidity. By what physical causes these sleeping and waking states are governed, human sagacity cannot yet divine.

Involved in equal mystery are the habits, mode of propagation, etc., of contagious diseases. Small Pox, for example, is a highly *contagious* disease, and yet has its periods of activity and repose—at one time it disappears entirely—at another a few sporadic cases are seen—again we see it scattered irregularly here and there, and lastly it comes as a great Epidemic sweeping over a whole nation.

Small Pox, though known in China 2000 years earlier, was not carried to Europe until somewhere about the beginning of the 8th century A. D., and the fact is equally certain that it was not known in America until brought here by Columbus. There are strong reasons for believing that Scarlatina and Measles were also imported from Asia—yet these diseases have become perfectly domesticated in this country, and preserve all their ancient habits. Every now and then we hear of cases of Small Pox, occurring in localities removed from the thoroughfares, where it has never been known before, and under circumstances which render it impossible to trace its origin—still it must be carried to such points, for the disease is only propagated by *contagion*, as it was unknown in Europe or America till imported.

Small Pox, Scarlatina, Typhus, etc., are transported not only in their mature form, but in the form of fomites. In the latter case the germ is united in some way to clothes, furniture and other inanimate substances, for indefinite periods, probably for years, and then from unknown causes is roused into activity. Typhus is sometimes carried about and spread in its most malignant form by persons who are not affected by the disease—the memorable instance of the *Black assizes*, Old Bailey, in 1750, when the Lord Mayor, two of the judges and other eminent persons, took the disease from prisoners and died of it, is often alluded to.

"It is probable that Yellow fever is caused by an insect or animalcule bred on the ground, and in what manner it makes its impression on the system, is but surmise—unless the animalcule is, like that of Psora, bred in the system, we could no more expect it to be contagious, than the bite of a serpent. We may therefore easily understand, that it can at the same time be transportable in the form of a germ, and yet not contagious."

Without wishing to take so broad a ground as insect origin for all, I must say that those diseases arising from morbid poisons, present strong analogies with insect life. The Itch is a contagious disease which may be transported from place to place in all seasons and all climates, and is unquestionably *propagated by insects*. Like other contagious and epidemic diseases it prefers filthy places and persons of filthy habits. Other cutaneous affections have their origin in animalculæ, and M. Donné, one of the best microscopic observers of the day, asserts that the pus of Buboes contains animalcules, which account for the transmissibility of Syphilis.

Having no favorite hypothesis to sustain, and no other end in view but truth, it is proper to state that I have never myself witnessed any facts which would add much strength to the opinion that Yellow Fever is transmissible. There is, however, a mass of facts collected by numerous authorities on this point, which must be received at least with respect. The appearance and spread of this disease has often been mysteriously connected with the arrival of vessels from Yellow Fever ports—as in the case of the black assizes, a vessel might originate the disease, though no case had occurred on board during or before a voyage. At the time I am writing, Yellow Fever has appeared in Mobile and New Orleans *a month earlier than it has been known* for a number of years, and in the midst of heavy rains which had fallen every day for a month preceding the disease. Vessels have been, for some months, in consequence of the Mexican war, coming, in unprecedented numbers, from Vera Cruz and other ports where Yellow Fever was prevailing. Now although we cannot point to the chain of cause and effect, the circumstances in connection with the strange habits of diseases known to be transmissible, are sufficient to excite suspicion.

The remarkable manner in which Yellow Fever occurs in our northern cities, where it does not dwell, and where the natives cannot be acclimated against it, would seem to lend support to the idea of importation. I do not recollect any *Epidemic* of this disease in Boston since 1693. In Philadelphia it had not been seen for more that thirty years previous to the memorable 1793, when it began to assume an activity hitherto unknown at the north; and in the latter part of that century and beginning of the present, it occurred frequently, not only in the large but the small inland towns, as Catskill, Winchester, Middletown, and numerous other points in the Eastern and Middle States. Since 1823 the disease has not been known north of Charleston, I believe, and it is difficult to assign reasons why it should have appeared so often in rapid succession and then disappear for a long series of years. If it depended on animal and vegetable putrefaction, such could not, I think, be its course. It would seem more probable that the germ of the disease, which is exotic, when transported to an uncongenial climate, may exist for a few years, but finally becomes exhausted and perishes. Let any one desirous of honestly investigating this subject read the thirty years war between the New York and Philadelphia schools, and he will find much material for sober reflection and doubt on the transmissibility of Yellow Fever.

Yellow Fever came at the north in 1793, and ravaged the towns almost without interruption, for a series of years, and no one can tell why or whence it came, or for what reason it has not been seen in New York and Philadelphia for more than twenty years. Nor can we tell from whence came the Hessian Fly, that appeared first in 1776, on Long Island, nor why it departed after laying waste the wheat fields for a number of years. It was called the Hessian fly, but its true origin I believe is yet unknown. This fly travelled only about fifteen miles a year until it passed from our land.

Dr. Rush makes the remark, that no practitioner in the United States is likely to meet with Scarlet Fever oftener than once during his lifetime, so rare was this disease in his day, and yet no Epidemic affection is now more common than this in our country. I never saw or heard of it in South Carolina (my native State) until about fifteen years ago, nor do I believe it ever occurred in the interior of that State before. My old preceptor, who had been in practice forty years, then saw it for the first time; and now it has been become domesticated there, and sporadic cases (like Yellow Fever here) are seen every year.

Dr. Hulse, the distinguished surgeon of the Naval Hospital in Pensacola, informed me in 1841, that he had been in that town eighteen years, and had never seen there a case of Scarlet Fever. Rochoux (a well known authority) mentions the singular fact that Scarlet Fever is unknown in the Antilles, and that the natives of these Islands *must live in France eighteen months or two years before they can become so acclimated as to become susceptible to this disease!*

It is difficult to say where is the *home* of Yellow Fever, but even in the West Indies it has its periods of repose and activity; sometimes lying dormant for ten years, as was the case from 1828 to 1838 in some of the islands. If it is a disease originally of *one* country, which has been transported to others, its native place is probably that where it occurs with most regularity.

It has been observed of those great Epidemics which traverse the globe (as Cholera, Influenza, &c.) that germs are left behind, which, for several years, give rise to sporadic cases of identical character; and it would not surprise me at any time to see the Cholera again spring up in an epidemic form in New Orleans. There are several well authenticated instances where it has recurred at successive periods in the same vessel, showing that a germ is left. Like the seventeen year locust, it might take a Rip Van Winkle sleep, and again awake to its work of destruction.

Those gentlemen who contend for the absolute non-transmissibility of Yellow Fever would do well to weigh these and all the facts of similar import, before they rudely condemn others of equal honesty and ability, holding opposite opinions. The argument is utterly inconclusive, though a thousand instances be proven that vessels or steamboats with Yellow Fever on board have gone to distant ports, or ascended the Alabama and Mississippi Rivers without spreading the disease. Half a dozen well authenticated facts to the contrary are amply sufficient to overthrow it. Yellow Fever, like many other diseases, cannot be propagated in certain localities where the local circumstances are uncongenial to it. You cannot carry it to the interior towns on the Alabama River because some local condition is wanting; still it would seem that the germ of the disease lurks about steamboats, as in those seasons when Yellow Fever prevails in Mobile, it appears almost invariably in the old boats lying up and repairing on the Bay or Rivers within ten or fifteen miles of the town. Small Pox is known to be one of the most contagious of all diseases, and yet it has not extended in our city for the last twelve years, though vessels are bringing in cases every winter, and occasional sporadic cases are occurring which cannot be accounted for. How often too do we see solitary cases of Scarlet Fever occurring in families without contaminating other children, and we have already mentioned the fact that this disease cannot be propagated in the Antilles.

Can any one of the anti-contagionists explain why these contagious diseases are not communicable at one time, and so deadly at another? or why the Asiatic Cholera should suddenly assume an Epidemic form and encircle the globe?

In conclusion (on this point) I would remark, that admitting my suggestions to be true, they do not afford any ground for the vexatious and ruinous quarantine laws which have been enacted against Yellow Fever. A vessel *with Yellow Fever on board* should not be allowed to lie near a town, but here the restrictions should cease. If Yellow Fever is transportable by vessels at all, the instances are so rare, as not to justify very rigid quarantine regulations. Commerce is one of the *great necessities* of society, and law-makers should take into consideration the injuries as well as the benefits of their acts.

As, according to the theory we are discussing, the Natural History of Yellow Fever is closely allied to the Natural History of Insects, it is proper that I should say a few words more on the latter. The Infusoria, or Microscopic animalcules particularly demand a passing notice, as few of our readers have access to original sources on this curious subject. It has, I think, been pretty clearly shown that the propagation of

Yellow Fever cannot be explained by the Malarial theory, and it must remain with the reader to determine whether the chain of analogies offered, render the Insect theory more probable.

"Were a naturalist to announce to the world the discovery of an animal, which for the first five years of its life existed in the form of a serpent; which then, penetrating into the earth, and weaving a shroud of pure silk of the finest texture, contracted itself within this covering into a body without external mouth or limbs, and resembling more than any thing else an Egyptian mummy; and which, lastly, after remaining in this state without food and motion for three years longer, should at the end of that period burst its silken cerements, struggle through its earthy covering, and start into day a winged bird—what think you would be the sensation excited by this strange piece of intelligence?"—*Kirby and Spence—Entomology.*

Wonderful and incredible as this story would seem, it is but a faithful picture of what occurs in the *metamorphoses* of the Insect world. The beautiful butterfly that flits around us on a summer's day has passed through all these miraculous changes. First crawling from the egg, we see the *larva* (serpent)—next comes the *pupa* (mummy,) and lastly the butterfly, that might with much more propriety be ranked with the bird of Paradise than the disgusting catterpillar from which it sprung.

The microscopic wonders, revealed by Leeuwenhoek and other old writers, which for a long time were regarded, at best, only as honest delusions or creations of the imagination, have been thrown quite into the shade by modern discoveries; but it is to the great work of Ehrenberg that we are more particularly indebted for our greatly augmented and more positive knowledge of *Infusoria.*

If a small portion of animal or vegetable matter (as a leaf or piece of flesh) be immersed in pure distilled water, and allowed to remain for a day or two, and a drop of the fluid be then placed under the focus of a powerful microscope, it is seen to swarm with myriads of animalcules which are termed *Infusoria.* A very faint idea may be conceived of the infinite extent of these minute forms of insect life from the simple fact stated by Ehrenberg, that five hundred millions (almost as many as the aggregate of the human race) may exist in a single drop of water!

The term *Infusoria* has been used as a generic one to embrace all microscopic *animalcula;* there are, however, forms which should not come under this head. Like the stars in the heavens, the number of their species increases just in proportion as our artificial vision is perfected, and we have every reason to believe that countless species still exist, too small to be reached by our most powerful microscopes. The infusoria proper, which are found in fluids, are of course more easily seized and examined than those minute microscopic beings that are floating through the air.

Ten years ago, Ehrenberg had described no less than 722 species of Infusoria, and many new ones have been added since that time. Already has observation gone so far as to make it seem possible that there is no form of matter which is not composed of living, dead, or fossil animalcula. Every breath of air we breathe, every particle of fluid or solid we swallow, all the water of the land and of the sea, every solid of the earth we tread upon, is known to abound with them. Many rocks, as the lime stone and cretaceous formations and whole geological strata are composed almost entirely of fossil animalcula ; even the solid gun flint is largely indebted to them. Ehrenberg has described 76 species of fossil Infusoria, belonging to 15 genera. It has even been asserted by a distinguished naturalist that the living muscles are composed of animalcules.*

When we stand before the fossil remains of the Mastodon or the monster Saurien of Alabama, we are lost in wonder at the magnitude and grandeur of the structure ; but far more wonderful and incomprehensible in reality is the animalcule whose length is but the 30,000th part of an inch ! How is it possible, that a living animal, possessing all the complicated machinery necessary to animal life, can be crowded into a portion of space so infinitely small ? It has a head, with teeth—a body with an alimentary canal and complete digestive apparatus—a muscular system with the necessary organs of locomotion—organs of generation—in short, all the apparatus necessary for the existence of an independent being, relying upon external relations.

It is a common impression that Infusoria are found only in stagnant waters where putrefaction is going on, but this is a great mistake—it is true that they are more abundant in such situations, but they abound also in pure lakes, and in running streams, particularly around aquatic plants. The broad ocean too abounds with them, and its beautiful phosphorescence, so often described, is attributable exclusively to myriads of these minute beings. Backer has described 8 species of these phosphorescent animalcules.

We read of, red snow, the color of which is ascertained to arise from animalcules—also of water of various colors—the colors sometimes rising or falling, as the animalcules rise up or sink down. The filthy scum on stagnant pools is but a mass of infusoria ; and we are told that extensive and fatal epidemics occur occasionally amongst fish which are attributable to infusoria. Kirkby and Spence tells us that the " showers of blood" recorded by historians, are ascertained to be the excrement of a species of butterfly—one of the *Lepidoptera*—these showers cover every everything.

Though infusoria are most abundant in warm weather, they are also found in winter, beneath the ice, in frozen streams. The researches of Ehrenberg agree with those of Spalanzani, in showing that cold is dangerous generally to infusoria and especially to the *Rotatoria* (which are of the most perfect organization) and is more injurious to the living animal than their eggs—both the animal and the eggs perish by sudden heat, but sustain it better when gradually applied—some species support greater heat than others. These facts are interesting in connexion with certain experiments showing the disinfecting action of heat—contagious and epidemic diseases have been expelled and are best expelled from vessels, by closing them up and heating the confined air to a high temperature—*the germ of the disease is thus destroyed.*

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* The various facts given are mostly on the authority of Ehrenberg, Mandl, Dujardin, Donne and Edwards—well known authorities.

Light is favorable to the production of Infusoria, but not indispensable, as some species are found in the deepest mines. *Too strong a light* is unfavorable to them and if our theory of Yellow Fever be correct, this may be the reason why the morbific cause is most active at night.

Infusoria are variously acted on by poisonous substances soluble in water—those of fresh water are instantly killed by a drop of salt water though the latter has myriads of its own. Strychnine and many other substances kill them instantly—they swallow Rhubarb with impunity—calomel, corrosive sublimate and camphor do not kill them for some hours. Wine and rum, as well as sugar, says M. Dujardin, kills a great many of those animalcules found in potable water,—a fact, with regard to which, the great mass of the population of Mobile would seem to have as strong an *instinct*, as had Jack Falstaff of the Blood Royal, if we are to judge by the immense amount of ingeniously contrived alcoholic compounds swallowed daily in our pious city.

Infusoria are bred in different ways—some are oviparous—some ovo-viviparous—others viviparous; lastly, many are gemmiparous and they propagate with inconceivable rapidity. Direct experiment has shown that we may obtain from a single one of the *Rotifers* (Rotatoria,) a million on the 10th; four millions on the 11th; and sixteen millions on the 16th day; and the progression is still more rapid in the Polygastric Infusoria. But perhaps the most prolific of all living things are some species of Aphides (plant-louse.) The following curious extract is from Kirkby and Spence's Entomology:—"As almost every animal has its peculiar *louse*, so has almost every plant its peculiar *plant-louse;* and, next to locusts, these are the greatest enemies of the vegetable world, and like them are sometimes *so numerous as to darken the air.* The multiplication of these little creatures is infinite and almost incredible. Providence has endued them with privileges promoting fecundity, which no other insects possess; at one time of the year they are viviparous, at another oviparous; and what is most remarkable, and without parallel, the sexual intercourse of one original pair serves for all the generations which proceed from the female for a whole succeeding year. Reaumur has proved that in five generations one Aphis may be the progenitor of 5,904,900,000 (billions) descendants; and it is supposed that in one year there may be 20 generations!!!"

With these few facts before us, how much more easily may we account for the spread of yellow fever from a focus, by the insect, than by the Malarial hypothesis—here is something tangible and comprehensible.

Not only the living, but *dead* animalcules *may* be a cause of disease—those who prefer this doctrine may, if they like, appropriate them in a putrifying mass to the support of their malarial notions.

In the May No. 1845, of the London Quarterly Journal of the Geological Society, may be found an exceedingly interesting article (taken from one by Ehrenberg in a Berlin Journal) "on the muddy deposits of the mouths of various Rivers, and the infusoria found in those deposits."

Ehrenberg has discovered in the mud now depositing "forms of Microscopic life and Infusoria identical even in species with those found in the fossil state in the oolitic and cretaceous ormations in every quarter of the globe."

It has already been stated that the fresh water and marine animalcules are entirely different, and Professor Ehrenberg has established the novel fact, that "the microscopic animalcules found in the marsh lands at the head of tide water in the Elbe (and so with other rivers) are the same as those in the ocean—possessing silicious and calcareous skeletons. These organic forms, which are better preserved at the depth of several feet, than on the surface, existed in the arable land of the valley of the Elbe, which had been accumulating for thousands of years, and in this way is explained the origin of this soil in a more satisfactory manner than has hitherto been attempted." The marine animalcules have been carried up with the tide, killed by the fresh water, and largely assisted in forming a deposit which heretofore has been attributed to the *river* deposit alone. The examination of the river at Gluckstadt and Hamburg has proved the existence there of 58 different species of marine animalcules.

Some idea may be formed of the extent of this putrifying mass, from the fact stated by Ehrenberg, that one cubic foot of every 20 of the alluvial islands of the Elbe is composed of animalcular remains, chiefly of marine origin. He states farther that the great bulk of the deposit is sand which under the microscope is found to be the *silicious shells* of extinct animalcules. From these facts it would seem that at leasst one half of the whole deposit is living, dead, and fossile animalcula. To these facts is attributable the fertility of these islands and marsh lands.

Yellow fever has a mysterious connexion with the seaboard and embouchures of rivers, but I will not pause to speculate on this point—it does occasionally wander a short distance from tide water and I have under the head of contagion explained the manner in which this might occur.

The habits and instincts of larger insects are obscured by numerous impediments, but how much more perplexing must be the natural history of those which can only be reached by powerful microscopes? We have learned much about the infusoria proper, but myriads of minute beings might inhabit the air and even congregate in such numbers as to dim the light of the sun without our being able to seize and observe them. Denying animalcules the power of flight, which would be absurd, there are still ample provisions for their transportation long distances either in the form of egg or perfect animal. We have already seen how they are transported by water and by vessels, and there is reason to believe that they may be taken up with aqueous vapor and carried off by the winds. Even the Gossamer spider will sail upon his little web great distances. A shower of them fell upon the English vessel Beagle, in her voyage round the world a few years ago, when 60 miles from land.

To illustrate the influence which currents of wind may have in their distribution, the following facts are taken from an article in the Feb. No., 1845, of the London Quarterly Journal Geolog. Sciences, by the distinguished naturalist, Chas. Darwin, who made the voyage in the Beagle.

Many scattered accounts have appeared, concerning the dust which has fallen in considerable quantities on vessels at sea, great distances from land. Mr. Darwin has collected the details of 15 distinct instances in some of which dust fell for several days. It has several times fallen on vessels when between 300 and 600 miles from the coast of Africa; and it fell in May, 1840, on the Princess Louisa when 1030 miles from Cape Verd, the nearest point of the continent.

The instances related are given with such detail, and are so well authenticated as to leave no room as to their accurary. The dust is often so abundant on the African coast as to cover every thing on board the vessels, as we often see dust over our furniture during spells of dry weather. Particles as large as the 1000th part of an inch have been blown to a considerable distance, on one occasion, 330 miles, and the atmosphere became hazy and the sun was dimmed. Mr. Darwin follows these facts by this remark—"The fact of particles of this size having been brought at least 330 miles from the land, is interesting as bearing on the distribution of the sporules of Cryptogamous plants and the ovules of Infusoria." Again he says, "Professor Ehrenberg has examined the dust collected by Lieut. James and myself, and he finds that it is, *in considerable part, composed of Infusoria, including no less than 67 different forms;* the little packet of dust collected by myself would not have filled a quarter of a tea-spoon, and yet it contained 17 forms.

One of the most highly organized and the most interesting in connection with our subject, is the *Rotifer*, (Rotatoria,) which is found not only in moist, but in perfectly dry places. It possesses the remarkable quality, first observed by Leeuwenhoek some 150 years ago, of remaining in a dry and apparently lifeless state for an indefinite period, and then being again resuscitated by the application of moisture. It is found not only in the parched sands of the plain, but in the dust of the gutters on the house tops, exposed to the burning summer sun. The application of moisture restores them immediately to life and activity.

Here we have the proof that both the animalcule and its germ may lie dormant, as is the habit of certain diseases, and then be brought into activity when its appropriate stimulus is applied. We have the evidence, too, that they may be transported through the air to a distant point, and there abide their time, as do the fomites which transport contagious diseases. What are the causes, meteorological or other, which call them into action, we are as ignorant as we are of those which govern larger insects, as the Aphides, the Hessian Fly, the Cotton Worms, &c., or as we are of the causes which regulate the temperature, the quantity of rain, or the electric states of the atmosphere, in different years.

It is difficult to conceive that the various forms of fever described should arise from a common source, and as chemistry has failed to detect a gas or emanation which can produce any one of them, their causes perhaps may be sought with more success in the different forms of Insect life. Works on Poisons have classified and thrown into separate groups those substances which have general resemblances in their modes of action; and so closely do articles of the same group similate each other in effects, that we are often much perplexed in distinguishing them. The Narcotic poisons, for example, though derived from different plants, and differing in their analysis, will often produce symptoms so alike as to render it impossible for us to decide, under which a patient is laboring. The same confusion will be found in the poisonous effects of different snakes, spiders, &c. In like manner, fevers, if arising from insects of the same *genus*, might present some general characteristics in common, and yet preserve *Specific* differences.

Ehrenberg, Mandl and Dujardin inform us that different animalcules are found in different localities. Stagnant waters on calcareous soils contain Infusoria which may be sought in vain in those of Argillaceous soils. The latter, ferruginous waters, those of turf, those of *ditches around habitations*, all have their peculiar inhabitants. Again, we may seek in vain for Infusoria in one season which are found in another, and no reason can be assigned for their appearance or disappearance. Here we have another analogy with the different types and habits of fevers.

The observations of Ehrenberg did not detect Infusoria in the dews, and yet there are strong reasons for believing that they exist here. By operating on moisture condensed from the atmosphere, Moschati, Guntz, Brocchi and Rigaud de Lisle, Vauquelin, Rigaud and Julia have shown incontestibly the presence of animal matter in air, and it is highly probable that it exists in an organic form. Some very well conducted experiments to the same effect have been made by Professor Riddell of New Orleans, which may be found in the Medical Journal of that city.

Professor Jackson, of Philadelphia, in a paper published by him in 1824, (I think, but it is mislaid,) informs us that during the last Epidemic of Yellow Fever in that city, the microscope detected immense numbers of animalcules in the Black Vomit, and none in the fluids ejected from the stomachs of those laboring under other fevers in the hospital at the same time.

We know that certain cutaneous diseases are produced by animalcules—that animalcules and little worms are very often found in the various fluids of the body, as the blood, urine, bile, &c.—also in the solids, as the brain, liver, eye, &c. Linnæus gives us a case of Dysentery, clearly produced by what he calls the *Acarus Dysenterica*. M. Donné, as before stated, has discovered in the pus of Buboes, animalcules which are constantly present, and which he regards as the cause of the transmissibility of Syphilis; and it is highly probable that the same discovery will yet be made for Small Pox, Plague, Cholera and other diseases. It is possible that Mercury and Iodine in Syphilis act as specifics, like sulphur does on Itch, by poisoning the animal cubs; and there is no reason why specifics might not be discovered for Yellow Fever and other diseases.

To one living on the Gulf of Mexico, it would look like a waste of time to speak of swarms and migrations of Insects. At the very moment I am writing I am annoyed by gnats, bugs, moths, &c., in such numbers that an inhabitant of a northern latitude could not conceive how I can connect two sentences together, and I confess that sometimes they are so troublesome that I am thinking more of my persecutors than the subject before me. Facts however that are before us constantly, cease to excite reflection, and it may be well to give a few prominent examples touching the *Migrations* of *Insects*.

At the moment I am writing I see by the newspapers that a brown fly of peculiar character, and which no one recollects to have seen before, has appeared in and around Cincinnati in immense clouds, covering the country for miles. No conjecture can be formed respecting their point of departure. Kirby and Spence give us the following account of gnats :

"We are told that in the year 1736, they were so numerous, that vast columns of them were seen to rise in the air from Salisbury Cathedral, which at a distance resembled columns of smoke, and occasioned people to think that the Cathedral was on fire. A similar occurrence, in like manner giving rise to an alarm of the church being on fire, took place in July, 1812, at Sagan, in Silesia. In the following year, at Norwich, in May, at about six o'clock in the evening, the inhabitants of the city were alarmed by the appearance of smoke issuing from the upper window of the spire of the Cathedral, for which at the time no satisfactory account could be given, but which was most probably produced by the same cause. And in the year 1776, in the month of August, they appeared in such incredible numbers at Oxford as to resemble a black cloud, darkening the air, and almost totally intercepting the beams of the sun." Even in "Lapland their numbers are so prodigious as to be compared to a flight of snow when the flakes fall thickest or to the dust of the earth."

The instincts by which insects are at certain times impelled to *emigration*, even to great distances, are very strange and unaccountable. Sometimes flights of them are met far out at sea. "De Geer has given an account of the larvæ of certain gnats, (Tipulariæ,) which assemble in considerable numbers for this purpose, so as to form a band of a finger's breadth, and from one to two yards in length. And what is remarkable while on their march, which is very slow, they adhere to each other by a kind of glutinous secretion ; but when disturbed, they separate without difficulty. Kuhn mentions another of the same tribe, the larvæ of which live in society and emigrate in files like the caterpillar of the procession moth. First goes one, then follow two, then three, &c., so as to exhibit a serpentine appearance, probably from their simultaneous undulating motion and the continuity of the files ; whence the common people in Germany call them *heerwurm*, and view them with great dread, regarding them as ominous of war. But of the Insect emigrants, none are more celebrated than the locusts, which, when arrived at their perfect state, assemble, as before related, in such numbers, as in their flight to intercept the sunbeams and to darken whole countries ; passing from one region to another, and laying waste kingdom after kingdom," &c.

But it is needless to multiply iustances of this kind, and if any one should be at all incredulous, let him spend a night in a southern swamp. I will add one very singular example of the instinct of insects :

"There are annually two generations of the Angoumois Moth, an insect destructive to wheat. They first appear in May and June, and lay their eggs upon the ears of wheat in the fields ; the second appear at the end of summer in autumn and lay their eggs upon wheat in the granaries. These last pass the winter in the state of larvæ from which proceeds the first generation of moths. But what is extremely singular as a variation of instinct, those moths which are disclosed in May and June in the granaries, quit them with a rapid flight at sunset and betake themselves to the yet unreaped fields, where they lay their eggs; while the moths which are disclosed in the granaries after harvest, stay there and never attempt to go out, but lay their eggs upon the stored wheat. This is as extraordinary and inexplicable as if a litter of rabits produced in the spring were impelled by instinct to eat vegetables, while another produced in autumn should be as irresistibly directed to choose flesh."

The history of those great epidemics which sweep over the surface of the globe affords very strong support to the Insect theory. The Cholera, though not more remarkable than many other, may be selected for illustration. This disease, which started in Bengal, after assuming the epidemic form, travelled on until it arrived at the foot of the gigantic range of mountains which separates Asia from Europe—it seemed for several years unable to cross this immense barrier, but finally, like a river which had been pent up, it burst over into Russia in the dead of winter, when the mercury was almost freezing in the thermometer (and no doubt *quite* on the ridge of the mountains) and ravaged Moscow like a plague; and after 17 years of unceasing travel it completed the circle of the globe. Its general course may be followed from first to last, though there are many irregularities in the details—sometimes it turned to the right, sometimes to the left—now leaping over several hundred miles and passing on, or after a time retracing its steps and attacking towns which had congratulated themselves on an escape—usually preferring to follow great water courses and to prevail in summer, but at other times travelling over hills and sandy plains, and in the coldest weather. Wherever it prevailed, too, a tendency to reproduction remained for several years, as if germs were scattered in its track.

By what other than the Insect theory can these facts be explained?—No gas—no emanation—no form, in short, of inorganic matter could thus extend itself for 17 years around the globe, propagating as it travelled and scattering the seeds of reproduction behind it. All the theories which have been started, are absurd.

Sir Henry Holland, in tracing the erratic habits of insects in connection with this disease, says of them: "such are their frequent, sudden generation, at irregular and often distant periods, under certain circumstances of season and locality, or under other conditions less obvious to apprehension. The diffusion of swarms so generated and with rapidly repeated propagation over wide tracts of country and often following particular lines of movement," etc.

"Whatever is true as to the habits of insects obvious to our senses, is likely to be more especially so in those whose minuteness removes them farther from observation. Their generation may be presumed to be more dependent on casualties of season and place; their movements determined by causes of which we have less cognizance; and their power of affecting the human body to be in some ratio to their multitude and minuteness."—"Their direction to certain plants only—their settlements upon these in clusters and detached localities—the frequent suddenness of their change of place and disappearance, are all circumstances of curious analogy; as also the curiously abrupt limitation of some of those swarms, showing itself in definite lines of direction, along which their work of destruction is carried on."

Miasmatic fevers abound most in Southern latitudes and in marsh lands; and the reason assigned is the greater amount of vegetable matter which is here subjected to rapid decomposition. But it should not be forgotten that here, too, are to be found in great excess the various forms of Insect life, Infusoria, etc., etc. Every plant not only has one parasite, but it is estimated that there is an average of six to each plant. Some idea may be formed of the immense number of insects in warm climates when it is stated that naturalists have variously estimated the number of species in the world at from 300,000 to 600,000.

Köllar tells us "that the distribution of Insects is in exact proportion to the diffusion of plants; the richer any country is in plants, the richer it is also in insects. The polar regions which produce but few plants, have but few species of insects; whereas the luxuriant vegetation of the tropical countries feeds a numerous host."

But it is high time that this long and rambling essay should be brought to a close. No one is more fully sensible of its imperfections than myself, but were I competent to do ample justice to the numerous topics alluded to, far more extended limits would be required than can here be permitted. The reader need not be told how endless and complicated are the ramifications of the subject of Malaria. I have not attempted to elaborate fully a single point, and my object has been simply to attract attention to certain phenomena of yellow fever which I think have been too much overlooked, and to lay before the profession, in connexion with them, some material which may serve for reflexion.

IV.—*Reply to Doctor W. M. Boling's review of Doctor Lewis' Medical History of Alabama, with some new facts and remarks, in relation to the diagnosis and identity of the fevers of the State.* By P. H. Lewis, M. D., of Mobile.

"The intellectual worth and dignity of man are measured not by the truth he possesses or fancies he possesses, but by the sincere and honest pains he has taken to discover truth."

We regret our inability to make any acknowledgements to Doctor Boling for the general expression of his opinion, that the essay entitled the "Medical History of Alabama" will prove of great value for reference, and that as a whole "the paper possesses high merits." Praise from a pen so prolific of censure and affecting so much satire, is doubtless intended to sooth and palliate the wounds, a self-satisfied critic imagines he has inflicted; but as the Dr.'s blows have been too feebly made to take effect, his commentations like his censures have fallen short of their purpose.

Believing that a mere partisan controversy is not only unproductive of good, but exhibits its actors in an unenviable *light* before the public, we shall confine ourselves to a defence of the facts, and the conclusions based upon them, which the essay contains. And if, in doing so, it should become apparent to the reader, that this reviewer, in devoting 64 pages to a "deformed, unfinished essay, has been actuated by feelings and motives, other than those which should influence the enlightened physician; be the fault with him and not with us.

We ask permission in this place to state, that essays on the subject under discussion were to be presented by the first of December, and that such was the difficulty in collecting facts and incidents relating to a new and difficult subject, that the essay was not begun in earnest until the first of September. From this time until the first of December the interruptions incident to Hospital and private practice were such, that the author was unable to devote two consecutive hours to its preparation. After the Society had approved the essay and ordered its publication, it was the intention of the author to revise it for the press—but private matters of deep and absorbing interest prevented his doing so. He was aware that it went into the world "deformed and unfinished" and vulnerable in many respects; for this he apologised and in effect stated, that as the paper was presented to and accepted by the Society not as an attempt at a *Book* or finished production, but merely as the beginning of a Society record, which is intended to note the shifting and changing features of disease within the bounds of the State, he hoped a generous medical public, in consideration of the matter, would excuse the manner and style in which it appeared.

Before entering upon any discussions of the character, identity and complications of the diseases of the State, it is necessary to notice some of the more unimportant objections which Dr. Boling has raised to the medical history. * * * In reference to terrestrial emanations and the influence of soils in modifying disease, the author remarks, that "there will be no further advance in the science of Ætiology without the aid of Chemistry and Geology." The author here, as a matter of course, has reference to this particular branch of medical inquiry. Dr. Boling objects to this conclusion and says, that few will be found to admit the correctness of such an assertion. Now it so happens that Doctor Boling has published a treatise on remittent fevers, upon the first page of which he judiciously remarks, that the effects of malaria are sufficiently *striking* and *peculiar* as to produce confirmation (of its existence) in the minds of most *practical men* who have lived in tropical regions. On the same page the Doctor tells us, that the chemical and physical properties of malaria "remains yet to be satisfactorily explained." Doctor Boling here sanctions the general doctrine of malaria—we believe, it is the general opinion of mankind *that this malaria has its source* in organic remains or comminuted mineral substances. If this be so, how are we to obtain a knowledge of its chemical and physical properties (of which Dr. Boling says, we are entirely ignorant) without the aid of chemistry and geology?—Can the Doctor give "a logical" reply to this question, without sustaining "the assertion" of the author?—But as there is great want of stability and fixedness of purpose pervading his writings, it is possible that the Doctor since he wrote his essay upon fevers has changed his opinions, and now believes in the planetary, vulcanic or animalcular origin of disease. But if he will reflect upon these even, he must come to the conclusion, that in the event any one of those hypothesis should be true, chemistry and geology, or one of them at least, must play an important part in demonstrating its certainty to the world.

It is stated by the author, that in the territorial days of Alabama, when the population was thin and spare, and a very limited portion of the rich bottom lands had been brought into cultivation, the fevers were of a remittent and intermittent type and strictly non-malignant; but that these fevers not unfrequently glided into a continued irritative type, attended with great nervousness, twitching of the muscles, general emaciation—continuing for many weeks, and constituting what in those days was termed "nervous fever." With a few exceptions, to which the author called the reader's attention, he states that these cases of *nervous fever* were then the only ones of a serious or dangerous character. He could not discover, that any malignant disease, running its fatal course in a few days, existed anterior to 1817 and 1818—hence the adoption of the word "ataxic" to designate the character of disease belonging to that period. Doctor Boling considers the word "indefinite as characterizing a state of disease" and grows witty over our application of it in this sense. We have ventured the assertion that a single author of distinction cannot be found, who does not consider the word *definite* as "characterizing a state of disease;"—our recollection, at least, is much at fault, if Cleghorn, Cullen, Philip, Pinel, Brown, Forbes, Johnson, Chomel, Rush, Jackson and Bartlett do not use it in this sense. As for the application of it in this instance, we have recently discovered that every writer, on the early diseases of Pennsylvania and Virginia, made under the same circumstances, the same application of it that the author has. Our medical nomenclature may be so defective in many instances as to require alteration or amendment, but in this the term is so proper in its application and so firmly fixed by time and usage, that not even the distinguished authority before us is likely to effect it.

In relation to this ataxic or nervous fever Dr. Boling makes the following remarks. "It is no very uncommon thing in some parts of the State to see cases of remittent fever, which at first perhaps were rather of a mild character, by neglect or maltreatment assume the appearance here described, and in fact, the terms "nervous fever," "typhus fever" and "typhoid fever," are not unfrequently applied to them in this stage;—though, on careful inquiry, the peculiar characteristics of remittent fever may always be discovered to have marked their early stages.

When Dr. Boling supposes that the medical men of Alabama do not distinguish between the ataxic or nervous and the typhoid condition, he underrates their judgment and practical discrimination very much. Cases displaying the symptoms of one or the other are frequently met with and are easily distinguished. In the nervous, for instance, the tongue is covered with a white fur and always moist, the pulse is small, hard and firm, bowels constipated, muscles tremulous, those about the face and eyes frequently twitching, great watchfulness with soundness and acuteness of intellect. Cases of this description are of common occurrence among the wood-choppers about Mobile. It is unnecessary to describe here that condition recognised by medical men as "the typhoid," it is so entirely different, requiring such opposite treatment, that they never fail to distinguish between the two.

In relation to this particular period, the author of the Medical history goes on to state, that the people lived in a state of rude simplicity and enjoyed the most robust health. We believe that no one at all acquainted with the subject, will fail to admit that the pioneers of all new countries are not only blessed with the most athletic frames and hardy constitutions, but attain the greatest number of years allotted to man. The history of New-England, Pennsylvania, Virginia, Kentucky and Tennessee brings strongly to view the fact, that these woodsmen were strangers to disease and physical infirmities, and that in many instances 20 and 50 years elapsed, before the settlements they had planted were visited by malignant epidemics. Similar circumstances and results, connected with the early settlement of Alabama, being apparent to the author of the Medical history, he concludes a sentence with the remark that—"great longevity marked man's pilgrimage on earth." Dr. Boling, with more of the adroitness of the *tricster* than the ingenuity of a critic, *shows up the author*, for placing the "ataxic period," so propitious to health and longivity, at 7 years, from 1811 to 1818;—thus placing him in the ridiculous position of measuring the lives of men by such an inadequate rule. We believe it is apparent to those who have read that part of the essay, that although the author could obtain no account of the diseases of the country, prior to 1812, yet the inference he expected would be drawn from his remarks, was, that the description of persons to whom he alluded, had formed their habits and hardened their constitutions before that time. Again, it will strike any man, that though circumstances may arise, affecting a *rising* generation or new population, still the one preceding will in a great measure retain their early characteristics. In confirmation of this, we can here in Alabama point Dr. Boling to hundreds of pioneers of the country, who now at the age of 60 and upwards are treading with a firm and manly step upon the graves of their own children. And again, if we understand Dr. Boling, he wishes to impress it upon the reader, that these hardy, robust pioneers would be more liable to inflammatory diseases, than those living upon luxuries, fermented liquors, and passing their time in idleness and dissipation. The experience of all observers is so pregnant with facts to the converse of such a conclusion, that we will not insult the understanding by recapitulating them.

In progressing with the history of the State, the author remarks, that the fertile soil, beauty of scenery, and navigable streams of Alabama, began about the year 1815 to attract the attention of people at a distance—that from that time immigration was unprecedented, that villages numbering but a few dozen, boasted in 3 or 4 years a population of several thousand:—that wealthy planters from the adjoining states brought into the country thousands of colored laborers, who soon reduced the *heavy timbered* alluvial lands, which had escaped the feeble force of the early settler, to a state of cultivation. Hand in hand with agricultural improvements, villages and towns sprung up in rapid succession—the persons constituting the population of the latter, coming mostly from the cities of the North and Europe and bringing along with them luxuries, dissipation and pernicious habits. He (the author) remarks, that among the secondary causes so active in the production of disease, this change from the simple primitive habits of the people, to thoss of luxury and dissipation, was one which could not be overlooked, that the effect of those habits was not only immediate and direct upon those who indulged in them, but exerted a baneful influence upon the constitutions of their offspring. This is the true and only sense, or meaning, which can be derived from the author's remarks upon this subject. It is difficult here as in many other places, to ascertain the *drift* of Dr. Bolings inuendos and cavilling objections; but if we do not misunderstand him, he objects to the author's conclusions, and insists that the habits and mode of living introduced by this new population, could have had no agency in producing, as the author states, "that marked change in the character of disease so fully exhibited during this important epoch." Among the many causes assigned by the author, for the development of malignant inflammatory disease at this striking period in the history of the State, too much prominence may have been given to this one. But we will hear what Dunglison says on the subject.—"The remarks already indulged will have shown how detrimental the constant abuse of spirituous liquors must be to the liver, and to the functions of the stomach and intestines in general. There is hardly indeed a faculty—mental or corporeal—but is made to totter under the stimulation excited in it, by the pernicious habits of the dram-drinker." To the casual remark of the author that these pernicious habits had "a tendency to reduce the stalwart man to a state of decrepitude," Dr. Boling has applied no ordinary degree of ridicule. When he perused those witty passages the Doctor had certainly forgotten the authorship of an extraordinary production, intended for the eye of gentlemen occupying the sacred desk. We quote from Doct. Boling's Essay on Diseases of the Clergy—it is an elegant (not *inflated* or *bombastic*) sentence:—"One of the most important *conceptions* for the improvement of the moral and *physical* condition of man—one which has been the means of rescuing thousands from, of preventing millions from falling into a state of *physical wretchedness and misery*, and of moral degradation, below even that of the brute—the temperance cause, has it not mainly been carried out by the exertions of the clergy?" Doct. Boling, in his review, after giving to some of the author's sentences, an interpretation to suit himself, says he is reminded by them of the "cross readings" by which editors amuse their readers.

The above extract in connection with opinions advanced in the review, places the Doctor in the awkward predicament, of pandering on one occasion, to the vanity and prejudices of the clergy, or on the other of catering to the lowest of professional appetites—neither of which objects are easily reconciled with the dignity of that position to which his intelligence and acquirements have elevated him.

There is no part of the Medical History attacked which is not perfectly defensible, but to parry all the small thrusts of a reviewer, who writes as though fame consisted "in filling a certain space on paper" would be a waste of time and an unpardonable tax upon the patience of the reader. We will therefore confine our defence in future to material and important portions of the history.

In referring to the character of disease of the second epoch, Doctor Boling says: "Dr. Huestis, the most authoritative writer of his day perhaps in Alabama, gives an account of an epidemic which occurred in Cahawba in the autumn of 1831, which does not exactly sustain the author's views relative to the highly inflammatory character of the diseases of that day, nor in the epidemic spoken of, at least, do we find him speaking in any thing like high terms of the use of the lancet." Now, it is very evident, that Dr. Boling, by quoting the most authoritative writer of his day in Alabama, wishes to create the impression that the author of the Medical History so far from being sustained, is contradicted by this high authority. It is but reasonable to suppose that Dr. Boling has examined every paragraph and sentence contained in the history—if so, he could not avoid seeing that the author had made liberal extracts from the accounts given by Dr. Huestis of the epidemics which prevailed in Cahawba from 1818 to 1830. In these accounts Dr. Huestis dwells upon the state of high vascular action and excitement, imperfect remissions, tendency to local inflammation, buffy coat of the blood and inflamed condition of the membranes of the brain and stomach, characterizing these diseases. And as to bleeding he was the unqualified advocate of it, stating, that it was frequently necessary to draw blood three or four times during the course of one fever.—This is the authority by which Dr. Boling, by taking an account of a few cases occuring in one epidemic, and that epidemic taking place between 1830 and 1834, years in which the author of the Medical History says :—"inflammatory diseases of winter assumed rather a low typhoid type, whilst the *fevers* of *summer* displayed less of that high toned vascularity than had been their wont?" would invalidate the statements of the author. The reviewer who, by such partial extracts, by the suppression of important relative events, and by unwarrantable inferences, will attempt to break up the connection of genuine history, not only places himself without the pale of polite controversy, but weakens that confidence in himself, which should be reposed in the representations of one writing upon scientific subjects.

In another part of the review, Dr. Boling again uses this epidemic of 1831 as true congestive fever, thereby leaving the inference that the author of the Medical History is wrong, when he asserts that this disease did not make its appearance until 1834. Although we can satisfactorily demonstrate the great dissimilarity between congestive fever and the disease here described by Dr. Huestis, it is here unnecessary.—Doct. Huestis we presume is good authority, at least in relation to the diseases he had described and the conclusions to which he had arrived. In an unfinished paper on typhoid and adynamic pneumonia, which has been placed among the documents belonging to the Mobile Medical Society, he uses the following language in relation to "congestive pneumonia :"—"Many cases of a similar character occurred during the prevalence of the Malignant Pneumonia at New Orleans in 1814, noticed in my "Observations on the Diseases of Louisiana"—but since then I have rarely met with a case, until after the *appearance of congestive fever in* 1834. Since then, cases of this description are not uncommon on the Alabama River." Before attempting to correct others in matters of fact, Dr. Boling should inform himself properly, not only in relation to their authenticity, but the succession in which they run—he should be careful how he pushes his *little shallop* into waters he has never explored.

Before leaving this subject, we will state, that though the author of the Medical History is freely sustained by Dr. Huestis, yet, he relied more especially upon other sources, for his information—such, for instance, as Drs. Casey, Hogan, Johnson, Saxon, Watkins, Brown, Shakelford, Bohanan, Meeker, Miller and others not immediately connected with the profession. That the diseases of that day were of an open inflammatory character, and that calomel, antimony, nitre, salts and bloodletting were the agents in common use, not only in, but out of the profession, we have never heard denied by any one who is a competent witness.

Doct. Boling introduces his strictures upon the third or adynamic epoch, as follows:

"Before entering upon the third or adynamic epoch, we will turn back to make an extract from the first epoch.

"After tracing disease from its mild incipient action of early days, through the high toned phlegmasia of later times, until we arrive at the low state of typhoid affection that marks the character of all disorders at the present day, it will be discovered that the above division, so far from being the result of an hypothesis, is naturally and imperiously demanded." We would ask what does Dr. Lewis mean here by "typhoid affection." He cannot, of course, we must believe, have reference to the disease known under the names of the "typhoid affection," "typhoid fever," "dothinenteritis," "follicular enteritis," &c., for no one who is at all familiar with the diseases of Alabama, could be so far led astray, even by a favorite hypothesis, as to discover among our more prevalent diseases anything at all "answering to the description of this affection." With all of the Doctor's pretended ignorance of the author's meaning, it strikes us as being too plain to admit of a moment's doubt. We will here inquire, what is the general understanding not only among men in Alabama, but throughout the world, in relation to continued fevers of an intermediate grade, between pure synocha and pure typhus, or between the periodical fevers of autumn and true typhoid of winter. They are called typhoid, adynamic, ataxic or inflammatory, as the symptoms essential to one of these conditions may predominate in the case before them. If, for instance, in Alabama, a case of periodical fever, whether in the remittent or continued stage, is attended with many of the phenomena belonging to typhoid fever, it is called *typhoid*; so also with many of our diseases, especially pneumonia, in which this element is frequently manifest. If Dr. Boling with an eye, unclouded by prejudice, will but look over the writings of even such rigid and strict annalists as Louis and Bartlett, he will discover that they apply *typhoid state* or *typhoid condition* to one of the varieties of many diseases—as typhoid scarlatina, typhoid yellow fever, &c. &c. By which, according to the construction of this *new Daniel* they mean, *Typhoid fever, dothinenteritis*, &c. In the same way, all writers, except Doct. Boling, use the term *adynamic*, to denote a certain well known condition that frequently supervenes in most acute diseases, as adynamic scarlatina, adynamic pneumonia and adynamic yellow fever—but they do not by this mean, that it is *adynamic fever*. With a certain and fixed knowledge, that the diseases of the THIRD EPOCH, in becoming serious or dangerous, displayed in a prominent degree either the TYPHOID OF ADYNAMIC element or condition, the author used those terms to designate their character.

Doct. Boling, having started with an erroneous and most unwarrantable assumption, proceeds as follows. "We shall have to state, deriving our impression from another part of the essay, that Doct. Lewis has assumed the congestive fever as the type of the diseases of the adynamic epoch." Dr. Boling then proceeds to make such extracts from the Medical History, as in his opinions, sustains his assertion, then by bringing from a remote and different section of the history, the author's words "typhoid affection," by which he has already so sagaciously assumed that he (the author) means *True Typhoid Fever*, makes out his case and remarks—"hence Doct. Lewis must believe in the identity of congestive and typhoid fever;"—having out of this material erected an airy fragile castle, unequal in strength to his *pathological* prowess, the Doctor, Quixote like, proceeds with an extraordinary flourish of trumpets to its demolition.

We are here under the painful necessity of convicting this reviewer of a piece of *garbling*, which, for recklessness has no parallell. It is the suppression, not of a distinct or connecting passage, but of one half of a paragraph, which is inseparably connected with the part Doct. Boling quotes, and is alone illustrative of the author's real views and conclusions relative to the general character of disease during the 3d epoch. We will first *requote* from the review that portion of the passage which this writer extracts for his purposes.

"Causes of a prominent nature have been assigned for the change which took place in the character and temper of disease about 1818.—Whether those causes were adequate to the production of these new pathological features cannot be asserted with absolute certainty, yet such a conclusion is plausible and sustained in part by events in the history of other and older States. But for the change which we are now contemplating, the mind cannot fix upon any tangible, or supposed cause, with certainty or satisfaction. In many parts of Europe as well as America, it was noticed, about this time, that disease assumed an adynamic type. This change, though not so marked elsewhere as in Alabama, was still sufficiently prominent to attract general attention. Watson and other observers seem to hold the opinion that it was in some way influenced by the epidemic cholera, which immediately preceded."

So far, reference is made *only* to congestive fever, and here Doct. Boling *stops*, in the middle of the paragraph:—We will now quote the remainder of the *suppression*, of which we complain.

"Owing to the fact that many cases of congestive fever, (then a new disease in Ala.,) resembled the cholera in some of its symptoms, the same conjecture has been made here. But when it is known that the congestive fever of Alabama is identical with a disease, designated by the names "cold plague," "congestive typhus" and "cold sickness," that has occasionally shewed itself in certain localities along the Mississippi and Apalachicola low lands, since 1820, it becomes obvious that this particular form of disease is essentially indigenous to the country, and that its development in Alabama, was owing to local causes rather than any (*inexplicable*) foreign atmospheric agency. But this *particular form of fever and all speculation on the subject aside*, and it becomes our duty, so far at least as unerring testimony can make it, to state, *that diseases of every form, season and locality in the bounds of the*

State became at this particular time characterized by a low enfeebled state of the circulation, opposite that which previously existed; and that even the enlightened and observing surgeon, in cases of injury and violence, had frequently to stimulate and nourish, where but a short time before the most active depletion would have been required."

Notwithstanding this full and unmistakable explanation of the views of the author in relation to the diseases of this epoch, given independently of congestive fever;—connected too with the fact, that he has divided the State into three geological districts—the Granitic, Prairie, and Tertiary; describing different types of fever, as peculiar to each—only one of which, he calls *congestive*;—and that in another place, he emphatically says—that the diseases of winter changed about this time, from an inflammatory to a typhoid character;—Dr. Boling still insists, that Dr. Lewis has assumed the *congestive* as the type of disease for the third and present epoch. An inference not only baseless and shadowless, but raised in the face of the strongest possible array of opposing testimony.

Before dismissing this branch of the subject, we will inform Dr. Boling, that by turning to the article "Fever" in the British Encyclopedia of Practical Medicine, he will find that writers there, when designating a low condition of the vascular and nervous systems, opposite that which distinguishes the true and well marked phlegmasia, frequently use the words *typhoid*, *adynamic*, *atonic* and *asthenic*, as synonymous terms.

We will now pass on to that portion of the Medical History, which is devoted to the autumnal and typhoid fevers of that section of the State, known as the granitic or hilly region.

After a few remarks concerning the occasional cases of *congestive* intermittents that are met with, the author proceeds as follows: "The summer and autumnal fevers of this region are principally intermittent and remittent, of a mild nature, attended with little fatality; the only severe or grave form of febrile affection, being of a continued or typhoid character. Dr. Clark, in his esteemed letter, remarks, 'in August, September and October of 1840, in Benton County, typhoid fever prevailed as an epidemic, assuming frequently a malignant, obstinate and unmanageable character. It attacked, indiscriminately, individuals of all ages, without regard to sex or color. During the prevalence of this fever, we had also every grade and variety of intermittent and remittent fever, throughout the summer months, but all the *fatal cases* were of a typhoid character.'

Notwithstanding the occurrence of this and other partial epidemics, idiopathic typhoid fever cannot by any means be regarded as the prevailing disease of summer and autumn; it more properly belongs to the winter and spring months: yet, as the general tendency of the remittent is to the continued typhoid, taken in connection with those that are essentially so, and that is the only type which is considered grave or dangerous, we cannot err in stating that typhoid fever prevails in this section of the State *to some extent at least*." We presume that no one can mistake the author, in the instances quoted. Speaking of his own observations, he states, that he saw cases of continued fever, attended with bilious vomiting, quick compressible pulse, moist yellow tongue, stupor or coma,

&c., &c., in reference to which the following remark is made: "In these forms of fever, the disease generally lasted from ten to fifteen days and may be comprehended under that variety described by Chomel, as not being essential typhoid fever." Dr. Boling here wishes to know, if the author intends to convey the impression that Chomel pronounces a disease typhoid fever, and then declares it not *essential typhoid fever*; if so, says the Doctor, "he should have referred us to volume and page." We have not the volume on our table, but will give Bartlett's version of Chomel's account of cases of this description. Bartlett on Fevers of the U. S.—page 107: "Chomel admits several forms or varieties of typhoid fever, *not depending upon degrees of severity*. These are the *inflammatory*, the *bilious*, the *mucous*, the *ataxic*, and the *adynamic*. * * * * * The bilious variety is characterized by some yellowness about the lips and nose; a thick, yellowish, or greenish coat of the tongue; a bitter taste; nausea and bilious vomiting. Chomel regards this form of typhoid fever, which occurs oftenest, he thinks, during the summer and autumn, and in particular localities, as identical with the bilious fevers of authors." This corresponds precisely with the variety of fever seen by the author of the Medical history in the *up country*. Chomel gives it as a variety of *typhoid fever*, not *depending upon degrees of severity*. Touching any further information on the subject, we must refer Dr. Boling to M. Chomel himself, our only purpose being to show that this French writer gives to the author all the latitude he has taken.

The author, from the notes and testimony before him, describes this typhoid fever of the hilly portions of the State as follows:

"Typhoid fever, whether remittent at the first, or continued from the onset, is attended with extreme debility and great prostration of body, nervousness, irritability, perversion of the senses, stupor, sometimes delirium and pain in the head and limbs; thirst and heat of skin are frequently the variable, although prominent symptoms. The pulse is small and frequent, the bowels are slightly tympanitic and sore on pressure. The tongue is usually round and lengthened, dark in the middle, the edges inclining to fade, and always dry, but when it becomes moist it is generally a sign of convalescence, which may occur in 8 or 10 days, but the usual duration of the fever is from 15 to 25 days."

"In the treatment of the continued fevers of this section of country, whether they are idiopathic, or supervening upon a remittent type, local depletion cautiously practised, diaphoretics, sinapisms, emolient poultices, diffusible stimulants, and mild alteratives, are the remedies usually employed by judicious practitioners. Quinine has been used at different periods, in every variety of dose and form, but always tending to aggravate the disease, and increase that disposition to local inflammation which usually exist in the cases of that region."

The physicians throughout the Southern country are in the habit of calling all cases of continued fever, the leading symptoms of which, regardless of their anatomical characters, are those resembling the morbid phenomena of typhus, Typhoid fever. Nor are they alone in this understanding; they are sustained, analogically at least, by the most eminent writers in the world. We quote from London Edition, British Encyclopedia of Practical Medicine, page 175: "There can be no doubt of the existence of every intermediate grade between the common forms of fever, synochus and typhus, so that it becomes a matter of nicety to discriminate to which class a particular case, or number of cases, properly belongs." Watson, page 834: "There is no line of *genuine distinction* between continued fevers that can be relied upon." Again at page 845: "Although fever is, as I have stated, a *specific disease*, it assumes divers forms; and so dissimilar are some of its phases that they might seem to belong to totally different maladies." Watson, continuing his remarks upon the *inflammatory* and *typhoid* types of continued fever, says—"These forms I would have you bear in mind, not that you are likely to meet with many instances of *pure synocha*, nor of *pure typhus*, but because they furnish *standards* of *comparison*, towards which, in opposite directions, the fevers of different epidemics approach." The same writer also says—"Most generally of all, the disorder commences with *inflammatory fever* and ends with *typhoid symptoms*." Take, now, the intermediate grades, between the periodical fever of summer and the pure typhoid of winter, described by the author of the Medical History, and who can say, in view of the constructions of writers and understanding among medical men, that he has made a wrong diagnosis?

We here beg leave to diverge from the main thread of our narrative for the purpose of illustrating and enforcing the author's views, in relation to the change which took place in the character of the diseases of Alabama between 1830 and 1834. We quote from a writer, in fol. following whom time is never thrown away. Watson, page 845: "The difference is very striking between the kind of fever that I witnessed in London for ten years before the arrival of the spasmodic cholera in this country, and the kind of fever that has since prevailed and is now (1838) so rife around us. During the first of these periods, the antiphlogistic regimen was indispensable in the outset of the disease; in most instances, bleeding, either general or topical, was required, and well borne. There was no eruption to be seen upon the skin; the glands of Peyer, according to my own experience of the fatal cases, were almost invariably affected; and the mortality was very moderate. This *was an inflammatory phase*. The present epidemic offers a marked contrast in all these points. A large per centage of those who contract the fever die; after death we seldom detect any disease of the agminated glands of the intestine. We are taught by experience to refrain as much as possible from blood-letting; and almost from the beginning, or quite, we find it necessary to sustain our patients by a liberal allowance of strong animal broth. *The typhoid is now the prevailing type*. Moreover, all acute diseases have assumed, within the last ten years, in this town at least, an unusually *asthenic* character." After *summing up* the facts collected, in relation to the diseases of Alabama, the author, in effect, states: That periodical fever is the common endemic of the country at all times; that previous to 1830 it was *inflammatory*; since then, and especially in 1834, it assumed a *low typhoid type*, in some localities and sections of the country, adynamic, or in the language of Watson, it became unusually *asthenic*, offering a strong contrast to the fevers of the preceding period. As Dr. Boling is so very *critical*, it may be necessary to state, that we do not *assume*, in this analogy, that continued and periodical fevers are identical.

We will now return to typhoid fever, or a disease known in Alabama by that name. It is fearfully and rapidly increasing in the South, and it is time there should be some understanding in relation to its diagnosis and true character. Doctor Boling denies the existence of *true typhoid fever* in Alabama. Let us examine the question.

From the general tenor of Dr. Boling's remarks we infer that he belongs to that class of pathologists, who, with Chomel and Louis as their leaders in France, and Bartlett and Gerhard in the United States, believe, and as we humbly think, have established on rational and conditional grounds, that there is a distinct disease, which they call *typhoid fever*, arising from a specific poison, differing fundamentally from typhus and all others. As the English are unable to trace out such a distinction, in the continued fevers of that Island, it is probable that such authorities as Watson, Tweedie, Connolly and Forbes, may not be considered valid by Dr. Boling; be it so. We will test the question, upon which he (Dr. B.) and the author of the Medical History are at issue, by the light which Dr. Boling has set before us.

Doctor Bartlett, with a view to ascertain the geographical limits of typhoid fever, addressed a letter through the New Orleans Medical Journal to physicians of the South, on that subject. The following extract is from that letter. January No., 1847: "Doct. Boling of Montgomery, Alabama, in an elaborate and valuable article on remittent fever, published in the American Journal, speaks of *protracted cases, attended with spontaneous diarrhea and tympanitic distension of the abdomen*, and a feeling of pain or uneasiness *between the umbilicus and right iliac fossa*. He also reports two *protracted cases*, in which he found ulceration of Peyer's glands after death. Now it seems to me *hardly possible to doubt, that these were*, all of them, *cases of true continued typhoid fever*." To this portion of Dr. Bartlett's letter, Dr. Boling makes an early and direct reply: it is to the point. We quote from a letter written by Dr. Boling to the Editors of the N. Orleans Journal, March No., 1847, page 685. "By the way, to the remark of Professor Bartlett, in the last No. of your Journal, page 552, 'now it seems to me hardly possible to doubt, that these, all of them, were cases of true continued typhoid fever,' I must answer, that with me there is no doubt whatever on the subject—emphatically they were not such." The want of courtesy distinguishing this reply, is thrown entirely into the shade by the coolness and confidence with which it is made—"emphatically they were not such"—is the indignant and conclusive disclaimer to Dr. Bartlett's carefully formed and deliberate conviction, that Dr. Boling had described, under the head of *remittent*, *true typhoid fever*. And who is Dr. Bartlett? For many years the campanion of Louis in pathological investigations; for a long time a practitioner of eminence in Lowell, where his [improved] opportunities for the study of typhoid fever were unrivalled; since then, a lecturer on Morbid Anatomy and the Practice of Medicine, in various schools of high standing; and, finally, the author of a work on the fevers of the United States, the most perfcct, probably, of this age, and so long as American medical science shall exist, will form one of its most conspicuous monuments.

This is the pathologist, whose calm and well fortified opinion Dr. Boling sets aside with one dash of the pen—"emphatically they were not such." But it appears that some further correspondence has taken place between Drs. Bartlett and Boling, in reference to these cases. At page 337, Bartlett's new work on fevers, we find the following note. "Since writing the above, I have had a correspondence with Dr. Boling in regard to the most striking of these *two cases*. His letter to me contains nothing to induce me to change the opinion already expressed, in regard to the true character of these cases." So it appears that Dr. Boling has argued the case, but still, that eminent man can find nothing to "change his opinion." As Dr. Boling has been treating *true typhoid fever*, without a knowledge of its true character, this, too, after describing in his own cases the characteristic lesion of the disease, we might, with the utmost propriety, pass over his opinions upon these subjects as being utterly worthless; but this would not settle the question under examination.

The author of the Medical History, although satisfied that true *typhoid fever* prevailed to some extent in the hilly region of country, during the autumn, and that all cases of remittent fever that become continued, were attended in this stage with symptoms of a typhoid character, was unwilling to admit that it was the prevailing disease during this season. When he comes, however, to comment on the diseases of winter and spring, relying upon his own experience, corroborated by the observations of Drs. Hogan and Clark, he does not hesitate to state, that typhoid pneumonic, and typhoid fever, were the most usual forms of fatal disease, met with during these seasons; and this, too, in every section of the State,—these disorders being modified in some localities. Notwithstanding the mass of evidence before him, (for the reader is referred to the Medical History—diseases of winter and spring) Dr. Boling still expresses doubts. After a few extracts, made with his *usual impartiality*,* from that portion of the Medical History, Dr. Boling remarks: "There are many physicians of our acquaintance who are much inclined to doubt whether the genuine typhoid fever has ever appeared among us. Will these doubts be removed by the lengthened quotations we have made, after comparing them with the account of typhoid fever as given by Bartlett?" Dr. Boling, then, acknowledges Doctor Bartlett as authority; he is the standard. After the occurrences just detailed, we should think this a most painful admission on the part of Dr. Boling; but we commend his humility, and consent, that Bartlett shall, in part at least, decide the question. We quote from Bartlett's new treatise on fevers, page 85. "Doctor Wooten, of Lowndesboro, Alabama, says—'There are physicians in our State who contend that we have no fevers except those of a remittent or intermittent type. But my experience justifies me in declaring this to be an error. Typhoid fever does exist here. It appears at all seasons of the year; but I think it is most common in spring and early summer. Its occurrence is far more frequent of late years than formerly. In 1836, I saw one case of it. In 1837, I had

* Dr. Clark's observations, upon which the author placed much reliance, are omitted by Dr. Boling.

a very serious attack of it myself; I was seen by some half dozen experienced physicians, all of whom spoke of it as a *very rare case.* It has gradually grown more and more prevalent, until it is now looked upon as a very common disease. It is unnecessary to describe the symptoms of this fever. It is sufficient to say that they are *described by you* under the head of *Typhoid Fever*, and that it is unquestionably the disease for which you inquire.' "

If Doctor Boling doubts the correctness of the diagnosis, made by the author of the Medical History, what will he say to the following. We again quote from Dr. Boling's *standard authority.* Bartlett on Fevers, new edition, page 127: "Typhoid fever, like all other continued affections, is sometimes more or less mixed up with and influenced by the pathological element of periodicity. This will happen most frequently, and be most strongly marked in malarious regions, and during the prevalence of remittent and intermittent fever. Dr. Wooten, of Lowndesboro', Alabama, in a letter to me says, "I may remark that I have often seen typhoid fever complicated with regular remittence—that is, typhoid fever and remittent fever existing together; and I have cured the paroxysmal exacerbations, whilst the disease essential to typhoid fever continued; and I have frequently found it necessary to do this, before the more formidable disease could be influenced by remedies. I have seen such cases in the practice of physicians, who supposed them to be remittent or bilious fevers, in which the bowels had become diseased as a consequence of fever. I think it is a very common error. The malarial influence, frequently, so preponderates in the symptoms of inflammatory diseases in our dlimate, as to obscure the real disease for many days; and in such cases it is easy to look upon such as the cause of the structural lesion, whilst, in fact, the latter has acted as the exciting cause to the manifestations of the former. Doctor Wooten says that typhoid and remittent fever exist together, and Dr. Bartlett, the most ultra of rigid and exact constructionists, sustains him. We wish the reader to bear this in mind.

Since writing our essay on the Medical History of Alabama, our information relative to this affection, as it exists in the State, has been more complete and satisfactory. In Shelby County it appeared in the spring of 1846, affecting more or less every family, in a densely populated settlement. Doct. White, formerly a pupil of Doct. Bartlett, describes it as true typhoid fever, uncontrolled by quinine, and spreading, as he believes, by contagion. We have received from Dr. J. C. Knox, of Talladega, a letter of a late date, from which we make the following extract: "I have seen a disease among the negroes on two plantations in this county, and also a few scattering cases elsewhere in the county, which was *precisely such a disease* as is described by writers as *typhoid fever.* The disease of which I speak corresponded, not only in its external symptoms, but also in its post mortem appearances. *The mesenteric glands were enlarged and inflamed. The mucous surface of the small bowels was extensively inflamed, and in some cases ulcerated. And so far as I could determine, there were no other lesions* which could be set down as *characteristic of the disease*, as the other organs seemed to be in a healthy condition. It is useless for me to go into an extended description of the various symptoms of the disease, as they were, in the main, just such as have been minutely described by Bartlett and others." In the prairie region, as Sumpter and Marengo, typhoid or continued fever has prevailed to considerable extent during the past spring and summer, and in some localities with great malignity; many of the youthful of both sexes having fallen victims to it. We regret the impracticability of inserting here such a minute account of the disease, as the notes furnished by medical friends would enable us to give: sufficient to say, that many of the cases, especially in Sumpter, the hot bed of congestive fever, were continued fevers of the adynamic variety, terminated in 8 or 10 days, and were exceedingly violent and unmanageable.

Dr. Clark, of Woodville, Marengo County, detailed to us many cases. He and Doctor Langhorn made six autopsies. In these cases the disease was from 20 to 40 days in running its course. There was enlargement of the mesenteric glands, with extensive disease of the ileum, in two of which, this intestine was perforated. They do not mention any thing of the *rose colored spots* spoken of by others.

At this time in Mobile, there are many cases of protracted continued fever; Dr. Nott and myself are treating three cases in private practice, two of which are certainly typhoid fever, the other coming more closely up to the descriptions given of Irish Typhus. The subjects are whites, regular citizens of the city, and between 15 and 20 years of age. In one of these cases, *rose colored spots* have been distinctly traceable for the last four days. Doctor Knox, in the letter just quoted, says, that in the cases he has treated in Talladega, there was a constant desire to eat, which continues throughout the disease; the same desire is conspicuous in the majority of cases occurring in Mobile; nothing but the strongest kind of food will satisfy their *cravings*.

On yesterday we examined the body of a young man, who died in the Marine Hospital, after 28 days illness. The mucus coat of the ileum was of a *dark greyish* or ash color; four of the elliptical plates, situated at the lower extremity of the intestine, were *raised* and *thickened;* in the lowest of these diseased patches there was a small *gangrenous* ulcer, corresponding in size to the finger nail. The spleen was enlarged and softened, the mesenteric ganglia but slightly altered. In two cases of *Ship Typhus* examined also in the Hospital, the mucus membrane of the small intestines was of a dark red color, but no special alterations could be discovered in the elliptical plates.

We could extend and enlarge upon this testimony, but it is wholly unnecessary to our present purpose. Doctor Boling, and the *physicians of his acquaintance*, may still deny, if they like, the existence of typhoid fever in Alabama; the medical public, we are confident, will sustain the author of the Medical History; and possibly teach Dr. Boling that a mere *opinion*, dogmatically promulgated, will not rule the minds of men, nor stay the hand of investigation.

Doctor Boling, at last, quotes a short passage from the Medical History, the truth of which he fully admits. As it is more than pleasant even for a moment, to exchange kind civilities with an old friend, we will also insert it. "Taking into view the rapid decline of severe autumnal fevers, together with the lessening mortality attending them and the rapid increase of those diseases just pointed out, the winter and spring diseases may now be regarded as the most fearful maladies within the borders of the State. To the negro, whose organization is such as to endure the heat of summer with impunity, these diseases which come on the chilly blast, and are nourished by cold and moisture, are peculiarly noxious and alarmingly fatal." The diseases alluded to are Rubeola, Scarlatina, Pneumonia, and Typhoid Fever. Immediately following, he quotes another not so much to his liking; it is as follows: "The exanthematous affections like those of thoracic viseca, are rapidly increasing, and if we are to judge by the effects generally produced by physical changes, the day is not far distant when they will become the prominent disorders of the State, and the affections of summer and autumn gradually yield to an improved state of cultivation." Upon this the Doctor remarks as follows: "We are somewhat at a loss how to construe the above sentence. Does Doctor Lewis really mean to say, that the exanthemata are increasing in proportion as our autumnal diseases are disappearing—both being the result of 'physical changes,' and 'an improved state of cultivation?' Or, merely, that without any proportionate increase, this class of diseases will rise into importance in consequence of the partial disappearance of others, which have heretofore attracted our principal attention, and by which they have been, as it were, overshadowed? However, we can conceive that even a proportionate increase in the number of cases of the contagious exanthemata, would probably occur in a more closely settled and as a general rule, therefore, more highly cultivated section; not in consequence of an improved state of cultivation, *per se*, but from the greater and more constant intercourse among the inhabitants, facilitating the spread of diseases of this character."

It is a very difficult matter to give a complete and satisfactory reply to Dr. Boling's inquiry. We know, however, that in all countries, even those of the extreme North, during the transition from its forest state to a well improved and highly cultivated condition, periodical fever is the prevailing disease; and that, after "physical changes" have wrought a revolution in the surface of the earth, so that the vegetable remains have become completely exhausted, these diseases disappear. In confirmation of this, it is only necessary to refer to such cities as Rome, Marseilles, Philadelphia, Charleston, New Orleans and Mobile. With the exception of the suburbs, a case of uncomplicated remittent fever is very rare: whereas, at one time, this was the predominant disease in these localities. Doctor Boling cannot be insensible to the change, in this respect, now going on in Alabama. The fact that, as a country is improved, periodical fevers are superseded by those of a different type, has given rise among writers, at the North, to the opinion that there is an "antagonism between typhoid fever, on the one hand, and intermittent fever and phthisis on the other." Whilst at the South it is the impression (of which we *once partook*) that yellow fever *drives remittents* from our seaboard cities. After a laborious investigation of this subject, in connection with the malarial hypothesis of disease, we are driven to the conviction that these opinions are completely overthrown by the facts, at least as they exist in Alabama. We have reason to know that tubercular phthisis, in five instances terminating in death, was complicated with intermittent fever. At no time, during the last 16 months of their existence were these unfortunate patients exempt for eighteen consecutive days from regular paroxysms of intermittent. A reverend gentleman of distinction, now in low health in Mobile, exposed himself whilst laboring under a slight attack of Laryngitis, to the malaria of the Escumbia, and was seized with periodical fever. We had occasion to visit him two years after; disease by this time had made deep inroads upon the left lung,* the periodical affection still playing a prominent and pernicious part. It is well to mention that with the exception of the latter, these subjects were natives of the State. In like manner do we find this busy agent embracing every malady that comes within its reach; even the yellow fever cannot approach its haunts without being influenced by it; instead of an antagonism, it seems to have a great affinity for every disorder; to banish it from a locality, you must destroy the source whence it originates.

Dr. Boling's suggestion, that an increased prevalence of the Exanthemata is owing to the contagious nature of these diseases, explains it only in part, and so far merely as they are concerned. Facts and observation teach us how it is that intermittents disappear; but what constitutes the producing cause of these new disorders which take their place, is left to conjecture. We only know, that whilst the one disappear before the march of improvement, the cultivation of the soil and increase of population, the *others spring into existence.*

CONGESTIVE FEVER.

At the same time that it is impracticable to follow Dr. Boling, very closely, in his winding and uncertain path, it is difficult to get a fair and direct issue with him. We will endeavor, however, not to misrepresent him. In his comments on that portion of the Medical history, devoted to the congestive fever of the prairies, he seems to find no positive objection to the general description given of that disease; but asserts, with that reckless and unreflecting confidence which distinguishes his writings,—that he is unable, from these descriptions, to discover the distinctive phenomena characterizing this disease. The physicians of Alabama will doubtless be surprised to learn, that there is a medical writer, a critic forsooth, living and practising in the South, who openly declares that there are no "symptoms pathognomonic of congestive fever"—a disease, which of all others is most peculiar and easily diagnosed. For this extraordinary conclusion it is difficult to account, and the only way in which it can be done, is, by supposing that Dr. Boling has mistaken some forms of remittent fever, where this congestive element of disease was but partially displayed, for the genuine disorder. If an observer is so defective of correct perception, as to mistake *typhoid fever, follicular enteritis*, for remittent fever, it is possible he may have mistaken modified remittent for true congestive fever:—hence his inability to give to the observations and descriptions of others, that force and effect to which they are entitled. *Apropos*—The author of the Me-

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* In 60 cases of acute and chronic disease of the lungs, treated in the Marine Hospital, (two thirds of them being complicated with intermittent,) the left lung in 45 seemed to be the one more especially involved.

didical history, when attempting to place before the reader the fact, that congestive fever was very partial to some localities, while it did not appear in others, uses the following language in relation to Montgomery, Dr. Boling's place of residence: A correspondent, residing for many years in Montgomery, a town situated amidst diluvial elevations of the prairie region, informs us, that he "has not seen any case of congestive fever, such as has been described by medical men living in other sections of the State—that he has seen cases of simple fever *collapse* or become complicated, (under the influence of injudicious remedies) resembling in that condition the congestive state." From abundant testimony it would appear, that this town is exempt from the disease in its true characteristic form; take, for instance, the able paper of Dr. Boling on the fevers of that vicinity. He describes no case of febrile affection, which physicians of certain localities, in Green, Marengo and Dallas, would recognise as congestive fever." When the author of the History made this respectful reference to Dr. Boling's work on remittent fever, he little dreamed that he was treading on such dangerous ground. As Dr. Boling's descriptions of disease, coupled with the statement at the close of his treatise, that all his observations in relation to remittent fever "are made with reference to the disease as it prevails here in Montgomery and in its immediate vicinity"—did not impress the author with the belief that he had seen *true* congestive fever, he naturally concluded that, for want of a practical knowledge, he avoided any special attempt at description. But it appears from his indignant rebukes at our bare intimation that he had not described the disease; that he comprehends in that essay *an account* of all the fevers common to the State. Although we avoided any other than respectful and complimentary allusion to Dr. Boling's treatise on Fevers—notwithstanding that it came within our province—and even now would be pleased to avoid any notice of it, as *descriptive of the prominent diseases of the State*; yet, as it is unceremoniously and indecorously thrust into our face, it will become necessary, *bye and bye*, to examine its pretensions to the character lately claimed for it.

Dr. Boling has assumed for us, that congestive fever is a disease *sui generis*, differing radically from all others. We wish in this place to have this question defined and understood. If Dr. Boling means that we believe congestive fever to be as distinct from, and dissimilar, to *remittent fever*, as small pox, for instance, is from remittent fever, then we object to such a construction. The author of the Medical history has been as explicit on this subject as his use of words would allow him to be; and that the question, as raised by him, may be fairly stated and understandingly discussed, we will make some short extracts from that paper:

"In relation to disease as it is presented during this epoch, (from 1834 to 1847,) it has already been intimated that there was a display of certain specific differences in various sections and localities of the State which could not be reconciled in any other way than by supposing that they were owing to some peculiarity attaching to the organic nature of each locality and region.

That the chemical character of the soil varies with the geolocical systems or formations, and that the morbific agents are as varied as these, is so evident as to require no argument. In fact, it is upon this hypothesis that the old and well established doctrine of malaria alone can rest with security :—for if disease were uniformly the same in regions distinguished by separate and peculiar physical characters, even in the same latitude, then that theory which is based on the supposition that these different formations would give out emanations that are identical, would not be entitled to serious consideration. But we find in Alabama that, although the diseases may belong to the same family, yet they are distinguished by certain differences as apparent as are the physical characteristics peculiar to the regions where they respectively prevail."—

After giving such an account of the geological structure of the State and chemical character of the soil, as his information would allow him to make ; together with a brief sketch of the character of disease, peculiar somewhat to each division, the author concludes as follows :

"The exanthematous fevers, as small-pox, chicken-pox, measles, and scarlet fever, occupy amongst the groups of diseases what is called the type family. In their mode of access, phenomena, changes, periods or stages, duration, impossibility of interfering by art with their course and perfect immunity after attack, constitute a perfect resemblance; yet a characteristic eruption stamps them as distinct diseases, caused by a specific poison or contagion.

The family of autumnal fevers that have occupied our attention, do not, in their mode of access, phenomena, changes, periods or stages, duration, impossibility of interfering with their course by art, &c., &c., discover the same resemblance one with the other, as do the exanthematous fevers—still, their origin, history and great affinity, entitle them to be classed as one of the families of disease that belong to our State."

With these qualifications and *unaltered conclusions*, it now becomes important to state, what we mean by congestive fever, as constituting, in this *family of diseases*, one that should be classed as a separate and distinct malady.

In looking over the books, we meet with the terms typhoid pneumonia, typhoid yellow fever, etc., which means, that this pathological condition, called *typhoid*, has been superadded, or has supervened upon a distinct and separate disorder, as the case may be. Louis and Bartlett both inform the reader that this element is not only very prominent in disorders wholly different from *typhoid fever*, but also in morbid conditions arising from injuries. Notwithstanding this, patient and rigid investigation has established the fact, that there is a distinct disease, a disease *sui generis*, called *typhoid fever*;—and this, too, in a large family or circle of diseases, resembling it very closely, and contended by a majority of writers, to be identical. In like manner do we meet with the terms adynamic pneumonia, adynamic typhoid fever, adynamic yellow fever, etc., by which is meant that this morbid element has been superadded to the phenomena essential to these respective maladies. We will now be able to get at the question :—Does this striking pathological element or condition, called *adynamic*, ever exist alone, from the access of disease, throughout its course and to its termination in death or a healthful reaction? We are driven by personal observation and experience to say that it does. If this then brings us to the conclusion, that congestive fever is a disease *sui generis, differing radically from all others;* we most readily embrace it.

Dr. Boling says that this question is of importance to the medical men of the South; let us examine it then free from prejudice or party bias and in the language of a writer on typhus and typhoid fever—"whether the two diseases be or be not specifically and nosologically unlike each other, it is equally important that the *wide differences* that do exist between them, should be pointed out and their real relations be established."

To understand the true character of this disease, and the nature of that connection which exists between it and remittent fever, we should enquire what were the peculiarities, if any, attaching to it, in the primitive days of its existence in this country. If a naturalist, who wished to ascertain the differences, anatomical and mental, between the Caucasian and African, were to confine his investigations to the *quadroons* of Mobile and New-Orleans, he would discover but few appreciable differences; but if he should take one of the primitive stock, the real African, he will have no difficulty in discovering the well marked characteristics distinguishing one race from the other. Thus it is (if we are permitted the *simile*) not only with congestive, but yellow fever;—distinct and peculiar at one time, or in some localities, we again find them so changed and modified, by mixing with kindred affections, as to loose much of their identity.

We will introduce some authentic information touching the early history of this disease in the Southern country.* Surgeon Harnly, in relation to the garrison at Baton Rouge, remarks that the soldiery suffered in 1821, 1822, and 1823, with a disease called the *cold plague*, during which years the village was free from disease. He supposed that the great mortality among the troops was occasioned by their exposure, whilst getting timbers from the Mississippi swamp. Another report made, it appears, by some other surgeon, from the same station, mentions among the deaths that occurred—twelve of "*Congestive Typhus*;"—doubtless the disease, then more commonly known as *cold plague*. The following satisfactory account of this disease is from a report made from Baton Rouge by Surgeon Genl. Lawson in 1832.† "By far the most fatal disease of Louisiana is the congestive form of fever, or as it is called here the *cold plague*. It is an insidious enemy, attacking most commonly the weak and enfeebled, and those laboring under mental depression. In many instances the subject of the disease, before he himself or those around him are aware of it, *becomes cold in the extremities, and on the superficies of the body generally*, with the exception perhaps of the region of the chest; the blood retires to the interior of the system, and the patient is at once prostrated. The vital organs being overwhelmed, the system of itself cannot react, and not unfrequently all the means of art are of no avail in removing the load of oppression. There are other instances, however, in which the disease, though always insidious in its invasion and never without danger, is less severe in its attack." In what particular does this disease, described by Surgeon Genl. Lawson, resemble any

* Forry on the Climate of the U. S.

† Forry.

other in this country? Without *the knowledge of the patient*, the extremities and superficies of the body become cold, the vital organs being overwhelmed, the *system* of *itself cannot react*—in some instances the attack is less severe, but never without danger. Where, pray, can be fouud in this description any thing approaching a regular paroxysm of remittent fever, or even the *initial chill* of this disease?

Whilst a student in the Orleans Charity Hospital, an incident of some interest, touching the resemblance between Asiatic Cholera and *cold plague*, occurred. In July 1833 we received into the Institution two cases of disease, which were announced to Dr. Ker, who visited the Hospital shortly after, as two very bad cases of Cholera; after an examination the Doctor pronounced them not Cholera but a disease called *cold plague* or *congestive fever*, cases of which he had occasionally seen for the last ten years, and it was more fatal even than the Cholera. Several gentlemen present were disposed to doubt the correctness of the Doctor's diagnosis; but after he had pointed out the differences between the two diseases, yielded their objections. The man, who then should have called these cases remittent fever of *any form* or *variety*, would have been laughed at. But to proceed.

In a sketch on the resources and climate of Cape Florida—published in the Gazette of that territory—Dr. Crews incidently remarks that in 1824 a number of white laborers were engaged in making some improvements contiguous to the low lands of Apalachicola. A fatal disease made its appearance among these people in July, and the Doctor being in the neighborhood officiated as their medical attendant. The following quotation embraces all he has to say on the subject. "This disease, though new to me, is of frequent occurrence on the lower Mississippi and is called there the *cold plague*, probably *congestive typhus* would be a more appropriate name for it. It was ushered in by cold skin bedewed with a sweat equally so. In these cases the coldness of the surface was far more intense, than in the collapse stage of any other disease. There was much uneasiness about the stomach and efforts to vomit were frequent. The pulse was small and frequent, and in a majority of cases became imperceptible in eight or ten hours. The bowels were hard to move, and when evacuations were procured they consisted mostly of a little blood and mucus. The difficult and labored breathing, great internal heat and thirst in these cases constituted a degree of suffering and torture which I have never seen equalled. Five out of nine that I treated died." Now mark the language of Dr. Crews. It is called *cold plague*, but "*congestive typhus* would be a more appropriate name;"—and when we take into consideration the great loss and perversion of nervous power, with the recession of blood from the surface, a name more completely comprehending this peculiar condition could not be found. Who from this description can trace the least resemblance or connection between this *strong* disease and remittent fever?

We here introduce some passages from an exceedingly able treatise on this disease by Dr. G. W. Wright, at one time Editor of the Western Medical and Physical Journal, and, we believe, now a teacher in the Cincinnati Medical School. We cannot discover from the paper the particular locality where the Doctor made his observations, but it ful sensation to the hand; the stomach may remain undisturbed, but most commonly there is nausea and a vomiting.

Doctor Johnson says, there is in some instances such a degree of coldness as to produce shaking, but that the patient is *not conscious of coldness.* We have noticed this *shaking* or *tremulousness* in a few instances; in these it followed and seemed to be caused by great physical exertion on the part of the patient. We have noticed the same phenomena, under like circumstances, in *ataxic fever.* We wish the reader to notice, particularly, that line in Doctor Johnson's description, touching the action of the heart. Doctor Boling, we believe it is him, says it is *strong and loud.* Doctor Johnson, with all the simplicity of nature itself, says, and says truly, "the action of the heart is so *feeble* as not to be felt with the hand on the chest."

Doctor R. Johnson, formerly of Willcox, late of Munroe, wrote out a reply to our interrogatory letter of June, 1846; it did not come to hand, however, in time for insertion in the Medical History. From 1832 to 1840, the Doctor was actively engaged in the practice of his profession—to practical tact he brought acuteness and strength of intellect. His letter we copy, at least that portion of it which relates to this *particular* subject. "Your interrogatory in relation to the identity of remittent and congestive fever, I have no doubt will strike many of our Doctors, especially those who have lately entered practice in the South, as extraordinary and uncalled for; but to me, witnessing as I did the awful effects of congestive fever on the Alabama River in 1834 and 1835, it creates no surprise. In those days I never heard it suggested that the disease had any connection with remittent fever; so far from it, that it was called, in 1834, the cold *plague.* It came on with cold skin, cold sweat, sickness at stomach, oppression of the lungs and great anxiety. There might have been, tn the course of the afternoon, a slight return of warmth to the surface, with less difficulty of breathing, and some *drying* up of the sweat; upon which the patient would grow animated and cheerful: but the observing medical man, who had been taught a few painful lessons, would, by a *quick compressible* pulse, feeble action of the heart, and a want of a substantial natural warmth of the skin, be apprised that the strong hand of disease was still upon the sufferer. To be as short as possible, I regard congestive then, and congestive now, one continued state of peculiar diseased action, from the onset to death or recovery. In regard to its fatality, I think that in 1834 and 1835, one half of those attacked died.* It was confined

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"Tom Judge of Lowndes—I think it was Tom—was coming up the River once, from Mobile, when a gentleman from some Northern State, going to settle in Selma, walked up to him and inquired if there were any *Alligators* in that stream. Tom took the dimensions of the customer with his eye, looked him in the eye, and ascertained that he was soft; and then dolorously sighing, answered—

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"In 1835, intermittents and remittents prevailed from the first of May until the first of July; they then began to decline, and by the middle of the month the country was exempt from disease of every kind. The congestive fever made its appearance early in August, and continued its ravages until the middle of October. The following case, condensed from notes taken at the time, will best serve the purpose of description.

Mr. A., aged 24, native of the State, of robust constitution, came to my office at 9 o'clock in the morning for advice. Says he has been for the last week engaged in superintending "*some work*" in an adjoining prairie swamp; went to bed last night feeling well, awoke this morning at daylight, since when, he has been weak or languid, not particularly sick, but is unable to shake off a restless, desponding and uneasy feeling. Found his pulse not exceeding 100, but small and deep seated, skin cool and damp, which was attributed to exposure, the atmosphere being warm and very damp. Says he is thirsty but *does not feel chilly;* his difficulty mainly consists, to use his own language, in an "inability to get my breath." In two hours after he returned home, some two miles from my office, he wrote and despatched the following note: "Come and do something for me or I shall die." Did not see him until 3 o'clock in the afternoon; his condition then, was nearly as follows—skin cold, of a pale blueish color, muscles soft, unless put upon the stretch by exertion, profuse perspiration over the entire surface, standing in large drops on the chest and forehead, tongue cold, pale and inclined to a livid hue, pulse frequent, small and thready. Action of the heart changed to a *tremulous flutter*, with now and then a violent pulsation, causing the patient to start, urine abundant and colorless, bowels torpid, slight nausea, countenance haggard, expressing the deepest agony or

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"Was your friend drowned?" inquired the green 'un.

"No; he died of that most horrible of Southern diseases, the *congestive fever.*"

After a pause, Spooney essayed again—"What caused the disappearance of the Alligators?"

"They died of the same disease," replied Tom, looking the stranger steadily in the face, with a sepulchral expression.

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physical trouble, very restless, walks rapidly over the floor for a moment, then sinks exhausted, wants an emetic to relieve his breathing, is perfectly sensible. About 10 o'clock at night the perspiration sensibly diminished, pulse improved and the breathing became easier. This partial improvement continued until 6 o'clock in the morning. At 10 o'clock in the morning found him much worse—pulse imperceptible at the wrist, tongue and lips livid, skin icy cold, dependant portions wilted and shrivelled; other symptoms about as yesterday. After a doubtful struggle of 6 or 8 hours, during which time *there* is *no complaint* of *chilliness*, but great heat and burning, the pulse again becomes perceptible, a slight degree of warmth returns to the surface, and hope again enlivens the household. The next morning, I was summoned early. He began to grow worse at 2 o'clock, made several efforts to vomit; had not been chilly; he now speaks in monosyllables, says he "is sensible, but has no breath to talk." Is extremely restless, skin cold, blueish and mottled about the back—each expiration is attended with a harsh distressed murmur, cannot bear any covering—finally, in a paroxysm of wild dispair, rises from the bed, rushes to a window, and whilst holding to the facing, is seized with a convulsion. He expired in a few minutes, after being laid upon his couch; having been ill but 52 hours."

In this case, notwithstanding the most active means were used, there was no evacuation of fæces from the bowels. During efforts to vomit, a little water mixed with mucus was occasionally ejected.

By reference to notes and the recollection of conversations had with Drs. Gantt and Herbert, who did a leading practice in the county, the foregoing case represents the general condition and symptoms of those attacked—the difference in the cases, consisting in degree, or the viscera more especially implicated. For instance, in the case described, it appears that the first *appreciable* derangement of function, was that of the respiratory apparatus; this grew more and more prominent, until the lungs totally failed in their office. In many cases there was pain in the head, flushed face, watery injected eye, sensibility to light, and an early supervention of delirium. In this variety, labored and difficult respiration was not so prominent; this, together with the absence of symptomic gastric complication, caused practitioners to refer to the brain as the seat of mischief. Again, there were cases (and probably a plurality were of this description) where early nausea, sinking at the stomach, vomiting and frequent stools of sero-mucous matter, were the most prominent and urgent symptoms of disease—here, the stomach and intestines were pronounced to be the points most especially involved. Although one or the other of these determinations may have existed in one case, or they all may have been manifest in another, producing death in 24 hours; yet, as a general rule, the assemblage of symptoms characterizing the disease were very uniform.

With a majority of those attacked, there was in the course of 24 hours, a return of warmth to the surface, and in fact a general improvement, showing a strong effort or disposition on the part of nature to react—but if this reaction amounted only to a gentle glow of warmth on the surface, the pulse continuing quick, and perspiration profuse, the ensuing morn witnessed, in a more aggravated form, the return of all the perilous and depressing symptoms so peculiar to the disease.

The formation of the *true and healthful* reaction could always be distinguished from the false and ephemeral. In the first case, the *perspiration sensibly diminishes;* as the skin becomes dry, the capillaries fill, and a deep general warmth ensues; the pulse, which a short time since may have been extinct at the wrist, is now perceived, and as it becomes raised more to the surface, it lessens in frequency and increases in force. We did not see a case, where the skin became *dry and warm*, that the patient did not recover, and that very rapidly. During the improving stage, evacuations of a bottle green color, and about the consistence of grape jelly, took place; often as much as a gallon passed in 48 hours: this remarkable colluvies was inodorous, devoid of bile, breaking into pieces like the coagula of blood. Occasionally organic or functional disorder followed the patient, but *relapse* or *second* attacks did not occur.

Our attention thus far has been directed to the graver and more malignant cases, about one third of which proved fatal between the 2nd and 3d day. There were those of a much lighter and milder grade, distinguished by the same phenomena—such as cool skin, continued perspiration, thirst, quick thready pulse, interrupted respiration, uneasiness and occasional sighing. These symptoms would deepen and become more urgent in the morning, a partial reaction taking place in the afternoon. In these as well as those previously described, there was no marked remission or intermission; the patient continuing in a pathological condition, until a *dryness* or *permanent warmth* of *skin* announced a restoration of the functions of the body. In no description or variety of case, could there be detected, even in the *cold stage*, that *shivering* or sensation of *coldness* that causes the patient in intermittents to *seek* the *fire*, the *sun*, or *warmth of clothing*:—On the contrary, there was an earnest cry or impatient demand for *cold drinks*, *cold fresh air*, constant fanning and exposure of person. This was the congestive fever which prevailed along the prairie, creeks and low humid swamps of Willcox and Dallas counties in 1835 and '36. The medical men at that time, did not dream of classing it with the intermittents and remittents of the country.

From the history of this disease, as presented in the foregoing pages, we will now endeavor to sum up something like a

DEFINITION.

CONGESTIVE FEVER,

Known at different times under the appellations of *cold plague*, *congestive typhus* and *cold sickness*, made its appearance on the Mississippi river and in some localities in Florida about the year 1821, and in Alabama in 1834. Although its geographical limits embrace most of the South-Western States, it is chiefly confined to the rich, mixed and humid soils of creeks, sloughs, rivers and prairie marshes and swamps. It is an acute affection, occurring at all periods of life, but much more frequently between the age of 15 and 35, attacking the native as frequently as the foreigner. It prevails most generally in the months of

July, August and September, and never to any extent for more than three years in one locality—usually following the upturning and exposure of that description of soil, whence it seems to derive its source. It may appear twice or oftener in the same subject, but second attacks are usually modified or complicated. With the same degree of exposure whites are far more liable to it than blacks. Its poison does not seem to attain any great circumference or altitude. It commences with languor, anxiety, oppression about the chest, quick and small pulse and lowering of the temperature of the surface; these symptoms may continue with more or less prominence from four hours to two days before the subject is forced to take his bed. In other instances it is insidious in its approach, so much so, that, at the first moment of alarm, the patient will be found in a state of great depression, attended with an alarming perversion and derangement of all the functions; those of the skin, lungs and heart being most conspicuous. The disease having taken hold proceeds rapidly with its work. The pulse becomes quick and thready, appears deep seated and often becomes extinct at the wrist. The muscles become flaccid and soft. The action of the heart becomes feeble, intermittent, tremulous or fluttering, with now and then a struggling pulsation. The coldness of the extremities extends rapidly to the superficies of the body, the sternal portion of the chest alone retaining its heat; this coldness is far more active and intense in its character than that of any condition in other disorders. Sweat is always present, sometimes very finely diffused over the surface, but usually profuse, standing in large dew like drops on the chest and forehead,—it is cold and inodorous. The skin is generally bronzed or livid,—dependant portions shriveled and wilted. The tongue presents various appearances, usually covered with a thin yellowish fur; in violent cases it is cold and livid like the lips;—we do not, with one exception, recollect to have ever seen it dry. Sometimes there is fullness about the region of the stomach; in almost every case there is a *sinking sickness*, as the patient calls it, about the epigastric and pericordial region; this character of distress is such in some cases that the patient says he cannot describe it. Positive nausea is not very usual, occasionally a grass green fluid, mucus, or the ordinary contents of the stomach are vomited; *retching to vomit*, especially as connected with efforts at inspiration is very frequent. The evacuations are sometimes thin, pink colored, containing a little mucus, but not very profuse;—most usually there is either suppression of all the excretions, or frequent attemps to stool, a little blood and mucus only being voided. Copious, pale or *rice water discharges* we have never seen, except in cases where this peculiar element of disease (congestive) had supervened upon others. There is seldom positive pain in the head, back or limbs. The function of respiration is very imperfectly carried on, being labored and often interrupted; even in cases where it is not obviously labored and difficult, the frequent sighing and great effort at inhalation, with now and then a harsh prolonged expiratory sound, discovers the defect of this function in all cases. The physiognomy is anxious and painful; the sufferer will not permit clothing to be placed upon him,—there is a horror of having the shutters closed, he wants fresh air, requires to be fanned, says he is burning up, and demands water frequently. In moments of threatened suffocation the nervous energy is rallied, he rises from bed, walks the floor for a minute or so and falls or throws himself on the bed; the exhaustion now is complete, he lies in an unconscious state for a moment, and is again aroused to a sense of his suffering.—A more complete state of exhaustion aud prostration during the intervals of these painful struggles cannot exist. There is such insensibility of surface as usually to defy the effects of the most stimulating applications;—cruel and mis-directed applications of positive heat has been known to *char* the muscles of the leg without producing pain. The eye is clear and often protruded as in asthma. The intellectual faculties, in most cases, continue good until the closing hour of life. The foregoing symptoms are, or most of them, present in all cases; they may vary in prominence, some predominating in one case, and others in another;—THEY DIFFER GREATLY IN DEGREES OF SEVERITY, but in number and combination are uniformly present. These symptoms, we believe, seldom or never, without the interference of art, give place to a normal reaction of the powers and functions which they involve. Death usually takes place between 24 and 60 hours, in some cases not before the fourth or fifth day.—In many of those cases where the disease continues from day to day, a slight reaction takes place in the afternoon; the skin becomes partially warm; the pulse rises, so as to be perceived at the wrist; the lungs give token of a freer action; the tortured features relax, and the patient congratulates himself that the fearful ordeal has been passed. The improvement, *though great*, is but comparatively so:—the pulse is still quick, small and compressible; the hand, placed for a minute on the surface, discovers not only a want of substantial warmth, but a coldness;—also great relaxation, or a want of ordinary firmness and consistence in the capillary net work; though smiling and cheerful, there is restlessness with an occasional sighing. A few hours more elapse, and the disease, *scattered* only for a moment, gathers again with increased terrors; nature's efforts, unequal to the struggle, soon yield the victory to this *strong* malady. Death takes place not as a consequence of a series of disordered and diseased actions as in other affections, but by the *direct and positive agency* of that poison, which makes its impression, we know not how;—but produces phenomena unlike any other, and destroys life in a manner peculiar to itself.—But nature, when materially and powerfully aided by art, often reacts, and the patient recovers. In these cases the indications of improvement are clear and decided. The perspiration diminishes, the pulse, in becoming perceptible, is not so very quick, appears to be nearer the surface, and has considerable stamina; the breathing becomes easy; as the surface dries it becomes warm, and the shrivelled shrunken portions of the skin fill out; the reaction set up here is not ephemeral, but permanent. From this point, there is little danger, that the patient will fall back into that condition from which he has just emerged;—local inflammation, fever, dysentery or diarrhea may follow as a sequence—but with good management, recovery is very rapid."

Can Dr. Boling find in the forgoing portraiture of this disease "no symptoms pathognomonic of congestive fever?"—Will he acknowledge to the world, that he can find nothing here, distinguishing this disease from remittent fever?—Let us call his attention for one moment to a paroxysm of the last mentioned disease as described in his work on the fevers of the time worn, *pebbly* hills of Montgomery. We shall abbreviate. "They (intermittent and remittent) are varieties of the same disease and present corresponding pathological changes. * * * * The development of the first exacerbation is generally preceded by a *slight* chill of shorter or longer duration, sometimes by a well *marked ague*, and in others a sensation of coldness with shivering is felt, especially when the patient turns in bed, or in any way disturbs the covering, for *several hours*; the *entire surface, even the extreme portion of the toes, feeling preternaturally hot to another person at the same time.* * * * During the existence of the cold stage, whatever form it may assume, the patient suffers much from *pains in the loins, and indeed in all the larger joints.* * * * The pulse is small, the action of the *heart is laboring and strong*, the sounds and impulses being increased; with the developement of the hot stage, they are still *further augmented* and abate during the remission—during the height of the exacerbation the pulse becomes *moderately full at least*, sometimes *very large*." After some remarks on the tongue, state of the stomach, bowels, evacuations &c., the Doctor says: "The symptoms as connected with *the skin*, as *with all the other organs vary both with the period* of the disease, and the particular period of the exacerbation. In the forming stage of the first exacerbation the extremities will generally feel cool or cold to another, *though this is not always the case, even when complaints of suffering from cold are loudest on the part of the patient.* * * * Soon the general temperature *increases, and the whole surface becomes hot, and a vivid flush makes its appearance not only* on the face, but *occasionally over the body*, in patients at all plethoric or of a sanguine temperament. This continues for a longer or shorter period, dependant in a great measure on the type the fever is going to assume; for it will be shorter in the paroxysm of a quotidian than of a tertian; when *the heat and redness of the surface decline, perspiration appears* &c. &c." This completes the paroxysm.—Can there be two conditions more opposite than those distinguishing congestive fever, as described by us, and those distinguishing remittent, as described by Dr. Boling. In the one (remittent) the skin may be warm and the patient feels cold;—in the other, the *skin is cold* and the patient *complaints of great heat.* In one the pulse, small at first, rises with the exacerbation, becoming large; in the *other it is thready and quick, and grows finer until it becomes imperceptible.* In one there are violent pains in the joints; in the other we hear *nothing of them.* In one, the action of the heart is loud and strong, augmenting in force as the disease progresses; in the other, it is *weak, feeble* and *tremulous.* In one, the temperature of the surface is rapidly augmented; in the other it is as *rapidly lowered.* In one, the remission is marked by *diminished* heat and perspiration; in the other (if any), by *increased* heat and *diminished perspiration.* In one, the disease, if left to the curative powers of nature, will generally yield; under the same circumstances the other *would be certainly fatal.* In one, death only occurs after a series of varied pathological changes; in the other, it is induced by the direct and immediate agency of the morbid poison. These are only a few of the many characteristic symptoms distinguishing the two diseases; in fact, there are very few in common to the two disorders.

We have now concluded what we have to say in relation to this disease in its true and natural character, such as it presented in the days when it first made its appearance in the State, and is still seen in some localities. We believe the candid mind, willing at all times to yield to the force of reason and truth, must admit, that the general assemblage of symptoms characterising it, are unlike those of any other; differing wholly and radically from them. Should we, however, be mistaken, and the judgment of the profession is against our conclusion, it will, at least, do us the justice of admitting, that we have honestly and faithfully pointed out "the differences that do exist between this and *other diseases*."

In view of any misrepresentations that may hereafter be made, we wish to qualify somewhat our conclusion. Like "*pure typhus and pure synochus*," there may be comparatively but few cases of *pure periodical* and *pure adynamic fever*—but that they do exist cannot be doubted.

A few remarks in relation to intermittent congestive fever, and we will pass to other subjects.

After describing the congestive fever of 1834, 1835 and 1836,—the author of the medical history states, that, as a general rule, this disease has become modified since 1835, maintaining only in a few localities its original marked violence;—he then proceeds to notice the complicated varieties, as described by Drs. Mabry and Wooten, and are now the common forms of the country. We know from experience, that simple intermittent fever is often complicated with congestive fever;—in fact, this mixed form of disease is so general in some sections of the country, as to give rise to the belief, that intermittence is a necessary element to congestive fever. Several paroxysms with the usual stages of intermittent fever may occur before this adynamic element becomes manifest; probably very indistinct in one paroxysm, and threatening the life of the patient in the next. Observation has, however, impressed us with the belief, that if this morbid condition is manifest during the paroxysm of intermittent, close observation cannot fail to trace it out during the apyrexia. Dr. Mabry, for instance, tells us, that during the intermission of congestive intermittents, there is restlessness, loss of appetite, and a painful apprehension of impending evil. Doctor Wooten in his cases speaks of a *sweating stage succeeding the chill*; reaction, if *any supervenes*, being very imperfect;—he also says that cool extremities and a clammy sweat continue until the next paroxysm. Dr. Gale, in a practical paper, describes individual cases of congestive intermittents, tertian variety, where during the apyrexia the patient would walk about his grounds, believing he was well. The Doctor at the same time was enabled, from unmistakeable signs of disease, to warn him of his perilous situation. We have seen similar cases, where it was difficult to convince the patient that he was then laboring under this disease. Doctor Cook of Opelousas, La., says that congestive fever is not so prevalent as public belief credits—they do not bear to bilious fever a ratio of over two per cent. He says, that it is commonly intermittent,—at the same time he is careful to state, that in the second chill the pulse is small and threadlike, and that *in this paroxysm* there is *no reaction*, at least, "never the full condition constituting fever." The same excellent writer says, that cases are not uncommon, in which this congestive condition "is not preceded by *increased heat of skin* or *febrile* pulse."

When asked, why it is that in congestive fever reaction or fever does not succeed the chills; the reply is, that the chill is so *severe* and so *deep* that the system cannot react. We would ask gentlemen, who are ever so ready with this explanation, if they have ever compared carefully *the chill or the formation* of that *cold condition*, characteristic of adynamic fever, with the chill or ague that ushers in intermittent or remittent fever. Turn to any book that describes the intermittent fever, or go to those localities, designated by the author of the Medical History, where intermittents prevail in a most aggravated form, and note the phenomena of a chill. The sufferer feels cold, if it is an ague or *deep* chill; he says: "I feel cold in my very bones;" he desires fire, covers himself deep in blankets, (no suffocation here) his suffering instinctively causes him to call out for any and every means calculated to impart heat to the body. (And this too, Doctor Boling says, when the surface is warm to another.) In the other case, whilst the surface is cold as marble, the patient complains of heat and burning: To give him heated air to breathe or to cover him, head and body, would be to apply the most horrible of tortures. A fleeting sensation of coldness, attended with a momentary tremulousness, may exist now and then, but any number of the phenomena of a true chill or ague are never present.

We will not say, whether this periodical element of disease, belongs properly to congestive fever, or is merely mixed up with it as in other disorders. A few facts, connected with diseases, that are known to be wholly and fundamentally different from remittent fever, may throw some light upon this subject.

In that portion of this communication, relating to typhoid fever, we have already shown, from the very best authority, Bartlett and Wooten, that remittent and typhoid fever are often complicated, in malarious regions. We have also had occasion to state, that intermittent fever is often engrafted upon pneumonia, phthisis, catarrhs, &c.; Doct. Boling has published an article in the May No. 1847, of the New Orleans Medical and Surgical Journal, which establishes the fact that remittent fever is frequently complicated with tetanus. If intermittent and remittent, then, have such an affinity for diseases that do not usually prevail in the same season with it; it is but reasonable to suppose, that it would readily amalgamate with those, which prevail in the same months, and seem to be developed under the same meteorological influences.

In our articles on yellow fever, published in 1845, we mention particularly a great number of cases which we called *intermittent and remittent yellow fever*; in these we had little difficulty in making a correct diagnosis, the pathological condition peculiar to yellow fever being easily discovered during the apyrexia. Doct. Bartlett, after noticing this form of disease, as described by us, remarks: "other diseases are frequently *mixed up* with this periodical element in pathology; and not only is there no reason *à priori* why such should not sometimes be the case with yellow fever, but it would be a very singular circumstance if this disease alone should be exempt from this complication." It is well known, that in Egypt and other places, the plague never prevails without a marked increased prevalence of remittent fever, and that these diseases are frequently complicated, and the resemblance between them is so great for the first day or two, that physicians often fail in their diagnosis. Topkin, in his work on the malignant epidemics of Germany, describes forms of disease, resembling somewhat congestive fever, and others, still more closely the asiatic cholera, both of which were intermittent; the patient going into a fatal collapse after the second or third paroxysm. These diseases prevailed in that country, only for a very limited number of years. Berserius and other writers describe the same complications as connected with the cholera and remittents of India.

It strikes us, that the suggestion, thrown out in the Medical History, in relation to the non-identity of malaria, will explain this connection between congestive and remittent fever. In his geological sketch of the State, the author shows that the chemical character of the soil differs materially in different localities.—If these give out emanations peculiar to them,—as lead and mercury for instance—and fevers have their origin in these emanations, it cannot be expected that they would be identical. Upon such an hypothesis, we can imagine how it is, that the same meteorological influence which causes to rise from the earth one noxious agent, should start into being those of a different but kindred nature. Thus united they may produce, in some seasons and localities, a disease of a blended character;—and in others, the one agent may alone exist, or its influence so completely preponderate as to impress its separate and peculiar effects upon the constitution. We will here conclude this branch of our subject.

It appears that a writer by the name of Dr. Parish in " strictures on the use of the term congestive" states that this congestion is the " consequence of diminished nervous power, and nothing more nor less than that state which occurs in all cases of sudden prostration. Doct. Boling, who *is usually* of *the opposition*, contends, that congestive is expressive of this state in congestive fever, and that it may depend upon *perverted innervation*—" but certainly not diminished nervous power,"—(this is a nice distinction truly.) To sustain his view of the matter Dr. Boling says :—" the action of the heart will be found *strong*, as indicated by the loudness of its *sounds*, and the *force of its impulse* :"—he also remarks that muscular prostration is rather *apparent* than *real*, the patient being able to jump out of bed and walk about while he is pulseless. With an eye to this passage in Dr. Boling's treatise on fevers; the author of the Medical History *penned* the following passage.

" Notwithstanding the exhibitions in this disease of *apparent strength*, (regarded by many as real,) we are firmly convinced that great muscular prostration exists. The patient is capable of these surprising efforts only at occasional intervals. In a moment of extreme agony, with his lungs and heart oppressed with dark blood, like the victim of asthma, he exclaims, " I cannot breathe, I am smothering," and by an instinctive struggle his nervous energy is rallied for a moment—he starts up—rushes for the open door or window and falls powerless on the floor. It is said by some that the action of the heart is "loud, strong and tumultuous"—hence, they conclude there cannot be diminished nervous power. True, it is "loud and tumultuous," and often beats violently against the thoracic wall; but it is that tremulous irregular action (often seen from depressing poisons) where the heart painfully labors to force on the stream of blood which flows in too fast for its exhausted powers. The patient often complains of fullness and oppression, and one ear placed over the heart, conveys to the mind the idea of a distended organ laboring in vain to free itself. We have here nothing of the bold, distinct measured pulsations which belong to the heart in many of the simple phlegmasiæ."

These remarks, which really lean more towards Dr. B.'s conclusions than the facts will warrant, have been commented upon by the Doctor at considerable length. He compares favorably for himself, the strength of one laboring under congestive fever, to the great debility of those who are in the last stages of typhoid fever and asthma. Although the analogy, when we take into consideration the great difference in the diseases, is not a legitimate or fair one; yet, even in typhoid fever or asthma, "during the hours of impending dissolution," if anything occurs, producing great alarm or emotion of any kind, the blood will be so forced upon the heart as to make it beat violently for a while, and the patient may even start from his couch. But as the whole of Dr. Bolings' theory of congestive fever, as given in contra-distinction to Doct. Parish, is based on a simple fact, it is easily settled. The Doctor says,—"the action of the heart will be found strong, as indicated by the loudness of its sounds and the force of its impulse"—since he infers nervous power cannot be diminished, &c. We have searched in vain for authority to sustain Dr. Boling—his assertion, in this particular, stands contradicted by all of the medical men of the South; at least, so far as they have made their observations known. As for Doct. Parish's statement, that this congestion is nothing more than a prostrated condition, such as occur in other diseases, it is unnecessary to say more than that he could not very readily get more wide of the mark in any statement, than he has in this? the two conditions are wholly and entirely unlike, except in general debility. Had these gentlemen not been so far misled as to have entirely mistaken the facts; much ink and paper would have been saved.

In relation to that unfortunate remark contained in the Medical History, "Dr. Boling has not described any cases of fever which the physicians of certain localities would not recognise as congestive fever, &c., we regret that our limits (having learned from the publisher that the Journal is full) forbid making such extracts from his pamphlet, as are necessary to do the Doctor full justice. His paper is before the public and we must refer the reader to it. What Dr. Boling says in that paper, in relation to pernicious remittents or congestive fever, is mostly in general terms;—like Wood, Bartlett and Clymer, he seems to have received his impressions of the *varieties* of pernicious remittents, not so much from a study of the congestive fever of Alabama aud Mississippi, as from a thorough examination of foreign writers;—who *bye* the way are far more liberally noticed than those residing in this country. There is a variety of this disease, however, about which the Doctor speaks from personal observation ; as it is one of common occurrence throughout the country, and necessarily of interest, we will quote the Doctor's description of it.

"The term *insidious* is applied to pernicious remittents, in which the first exacerbations, of a very mild character, are followed by a sudden and unexpected explosion of disease. As a large majority of the cases that I have seen deserving of this title have occurred in children under nine or ten years of age, I shall confine my description to the appearances presented in them. After having had one or two exacerbations of fever, so slight as scarcely to attract attention—perhaps not confining the patient to the bed or cradle more than an hour or two, which time is generally spent in sleep, at the regular febrile period, perhaps tertian, perhaps quotidian,—the most violent train of symptoms suddenly and unexpectedly set in. The skin is of a pale purplish cast, at first hot and dry, soon followed by profuse perspiration, warm in the beginning, afterwards, especially where the paroxysm is about to terminate fatally, cool and clammy. Stupor, strabismus, and frequent convulsions are present. The tongue is most generally smooth, clean, moist and relaxed, and the abdomen tumid. Just preceding each convulsion, there is frequently a rumbling sound in the abdomen, produced by a change of place of flatus and fecal matters, and during the period of their continuance, evacuations are apt to take place from the bowels. These are composed principally of undigested food which had been taken during the previous remission. Portions of half ripe melons, including the seed, various kinds of nuts imperfectly masticated, raisin skins, &c., are the articles most generally found. This state of things is also frequently aggravated by a large dose of calomel, or some other purgative forced down the child at the moment of the first alarm, by the parents, simultaneously with sending for the physician. Many children die in the first exacerbation of this kind."

And this is the *insidious form of congestive fever* which the physicians of this State have been treating for the last 14 years. We doubt if the practical men of Alabama and Mississippi can restrain their mirth, whilst reading the Doctor's account of it. It occurs, says he, in persons under *nine and ten years of age*, the skin " at first hot and dry is soon followed by perspiration." The poor child, whilst convulsed, from gastro-intestinal irritation, vomits *half ripe melons*, including *the seeds, nuts, and raisin skins.* We believe, that we have at last discovered the Doctor's reasons for saying, that the aid of chemistry is not required in Etiological investigations.

It is not only impracticable, but altogether unnecessary to follow Dr. Boling in his comments on that portion of the medical history, devoted to the yellow fever of Mobile. The Doctor says that he has never seen yellow fever ; and if we make his remarks on this occasion a criterion, we would say, that he had never studied its *literary* history—at all events, he has not touched the merits of the question which he affects to examine. We will merely remark, that for a series of years, the medical world was divided upon the question, growing out of the supposed contagious nature of this disease. The contagionist contended that it was a specific disorder, a disease *sui generis ;* contagious and transmissible. The non-contagionist contended that it was but a variety of bilious fever, was controlled by the same laws, and of course not contagious. In the violent and heated contest that ensued between these parties, reason was put out of view, and we may probably say, that falsehood usurped the place of truth. After the disease had ceased to exist in Northern cities, the public mind calmed down, and a new generation of medical men began the investigation of these questions, upon their merits. As it was not necessary, that the question of the identity of yellow and bilious fever should be connected with that of the contagious character of the former, of course these were properly divided. With a few honorable exceptions, the physicians entitled from observation and experience to judge, have finally arrived at the conclusion, that yellow fever is a disease *sui generis*, and that it is not contagious. We make these assertions with the most perfect confidence in our ability to sustain them.

The author of the Medical History in his remarks upon the diversity of opinions in relation to the modus operandi of quinine makes the following allusion to Dr. Boling :

"Dr. W. M. Boling has maintained, in able and elaborate articles on the use of quinine, that it is contra-stimulant and highly serviceable in the treatment of *inflammatory affections*. Inflammatory diseases, strictly so, are now rare among us ; they belong, as we believe the preceding pages will show, to a period long since gone by. We have examined somewhat the notes of cases upon which Dr. Boling predicates his opinions. Complicated as they are, these cases cannot be properly classed with the phlegmasiæ. With a gentleman who, like Dr. Boling, has so materially contributed to the medical literature of the South, and reflected so much honor on his adopted State, it is painful to differ ; and nothing but the obligations, resting upon an impartial chronicler, could force us into such a position. We may be wrong and Dr. Boling right, be this, however, as it may, he has too much liberality not to agree with us in the sentiment of the poet,

"One man's word is *no man's* word,
Truth demands that all be heard."

From the *fluttering* betrayed by the Doctor, in his comments upon this passage, it would appear, that he was wounded by it ; the author certainly did not intend that it should have any effect of that kind. The Doctor complains of the author's summary way of settling matters ;—and says that the cases alluded to, do not "all properly belong to the phlegmasiæ." In this we agree with Dr. Boling ; but as he seems to derive his opinions from the leading case reported by him, and it was from that case, that the author obtained, as he thought, a knowledge of Dr. Boling's conclusions in relation to the effect of quinine upon the circulation in the phlegmasiæ—we will insert it here. Justice late is better than never.

"As to the modus operandi of quinine, I have been able to form no very satisfactory opinion ; the observations of one day generally altering or modifying the opinions predicated upon the experience of a previous day. At one time I was disposed to look upon it as a sedative or contra-stimulant ; and as a general rule, this is its most manifest effect ; and yet I have seen a *very few* cases in which it *appeared* to act as a stimulant. Its most general effect, however, is that of a sedative; more certainly reducing and controlling the action of the heart and arteries than any remedy with which I am acquainted. At another time I was disposed to think, that this controlling influence was only exerted in those labouring under the influence of malaria; but in the advanced stage of a case of endopericarditis, in which nothing in the circumstances of the patient, or the character of the accompanying fever, indicated a malarious taint, I was able, by administering, night and morning, an enema containing grs. xx. of quinine, to moderate the rapid progress of the disease, and to reduce the pulse from 110 to 80, and to keep it at that standard, so long as the use of the quinine was persevered in. The influence of the quinine was satisfactorily proved by the fact, that the omission of an enema was invariably followed during the next twelve hours, by a rise of the pulse to the original standard. One of the remedies used in this case, before the quinine, was digitalis, which had no effect whatever in controlling the pulse. The case eventually proved fatal, in consequence of its becoming complicated with gastro-enteritis, induced by a moderate, but for the safety of the patient, too free a use of calomel and tartar-emetic. But to return; it would not be considered scientifie to call it a specific, and yet, in malarious diseases its effects seem almost *antidotal*. In almost every case, whatever the nature of the disease, supposing the system at the time to be labouring under the influence of malaria, either as the principal curative agent, or as an important adjuvant, the best effects may be anticipated from its effects." As to Dr. Boling's opinion in regard to the modus operandi of quinine, he certainly leaves us no room for doubts;—for in the preceding passage, after mentioning previous doubts, he says, that its general effect is that of a sedative, *reducing and controlling the action of the heart and arteries with more certainty than any other remedy*. (In casting the *mind's eye* for a moment over the materia medica, this language of the Dr. sounds *strong and strange*.) It will also be discovered from the quoted passage, that Dr. Boling distinctly avers, that this action of quinine is not confined to diseases of a malarious taint but those of a strictly inflammatory character, as the case in point imports. We would now enquire of the reader, as to the sufficiency of the doubtful case, reported by the Doctor, to the strong and important inference which he draws from it. Notwithstanding quinine *moderated* the rapid progress of the disease, still the patient died: but, says the Dr., it was in consequence of its becoming complicated with gastro-enteritis, induced by a moderate, but for the safety of the patient, *a too* free use of CALOMEL and TARTAR EMETIC. So it appears, after all, that this reduction of the pulse may have been induced by calomel and tartar emetic, which the Doctor administered, but keeps in the *back ground* until it becomes necessary to account for the death of the patient. It is believed *by some* that tartar emetic will reduce the action of the heart and arteries.

There is another case reported by the Doctor, more in point than the great mass of those which he instances. We will admit that it is strictly inflammatory and see how far it sustains the Doctor's conclusions. We will give so much of the case only as relates to the treatment pursued.

The subject was a hog-drover from Tennessee and, of course, strong and muscular; had been ill *at spells* for some weeks, before the Doctor is called.

"He complains much of soreness and tightness around the chest, and also of headache; tongue rather dry, rough and white; skin hot; pulse 100, and moderately full and sharp; respirations 30; face flushed and livid: bowels costive; catches at imaginary objects during his sleep; owing to the severity of his cough, he has for some time rested badly, more especially for the last three or four nights; he is quite thirsty. ℞. Ant. potass. tart., gr. ii; Tr. opii, gtt. xxiv; mucilage acaciæ, ℥ iv. —M.; a tablespoonful to be taken every second hour; and ℞. Calomel, pulv. ipecac. c. āā gr. v.—M.; to be taken at bed-time.

26*th*. His situation remains much the same; pulse 100; respiration 28; skin hot; he has had one thin bilious evacuation. Continue the antimonial mixture. Evening.—Same. Continue same.

27*th*. Still as he was. ℞. Ant. potass. tart., gr. iv; Tr. opii, ʒ ss; mucilage acac., ℥ iv.—M.; a tablespoonful every third hour. Under a continuation of the same treatment his symptoms continued unabated up to the evening of the 28th, with this difference only, that he had got rid of the chilly sensation, and the consequent febrile paroxsyms, the fever being continuous; yet judging from his own sensations he thought himself decidedly worse. At 7 o'clock in the evening I gave him, as an experiment, ten grains of quinine. At 10 o'clock I returned, and found a very appreciable improvement in him. His pulse 94; respiration 26, and his skin a little moist. ℞. Quin. sulph., gr. xxiv; massæ hydrarg., gr. viii.—M.; fiant pilulæ viii; two of them to be taken every second hour.

29*th*. Pulse 86; respiration 26; skin and tongue moist; no thirst; cough less frequent; tinnitus aurium. Continue pills. Evening.—Pulse 78; respirations 26. Continue pills.

30*th*. Through forgetfulness the pills were not procured for him last night. The tinnitus aurium has abated; pulse 86; respirations 28; cough more troublesome; skin dry. ℞. Quiniæ sulph., gr. iv; antim. potass. tart., gr. i; massæ hydrarg., gr. viii.—M. ft. massa in pilulas viii dividenda; two of them every third hour. Evening.—Pulse 76; respiration 26; skin moist.

31*st*. Pulse and respirations the same; coughs but little, and with scarcely any expectoration. The sibilant râle is only heard occasionally at distant points; the sonorous râle continues to be heard extensively; the crepitating râle is heard over a small spot below the nipple. Continue pills.

January 1*st*. Pulse 72; respirations the same; skin and tongue moist; cough and expectoration both diminished, and the small quantities of matter expectorated is a thin transparent mucus resembling that of the first stage of bronchitis. Continue pills.

2*d*. Pulse 70; respiration and other symptoms the same. Continue pills.

3*d*. Pulse 68, soft; skin and tongue moist; no thirst; scarcely any cough; no morbid sound in chest. Continue pills.

4th. Pulse 64 ; respirations 24 ; some appetite ; gums a little sore. He continued to take the quinine, gr. viii, three times a day for a short time.

On the 7th he was able to sit up part of his time ; he continued to improve, and recovered his strength rapidly."

Doctor Boling indirectly attributes the favorable influence exerted by the treatment in the foregoing case, to the quinine which was administered ; and of course infers from it that this salt reduces the action of the heart and arteries, and exerts a favorable influence in the treatment of the true phlegmasiæ. Now, as calomel and ipecac, or blue pill, nitre and tartar emetic were given from the onset to the termination of the case until *the gums at least became sore*, we imagine that they had some agency in the favorable effects produced.—The treatment of this case reminds us of an incident that occurred not long since in one of our Southern cities. A sick man was left by his medical attendant, with directions to give brandy and water very freely, which was done. During this state of the case some of the friends brought in a Homeopathist, who put *two drops* of liquid in a tumbler of water and directed that a tablespoonful should be given every two hours ;—at the same time telling the nurse to give the patient some brandy and water every *half hour to quench his thirst.* The patient rallied from his collapse, and of course the general inference was, that recovery was owing to the *little drops.* Is not this the way Dr. Boling cures his patients. He gives the *little drops* (quinine) but takes good care to *throw* in the brandy (calomel, tartar emetic and opium) and water.

The other cases reported by Dr. Boling in that article* are, with one or two exceptions, not strictly inflammatory; all of which he admits. All that we contend for is, "that quinine will not reduce the pulse, nor in any way exert a favorable influence, when given in the true phlegmasiæ. It is due to Dr. Boling to say, that in the article referred to, he so modifies his opinion as to doubt, whether as a general rule quinine is serviceable in any other cases than those of a malarious complication.

We could pursue this examination of the articles on quinine and remittent fever to the still greater disadvantage of Dr. Boling ; but as we are satisfied with our defence, we have no disposition to "carry the war into Africa." We know that Doctor Boling is behind no man in a knowledge of French and English pathology, and that he is an able and discriminating physician ;—but when he permits his *pathological vision* to become so deeply colored by the modified, mixed and uncertain character of fevers that prevail in Montgomery, as to suppose they represent the types of other localities, he does himself great injustice.

Had Doctor Boling studied the French and Italian authors less, and the diseases of the State, as presented in different localities, more, he would have produced a work more true to nature, and satisfactory to physicians of the South than the one before the public.

In concluding, it is our desire to say, that if we have misrepresented

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* Pha. Journal. July 1844.

Dr. Boling, it is not intentional;—and should any harsh epithet have found a place in our communication, it has not proceeded from any feeling of unkindness. We have occasionally thought, that the Doctor, in indulging a too prurient ambition for criticism, has sought to misrepresent or do us injustice;—resentments raised under feelings of this kind passed away however, with the same rapidity, with which they were formed.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

[The large quantity of original communications contained in the present number, has almost entirely closed the second part of the Journal. We have on hand a long and able communication from our correspondent W. M. B., which we hope to publish entire in our next number.—Eds.]

I.—*On Poisons, in relation to Medical Jurisprudence and Medicine.* By Alfred S. Taylor, F. R. S., *Lecturer on Medical Jurisprudence and Chemistry, in Guy's Hospital, and author of "Medical Jurisprudence."* Edited with notes and additions by R. E. Griffith, M. D., &c. Philadelphia. Lea & Blanchard. 1848. pp. 670.

We have bearly space to acknowledge the reception of this recent work in our present number. In our next, we shall probably give an extended notice of the book, as doubtless it contains a large amount of new and valuable information. Dr. Taylor has already given proofs, in his "*Medical Jurisprudence*," of his learning and practical turn of mind. The subject is interesting in itself and in the hands of "Taylor," it will, we believe, excite much attention in this country.

II.—*Introductory Lectures.*

1st. By J. R. Micthell, M. D., Professor of the Practice of Medicine, Jefferson Medical College, 1847.

2d. By Thos. D. Mütter, M. D.

3d. By John P. Harrison, M. D., Professor Practice Medicine, Ohio Medical College, 1847.

4th. By Geo. R. Grant, M. D., Professor Practice of Medicine, Memphis Medical College, 1847.

5th. By O. W. Holmes, M. D., Parkman Professor of Anatomy, and Physiology, Massachusetts Medical College, 1847.

6th. By S. Henry Dickson, M. D., Professor Practice of Medicine, New York University, 1847.

Some of the above Lectures are conceived in good taste, and expressed in a neat and concise style; others again are too laudatory of self and *confrères*. This however may be pardoned in a *novice* who has just been elevated to a Professor's seat, and suddenly finds himself metamorphosed into a Teacher of the great truths of medicine! Most of the Introductory Lectures which have fallen into our hands, seem to have no definite object in view; they are too discursive, too erratic, aim rather to excel Cicero than Hippocrates. In an introductory, the Professor we think should aim to lay down the principles—give the outlines of his particular branch; he should strive to be eloquent in the enunciation of the truths of medicine—inspire the student with an ardent love for the science, and point out the course he should pursue, and place before him objects and ends worthy of attainment.

Part Third.

EXCERPTA.

1.—*A Course of Lectures on the Physical Phenomena of Living Bodies.* Delivered in the University of Pisa, by Professor MATTEUCCI, F. R. S.

(Translated for THE LANCET, By S. J. Goodfellow, M. D., London, late Physician to the Cumberland Infirmary.

(*Continued.*)

LECTURE V.

Digestion.

THE existence and preservation of an animal depend upon the introduction, at regular intervals, into its body, of certain peculiar substances, termed *aliments.* These substances, for the most part solid, undergo, in the digestive apparatus, a series of modifications, by means of which they are separated into fecal matters, which are expelled, and into others, which are mixed with the blood, into which they ultimately become transformed. The final end of digestion is the preservation of the integrity of the organism, by restoring to the blood the proximate principles of which it is constantly being deprived by the act of nutrition. Reason leads us to believe that all the parts of the organism are transformed and renewed more or less rapidly; and experimental physiology furnishes us with a certain number of experiments leading to this conclusion, which it would be very desirable to see varied and extended.

To divide and make soluble the alimentary substances, in order to facilitate their absorption, summarily expresses what occurs during digestion. Nothing more, therefore, is witnessed in this function, beyond that which is purely physical, than the modification of the condition of the aliment.

Before entering upon the physico-chemical phenomena of digestion, I would briefly state some generalities with regard to this function.

All alimentary substances may be arranged, so far as their chemical composition is considered, under three well characterized categories; in the first are comprised, neutral azotised substances, as albumen, fibrine, and caseine; in the second, fatty matters; and under the third, gum, starch, and sugar, the composition of which may be represented by water and carbon. Experiment has shown that alimentary substances coming under the two last heads, are insufficient for the alimentation of an animal, and that it is necessary that they should always be joined to those belonging to the first.

We shall further see what share the alimentary substances comprised in these several categories exercise respectively in the functions of the animal economy.

With regard to the substances of the first class, I cannot pass over the important discoveries lately made by Mulder and Liebig—that albumen, fibrine, and caseine, are identical in their composition; in all three, the proportion of carbon to azote is as eight equivalents of the first to one of the second: they seem to differ from each other only in the small quantities of phosphorus and sulphur which accompany them; these taken away, there remains a principle common to all, which Mulder has termed *proteine*, and the formula of which, according to Liebig, is, C_{48} H_{36} N_6 O_{14}. We must, then, consider these substances, although endowed with physical properties so different, as isomeric, and only as modifications of proteine. Another important fact, discovered by Dumas and Liebig, is, that *vegetable albumen* and *animal albumen* are identical; also that in corn flour their exists a substance analogous to *caseine*, and that in gluten there is found a resemblance to animal fibrine. There is not, then, any essential difference between the aliments of herbivorous and carnivorous animals, except that the first take them from plant, and the second, from other animals.

And since the composition of the blood, as well as that of the greatest number of animal and vegetable tissues, is analogous to that of the neutral organic substances which I have mentioned, since they are found, when forming part of the animal organism, without any change of chemical composition, and only contract, during nutrition, a new form, it is natural and just to admit that, in the act of digestion, the neutral azotised alimentary substances simply enter the blood in the state of solution, and without undergoing any other alteration.

The isomerism of these substances is equally shown in the beautiful discovery of Denis, that fibrine is converted into albumen when dissolved in a solution of nitrate of potash. What is still more curious, with regard to this fact, is, that this conversion can only be effected with the fibrine of venous blood, and that that of arterial blood is neither dissolved in the nitre, nor transformed into the albumen. Scherer exposed fibrine of venous blood to an atmosphere of oxygen, and saw the oxygen converted into carbonic acid, and the fibrine lose the property of being changed into albumen in a solution of nitre.

Some physiological experiments have long since proved that the digestion of similar alimentary substances is a purely physical act, and that it is effected independently of the living organism. Not one of you is ignorant of the celebrated experiments of our countryman Spallanzani, that flesh, gluten, and coagulated albumen, introduced into the stomach in perforated metallic tubes, become dissolved and digested as if they were free in the stomach. This solution is effected, as we have seen, by one of those actions which we described in the first lecture, termed catalytic, or actions of contact.

The recent experiments of Melsens, and particularly those of Bernard and Barreswil, have shown that the gastric juice contains a free acid, which will be the lactic, holding in the solution a peculiar substance, called *pepsine*, which has been obtained tolerably pure. It is this substance which Payen has recently investigated, and which he has called *gasterase*. The acidity of this gastric juice is more or less, according to the quality of the aliments; while fasting, the acidity in the stomach is less strong. It increases by contact with aliments, and is strongest when they are composed of fibrine, albumen, &c. Here I have, in some glasses, an infusion of pepsine, to which I have added a few drops of hydrochloric acid. In one of these little glasses I have put some coagulated albumen; in another some fibrine. These, so prepared, have been kept for ten or twelve hours in an atmosphere heated to 86° Fahr., and the albumen and fibrine have already disappeared to a great extent; there remains only some slight traces already transparent on the edges which will also shortly disappear altogether. If I neutralize the acid, and evaporate the solution, I can easily reproduce the albumen and fibrine, which have not undergone any change in their nature, and have only been dissolved by their contact with the acid infusion of pepsine. This substance acts, then, in dissolving fibrine and albumen as a body endowed with catalytic properties, and it is by an action of contact that their solution is effected. It is only in the stomach, or by certain glands which are situated in its mucous membrane, that the acid solution of pepsine or the gastric juice is separated. I have kept some pieces of small and large intestine in a solution slightly acidulated with hydrochloric acid; but it never acquired the solvent property; it only became gastric juice by contact with the membrane of the stomach.

The property with which the pepsine is endowed constantly requires the presence of a free mineral or organic acid. We have just seen how the catalytic action of this substance is modified when dissolved in an alkaline solution. I should here mention that pepsine loses its properties, and becomes insoluble, if it be heated to 50° centigrade, (122° Fahr.)

The neutral azotised substances dissolved in the stomach by the liquid acid, or by the catalytic action of the pepsine, penetrate into the blood, through the walls of the capillary bloodvessels of the stomach, solely by imbibition. Water and coloured alcoholic drinks, introduced into the stomach, are also absorbed. They do not pass this viscus, and are not found in the chyle, and yet they find their way into the blood. Bouchardat and Sandras fed some animals with fibrine, coloured with saffron or cochineal, but could never discover the colouring matter in their chyle. Again: animals that had been fed upon fibrine, and others that had been kept for some time fasting, and then killed, always furnished an identical chyle. The matter found in the intestines did not differ, except that in animals fed upon fibrine a small quantity of that substance remained in the stomach only partially dissolved. We know also, by the celebrated discoveries of Tiedmann and Gmelin, that the quantity of fibrine found in the lymph and the chyle, after a long fast, is not less than that which is contained in it immediately after digestion. The results are the same when coagulated albumen, gluten, or caseous matter, is used instead of fibrine. The digestion of these neutral azotised substances consists, then, in their simple solution by an action of contact, and the absorption of this solution, which takes place principally in the stomach.

Nothing is, therefore, more physical than this part of digestion. The mastication of elements impregnated with a slightly alkaline and warm liquid resembles that physical operation which is practised in our laboratories, for reducing a body to powder in order to facilitate its solution. The gastric juice which the stomach always secretes at the time of digestion is an infusion of pepsine in acidulated water; and when it acts upon coagulated albumen, fibrine, or caseine, the solution of these substances takes place in the stomach, as in any other receptacle suitably warmed.

The movement of the walls of the stomach favours the action of the infusion of pepsine upon the substances to be dissolved, as all agitation assists the reaction of two bodies partially dissolved, or the solution of a solid in a liquid. This movement of the walls of the stomach is useful in another way, because, by constantly renewing the points of contact between them and the matter contained within the organ, the absorption of the liquid portion of this substance is more easily effected. The influence of a section of the nerves of the eighth pair, in disorders of digestion, may be partly attributed to the cessation of these movements of the stomach, which are certainly due to the action of these nerves. Their section, moreover, occasions great disturbance in some other functions indispensable to the integrity of the animal economy.

I will now speak of the digestion of amylaceous matters, which has been much elucidated by a beautiful experiment of Sandras and Bouchardat.

This experiment may be easily made. Some drops of pancreatic juice added to a certain quantity of cooked flour or starch paste, at the temperature of 95° or 104° Fahr., quickly dissolves them; the liquid becomes transparent, and ultimately all trace of starch disappears. The same effect takes place if, instead of pancreated juice, we select some portion of the pancreas of a pigeon, or of any other animal. I take the pancreas of a pigeon, bruise it, and add the substance thus triturated to the starch, and heat it to 104° Fahr. The starch becomes dissolved, and is converted into dextrine or sugar. It is into this condition that amylaceous substances are brought previously to their absorption. There exists, therefore, in the pancreatic juice, and perhaps as Magendie affirms, also in the saliva, a substance which acts upon starch, like *diastase.* It is singular that this action requires the presence of a free alkali. If the pancreatic juice be made acid, it ceases to act upon the starch, and, according to Bernard and Barreswil, acquires even the property of acting upon neutral azotised substances. Hence we conclude that one and the same organic substance is endowed with the property of dissolving starch as well as the neutral azotised substances, only that to act upon the first a free alkali is necessary, and in the case of the second, a free acid. It still, however, remains to be proved whether the starch thus converted into dextrine and sugar by the saliva and the pancreatic juice passes in this state into the blood, or whether it be not first converted into lactic acid. It is only in the blood of some diabetic persons that sugar has been found. The supposition that the conversion of starch first into dextrine, then into sugar, and lastly into lactic acid, which becomes absorbed and passes into the circulation, seems to be more in accordance with facts.

We must not forget the important discovery made by Fremy, of the property which certain thin animal membranes acquire from a contact with water for a certain time, of converting large quantities of sugar into lactic acid.

Those azotised substances which in certain conditions are apt to excite the lactic fermentation, assume another state, which I am disposed to consider as a more advanced state of transformation, the nature of which is as yet unknown, and which is not attended with the production of more lactic acid by their action on sugar; on the contrary, they assist in exciting the alcoholic fermentation, by converting it into carbonic acid and alcohol: besides, we know that a solution of sugar injected into the veins of an animal quickly becomes apparent in the urine.

We may, then, conclude, by yielding our consent to the knowledge derived from organic chemistry, and relying upon the well-known results of the simple play of the actions of contact, that starch is convertible, in the intestines, into lactic acid, after passing, most probably, through the intermediate states of dextrine and sugar.

It will not be unreasonable, nor in opposition to actual knowledge, to suppose that a portion of sugar into which starch has been converted, not only undergoes the lactic fermentation in the intestines, but also another transformation analogous to that in the midst of which we now know infusory animalcules to be developed. The recent experiments of Gruby and Delafond have established beyond doubt, that a great number of these animalcules are found in the stomachs of herbivorous quadrupeds.

I cannot here pass over the researches which have been conducted for discovering the cause and treatment of diabetes.

Bouchardat first promulgated the opinion, which has been generally adopted, that in this malady the starch is converted into sugar in the intestines, and that in this state it passes into the blood and urine; and a diabetic regimen has therefore been prescribed, composed principally of the neutral azotised substances, of which amylaceous matters have formed no part. In some cases restoration to health has followed this mode of treatment.

This opinion, however, has been very much shaken by the numerous experiments of Dr. Capezzuoli, which tend to prove that the quantity of sugar in the urine of diabetic patients bears no relation to the quantity of starch in the diet; and that, under the use of a strictly azotised diet, as much sugar was formed as under one from which amylaceous substances had not been excluded.

It is true that this experimentalist found sugar in the contents of the intestines of diabetic persons, as also in the matters vomited by them, but only after a meal composed of amylaceous substances. But this was not peculiar to diabetic persons, for there was as much sugar found in the healthy man under these circumstances as in the diabetic. This fact of the transformation of starch into sugar, thus shown by experiment, is of great importance in connection with the theory of digestion.

Dr. Capezzuoli, moreover, found traces of sugar in the blood and other contents of an abscess, in a diabetic patient. This abundant production of sugar in those maladies which are constantly accompanied by great emaciation is as yet unexplained.

I shall now direct your attention to the digestion of the fatty matters which are consumed in such large quantity by carnivorous animals, and which are carried to the adipose tissues, without undergoing any modification in their composition, and I shall avail myself of this opportunity of saying a few words relative to the origin of fat in herbivorous animals. Liebig maintains that it is produced by means of a transformation of the starch, which loses a portion of its oxygen, which escapes from the organism in combination with carbon. Dumas, Boussingault and Payen, on the contrary, are of opinion that the quantity of fatty substances in hay, beetroot and straw was sufficient to account for that found in animals fed on these aliments. Boussingault has shown the truth of this assertion. He found that the milk of a cow, whose food, during a certain time, contained 1614 grammes (fifty two ounces, two pennyweights, thirteen grains, troy) of fatty matter, yielded 1413 grammes, (forty-five ounces, twelve pennyweights, thirteen grains, troy)—thus leaving an excess of fat in the food of 201 grammes (six ounces, nine pennyweights, nineteen grains, troy) over that furnished by the products of the animal. The same chemist also found that a greater quantity of fat was produced in pigs and geese then was contained in their aliments. Persoz arrived at the same result.

It cannot, then, be denied, that the animal economy possesses the faculty of transforming a part of the food into fat. Unfortunately, chemistry affords us but little assistance in explaining this transformation.

On the other hand, it has been proved by physiological observations, that animals fed on fatty substances furnish a chyle more abundant and more milky than usual, and that these matters may be again separated from it. Small fat globules may also be perceived in it by the microscope.

The experiments of Sandras and Bouchardat have put this conclusion beyond doubt. By feeding animals upon oil of sweet almonds, these chemists found oil in the chyle, and the same result was obtained by substituting suet. When wax was given, only a small quantity of it was found; but it was much increased by previously dissolving it in oil.

These chemists also examined the contents of the stomach and intestines of animals which had been fed exclusively on fat, and they detected in the former organ, when cold, a large portion of it in a solid state, surrounded by a very acid liquid; and that in both the large and small intestines also, there was a thick pap, from which they separated, by means of ether, a considerable quantity.

From these facts, of the reality of which I have convinced myself, it follows that these fatty substances do not undergo any change in the stomach, and that they are carried into the intestine simply liquefied, or nearly so, by the heat of that organ. Nor was any change effected by the action of gastric juice upon fat out of the stomach. The alkali of the bile and pancreatic juice neutralizes the acid of the gastric juice, which affords a new proof that its dissolving action upon azotised substances ceases in the intestines. It is difficult, by the aid of analogies deduced from chemical facts, to distinguish what becomes of fatty substances after they leave the stomach. It is certain that they become absorbed, and that the lacteals are almost the sole agents endowed with this function.

I have endeavoured, by means of experiment, to clear up the obscurity which prevails in this part of the digestive process. For this purpose I poured into a mattrass a solution of twenty-five grains of caustic potash in 300 grammes (nine ounces, thirteen pennyweights, eighteen grains, troy) of distilled water. This solution had not any perceptible alkaline taste, and acted but feebly on turmeric paper. It is less alkaline than lymph or chyle. I heated this mattrass in a sand-bath, to the temperature of 35° to 40° centigr., (95° to 104° Fahr.,) and then added some drops of olive oil, and after shaking it, instantly perceived the liquid became milky, and assumed the appearance of milk, even to the extent of being mistaken for it. The liquid so obtained, left to itself, preserved its resemblance to milk, and separated into two layers, the upper one being more opaque, and in which there were evidently some little globules of fatty matter, while the lower was less opaque, although it always preserved its milky aspect. I filled a piece of intestine with this emulsion, and plunged it into the alkaline solution above described, keeping the temperature from 95° to 104° Fahr. After a certain lapse of time, it became turbid, and assumed the characters of the internal emulsion, and it may certainly be believed that a portion of it passed through the membrane, and escaped into the surrounding liquid.

Another experiment which I made appears still more conclusive. I filled an endosmometer with a weak alkaline solution, and placed it in the emulsion. The membrane employed was, as usual, the urinary bladder of an ox, and the two liquids were, at the commencement of the experiment, at the temperature of 30° centgr., (86 Fahr.) Endosmose then took place, and the emulsion penetrated to the alkaline solution, and raised a liquid column thirty millimetres (a little more than an English inch) in a very short time.

Look at the physical phenomena which, without resolving all the particulars of the digestion of fatty substances, contribute, nevertheless, to render it less obscure. The lacteals, terminating as they do in closed extremities, and enveloped or covered by the intestinal mucous membrane, are, specially in young animals, full of an alkaline liquid closely analogous to lymph. After digestion, particularly if the animal be fed on fatty substances, the liquid in the lacteal only differs from what it was before by the addition of oily corpuscles, which give it the milky appearance. It is reasonable to admit that this chemical affinity between the alkaline solution and the oil which produces the milky liquid, takes place equally through the walls of the lacteals, which can as certainly permit the imbibition of the alkaline solution as the milky liquid.

The phenomena of endosmose, of which I have already spoken, may also be included among the probable causes of lacteal absorption. It is certain that absorption cannot physically take place, if the internal walls of the intestines be not bathed with a liquid with which the fatty body has some affinity. It is easy to show by experiment how the alkaline condition of the intestinal walls favours this absorption.

Fill two funnels with sand, equally packed in each; pour pure water upon one, and an alkaline solution upon the other. The liquids having passed through, gently pour an equal quantity of oil upon the two filters. The oil will remain many hours even on the surface of the sand wetted with the pure water; but in the other, on the contrary, it will disappear rapidly by the imbibing power of the sand, when moistened by the alkaline solution.

The neutral azotised substances which enter the blood, after being dissolved by the gastric juice, would rapidly destroy the neutral or slightly alkaline condition necessary for the preservation of the qualities of this liquid, were it not for the alkali of the chyle, as well as that of the lymph, bile, and pancreatic juice, which preserve its neutrality.

Chyle and lymph hold in suspension a great number of little grains, which are from 1 to 2000th of a line in diameter, and which appear formed of a fatty substance enveloped in a membrane, all which leads to the belief, that it consists of a body analogous to proteine. These granules exist in the yolk of the egg, in milk, chyle, lymph, and all the liquids exuded in the pathological cases, or destined for new formations. These granules have been seen to unite and form a globule, or cell, analogous to those of the blood; and this is why we regard them as the morphological elements of all the animal tissues. Within the last few years Donné has observed, that when milk has been injected into the veins, the globules disappeared at the end of a certain time, by being covered with an albuminous layer, as with a bladder, and that they became reduced to the state of the white globules of the blood, and lastly disappeared by being transformed into the red globules, when the blood assumed the same appearance as before the injection was made.*

The organic element, then, seems to be reduced to a vesicle constituted by a layer of albuminous matter, which collects and becomes organized round a nucleus, consisting chiefly of fatty matter. I will show you an important experiment, first performed by Ascherson: by placing an oily liquid in contact with albumen, the latter immediately coagulates, as you see. If you mix both together, and place a drop of the liquid under the microscope, you will see a group of vesicles, each of which is formed of a grain of fat, enveloped by an albuminous membrane, somewhat coagulated, and it will appear as if there were real fat-cells upon the object-plate. This result may be more conveniently seen by placing a drop of oil, and another of albumen, upon a plate of glass, and gradually bringing them into contact. It is curious to observe in the microscope the almost instantaneous formation of a very delicate, elastic membrane, which is quickly covered with numerous folds. Ascherson has proved, that this formation, effected by albumen and oil, is of a decidedly cellular nature, for by adding to it a drop of water he saw the cellules swell, and little drops of oil escape from them. By using diluted acetic acid, the cellules became so large as to burst; when placed in oil, on the contrary, they became contracted in size. These facts, which ought to be varied and extended, evidently belong to the phenomenon of endosmose, and cannot be understood without admitting the cellular formation. Here, then, is a physico-chemical operation, which leads us to the discovery of the formation of elementary granulations. Fatty substances and combinations of proteine are constantly introduced into the organism; they are found in all the animal fluids; the globules of fat which enter the lacteal vessels, and are there also found in the midst of an albuminous liquid, are not slow in becoming enveloped by similar membranes, and must, for this reason, form vesicles like those discovered by the microscope in chyle, lymph and blood.

In concluding this lecture, I shall add a few words upon the gases of the stomach and intestines, as inorganic substances, which more or less directly form an integral part of the animal organism.

Observation has shown that oxygen is scarcely ever found in the gases of the stomach, and especially of the intestines; but that in these cavities they are chiefly composed of nitrogen, carbonic acid, and a certain quantity of carburetted hydrogen, with sometimes a trace of sulphuretted hydrogen. A quantity of atmospheric air is introduced into the stomach along with the food. The oxygen of the air disappears in the stomach, perhaps by passing through the membranes till it reaches the blood, or, more probably, by taking part in the transformation of azotised albuminous substances into ferment. In this case, carbonic acid gas appears to be very abundantly developed, and it is said that enormous volumes of this gas are disengaged by some ruminants which feed upon fresh succulent herbs. It is curious to observe that the production and disappearance of this large quantity of gas in the stomach and intestines take place and succeed each other with such rapidity, that recourse may be had to chemical reaction to account for them. The presence of hydrogen has not hitherto been accounted for by any of the physico-chemical changes which are known to take place in digestion.

I have shown, by experiment, that oxygen is not necessary for the solution of fibrine and coagulated albumen by the gastric juice, as was supposed by Liebig. A portion of the stomach of a pig was placed, with some fibrine and coagulated albumen, in slightly acidulated water; the water had been boiled many hours, and the prepared liquid was covered by a thick layer of oil. The fibrine and albumen were dissolved in this bath quite as well as in a similar one which was exposed to the air.

* It may be necessary here to remark, that Donné's views are questioned by most physiologists.

The inorganic substances which are found in the organism are evidently introduced there, and form part of the food; they can only reach the blood by being dissolved in water, and the gastric juice of the stomach. Everything which is not reduced in this way is necessarily rejected with the excrements. Physicians never forget this truth in the selection and preparation of medicines. Experience has now proved that there is no reason to wonder that large doses of some inorganic salts produce no effect when introduced into the stomach; they are rejected as excrementitious.

LECTURE VI.

Respiration.—Gaseous Endosmose.

THE action of the oxygen of atmospheric air upon the venous blood, the changes which take place in the air introduced by respiration into the pulmonary cells, the modifications which the blood undergoes in traversing the capillary network in the thin walls of the bronchial vesicles,—these are the principal phenomena of the function of respiration, and will form the subject of this lecture.

The life of every animal, however low in the scale of organization, is essentially connected with those modifications by which the oxygen of the air is introduced into its substance. The organs, by means of which this action is carried on, are more or less developed, according to the medium in which the animal generally lives, and have a form and an organization strictly corresponding to it. In fishes, for example, the organ of respiration is a mucous membrane doubled several times upon itself, divided into filaments or thin plates, full of blood-vessels; it is always in contact with the water, which is introduced through the mouth, and passes out through the bronchial openings. Their whole organization is arranged so as to give the greatest extent of surface for the contact of the water in which atmospheric air is dissolved, and the vascular walls of the gills. In the common ray the gills have a superficies of 2250 square inches. In reptiles, birds, and mammifers, the respiratory organ consists of an expansion of the bronchial tubes, which ramify like the branches of a tree, and the most delicate extremities of which terminate in a great number of spheroidal vesicles, placed back to back, and surrounded with small blood-vessels.*

The respiration of some reptiles, at least during the first period of their lives, resembles that of fishes, as well as of mammiferous animals, and on this account are provided with both gills and lungs.

The movements necessary to this function are partly voluntary and partly involuntary, and have reference solely to the introduction of air into the lungs, and its subsequent expulsion. All the air-passages are dilated during *inspiration*, and contracted during *expiration*. The causes of the movements of the respiratory function are, the combined action of the muscular force, the elasticity of the osseous and cartilaginous parts of the thorax, and also that which peculiarly belongs to the walls of the air-vesicles, and lastly, the physical properties of the air. The whole thoracic cavity dilates during inspiration, and the air rushes into the bronchial tubes; during expiration this cavity contracts, the cells of the lungs, being elastic, resume their original size, and the air, thus compressed, and having its elasticity increased by the heat communicated to it in the lungs, is expelled. The action of a pair of bellows represents the whole mechanism of the respiratory movements.

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* The translator needs scarcely remind the readers of THE LANCET, that the anatomy of these organs, in the three great classes of animals named, is very imperfectly, indeed erroneously expressed, for in neither of them, except perhaps in the mammiferous, do those organs bear even a remote resemblance to that to which the author has likened them.

In fishes this movement is carried on independently of the ribs; the bronchial arches open, the plates separate, the water comes in contact with them; they then close, and the water escapes by the bronchial fissure, which remains open until the operculum falls. In the lower animals respiration is less energetic, and the respiratory movements are almost involuntary. In annellides and molusks the current of water in which the air is dissolved is increased by the movements of the vibratile cilia, which are placed on the branchiæ of these animals.

A man introduces into his lungs, in one inspiration, a little more than half a pint of atmospheric air. The air expired contains from three to five parts in 100 of carbonic acid; and after a very deep expiration, from six to eight parts in 100 have been found. In the same time, the air inspired lost from four to six parts in 100 of its oxygen.

The numbers I have mentioned are chosen in preference to many others, as most worthy of confidence. These being assumed, it is easy to calculate the quantity of oxygen which a man absorbs in respiration in a day, supposing that from fifteen to twenty respirations are made in a minute. According to Lavoisier and Seguin, the oxygen consumed in the respiration of an adult man is 1015 grammes, or thirty-five ounces, 402 grains, avoirdupois. The oxygen which disappears in the respiration of man and birds is very nearly equal in volume to the carbonic acid which is expelled. Some very scrupulous observers have found that the volume of oxygen absorbed in respiration is more considerable than that of the carbonic acid produced. This difference is particularly seen in carnivorous animals, for Dulong found that the oxygen consumed was sometimes double the volume of the carbonic acid formed.

By making an animal respire in a given quantity of air, Dulong and Despretz have placed it beyond doubt that a considerable quantity of azote is always produced, more than could be accounted for by the disappearance of the oxygen consumed from the respired air. This fact shows that the azote thus inhaled in excess is derived from the food, and perhaps also from that azote which we said was found in the stomach and intestines, as the residue of the air which is introduced with the food. And if the quantity of azote contained in the air is invariable, Boussingault has shown that this is derived from the quantity of this gas which some plants absorb.

The changes which respiration produces in atmospheric air equally take place in air held in solution in water. It is well known that in common water and sea-water a certain quantity of atmospheric air is held in solution, which may be disengaged by boiling, or by placing the water in contact with other gases than those which it holds in solution, or still better, by the use of the air-pump. These physical phenomena are carried on according to the well-known laws of the absorption of gases by liquids, which were discovered by Dalton.

The experiments of Morren have made it equally clear that a certain quantity of carbonic acid is also held in solution in these waters, which seems to vary in an inverse ratio to the oxygen contained in it at the same time. The proportion of oxygen found in a given volume of air in solution in water is greater than that in atmospheric air. Humboldt and Gay Lussac found thirty-two parts of oxygen in 100 of air taken from soft water. According to Morren, the quantity of oxygen in the sea varies at different hours of the day, being the greatest at noon; the contrary is the case with carbonic acid.

Fish absorb a portion of this oxygen, and give back carbonic acid, which is also absorbed in the water, and it is only by the continued solution of fresh portions of atmospheric air that the respiration of these animals continues to go on. This is the reason why fish soon die in water deprived of air by boiling, or when covered with oil. I will here relate an experiment which is peculiarly my own, and which I made a long time ago, upon the respiration of the torpedo: One hundred parts of air dissolved in the water of the Adriatic, taken near an estuary, was composed of carbonic acid, 11 parts; of nitrogen, 60.5; and of oxygen, 29.5. A large torpedo was kept for forty-five minutes in about a gallon of this water. The torpedo was frequently excited, and we obtained many shocks from it, and it soon died. The air in solution in the water did not contain a trace of oxygen; 100 parts of it were found to contain thirty-six of carbonic acid, and the rest azote. Experiment has proved that these changes in the atmospheric air in the contact of a living animal, takes place not only in the lungs, but also that the whole surface of the animal can effect, in different degrees, similar modifications. Frogs continued to live when placed in a determinate quantity of atmospheric air, even after we had removed their lungs, or prevented their respiration in some other way; and we found, after a certain time, that a portion of oxygen had disappeared, and been replaced by carbonic acid. Humboldt and Provençal have observed tenches to live without apparent suffering, even when their heads and gills were out of water, and the body alone submerged. Spallanzani and Edwards have also proved that the cutaneous respiration is indispensable in the batrachians, inasmuch that frogs live many days without lungs, but, on the contrary, they perish after a few hours when deprived of their skin, or if it be covered with varnish.

Sorg kept one of his arms in oxygen gas for four hours, after which he found that about two-thirds of the gas had disappeared. Davy made an analysis of the air injected into one of the pleural cavities of a dog, and found, after a little while, that it did not contain more than some slight traces of oxygen. The mechanism of respiration, and the chemical changes which accompany this function, are similar in all animals. The oxygen disappears in the respiratory organs, and the carbonic acid is at the same time exhaled from them: there is an excess of azote in the air expired over that which has been inspired; the volume of carbonic acid exhaled is never greater than that of the oxygen absorbed, and in some animals it is even less; and, lastly, the air exhaled is saturated with the vapor of water.

Whilst the respiratory act effects the changes which we have described in the atmospheric air, what becomes of it in the organism? You all know, that in respiration, the venous blood propelled into the lungs loses its black color, and assumes a beautiful vermillion tint, and becoming arterial, is returned to the heart, from which it is sent into all parts of the body. Any interruption to this transformation rapidly causes death.

I could adduce a great number of experiments, for the purpose of showing that the change from venous into arterial blood takes place in the lungs during the respiratory act. Bichat divided the trachea and an artery of a dog, and immediately applied a cock to the opening of each of them; having closed the cock of the trachea a little after an inspiration, the arterial blood began to turn black, and in the space of a minute, had become completely venous. The experiment was repeated by closing the cock of the trachea immediately after an expiration, and in a few seconds, the arterial blood which escaped was black. When the air was taken from the lung by a pump properly applied, the blood that immediately issued from the artery was black; but if, on the contrary, we propelled a little air into it, the blood preserved its red color for a long time. By carefully opening the cock of the trachea at regular intervals, jets of red and black blood appeared alternately. Here is a rabbit, with a cock fixed to its trachea; if you observe its peritoneum, which has been exposed, you will perceive that the red color of its vessels becomes dark when the cock has been closed a few seconds, and that the natural color is restored on its being reopened. In asphyxiated animals, all the tissues of the body, the kidneys, muscles, tongue, and lips, assume a blackish color. If the pneumogastric nerves of any animal be divided, the respiratory movements will soon be disturbed, and at the same time the blood will preserve its black color, and the lips, nostrils, and anus of the animal will lose their red color.

If, instead of introducing atmospheric air into the lungs of an animal, it is made to breathe in nitrogen, carburetted hydrogen, pure hydrogen, oxide of carbon, carbonic acid, deutoxide of nitrogen, or sulphuretted hydrogen, death will take place more or less quickly, and in the whole body only blood of a black color will be found. Besides atmospheric air, oxygen and protoxide of nitrogen are capable of maintaining respiration for some seconds. Perhaps in oxygen this function might go on for some time, but when this gas is breathed in a pure state, the respiratory movements are more frequent, the arterial pulsations are accelerated, and the whole blood becomes of a very brilliant red. In protoxide of nitrogen, respiration goes on for some seconds, without serious inconvenience; but, as in oxygen, the respiratory movements are accelerated, the cerebral functions disturbed, and a kind of intoxication supervenes.

We now know the phenomena which take place both in the air itself, and in the organism, during respiration: oxygen is absorbed, carbonic acid exhaled, and the black venous blood changed into red arterial blood; and these two modifications are carried on in the same organ in which, by its peculiar structure, the atmospheric air, which loses its oxygen, and the venous blood, which becomes red, are almost brought into contact, or separated by an extremely thin membrane.

Are these modifications of the air and blood phenomena which only take place in the living body? Are changes analogous to those which occur during respiration, seen for some time in venous blood taken from a living body, and exposed to the oxygen of the atmosphere? The most simple experiment may soon answer these questions, and leave no doubt of the entirely physico-chemical nature of this function. Here is a mass of blood which has been coagulated for some hours; you see that its surface is red, but that the surface of a piece cut off with a knife is black. In a few seconds, you will see it also become red. If I bring carbonic acid in contact with the red surface of this clot, it almost immediately becomes black. If I pass a current of this gas through a liquid formed of blood, dissolved in water, it will soon become black. This black liquid, if poured into a flask, full of oxygen, and shaken for a few seconds, loses its dark color, and becomes red. Sulphuretted hydrogen is the only gas which, even in very small quantity, affects the blood, so as to prevent its being restored, by oxygen, to the arterial state.

Since the time of Priestley, it has been known, that if blood which has turned black by the action of carbonic acid be placed in a moist bladder, held in contact with oxygen, the blood equally becomes red, and the interposed membrane does not hinder the change of color. It is then proved by experiment, that the change of color in the blood from black to red, which constantly accompanies the introduction of oxygen into the aërial vesicles of the animal, under circumstances indentical to those which I have just pointed out, is a phenomenon of an entirely physico-chemical nature, consisting in the action of oxygen upon a liquid which originates in the living organism.

What is then the nature of this change? What are its laws? These particulars ought still to occupy us, and in these investigations we shall avail ourselves of the beautiful researches of Magnus.

If venous blood obtained by opening the vein of a living animal, be collected in a receiver containing pure hydrogen gas, and agitated with it, a certain quantity of carbonic acid will be found, which assuredly cannot be the result of any chemical combination of the hydrogen with the elements of the blood, nor can it be accounted for by supposing that the carbonic acid is expelled from the blood by affinity of the hydrogen for the body with which it is thought to be combined. Carbonic acid, therefore, is held in solution by the blood itself, and is set at liberty by the hydrogen, from the mere action which one gas exerts upon another of a different nature, dissolved in a liquid. Had arterial blood been used instead of venous, a smaller quantity of carbonic acid would have been furnished by it. Nitrogen equally produces the disengagement of carbonic acid, the quantity of which from venous blood is more than double that obtained from arterial blood.* By this method we not only obtain the carbonic

* The tables from Magnus do not show this. In fact, it appears from them, that

acid, but also oxygen and nitrogen, which are disengaged with it. The results obtained by Magnus deserve so much confidence, and are so important as to induce me to communicate to you the numbers found by this chemist. He extracted and analyzed the gases dissolved in the blood, by means of a peculiar apparatus, with which he made a vacuum over the blood, and collected the gases thus disengaged. If I were to introduce, under the vacuum of a barometer, a certain quantity of blood just taken from an animal, you would see the column of mercury fall considerably, and by this means the gases of blood may be collected. The following is a table of the numbers obtained by Magnus:—

	Obtained of Gas Cubic Centimetres.†	Cubic Cent.	
Arterial blood of a horse, in 125 parts	9.8	5.4	Carbonic acid.
		1.9	Oxygen.
		2.5	Nitrogen.
Venous blood of a horse, in 205 parts	12.2	8.8	Carbonic acid.
		2.3	Oxygen.
		1.1	Nitrogen.
Arterial blood of a horse, in 130 parts	16.3	10.7	Carbonic acid.
		4.1	Oxygen.
		1.5	Nitrogen.
Venous blood of a horse, in 170 parts	18.9	12.4	Carbonic acid.
		2.5	Oxygen.
		4.0	Nitrogen.
Arterial blood of a calf, in 123 parts	14.5	9.4	Carbonic acid.
		3.5	Oxygen.
		1.6	Nitrogen.
Arterial blood of a calf, in 108 parts	12.6	7.0	Carbonic acid.
		3.0	Oxygen.
		2.6	Nitrogen.
Venous blood of a calf, in 153 parts	13.3	10.2	Carbonic acid.
		1.8	Oxygen.
		1.5	Nitrogen.
Venous blood of a calf, in 140 parts	7.7	6.1	Carbonic acid.
		1.0	Oxygen.
		0.6	Nitrogen.

Taking the mean of these numbers respectively, and reducing it to the proportion of 100 parts of blood, we find—

	Cubic Centimetres of gas.	Cubic cent.	
In 100 parts of arterial blood	10.4276	6.4967	Carbonic acid.
		2.4178	Oxygen.
		1.5131	Nitrogen.
In 100 parts of venous blood	7.6825	5.5041	Carbonic acid.
		1.1703	Oxygen.
		1.0081	Nitrogen.

It is much to be desired that the experiments of Magnus should be repeated and extented, in order to obtain the absolute quantity of different gases in the blood.

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the absolute quantity of carbonic acid extracted from arterial blood is as great as that obtained from venous blood, and in some instances greater.—TRANSLATOR.

† A cubic centimetre is .061, or a little less than one-sixteenth of an English cubic inch.

The following conclusions are of no little importance in the theory of respiration:—

1. A larger quantity of gas is contained in arterial than in venous blood.
2. The quantity of oxygen found in the arterial blood is double that of venous blood.
3. The proportion of oxygen to the carbonic acid found in arterial blood is from one-third to nearly one-half; while in venous blood, it is only one-quarter, or even one-fifth.*

When we reflect, lastly, upon the means employed to extract the gases from the blood, such as hydrogen or a vacuum, it is plain that these gases are dissolved in it; and we must therefore admit, that when disengaged from the blood, they are set free by the presence of other gases, in obedience to the physical laws regulating the changes which take place between gases dissolved in liquids and those in a free state.

We have seen that the change of colour which venous blood undergoes, prior to its becoming arterial, owing to the oxygen, takes place even when the oxygen is separated from the blood by a membrane. It is necessary to prove now that these phenomena (that is to say, the reciprocal action of the gases, and the change of colour of the blood) can take place out of the body, through layers of these membranes, and in obedience to physical laws.

Any gas contained in a well-closed bladder, soon penetrates through its pores, and, at the same time, the atmospheric air is introduced in its place. If the external gas were of limited volume, with regard to that contained in the bladder, the exchange would soon cease, and a mixture of the two gases would be found both outside and inside the bladder. Place a bladder, filled with water slightly acidulated by carbonic acid, under a bell full of hydrogen, oxygen, or nitrogen, and part of the carbonic acid will leave the water, and be found free in the bell. At the same time, a portion of the external gas will have supplied its place by becoming itself dissolved in the water. As a general rule, two gases, one of which is free or dissolved in a liquid, and the other separated from the first by an intermediate membrane, act upon each other, and mix in definite proportions.

It would be very desirable if the laws of this phenomenon were settled by a long series of experiments, having regard to the reciprocal nature of the gases, their density, and the kind of membrane interposed. Probably a phenomenon analogous to endosmose takes place between gases. Here is an experiment, which shows how gases act through membranes, and proves that the change which takes place is similar to endosmose. I partially fill the lung of a lamb, recently killed, with oxygen gas, after having carefully extracted the air, as much as possible, by suction. The trachea being tightly fastened, I introduce the lung into a bell full of carbonic acid, inverted under water. In a few seconds, the lung swells, and is as much distended as the size of the bell will allow. I have analyzed the gas after this experiment, and found that the carbonic acid had penetrated into the pulmonary cells, and that the oxygen had been disengaged. The exchange, however, had not taken place in equal volumes; for the carbonic acid introduced into the lung was more than the oxygen which left it. In a lung thus prepared, I found after four hours, that the gas contained in it was composed of two-thirds oxygen, and one-third carbonic acid; while that in the bell was a mixture of one-quarter oxygen with three-quarters carbonic acid.

Soap-bubbles full of atmospheric air or hydrogen, made to fall into carbonic acid, drew the attention of Marianini to a phenomenon similar to that observed in the lung. The bubbles increase in size, and it is curious, that when thus dilated they fall to the bottom of the vessel which contains the carbonic acid.

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* The tables show that the relative proportion of oxygen in venous blood, to the carbonic acid, is from a little less than one-quarter to less even than one-sixth.—TRANSLATOR.

The excess of carbonic acid which has penetrated the bubble is the cause of the augmentation of volume and weight, and is able to counterbalance the diminution it suffers by the increase of volume; but at the same time, the layer of water of the bubble certainly dissolves carbonic acid, and thus becomes heavier.

I have endeavored to hold a bladder having very thin walls, and exactly closed, when full of oxygen, in contact with carbonic acid, previously taking the precaution that the bladder should not be wet. The enlargement did not take place: nevertheless, after a certain time, an exchange was discovered between the gases, but the carbonic acid introduced did not exceed the oxygen which had escaped. Lastly, I tried filling a lung entirely with carbonic acid, and introducing it in this state into oxygen: the lung contracted, the two gases mixed, but the volume of oxygen introduced was less than that of the carbonic acid withdrawn. In all these instances, besides the reciprocal action of two gases through a membrane, we must take account of the presence of the water which bathes the membrane, in which carbonic acid is soluble. The liquid acid thus formed is presented on one side to a gas different to that which has there been dissolved, and in regard to which the free gas acts as if in vacuo. We must, therefore, account for the introduction of a larger quantity of carbonic acid into the bubble or the lung, by attributing it either to a peculiar action of the two gases, which might be called gaseous endosmose, or to an effect of a gas first dissolved and then exhaled. To throw light on this question, it is necessary to use gases which have no affinity for water. We must also recollect the laws of the diffusion of gases in the air, discovered by Graham. The diffusive powers of gases in the air, when they are separated from it by a membrane, or by a layer of plaster, are in proportion to the square roots of their density. Since the last researches of Valentin and Brunner this law has been verified in the phenomenon of respiration.

Some facts of experimental physiology which I have still to mention will give every possible evidence for our conclusions. Spallanzani, Nysten, Martigny, and Edwards, expelled the air from the lungs of some frogs by applying pressure upon the chest and abdomen, with all the precaution exercised by these scrupulous observers. In this state some of them were plunged into hydrogen, and others into nitrogen. Dogs, rabbits, and a great many other animals, were subjected to these experiments when prepared for them, either according to the method just described, or by artificial respiration. It was always found that the hydrogen or azote was absorbed, and that in their place carbonic acid and azote were exhaled; in pure nitrogen, only carbonic acid was given out. By introducing a mixture containing more oxygen than exists in atmospheric air, after having emptied the lung by means of a syringe, it was found that the carbonic acid exhaled was in a larger proportion than that disengaged while breathing the air. Frogs emitted carbonic acid in hydrogen and azote, even after being deprived of their lungs.

After all that has been said, we cannot hesitate in coming to the following conclusion:—that the respiratory function is a purely physico-chemical phenomenon; that the gases dissolved in the venous blood are set free by the absorption of other gases; that a portion of the carbonic acid of the venous blood is exhaled by its absorption of the oxygen of the atmosphere; that the carbonic acid expired—at least, the greatest part of it—is not formed in the lungs; that this gas exists dissolved in the venous blood, and is set free during the act of respiration, in presence of the oxygen which is introduced in its place, in the same manner as it is with azote or hydrogen in the artificial respiration of these gases; and lastly, that it is evident, from the experiments of Magnus, that the quantity of carbonic acid gas contained in the five pounds of blood which pass through the lungs in a minute is nearly double that which is exhaled in the same space of time.

LECTURE VII.

Sanguification.—Nutrition.—Animal Heat.

In the last lecture, I showed that during respiration a portion of the oxygen of the inspired air disappears, and that in its place is formed an equal or smaller quantity of carbonic acid; that the expired air is saturated with aqueous vapour, and that at the same time that these changes are effected in the lungs, the venous blood is converted into arterial. We have also seen that all these phenomena take place as well out of the living body, and under the same conditions, as when they are carried on within it. It remains to examine the particulars of this change in the blood. Which of the organic elements of the blood undergoes this change, and in what does it chemically consist? If I must give a direct answer to these questions, I must confess that, up to the present time, the experiments tried in order to resolve them, have afforded but little information, and among the immense number of attempts that have been made, I can only choose those which appear to be on the whole the least imperfect and contradictory. Microscopical observers of the present day define the blood as a liquid chiefly composed of water, in which are dissolved different salts, albumen, fibrine and oily particles, and which hold in suspension a great number of red globules, of a regular form, and greater or less diameter, according to the different species of animals, and resembling a kind of vesicle with a coloured envelope soluble in acetic acid. I wish to show you a beautiful experiment of Müller's, which will give you a correct idea of this composition of the blood.

I pierce the hearts of several frogs, and receive the blood that flows from them upon a paper filter; a yellowish liquid escapes through the filter, and the red globules remain upon it. In a few seconds you will see the filtered liquid coagulate, and the clot will be composed of fibrine. Thus we have, on the one hand, the colouring matter, and on the other, the serum in which fibrine is dissolved. If the blood had not been filtered, the fibrine would equally have coagulated, but would have enclosed the globular matter; and this is what takes place in the blood out of the living body. According to circumstances purely physical, as the temperature of the blood when drawn, the density of the serum, the different proportions of globules and fibrine, so does the coagulation of the blood take place more or less quickly, is more or less abundant, and the coagulum formed offers greater or less resistance.

When we take only the coagulum which is formed in a mass of blood left to itself, and treat it with oxygen, we see it assume a red colour. This coagulum exposed to the air, and then cut, has a blackish colour inside, and a red colour outside. The fresh surfaces formed by the incision, when exposed to the air, soon become red. It is undoubtedly the globules of the blood which undergo this change of colour by contact with the air. Baudrimont and Martin Saint-Ange have lately shown, that during the period of incubation, the absorption of oxygen and exhalation of carbonic acid are carried on through the calcareous envelope of the egg; and they have proved that if these processes are prevented, the red globules are not developed in the embryo. It still remains to be proved whether the globules become red merely by absorbing oxygen, or by losing carbonic acid during respiration; or if, on the contrary, the blood becomes venous on account of the greater quantity of carbonic acid with which it is charged, or by reason of the smaller quantity of oxygen which remains in it, or if it be the effect of both these circumstances combined. Exact experiments on this point are wanting. Magnus had proved that venous blood, by losing the greatest possible quantity of carbonic acid, becomes less dark, but without acquiring a vermillion colour. This fact would lead us to suppose that the two causes simultaneously effect the change of colour which the blood undergoes during respiration. I must add, that if all the serum be carefully separated from the coagulum, and the latter be then washed with distilled water, to take away all trace of the serum, in this condition it will no longer assume that beautiful red colour which it acquires by contact with oxygen when it is immersed in serum.* Here is a saturated solution of bay salt, which I pour drop by drop upon the coagulum of the blood. You see that the points upon which it falls acquire a red colour, while the remainder of the surface does not change. It would appear from this, that the salts of the serum are concerned in the modification which the colour of the blood undergoes when oxygen is present. It may therefore be said that the presence of serum influences the change of colour in the blood, by becoming charged with a portion of carbonic acid, of which it is afterwards deprived by the oxygen.

But in what does this change of colour of the bloodglobules chemically consist? With regard to this question, Science is still in the dark. The great quantity of iron (five or six per cent) which always exists in the blood-globules, and which is not found in so large a proportion in any other animal substance, has led to the supposition that this metal, which is sometimes found as peroxide, and sometimes as a carbonate, cannot but influence the change of colour in the blood. In fact, the oxygen expels the carbonic acid from the carbonate of iron, and the carbonic acid, in its turn, replaces the oxygen of the peroxide, according to the relative proportions of oxygen and carbonic acid which are at liberty to act upon the oxide of iron.

Mulder and Liebig seem to have embraced these opinions. All the best supported clinical results seem to prove that the use of iron in certain maladies in some degree revives the colour of the blood. Nevertheless, Scherer has lately asserted thst he has obtained the colouring matter of the blood, entirely devoid of iron. If this observation of Scherer's be ultimately confirmed, and if it be also proved that this colouring matter, deprived of iron, undergoes by contact with oxygen and carbonic acid the changes that we have seen take place in the blood-globules, we shall be obliged to renounce the opinion, that iron is instrumental in changing the colour of the blood.†

The arterial blood, propelled by the unceasing contractions of the heart, as well as by the successive distensions and contractions of the arteries, owing to their peculiar elasticity, reaches the smallest capillaries with this red colour. Always circulating in them, it passes through all the tissues, loses its red colour, and returns by the veins to the heart, to be again subjected to the action of the lungs. It is during this passage of the arterial blood through the capillaries, that nutrition is said by physiologists to take place. In this science, it is admitted that all parts of the animal tissues are constantly renewed and transformed, and that these phenomena vary in intensity, and are proportional to the different degrees of activity in the capillary system peculiar to the various tissues. To speak the truth, the experimental proofs of this continual renovation are wanting, and that which is afforded by the colouring of the bony parts of animals fed upon coloured substances, and by their losing this colour on their

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* This is a very unsatisfactory experiment; for, by washing coagulum with distilled water, it would necessarily be deprived of a good deal of its colouring matter, and nearly all the red particles would be destroyed, or more or less injured. It would be quite as difficult to separate the serum from the red particles by this process of ablution, as to prevent their separation from the fibrine of the clot.

† It may not be out of place here to allude to the views upon this subject lately promulgated by Dr. G. O. Rees. According to this ingenious observer and able chemist, the venous corpuscles contain a fatty matter, in combination with phosphorus, which, on coming into contact with the oxygen of the atmosphere, during the respiratory act, is consumed, and combining with that oxygen, forms carbonic acid and water, which are expired, and also phosphoric acid, which, uniting with the alkali of the liquor sanguinis, forms a tribasic phosphate of soda. This salt, it appears, has the property of acting upon hæmatosine, so as to produce the bright arterial tint. That this tint is probably owing to this salt in solution in the serum, is rendered likely, by the fact, that it is found in much greater quantity in the serum of arterial than in that of venous blood.—Trans.

food being changed, has always appeared to me insufficient. It must, however, be confessed, that this renovation is proved by the accumulated evidence of physiological facts. Were I to mention here all the experimental deductions which are wanting, and which would be necessary to explain the act of nutrition, I should occupy a much longer time than we can at present bestow on this subject. The blood-globules, not forming a part of any tissue, but still being essential to nutrition, may be regarded, with some probability, as the catalytic body, which excites or sets in action the transformation of tissues, and their constant renovation. An analogy of this character of the globules is made apparent in the necessity which exists for their being charged with oxygen, in order to acquire this property.

It may also be remarked, that as in vegetables the *diastase* changes starch into dextrine, which is again transformed into cellulose and lignine—that is to say, into isomeric substances, so, in the same way, may the blood-globules convert albumen into fibrine, and this change certainly takes place in the embryo.

I wish I could say that experiment had shown the reality of these changes, as in the case of starch. I have made many attempts with this end; but the results I have obtained still leave me in doubt. I kept some albumen of an egg, mixed with a small quantity of the blood-globules of a fowl, exposed to oxygen for a month, at a constant temperature of 104° Fahr. A receiver, into which flowed hot mineral water, afforded a medium of an unvarying degree of heat. I saw that the oxygen partly disappeared, that it was replaced by carbonic acid, and that a great number of reddish flakes were deposited at the bottom of the receiver; yet the original liquid was limpid, and scarcely coloured. These flakes, when examined, did not appear to be identical with fibrine. Nevertheless, I would not conclude from these negative results that the principle on which my experiments were founded was false. This is a subject which requires longer and more varied researches.

To return however, to our original subject. During the act of nutrition, one part of the arterial blood disappears, and is replaced by an excess of carbonic acid in the venous blood. The oxygen combines with the carbon in the capillary vessels. It is certainly in them that this combination takes place; and since we find that the volume of carbonic acid expired is not sufficient to account for the oxygen which has disappeared during respiration, we must admit that not only the carbon unites with the oxygen to form carbonic acid, but that the hydrogen, which forms a part of the organic elements of the blood and tissues, also combines with the oxygen to form water. Here then is another instance of combination besides that of carbon.

The acetates, tartrates, and oxalates, which enter the blood in a state of solution, are expelled by the urinary passages in the form of carbonates. Benzoic acid, introduced into the circulation, escapes, in the state of hippuric acid, by the same passages. In concert with Professor Piria, I have tried introducing a solution of salicine into the blood of a living animal. After some time, a substance, derived from salicine, was discovered in the urine, which had the property of forming a violet precipitate with salts of iron.

An important observation, recently made by Dessains, deserves our notice. By boiling hippuric acid in a solution of hydrochloric acid, benzoic acid was precipitated, and we obtained a solution of hydrochloric acid, combined with a sweet azotised substance, which is the sugar of gelatine of Braconnot. It is known that this substance is obtained by treating neutral azotised matters, as proteine and gelatine, with acids. It is also known that hippuric acid in herbivorous animals is the substitute for the urea in the carnivorous. We discover from this that the sugar of gelatine is one of the first products of the transformation of those neutral azotised substances which are the materials of our tissues. We may also understand how, by adding benzoic acid, which combines with those substances, hippuric acid is obtained.

All these facts placed it beyond doubt, that the chief chemical action observed in this circulation of the blood, and in nutrition, is a kind of combustion; that it is a combination of oxygen with carbon and hydrogen. Yet, I repeat, there is even now great obscurity in our knowledge of the order of these phenomena. What is the difference, in chemical composition, between arterial and venous blood? What is the nature of this difference in the blood before and after its passage through the kidneys, the liver, and the various tissues? These are some of the numerous questions which ought to be resolved by exact experiments, and by researches, all agreeing in their results, before prosecuting our investigations upon the phenomena of nutrition and secretion.

As we have seen, the aliments pass into the blood after having undergone various modifications by the act of digestion. But many of these, in their natural state, are identical with the organic elements of the animal tissues, as, for example, the neutral azotised substances, and also fatty substances, which are found in the adipose tissues, scarcely, if at all altered. It would be unreasonable and absurd to admit that urea, carbonic acid, and water, which are the definite products of the transformations effected by nutrition, are furnished from those organic elements which have been introduced into the blood with the food. We must believe that these products result from the transformation of the tissues themselves, and are replaced by new organic elements derived from the food. In fact, the production of urea takes place in animals fed for a long time upon sugar, starch, or gum, just the same as before the use of such a diet. The same thing has been remarked in animals that have died from inanition.

For the sake of rendering this more apparent, I will cite to you some examples from the work of Liebig, on "Organic Chemistry applied to Animal Physiology."

A serpent kept for some time without food, and then allowed to feed on a goat, rabbit, or fowl, passes in its excrements the hair and bones of the animal devoured, exhales carbonic acid and water, and discharges by the urinary passages only urate of ammonia. It afterwards regained its usual weight, and no trace remained of the animal it had devoured. Let us analyze this simple case of nutrition. The urate of ammonia contains one equivalent of nitrogen to two of carbon; the muscles and blood of the animal eaten contained eight equivalents of carbon to one of nitrogen, and if to this we add the carbon of the fat and brain of the devoured animal, it will be apparent that the serpent consumed more than eight equivalents of carbon to one of nitrogen. In the excrements, only two equivalents of carbon are found; the six equivalents which are wanting must have been expelled in the form of carbonic acid. It is unnecessary for me to repeat that the urate of ammonia and the carbonic acid are derived from the transformed tissues, and their place supplied by the organic elements (proximate principles) of the animal digested. It is universally true, that as much carbon and azote are found in the products generated by the transformation of the tissues from their contact with arterial blood, as the tissues themselves derive from the blood or food. What I have just said of the serpent equally applies to the lion and other carnivorous animals. In their urine there is urea only, in which the proportion of nitrogen to carbon is as two to one; and, as in the food of these animals the nitrogen is to the carbon as one is to eight, it follows that the excess of carbon introduced with the food over that carried off by the urine, disappears in respiration, is, in fact, burnt and converted into carbonic acid. The respiration of the lion is, however, much more active than that of the serpent.

The fifteen (232½ grains) or twenty grammes (310 grains) of nitrogen which a man loses every day in the urine, as well as the excess of nitrogen which he expires, are furnished by the neutral azotised substances of his food, or, more directly, by the transformed tissues which are replaced by those alimentary substances.

Boussingault proved, by experiment, that the whole of the nitrogen contained in a horse's food is not found in his urine, and thus demonstrated that the excess of nitrogen expired is also derived from the food.

It is impossible, in the present state of science, to say precisely through what series of modifications and intermediate products the muscles, the cartilages, &c., pass, in order to be converted into urine by the action of the oxygen of the blood-globules. By adding to the formula of proteine (which is also that of albumen, caseine, fibrine, &c.,) as much oxygen as is necessary to transform it into urea, and the excess of hydrogen and carbon into water and carbonic acid, much smaller quantities of the two latter are obtained than those produced in respiration. Here is a numerical example taken from the experiments of Boussingault, which I relate for the purpose of establishing more fully, that the carbon of azotised aliments converted into urea is much less than that which animals emit in the state of carbonic acid. A horse continued in perfect health when fed upon one and a half kilogrammes (a little more than three lbs. avoirdupois) of hay, and two and a quarter kilogrammes (about four and a half lbs. avoirdupois) of oats a day. Analytical researches show that the nitrogen in hay is 1.5, and in oats 2.2 per cent. Let us admit that all the nitrogen of the food was reduced in the blood to the state of fibrine and albumen, it would make 140 grammes (four ounces, 418 grains avoirdupois) of nitrogen introduced into the blood, and intended to take the place of the nitrogen which escapes with the constituents of the transformed tissues. The weight of carbon introduced at the same time with the nitrogen is 440 grammes, (fifteen ounces, 250 grains avoirdupois,) and 246 only of these can be converted into carbonic acid during respiration, since the horse parts with ninety-three grammes, (three ounces, 127 grains) of carbon in the urea, and 109 grammes, (three ounces, 375 grains) in the form of hippuric acid. But a horse, according to the experiments of this chemist, loses by respiration, in the course of a day, 2454 grammes* of carbon in the form of carbonic acid. It is therefore clear, that the carbon of the azotised principles of the food is only a small part of that which is found in the expired carbonic acid. Hence arises the necessity of other kinds of food, as starch, gum, or sugar, and fatty substances, to supply this insufficiency of carbon in the azotised elements. Whenever there is a rapid increase or growth in the animal economy, as in young animals, nature has supplied a food in which the proportion of carbon and hydrogen spent in respiration is augmented, by which the azotised materials destined for the growth of the tissues are economised.

Dr. Cappezzuoli has recently discovered, by determining the weights respectively of the fatty and neutral azotised substances in the egg of a fowl, successively as incubation advances, and in the chick itself, after leaving the egg, that about the seventeenth day of incubation—that is to say, a short time before the separation—a diminution in the quantity of both becomes perceptible, and that from this period these substances go on gradually diminishing. It appears that fatty substances also are not employed altogether in respiration, except when the starch, sugar, and gum, are not sufficient; and when this is the case, as in hybernating animals, and those which have remained long without food, their fat is seen to waste. The physiological destination of these substances appears to be primarily for the formation of the cerebral and nervous substance, and to fill the interstices of the cellular tissue, which last is not without importance in the functions of life, as it there forms a magazine, or store-house, for the materials of respiration.

I will now mention the hypothesis of Liebig, with regard to the influence of the bile in respiration. Physiologists no longer consider the bile as an excre-

* A gramme is 15.44 grains, or nearly fifteen grains and a half. An ounce avoirdupois is equal to 437.8 grains.

ment merely. This is evident when we reflect that Berzelius found but nine parts only of a substance like it in 1000 parts of human excrement; that is to say, that a man who secretes from 500 to 700 grammes of it per diem, only loses one fiftieth or one-seventy-fifth with the excrements. On the other hand, we cannot suppose that a substance containing so little nitrogen should be serviceable in nutrition; and, lastly we have just seen that it takes little or no part in digestion. Liebig is of opinion that, when poured into the duodenum, it forms a soluble compound with soda, and that it is absorbed and converted into carbonate of soda by yielding a part of its carbon to the oxygen. These views require to be substantiated by experiment, the more so that it is only in some pathological cases, and under the influence of certain atmospheric conditions, that traces of biliary matter have been found in the blood.

Whether these hypotheses of nutrition be well founded or not, one thing is certain, that an adult man absorbs about 1015 grammes of oxygen in a day.—The observations of Dumas, Andral, and Gavarret, and those more recently made by Scharling, give as their result, in the mean, that a man exhales in one day, 224 grammes of carbon, in the state of carbonic acid; that men exhale more than women; and children more than men; and that a larger quantity is extricated during a given time, when awake, than during sleep. A horse eliminates 2465 grammes of carbon, in the form of carbonic acid, consuming, for this purpose, 6504 grammes of oxygen. A milch cow exhales 2212 grammes of carbon, as carbonic acid, using 5833 grammes of oxygen. The quantity of nourishment must therefore be in proportion to the oxygen respired, and the carbonic acid exhaled. The activity of the respiratory movements, the density of the air expired, and the quantity of carbon introduced with the food, ought to be proportioned to each other, to preserve the materials of the animal economy. Letellier has lately proved, with birds and guinea-pigs, that the quantity of oxygen consumed in respiration is less, as the temperature of the air is higher. The carbonic acid exhaled at $0°$ was found, by Letellier, to be double that produced at the temperature of from $+$ 15 to 20 centig., ($59°$ to $68°$ Fahr.)

In animals whose respiratory movements are very active, their capillary circulation rapid, and the quantity of blood-globules very large, the fatty portions of their tissues is very small. This is the case with birds, the hyena, and the tiger. If the animals are allowed but little exercise, fat will accumulate in their tissues. The experiments of Treviranus teach us that, when their weight is equal, a cold-blooded animal consumes ten times less oxygen than a mammiferous animal, and nineteen times less than a bird.

Lastly, I think it important to mention here the results of a great many experiments made by Boussingault, to determine whether nitrogen is expired by gramnivorous animals, from a comparison between their food and their excrements. By taking the mean of his results, we find that a turtle dove consumes 5.10 gr. of carbon, in twenty-four hours; in the same time it expels 18 70 gr. of carbonic acid—(that is to say, 9.441 lit) and 0.16 gr. of nitrogen,—(that is 0.126 lit.) The nitrogen would be a hundredth of the volume of the carbonic acid—a proportion smaller than that found by Dulong and Despretz. The hydrogen consumed in a day is 0.07 gr. These numbers being granted, it results that a turtle-dove, which weighs 187 grammes, and which respires freely at the temperature of $+$ 8 to 10 centig., ($46°.2$ to $50°$ Fahr.,) by consuming 5.1 gr. of carbon, and 0.07 gr. of hydrogen in twenty-four hours, can develop the heat necessary to maintain its body at the temperature of $+$ 41 to $42°$ centig., ($105°.4$ to 107.3 Fahr.,) and that when exhaling also about three grammes of water by the lungs and skin.

It is, then, indisputable that an animal is a real apparatus of combustion, in which carbon is constantly burnt, and from which carbonic acid is regularly disengaged. Such a calorific apparatus has been constituted as to produce a nearly invariable excess of heat in comparison with the temperature of the surrounding medium. This excess varies according to the rapidity of combustion in this animal calorific apparatus, and according to the constant temperature of the medium in which it lives. One gramme of iron which is oxydized in the air, and a gramme oxydized in oxygen, certainly develop the same quantity of heat; but the latter is, perhaps, oxydized in a second, while the other requires several hours. Hence the immense immediate difference in the heat exhibited by each. Half a pound of grapes heaped together will produce considerable heat in fermenting; the same quantity placed in a layer emits the same quantity, yet it is not perceptible, because too much dispersed. It is thus that we are enabled to understand the difference in this respect between warm and cold blooded animals. We can entertain no doubt of the source of animal heat. It is found in the chemical reactions of respiration carried on in the capillaries, in the transformation of the tissues, and, above all, in the combination of oxygen with carbon.

I feel it unnecessary to explain the other hypothesis regarding the source of animal heat. In consequence of the fall of the thermometer when placed in contact with the tissues of an animal after division of the pneumogastric nerves or the spinal cord, it was concluded that *innervation* was the direct cause of animal heat; but it was overlooked that by this division of the nerves and spinal cord, respiration and the circulation of the blood were retarded. Instead of entering upon the discussion of similar hypotheses, it will be more useful to examine more particularly those chemical actions which we have considered as the only source of animal heat.

Philosophers have endeavoured to show the truth of these hypotheses: an animal exhales, during a given time, a certain quantity of carbonic acid and water, and at the same time develops a quantity of caloric, which can be measured by the quantity of water it is capable of heating (to a certain degree) during the same period. If the carbonic acid and water which the animal exhales are the products of the combustion of carbon and hydrogen, the heat developed by the animal ought, say philosophers, to be equal to that which the same quantity of carbonic acid and hydrogen would produce when burnt in air.

By recording the results furnished by a calorimeter, into which the animal was put, noting the temperature acquired by the water, and measuring at the same time the oxygen absorbed by the animal, or the products,—carbonic acid and water,—Dulong, and afterwards Despretz, found, that of 100 parts of heat produced by the animal, and ascertained by means of the calorimeter, eighty or ninety only were represented by the combustion of the carbon and hydrogen furnished by the carbonic acid and water emitted from the animal.

If we remember that the temperature of the animal placed in the calorimeter is always higher than that of the water which surrounds it, and that consequently the animal is growing cold during the experiment, this cooling may be thought to afford a plausible explanation of the excess found and, in fact, the numerous experimets of Despretz have shown that the excess of heat indicated by the calorimeter, beyond what is owing to respiratory combustion, is greater in proportion as the animal is young, and its temperature high. Besides, we know, from the beautiful experiments of Edwards, that young animals cool much more rapidly than adults.

These considerations are sufficient to show that the excess indicated by the calorimeter can be accounted for without having recourse to a special power —a vital property which engenders heat.

I ought to add, that after the death of the celebrated Dulong, an account of various unpublished experiments was found among his papers, relative to the heat developed by the combustion of hydrogen. This heat was much greater than previously found by Dulong himself, and Despretz. The number determined by the latest experiments of Dulong has since been confirmed by those of Fabre and Silverman. Now, by adopting this new number, we no longer find an excess of heat indicated by the calorimeter over that developed by the combustion of hydrogen and carbon, but, on the contrary, a deficiency.

There is therefore no motive to seek for other sources of animal heat than the chemical reaction of respiration and nutrition; but I think it would be wrong to draw an exact parallel between the results of experiments on ordinary combustion in a calorimeter, and that which takes place in an animal, and to admit only one of the numerous chemical reactions which take place within the same animal as the source of animal heat. And, in fact, the carbonic acid with which venous blood is charged—which is certainly a product of the combination of atmospheric oxygen with the carbon of the organic elements of the various tissues which have undergone some modification—cannot arise from carbon existing in a free state in these tissues, but rather in combinations which we are far from perfectly understanding.

The experiments of Dulong have now placed it beyond doubt, that a body, when combined with another, does not produce the same quantity of caloric by burning or uniting with oxygen, which it would emit if it were free. The heat produced by the combustion of bicarburetted hydrogen, the gas of morasses, or the oil of turpentine, in oxygen, forming water and carbonic acid, does not equal the caloric which the volumes of gas which compose them would have furnished if burnt separately; it is generally less. The experiments of Hess and Andrews, which were intended to show that in a given combination an absolute quantity of heat is developed, whatever be the state of the two bodies in union, have, hitherto, only been tried with successive combinations of the same body, as in the case of sulphuric acid which combines with different atoms of water.

If we confine ourselves to the chemical action of carbon and hydrogen with oxygen, to explain the production of animal heat, it will be difficult to account for the results which have lately been arrived at by Andral and Gavarret, in their investigations upon the exhalation of carbonic acid during the act of human respiration. According to the very extensive, and apparently very exact experiments of these two distinguished physiologists, the quantity of carbonic acid exhaled during respiration may vary much, according to the sex, age, and some peculiar physiological dispositions. The difference is comprised between the numbers 5 and 14,4, equally expressing by these the quantities of carbon (measured in grammes) which are necessary to form the carbonic acid expired in the space of an hour. The first of these numbers was obtained from a child of eight years old, and the other from a young man of twenty-six. It is worthy of note, that in children the temperature is decidedly higher than in adults, but in the latter, the heated mass being much larger, the loss of heat which they undergo must be proportionally great.

Andral and Gavarret have also found, that in women the quantity of carbonic acid exhaled is not increased at the age of puberty, but that this exhalation becomes more active when age or other causes put a stop to the phenomenon of menstruation.

In spite of this, no sensible difference of temperature is remarked in the female body, either before or after, or during the time of menstruation, nor in the state of pregnancy. And, without having recourse to the results of experiment, it is sufficient to consider, that in some maladies there is a rapid decrease of temperature; in others, on the contrary, a very great elevation throughout the body, without our being able to observe a corresponding variation in the function of respiration.

Let us then conclude, that, in the present state of our physico-chemical knowledge, we may safely admit that the chemical actions which take place in animals during the transformation of their tissues, under the influence of the atmospheric oxygen, are the source of heat in animals; that among these, the combustion of carbon and hydrogen ought to be considered as the principal but not the only one; and that further experiments are necessary to discover the exact relation between the heat produced by an animal, or which arises from chemical reactions which take place within it, and those which we are able to produce with our apparatus.

I will not leave this subject without mentioning, that in vegetables, also, the heat developed by germination is a phenomenon of chemical action, occasioned by the combination of oxygen with the carbon of the germinating grain. It is known, that in germination there is an absorption of oxygen and a disengagement of carbonic acid; that the *diastase* converts the starch into dextrine and sugar, which afterwards disappear as carbonic acid. It is curious, that in plants, as in animals, it is starch and sugar which, by combustion, disengage the heat peculiar to these bodies. It is also thus that we must explain the heat which accompanies the fecundation of plants, and it is for this reason that we see, in sugar-cane, beet-root, and carrot, the sugar disappear after flowering and fructification.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*Influence of Electricity in the Production of Diseases.* By M. Pallas, Principal Physician in Algeria.

Dr. Pallas has addressed a note to the Academy of Medicine prior to publishing a work he has undertaken upon the importance of electrical isolation in the treatment of certain diseases. The subject, he states, is especially important in relation to the etiology, nature and treatment of the diseases of hot climates; and may be summarily stated in the following propositions.

1. The greater number of diseases, and especially those which belong to the class of neuroses, are occasioned by the exaggerated influence of general electricity, of which clouds, storms, and marshy regions are the most fruitful sources. 2. Marshes, in their geographical constitution, and the effects which they produce upon the economy, present the greatest analogy with the galvanic pile. Thus their action is so much the more baneful as they contain certain proportions of water, and their activity is considerably increased when the water contains organic or saline matters in a state of solution. This explains why salt marshes and such as are near maritime rivers are the most insalubrious. The drying up or submersion of marshes produces analogous conditions to those of a galvanic pile deprived of humidity, or which is under water, and the effects of which are then insignificant. 3. The researches of philosophers and physiologists have shown that the electricity produced by our machines exerts a special action upon the nervous system. Experience and rigorous observation of facts prove that the diseases which are produced in a marshy atmosphere are primarily nervous, and become inflammatory only by the re-action of the nervous upon the vascular system, inducing consecutive local or general irritation. 4. The neuroses are occasioned, generally, by the effects of electricity, and intermittent fevers have a similar origin, that is to say, they are due to the electrical emanations of the marshy pile, which are very active in hot countries, and not to miasmata, which have never been met with. 5. Electrical isolation is a rational means of modifying this morbid influence, and is accomplished by attaching to ordinary beds, sofas, or chairs, legs of glass or resin. A great number of cases prove that the patients whom I have thus isolated, have been cured or relieved, several of whom had resisted all the ordinary means of cure.

"6. Just as light and air are the essential agents of vision and respiration, electricity is the functional agent of innervation, whose injurious action may be modified by isolation; which is to electricity what a shadow is to the solar light.—*Bulletin de l'Academie*, Tom. xii., p. 743.

2.—*Painful Crepitation of the Tendons.* By M. VELPEAU.

The man whom you have just seen is a dyer by trade, æt. 49, and his case deserves a moment's notice. A week since he endeavoured to raise a load, having his left hand applied to his hip. He felt a violent pain in his arm, and now we may perceive a slight swelling at the lower and external part of the forearm, unaccompanied by any change of colour or fluctuation. Of a regular and elongated shape, it is only painful during motion, while on applying the hand over it we may perceive a fine, characteristic crepitation; and it is an example of the *painful crepitation of the tendons* which was vaguely indicated by Boyer and Desault, described by me first in 1825, and has since formed the subject of the special writings of several authors. I first met with it in a case in the hospital of Tours, where it was suspected to be a fracture of the radius. The affection is especially observed among washerwomen, mowers, blacksmiths, locksmiths, and joiners, and when it is seated in the foot, among soldiers, huntsmen, &c. Excessive friction is the condition necessary for its production. In the forearm and wrist, where it is especially met with, its recognition is very easy, the crepitation it gives rise to being quite pathognomic, being neither like that felt in fractures, that of cartilage or emphysema: but which has been compared to the crepitation of starch or of hoar-frost—such as is produced by walking on the snow. Its seat is evidently the sheath of the tendons, and it is probably due to a slight inflammation, first causing too great a dryness of the mucous membrane, and afterwards giving rise to effusion. It it generally in no-wise serious, disappearing in a few days by rest alone; but it must not be absolutely neglected, for I have seen it in some cases give rise to a fungous transformation of the sheaths; and indeed there is no reason why all the changes which occur in diseases of the joints should not take place here. If there is much pain we apply leeches and poultices, and the resolvent lotions and compression: but rest is indispensable.—*Gazette des Hôpitaux*, No. 82.

3.—*On Abscess of the Breast.* By M. VELPEAU.

"Subcutaneous inflammation of the Breast proceeds much as an ordinary phlegmon. When the abscess is formed between the mamma and the chest, the swelling is considerable, the breast raised up, but after an incision the cure usually takes place rapidly. But when the phlegmasia invades the substance of the breast itself, it is rare to find only a single abscess produced. We sometimes see 10, 20, 40 or 50 manifesting themselves in succession. An instant's reflection will show that this result is a natural consequence of the anatomical disposition of the inflamed tissue. The glandular parenchyma consists of different lobules, each of which constitutes a little organ having its own function, and which may become heated and irritated under the influence of lactation. Each lobule does not attain at the same time the same degree of irritation. One first inflames, then suppurates, and constitutes a first abscess: a neighboring lobule then becomes affected and, in its turn, forms an abscess; and so it may go on with all of them until we have as many successive abscesses as there are lobules.

"This distinction of abscesses of the breast into at least three orders is of the highest importance; and if we do not adopt it, our ideas upon the subject will be but very vague, and devoid of all precision as respects prognosis and treatment. Parenchymatous abscesses may last four or six months, or a year even, according to the rapidity of their succession and their number. The subcutaneous abscess lasts only as long as an ordinary phlegmon; and the submammary abscess has not the long duration of the parenchymatous one.

"Each of these has again its special treatment. We may endeavor to procure the resolution of *subcutaneous abscess*, and that by ordinary means; and, if suppuration occurs, we open it promptly, in order to avoid the burrowing of the pus among the tissues. *Sub-mammary phlegmon* should be treated especially by general measures, and leeches around the nipple. Topical applications are of little use, as they are separated from the centre of inflammation by the whole substance of the mammary gland. When an abscess is formed here, its prompt evacuation is desirable : but the perception of fluctuation is difficult, for the pus is surrounded by a large mass of tissues, and the thoracic parietes have not fixity enough to serve as a point of support. Nevertheless, you may recognise the existence of pus by the following characters : 1. An acute phlegmon rarely exists more than seven or eight days without suppuration taking place. 2. The breast is raised up like a sponge, and if we press upon it, it seems as if it were lying on a bladder full of fluid. 3. We find the breast surrounded by a kind of inflammatory œdema. Having recognised the pus, we should let it out promptly, or we expose ourselves to seeing it traverse the gland and form one of those abscesses I call *shirt-buttons*. These abscesses, moreover, have a mischievous influence upon the chest, and may lead to a purulent pleurisy.—They may, too, penetrate into the cellular tissue for a distance, and give rise to a diffused phlegmon. The incision should be made into the most dependent part, the place of election being below and at the outer side of the nipple, but, in some cases, a projecting point of the abscess indicates the place at which the opening should be made. It is always advantageous to make the incision towards the circumference of the breast, because the gland itself is not touched, and its weight tends to expel the pus. The bistoury should be directed almost parallel with the thoracic parietes, so as to slide it in between these and the mamma. The danger of such incisions is not great, there being no large arteries to fear. *Parenchymatous phlegmon* requires an energetic and varied treatment. Bleeding, purging, and the so-called anti-lactal medicines. When pus forms, which is almost always the case, topical applications and incisions seldom prevent the successive implication of the lobules. Nevertheless, there is some advantage derived from the prompt opening the abscess, *if the patient agrees to it*; for you should recollect that, in practice, if you open one abscess and others form, she never fails attributing these to your proceedings. These details will, I think, suffice to show you how important it is to distinguish the different abscesses of the breast, and to explain to you the confusion which prevails in the minds of some surgeons as regards the treatment."—*Gazette des Hôpitaux*, No. 89.

4.—*On the Nature of the Liquid secreted by the Mucous Membrane of the Intestines in Cholera.* By M. ANDRAL.

M. Andral recently read a note at the Academy of Medicine giving an account of the researches he has been engaged in for the purpose of determining the nature of the peculiar white matter, resembling a decoction of half-cooked rice, which is found in the digestive organs of patients attacked with cholera, and which especially belongs to and characterizes that affection.—From the facts detailed, he drew the following conclusions :

"1. The white matter which fills the intestines of cholera patients, is not, as it has often been stated to be, a portion of the blood itself; for neither albumen nor fibrine are found in it. 2. It is nothing else than mucus rapidly secreted in large quantities, and for this reason modified in its qualities. 3. The essential microscopic character of this matter is its containing a very considerable number of cells, with nuclei perfectly resembling, as far as regards their appearance, the cells found in pus, although this matter in no other respect bears any resemblance to pus. 4. The examination of the blood of cholera patients shows that the albumen of the serum is maintained in its normal proportions. 5. The theory which refers the symptoms of the stage of cyanosis in cholera to the change which the blood has undergone by reason of a great and sudden loss of serum, cannot be admitted."—*Gazette Medicale*, No. 33.

5.—*On the Treatment of Typhoid or Entero-Mesenteric Fever, by the Black Sulphuret of Mercury.* By M. SERRES.

M. Serres has recently read some papers upon this subject before the *Academie des Sciences*, and as they have exited much attention in Paris, a notice of some of the principal points dwelt upon by him may prove acceptable to our readers.

He believes that the symptoms, progress and anatomical lesions of this disease all show that it belongs to the exanthematous fevers, and this fact constitutes the basis of the proposed treatment. The histories of measles, scarlatina, erysipelas, but especially of variola and vaccinia, prove that the amount of fever is proportionate to the amount of eruption; if this is discrete the fever is slight; if it is confluent the fever is intense; "it becomes confluent also by the change that takes place in the composition of the blood, and the phenomena of re-action which are developed throughout the system." As long ago as 1812, the author, together with M. Petit, endeavoured to demonstrate a like dependence of typhoid fever upon the amount of entero-mesenteric changes which were developed; and 35 years additional opportunities of investigating the subject at La Pitié and the School of Anatomie, where bodies are brought from all the hospitals of Paris, have conferred upon the proposition all the certitude attainable in medicine.

"If, as now stated, every eruptive fever is compounded of two distinct elements: of the eruption, which is the dominating element, and of the fever, which is the dominated element, the therapeutical course is traced out in this disease by this subordination of the phenomena. Reasoning indicates this, and medical experience has demonstrated it. In the remarkably faithful picture drawn by Sydenham of the progress and generation of symptoms in the small-pox (the passages are quoted, but they are or ought to be familiar to our readers), we recognise that of the typhoid fever which was furnished by M. Petit and ourselves. If, in fact, in the comparison of the two diseases, we form an abstraction of the eruption or fundamental portion of each, we find a perfect resemblance in the phenomena of the consecutive fever constituting them—the same infection of the blood—the same permanence in the course of the infection—the same saturation of the system with a deleterious principle. The bases of their therapeutics should partake of and reflect this uniformity. But, for the bases of therapeutics to assume such conformity, they must be able to extend to the foundation of these two diseases. And here is the difficulty. With respect to small-pox the etiology has never been contested. All agree that, beyond the affection of the skin, there is a general affection, having its vehicle in the mass of the blood. It is not the same with the etiology which is here given of typhoid or entero-mesenteric fever. Eminent observers, and whose consummate experience might well serve as a guide in medicine, have entertained an opposite opinion. They have seen in the disease only an enteritis, or an inflammation of the intestine, different degrees of which might explain the general and local symptoms by which it reveals itself."

"The treatment I propose consists in the administration of the *black sulphuret of mercury* in the form of pills, and the inunction of the abdominal parietes by means of the mercurial ointment every morning. Four grains of the black sulphuret are formed into a pill with tragacanth and syrup; and from four or six of such are given every second day. The treatment may be continued for six or eight days, provided no stomatitis occurs. If the mucous membrane of the mouth becomes inflamed the frictions are to be suspended and the sulphuret diminished or discontinued, applying alum gargles or slices of lemon to the gums.

"Although every one recognises that the gravity of the disease is dependent upon the amount of the intestinal eruption, no one has hitherto tried to treat this topically. Purgatives in general fulfil the first indication of treating the general poisoning of the system; but it is the mercurial purgative which alone exerts a special topical action on the intestinal patches. We cannot give proofs of this direct action; but from the effects which mercurials exert on analogical diseases, we are enabled to make an *à posteriori* induction upon the subject. We know that the application of mercury procures the abortion of variolous pustules. Mercurial frictions dissipate an erysipelas springing from internal causes—as they do the rose-coloured lenticular patches which appear on the abdomen in typhoid. The diarrhœa and distension of the abdomen in typhoid are certainly due to the irritation which the intestinal eruption determines upon the mucous membrane of the intestines; and both these symptoms are relieved under the use of the black sulphuret (although ordinary purgatives fail to relieve them), proving that this exerts a topical action upon the intestinal eruption, preventing or arresting its development. But it likewise exerts a more generally beneficient effect upon the organism, seeming as if it reached the cause of the disease itself;—the fever becomes less, the pulse diminishes in number, and the delirium abates—and this in so decided a manner as to be obviously the result of the medicine. By this method we do not abridge the duration of the fever. It continues, as under other treatment, for 3 or 4 weeks; but generally, when seen early, it is conducted through its course without any accident arising."

"Although the lenticular and rose-coloured spots on the abdomen, which constitute so characteristic a symptom of typhoid fever, differ essentially from the variolous pustule, yet the septic nature of the two diseases, the concomitant changes in the state of the blood, led me to the study of the action of mercury on these petechiæ. In the year 1845, this petechial eruption was remarkably abundant in most of these patients; but, under the application of the mercurial ointment, they disappeared very rapidly—the accompanying meteorism simultaneously diminishing. This double result led me to conclude not only that the mercury operated beneficially upon the petechiæ, but also upon the intestinal eruption, which constituted the foundation of the fever. If this last conclusion were correct, it was reasonable to suppose that, could the mercury be brought into direct action on the intestine, its effect would be still more prompt and efficacious; and, after an attentive examination of several pharmaceutical preparations of this metal, the black sulphuret seemed the best adapted to fulfil the desired indication."

Some particulars of a few of the cases which have fallen under M. Serres' notice are furnished, and he draws the following conclusions:—1. The fever and cephalalgia have been evidently influenced by the second or third day of the medicine. 2. The pulse has fallen below the mean, and even become remarkably slow. 3. No adynamic or ataxic accidents occurred; and, when adynamia appeared at the commencement of the disease, it was soon removed. 4. The quantity of ethiops employed to procure these results has not exceeded 50 grains, and several times but 30 have been administered. 5. Only a slight stomatitis, of which the patients hardly complained, was produced. 6. Convalescence was fairly established from the 8th to the 15th day, return to health always having been accomplished without relapse. 7. The patients left the hospital entirely cured in between 30 and 50 days; although they were encouraged to stay in as long as possible for the purpose of observing any relapse if such occurred.—*Gazette Medicale*, Nos. 33 and 34.

6.—*Treatment of Dropsy after Scarlatina.* By EDWARD CHARLTON, M. D., Newcastle.

Dr. Charlton describes the dropsy which occurs as a sequela of scarlatina as coming on in two different ways. In one, its invasion is intense and sudden, the body being distended with fluid in twenty-four hours, with high fever, full pulse, and almost entire suppression of the urinary secretion. In such cases free general blood-letting was found to be the most efficacious remedy; and this could be, in the cases which occurred to Dr. Charlton in the epidemic described in his pamphlet, the more readily adopted, as the intense attacks of dropsy usually supervened upon the mildest forms of the cutaneous affection. In illustration of this mode of treatment he subjoins an extract from a friend as follows: "The treatment depended much on the nature of the case. If the anasarca was great, with much oppression of the breathing, high fever, rapid but firm pulse, I found one good general bleeding to be followed by the most beneficial effects. This was succeeded by the administration of a mixture of nitrate of potass, liq. ammon. acet., and ant. tart. in moderate doses every two hours, with calomel and compound jalap powder at night, and the latter repeated in large doses in the morning."

Dr. Charlton has occasionally had recourse to the treatment recommended by Dr. Golding Bird. The patient was wrapped in flannel, hot baths were given every night, and he took the following mixture every four hours:

℞. Vin. ant. potass. tart. *m* x,
Jalep. ammon. acet. ℥ iij,
Syrup. papaveris, *m* x (?):

and the following powder every night:

℞. Pulv. ipecac. comp. grs. iiiss,
" hyd. c. cretâ, grs. v. Ft. pulv.

Elaterium and other drastic purgatives were also given with advantage.

In the other form of dropsy, where the swelling comes on gradually, with little fever, Dr. Charlton has recourse to similar treatment, with the exception of venesection, which is omitted. When dangerous head or thoracic symptoms supervened, as was sometimes the case, the free employment of drastic purgatives was found most efficacious. Of these, croton oil and elaterium were preferred. Whether the fluid had accumulated in the pericardium, pleura, peritoneum, or general cellular tissue, elaterium, in doses of 1-12 to 1-6th grain every three or four hours, produced the most rapid amendment. Stimulants exhibited at the same time with the purgatives are in some cases necessary, and have the best effect.

Much benefit is, in some cases, derived from the exhibition of the iodide of potassium during convalescence. In patients who were left anemic and debilitated with tendency to the scrofulous deposit, the preparations of iron were useful, particularly the citrate and the iodide. Lastly, change of air is always beneficial in restoring the patient.

Dr. Charlton has little faith in diuretics, as they appear to increase the renal congestion; leeches over the region of the kidneys answered better.

Account of an Epidemic of Scarlatina at Newcastle, 1847.

7.—*The Cause, Prevention, and Treatment of the Typhus Fever.* By I. Pidduck, M. D.

(Lancet, Aug. 14.)

The cause of typhus fever is the exhalation of a specific poison from the bodies of the sick, by which persons in health become infected with the disease, as in cases of small-pox, measles, scarlet fever, &c.

This poison may be destroyed by a temperature of 212 deg., whether by boiling in water or by hot air; it may also be diluted by washing and ventilation, so as to be rendered inert.

The prevention of typhus fever consists—

1. In separating the healthy, particularly the young, from the sick.

2. In removing curtains and carpets from the room, and cloths from the person of the sick.

3. In boiling linen and cotton garments, blankets and rugs in water, before they are washed, and in baking woollen cloth garments, which cannot be boiled, put into a sack, in an oven.

4. In washing the bodies of the sick, and the floors of rooms, with soap and water, and the walls and ceilings with lime.

5. In lighting fires in fire-places, and setting open windows and doors.
6. In keeping provisions away from the apartment of the sick.

But as this poison exerts a much more malignant and fatal effect upon persons whose health is impaired by exposure to malarious influence, it is of great importance that putrid effluvia from drains, dunghills, or privies should be carefully obviated.

The drains from houses should be covered in, cesspools and necessaries should be emptied, stagnant ponds should be run off, and every cottage in the country should be provided with a bricked cistern, covered with a wooden flap-lid, for the reception of all solid and liquid manure, which should be emptied and carried out on the land, as soon as it is full.

If these precautions are taken, there is no need for chloride of lime or any other disinfecting agents, which only correct putrid effluvia; they have no power to destroy poison. They are worse than useless when they lead to a false security and occasion the neglect of these more efficient means. In like manner, drinking to excess, especially ardent spirits, eating unwholesome food, such as bad potatoes, decaying vegetables, half-rotten fruit, musty or sour meal, unsound meat, stale fish, and drinking stagnant water, should be carefully avoided. Great attention should be paid to personal and domestic cleanliness. The house should be kept dry, warm, and well ventilated.

The treatment of typhus fever. This is better left to the medical practitioner in the locality, who is best able to judge as to the remedies most suitable for individual cases.

The following are the principles which guide the practice in the typhus fever of London:

1. To remove all offending matters from the stomach and bowels, an emetic of salt water or ipecacuanha is administered; then a grain or two of calomel, and fifteen grains of rhubarb, followed by castor oil if necessary.

2. After the operation of the emetic and purgative, the patient is washed all over with soap and water and put into a clean warm bed, with a fire in the room, and the window open.

3. Five grains of the chlorate of potass in a wineglassfull of camphor-mixture is ordered every six hours. The chlorate of potass seems to aid the vital energies in expelling the poison, evinced in the improved colour of the skin, and altered state of the secretions.

The diet consists of bread and milk, or gruel, seasoned with salt instead of sugar, light broth, and fresh, well-boiled vegetables; whey, sago-tea, or lime-blossom tea, and oatmeal toast-water.

The body linen and flannel vest are changed daily, and the sheets once a week; the dirty linen, cotton, and flannel are put at once into cold water, and boiled before they are washed.

From this statement, it is evident that the cure of typhus fever can no more be effected by medical treatment than the cure of small-pox, measles, or scarlet fever. The disease, once set in, must run its course. It terminates, naturally, on or about the fifteenth day. The object of medical treatment, therefore, is to avert its fatal tendency; or, in other words, to conduct the patient in safety through its different stages. If the disease do not admit of cure, much may be done toward its prevention. 1. By separating the healthy from the sick. 2. By destroying or diluting the poison. 3. By avoiding all those causes which impair the health and weaken the powers of resistance.

AMERICAN MEDICAL INTELLIGENCE.

Proceedings of the Medical Convention of the State of Alabama, held in Mobile, December, 1847.

MOBILE, December 1st, 1847.

The STATE MEDICAL CONVENTION assembled this morning at the Waverly House at 11 o'clock.

On motion of Dr. P. H. Lewis, the convention was organized by calling Dr. A. Lopez, of Mobile, to the chair pro tem., and appointing Dr. G. S. Pollard, of Montgomery, Secretary pro tem.

The gentlemen representing societies and counties came forward and enrolled their names as Delegates.

A List of the Delegates in attendance at the Convention :

Wm. B. Johnson, M. D., Marion Medical Society, Perry County.
Richard Clark, M. D., " " " "
A. G. Mabry, M. D., Alabama Medical Society, Selma, Dallas County.
Wm. Morgan, M. D., " " " " "
Thomas W. Mason, M. D., Wetumpka, Coosa County.
Edmund P. Gaines, M. D., Washington County.
G. P. Barnes, M. D., Claiborne, Monroe County.
John H. Woodcock, M. D., Baldwin County.
Geo. S. Pollard, M. D., Montgomery Medical Society, Montgomery County.
A. Lopez, M. D., Mobile Medical Society, Mobile.
John F. Innerarity, M. D., " " "
F. A. Ross, M. D., " " "
K. Kovaleski, M. D., " " "
Geo. A. Ketchum, M. D., " " "
R. Miller, M. D., " " "
W. B. Crawford, M. D., " " "
W. C. Hicklin, M. D., " " "
A. B. C. Dorsey, M. D., Monroe County.
Richard Lee Fearn, M. D., Mobile Medical Society.
N. Walkly, M. D., " " "
Y. Wolf, M. D., Mobile.
T. E. Pearson, M. D., Pickens County.
R. C. Ashe, M. D.
W. W. Adair, M. D., Perry County.
J. H. Lang, M. D., Mobile County.
G. Owen, M. D., Tuskaloosa.
— Evans, M. D.
A. R. Rembert, M. D.
E. H. Kelly, M. D.
B. R. Hogan, Alabama Medical Society, Selma, Dallas County.
John M. Langhorn, M. D., Perry County.

By resolution of Dr. Mabry the following gentlemen were appointed a Committeee to propose permanent officers for the Convention:

Drs. Fearn, Clark, Morgan, Mason, Woodcock, Barnes, and Gaines.

On motion, Dr. Mabry was added to the Committee as Chairman.

On motion, the Convention adjourned until to-morrow at 11 o'clock.

December 2d, 1847.

The Convention met pursuant to adjournment.

The Committee to propose permanent officers, reported the following :

For President—WM. B. JOHNSON, M. D., Perry County.

For First Vice President—R. LEE FEARN, M. D., Mobile.

For Second Vice President—A. G. MABRY, M. D., Selma, Dallas County.

For Secretaries—GEO. F. POLLARD, M. D., Montgomery; WM. B. CRAWFORD, M. D., Mobile.

For Treasurer—GEO. A. KETCHUM, M. D.——Signed by the Committee.

On motion, the Report was adopted.

The President appeared and took the chair, and after a few brief remarks, declared the Convention organized and ready to proceed to business.

The following preamble and resolutions were proposed by Dr. A. Lopez:

Whereas, It has become a settled conviction with the Medical Faculty generally, throughout the United States, in which the State of Alabama especially participates, that wide spread and formidable evils have attached themselves to the profession from sources hitherto overlooked; and that a climax has at length been reached, which, without a stern and undivided effort at once on our part, must inevitably tend to the utter subversion of that high and important position which it behooves us to sustain. And, whereas, recent demonstrations have been made by the assembled wisdom and virtue of our professional brethren at the late National Medical Convention held in Philadelphia, bearing immediately upon the consummation so devoutly to be wished,

1st. *Resolved*, That this Convention hail the proceeding of the late National Medical Convention as the harbinger that a new and irresistible momentum will be given to the moral and intellectual perfectibility of our profession, and that our sympathies are with them in the prosecution of the good work begun.

2d. *Resolved*, That in order to evince the sincerity of this declaration, we most strenuously recommend to the different Medical Societies of the State to appoint Delegates to the National Medical Association to be held in Baltimore on the first Tuesday of May next, and we earnestly recommend to the State Medical Association to be held in Selma in March next, the propriety of appointing Delegates to represent the Association in the same National Convention.

3d. *Resolved*, That this Convention appoint a committee to examine the code of Medical Ethics prepared and published by the late National Medical Convention, and if approved, recommend it to this Convention as a standard by which the Faculty of the State of Alabama be hereafter governed.

On motion, adopted.

Committee appointed under third resolution, as follows: Dr. Lopez, Chairman; Drs. Fearn, Lewis, Mabry, Dorsey, Barnes and Clark.

A preamble and some resolutions were received from Dr. Woodcock, which, after some debate, were laid on the table indefinitely.

The following resolution was proposed by Dr. Fearn:

Resolved, That a committee of seven be appointed for the purpose of reporting to this Convention on the laws and regulations of the State of Alabama affecting the practice of medicine, and the sale of drugs and medicines.

Adopted, and the following committee appointed:

Richard Lee Fearn, Chairman; Drs. Mabry, Clark, Lewis, Dorsey, Lopez and Mason.

The following preamble and resolution was proposed by Dr. A. G. Mabry:

Whereas, It is a fact that few will attempt to controvert, that annually for years past too many young men have entered the medical profession in the United States—a number far exceeding that which the wants of the country required, and the result of which has been the drawing off from agriculture, the mechanic arts, and other pursuits a large number of persons who were well calculated to succeed in the prosecution of such pursuits, thereby becoming useful members of society, and securing for themselves a comfortable support for their declining years; but who being unprepared for the responsible duties of practitioners of medicine by defective education, and the hasty process by which they have been manufactured doctors, and allowed to assume these duties, instead of becoming useful members of society, have, by an unwise selection of a profession, the duties of which they were not prepared to perform, sacrificed their time; and by lives spent in idleness and profligacy, often, far too often, bring disgrace upon themselves and reproach upon their families, their friends, and the profession of which they have been permitted to become members. This must be owing to the inducements which are held out to young men to enter upon the study of medicine, and these again to a considerable extent to the great number of medical schools with which our country abounds. These schools supposing that their fame and prosperity depend upon the number of students which they are able to attract to their lecture rooms, in order to increase their prosperity and secure the attendance of full classes, held out such inducements as have generally, if not universally, accomplished the object proposed, but have so reduced their fees, curtailed the time of study, and abandoned one after another, those requirements which were all at one time universally considered indispensably necessary to entitle an individual to the honors and privileges which belong to a degree of Doctor in Medicine, that so little is now required that almost every one who presents himself as a candidate receives these honors and privileges and is sent forth as a practitioner of medicine. This state of things has long operated as an evil upon the best interest of our country, and its correction is an event which we most cordially desire to see consummated.

Be it therefore *Resolved*,

1st. That we earnestly recommend to the Medical Schools throughout the United States to adopt the system of reform which was recommended by the National Medical Convention which assembled at Philadelphia in May last.

2d. That we and each of us do pledge ourselves to exert our influence in favor of such schools as shall adopt such a system, or show a willingness to do so, and withhold it from all such as shall fail to adopt it, or show a willingness to do so.

3d. That Alabama has no medical schools within her limits and does not desire one; but since she must receive doctors from the institutions of other States, she claims the right to express her sentiments freely upon the subject.

4th. That the qualifications required by the different medical schools in the United States of applicants for the degree of Doctor in Medicine are so limited as to be attainable by individuals of limited pecuniary means and very ordinary capacity.

5th. That hereafter no one should be permitted to assume the duties of practitioner of medicine in the State of Alabama who has not obtained a diploma from some respectable medical institution.

6th. That a heavy fine should be imposed by law upon all such as may fail to comply with the requirement of the resolution next above.

7th. That we consider the vending of nostrums and patent medicine in the State of Alabama an evil, and for the correction of which every vender should be required by law to affix to the medicine which he sells a label in English, showing the ingredients which it contains.

8th. That all persons who vend patent medicine and nostrums are governed by motives of self-interest and as they receive aid and protection from the law, they should be required to pay a tax proportionate to the amount of aid and benefit thus received.

Received and referred to Dr. Fearn's committee.

A preamble and resolutions on preliminary education was proposed by Dr. A. Lopez, which was received and laid upon the table until to-morrow.

The following resolutions were proposed by Dr. Mason.

Resolved, That a committee of five be appointed by the President for the purpose of preparing and presenting to the Legislature a memorial or petition, praying a tax upon all nostrum and patent medicines sold within the limits of the State.

2d. *Resolved*, That it be signed by the President and Secretary of this Convention.

Received, and referred to Dr. Fearn's committee.

The following resolution was proposed by Dr. Dorsey:

Resolved, That a committee of seven be appointed by the Chair to report a draft for the organization of a State Medical Society.

Received, and committee appointed as follows: Dr. P. H. Lewis, Chairman, (Dr. Dorsey declining,) Drs. Morgan, Barnes, Miller, Gaines, Ketchum and Woodcock.

Dr. P. H. Lewis proposed the following resolution:

Resolved, that all matters requiring legislative action be referred to Dr. Fearn's committee. Adopted.

On motion, adjourned until to-morrw 10 o'clock.

December 3d, 1847.

The Convention met pursuant to adjournment.

The Minutes read and approved.

The reports of Committees called for.

The committee on the proceedings of the National Medical Convention, Dr. A. Lopez, Chairman, submitted the following report:

The Committee to whom was referred the preamble and resolutions introduced on the 2d instant, calling upon this Convention to express their approval of the proceedings of the late National Medical Convention assembled at Philadelphia, beg leave respectfully to

Report, That they unanimously approve of the Preamble, as indicating a high sense of the obligation under which the Medical Faculty of the United States have been laid by the zeal, industry and professional pride characterizing the labors of the National Medical Convention on all subjects in which the dignity and welfare of the Physicians of the Union are concerned.

Your Committee consider the first resolution following, as equally entitled to adoption by this Convention, because it corroborates, by an expression of its sympathy, the acknowledgment set forth in the preamble.

With reference to the second resolution, calling upon this Convention to nominate through the committee a number of Delegates to represent it at the meeting of the National Medical Convention, to be held in Baltimore on the first Tuesday of May next, your committee are induced by common consent so to modify the original resolution so as to transfer this appointment unto the hands of the "State Medical Association" about to be formed, because when such Association is created, the existence of this Convention of necessity terminates by its limitation, and of course cannot be represented. The committee therefore beg leave to recommend to this Convention the adoption of the following amendments:

Resolved, That in order to evince the sincerity of this declaration, we most strenuously recommend to the State Medical Association about to be formed, and to all Medical Societies and Boards now in existence in this State, to appoint delegates to the National Medical Association to be convened at Baltimore in May next.

Your committee are solemnly impressed with the overruling necessity which demands a uniform and effective system of laws, whose moral obligations shall be directed to the preservation of self respect, which may conduce to it among the regular medical practitioners of this country, without reference to individual or sectional distinctions. They believe that the spirit which is now abroad is regenerating, and that there is "healing on its wings," and that it has laid its foundation upon principles neither light nor transient. They foresee that if this spirit be fostered, as it deserves to be, there must eventuate, beyond all chances of failure, a broad platform upon which the medical profession of this Union can meet, impelled by one common interest, and that interest the *dignity, honor* and benefit for which the practitioner of medicine has so anxiously sought. For this purpose your committee conceive no step so effectual as a code of *Medical Ethics*, by which we shall be mutually bound to each other, to preserve inviolable against all infraction, those moral tenets which, under all circumstances, distinguish the individual whatever be the relation in life he may be destined to represent. And, however direct and local may appear the need of laws by which this convention be governed, yet upon the subject under review your committee consider there should be one chain, whose links indissolubly united, shall convey the impression communicated to it throughout this land, and whose influence shall not only bind the physician by the obligation it imposes, but command the homage and respect of the community in which he may reside. To this end, therefore, your committee, after mature deliberation, have unanimously decided that they could not provide for such purposes any thing more suitable than the "Code of Medical Ethics" adopted and published by the "National Medical Convention," and would respectfully advise that this Convention commend by resolution, that the State Medical Association, as soon as formed, cause to be published as many copies of this code as may be required for distribution in this State.

Your Committee most especially beg leave to recommend to this Convention, that it unite in one voice in the most unqualified praise for the high toned, independent and self-sacrificing magnanimity by which the Medical Department of the University of Pennsylvania has distinguished itself in becoming the pioneer, to place the first foot upon the threshhold of the usages in this country, regardless of all consequences to themselves, and governed alone by an eye single to the general good of the profession they so honorably represent.

Your Committee express to this Convention their approval of the preamble and resolution offered by Dr. Mabry on the 2d inst., so far as they refer to the recent action of the National Medical Convention, and recommend them for adoption.

Your Committee, perhaps, have exceeded the duties rigidly defined by their appointment, but they rely upon this Convention to consider such trespass (if it be one) as intended to facilitate and expedite the business of the Convention by bringing under one supervision all such subjects as have a common bearing. All of which is respectfully submitted. Signed,

A. LOPEZ, Chairman.
RICH'D LEE FEARN, A. G. MABRY,
T. P. BARNES, A. B. C. DORSEY,
P. H. LEWIS, RICH'D CLARK.

Dr. Fearn's Committee on the Laws relative to the practice of Medicine and Surgery, and also the vending of nostrums and patent medicines, and to whom was referred various resolutions which had been offered, and all other matter requiring legislative action, submitted the following Report, which, after much debate and proposed amendments, was adopted as reported, as follows:

REPORT OF COMMITTEE.

The Committee appointed for the purpose of taking into consideration the existing laws and regulations in this State connected with the practice of medicine, and the sale of drugs, medicines, &c., and also for the purpose of reporting on several resolutions and suggestions which had been offered to the Convention, appertaining to these subjects, beg leave to report the following as the most important suggestions which the limited time and means have enabled them to make.

First. That all portions and parts of the laws prescribing the powers and duties of the several Medical Boards in relation to the examination and licensing applicants to practise Medicine and Surgery in the State be repealed; and that hereafter the resignation of the names of those who have presented diplomas from respectable Medical Schools, shall be evidence in all cases, of the right to practise Medicine and Surgery, and be admitted as such by all courts of justice in the State.

Second. That Apothecaries, and all other persons selling drugs and medicines of every description, be required to procure a license for that purpose.

That the power of granting said license shall be placed in the Medical Boards of the State, and that they be required, as a part of their regular duties, to examine all applicants for this purpose, under the same laws and regulations as they formerly examined applicants for the practice of Medicine and Surgery.

That any licensed Apothecary or Druggist who prescribes any medicine for any disease or affection, without the prescription or advice of a licensed Physician, shall be liable to all the fines and penalties imposed upon Physicians for doing the same without a legal license.

That all persons who practice Medicine, Surgery, &c., or who sell drugs and medicines without license, shall be subjected to the same laws that now exist and are especially applied to those who sell poisons to ignorant persons, slaves and servants.

Third. That all persons engaged in the sale of patent medicines and nostrums shall be compelled to affix to the article so offered by them, a label in intelligible characters, showing the ingredients of which it is composed, or failing to do so, shall be liable to all the pains and penalties inflicted by the laws on those who are guilty of assault and battery with intent to kill.

Fourth. That Coroners of each county be appointed by the Medical Boards in or adjacent to said county, and that the fees of office shall be so augmented as to induce competent persons to seek and attend to the duties of the office.

Fifth. That the Legislature provide, that when Physicians or licensed Apothecaries are called upon by the Solicitors for the State to give medical advice, or by the Coroner to make *post-mortem* examinations, or chemical analyses, it shall be considered professional, and remuneration recovered in proportion to services rendered, as in all cases in which they may be entitled to recover fees.

Sixth. That Assessors of taxes in every county be required to take a list of all births, deaths, and marriages, and that this list be recorded each year by the Clerk of the County Court, in a book especially appropriated to that subject.

Seventh. That Physicians and Apothecaries, for attendance and medicines furnished to slaves under mortgage, in the hands of trustees, agents, and all other circumstances of doubtful or obscure title, shall have a lien on the slave or slaves so attended or furnished, to the amount of their legally authorized bills, in the same manner and under the same regulations as the lien laws for the security of mechanics and laborers in relation to steamboats, &c.

Your Committee have closed their imperfect and hurried report; many subjects of reform, not embraced in these recommendations, have been under consideration, but they have been impressed with the opinion that the Legislature of our State, having rarely manifested a disposition to aid in medical reform, may not find the time at the present session to consider what we have already recommended. Respectfully submitted,

RICH'D LEE FEARN, Chairman.

A. G. Mabry, Rich'd Clark,
P. H. Lewis, A. B. C. Dorsey,
A. Lopez, Thos. W. Mason.

Report of Committee to establish a State Medical Association received and made the special order for to-morrow.

On motion, in accordance with the suggestion of the Committee on the National Convention, the second resolution in the preamble and resolutions offered on the 2d inst., was altered as it is recorded at present.

The following resolution was proposed by Dr. Hogan:

Resolved, That a committee of three be appointed by the Chair to memorialize the Legislature on the subject of the foregoing resolutions. Adopted.

Committee, Drs. Hogan, Rembert and Kelly.

The preamble and resolutions proposed by Dr A. Lopez on the 2d instant, were called up in order, when the preamble was withdrawn by Dr. Lopez, as conflicting with the resolutions of Drs. Fearn and Mabry.

The resolutions are as follows:

Resolved, That we adopt the sentiments on Preliminary Education recommended by the late National Medical Convention, viz: That henceforward every member of the Medical profession throughout the State exact a certain standard of Preliminary Education as a pre-requisite from every young man who may desire to enter his office as a student of medicine; and, being so exacted, to grant him a written certificate to that effect, specifying also the period of his admission into the preceptor's office, as a proper warrant and credential for the student when about to enter a Medical College.

Resolved, That a Committee of three be appointed by the Chair, to decide upon this standard; this Convention confiding to their judgment and liberality the adoption of such an one as will not bear too rigorously upon the applicant, and, at the same time, serve to exalt the future character of the profession.

Committee appointed by the Chair, Dr. A. Lopez, Chairman, Drs. Rembert and Kelly.

Dr. Mason proposed the following resolution:

Resolved, That this Convention will not countenance the common practice of regular practitioners signing a certificate recommending any nostrums or patent medicines. Adopted.

Resolution proposed by Dr. Fearn:

Resolved, That each member of this Convention be required to hand to the Treasurer the sum of Five Dollars; the same to be subject to the order of the President for the expenses of this Convention, and should there be any surplus, to be disposed of by the President as he may deem fit. Adopted.

Dr. Fearn proposed the following resolution:

Resolved, That the Committee for memorializing the Legislature be authorized to appoint delegates to urge these claims before the Legislature. Adopted.

On motion, adjourned until to-morrow 10 o'clock.

December 4th, 1847.

The Convention met pursuant to adjournment.

Minutes read and approved.

The Report of Dr. Lewis respecting the formation of a State Medical Association was called up, and submitted to the Convention by sections, and, after several alterations and amendments, was adopted, as follows:

The Committee appointed to take into consideration the propriety and expediency of organizing a State Medical Association beg leave to submit the following report:

Believing that various reforms and measures which the interest and prosperity of the medical profession of Alabama demands, cannot be secured without the united efforts of its members, and that this unity of purpose can best be effected through the medium of a well organized institution. And, believing further, that whilst such an Association is elevating the profession and illustrating its usefulness to the public, it will be the means of stimulating medical men to a more rigid investigation of disease, and fostering and sustaining a home medical literature, suited to the climate and the peculiar circumstances surrounding us, we earnestly make the following recommendations:

First. That when this Convention adjourns, the members present form a State Medical Society, and that all unfinished business be transferred to that society.

Second. That the title of the society be *The Alabama Medical Association.*

That for the immediate management of the Association, and the prosecution of business, the following regulations be adopted:

1st. That any regularly recognized practitioner of medicine in the State of Alabama be entitled to enrol his name as a member.

2d. That the next meeting of the Association be held at Selma on Wednesday, the 8th of March, 1848.

3d. That the officers shall consist of a President, two Vice-Presidents, two Secretaries and a Treasurer.

4th. That the officers of this Convention be adopted as the officers of the Alabama Medical Association until its next meeting.

5th. That a committee of five be appointed by the President of the Association to report at its next meeting a Constitution and By-Laws for its permanent government.

6th. That a Committee of two be appointed by the President to prepare and address a circular to the Physicians throughout the State, urging them to unite with this Association in advancing its objects.

7th. That some medical gentlemen from the different parts of the State be appointed by the President of the Association to report at the next meeting, subsequent to the meeting in March, 1848, on the character of the diseases that may come under their observation up to the time of meeting.

Committee appointed by the President under the fifth section, Drs. Dorsey, Kovaleski, Ross, Mabry and Mason.

Committee appointed under the sixth section, Drs. Fearn and Rembert.

The report of the Committee on Preliminary Education, Dr. A. Lopez, Chairman, was received and adopted, as follows:

The Committee to whom was referred the resolutions on Preliminary Education, with instruction to propose a standard of preparatory studies, which shall be required of applicants to be received as students of medicine in the offices of regular practitioners of medicine throughout the State, have given to the subject the consideration it merits, and beg leave respectfully to report,

That they fully appreciate the necessity of the requisition, and mindful of the appeal to our liberality, they trust that in recommending what they consider a preparatory course for young men who seek to enter among us as intelligent and honorable competitors they will not stand chargeable with the desire so to hem in the applicant by such hard conditions as to amount to a prohibition. Such is not their object.

The field is wide enough. All that we ask, is, that the convention discharge its duty to itself and to society, by providing that such place be occupied by gentlemen, who will neither directly or indirectly reflect discredit upon the medical character.

Communities are already too prone to strike an unjust average whenever the selection of the Physician is concerned. They say, (and perhaps with a show of truth) that the line of discrimination is scarcely perceptible that divides the shrewd and observing man who launches forth upon his own responsibility, irrespective of legalized right, and the alumni of the best schools, who leave the college walls unfit to fulfil the arduous and solemn responsibilities of their calling.

It is our duty then to rectify this perversion of public opinion, and it is the belief of your committee that the remedy must be sought in the first instant at the portals of your colleges. These avenues should not be opened to applicants without pretensions which shall be recognised as passports to their confidence, and if our medical professors are desirous to promote the standard of our profession, they must receive the countersign from the outposts.

The petitioner for Matriculation must present himself armed with such testimonials as will assure the professors that all has been done in the premises that is required, and this can only be accomplished by such a certificate as is advised in the resolution upon which the committee now report.

We therefore consider and recommend that all regular practitioners of medicine in the State of Alabama, should demand of those who desire to enter their offices as students, that they come thus prepared:

First. That they possess a good English education, such as is taught at the most respectable elementary schools.

Second. That they be sufficiently acquainted with the Latin language to serve them in comprehending the technical terms used in medicine, and to read and write prescriptions with facility and accuracy.

Third. A certificate from the last teacher as to the progress made in the studies prescribed.

Your Committee are aware that higher branches have been deemed requisite and so recommended, but they do not coincide with these views. It is true that the knowledge of such departments of learning as well as many others are desirable in order to perfect the accomplished Physician. But the object of your committee is to open the door to the meritorious and the talented, not to proscribe beyond reason.

With these considerations, the Committee respectfully submit their report.

A. LOPEZ, Chairman.
A. R. REMBERT.

The following resolution was submitted and adopted:

Resolved, That the proceedings of this Convention be published in the New-Orleans and Charleston medical journals.

On motion, that this Convention adjourn *sine die*, resolving into the State Medical Association, with the same officers as this Convention. Carried.

Then, The report of the committee to appoint Delegates to the National Convention in Baltimore in May, 1848, was received, and further consideration postponed indefinitely.

On motion of Dr. Mabry, the President appointed H. A. Wooten, M. D., of Lowndes county, to prepare and deliver an address at the next meeting of the Association.

On motion of Dr. Ross, That in the event of Dr. Wooten not accepting the appointment, the President be empowered to appoint some one in his place.

On motion, the Association adjourned to meet at Selma on the 8th of March, 1848.

WM. B. JOHNSON, President.

GEO. F. S. POLLARD, } Secretaries.
WM. B. CRAWFORD, }

NEW ORLEANS, MARCH 1, 1848.

WITHDRAWAL OF DR. FENNER.

To the Readers of the New Orleans Medical and Surgical Journal.

The undersigned, having withdrawn from the Editorial department of this Journal, begs leave to say a few parting words to those who fostered it in the beginning and who still extend to it their countenance and support. Having been one of the original projectors of the work, and laboured assiduously to maintain it for a period of four years, it may readily be supposed that he would feel something like a parental regard for its success. Such is the fact; and his earnest desire is that it may continue to prosper and increase in usefulness, until it shall realize the laudable expectations which prompted its undertaking. When the undersigned and his first colleague projected this work, there was not a single medical journal published in the United States, south of Kentucky. They had but recently settled themselves in New Orleans and were but little known either to the citizens or the physicians of the place. That an organ of intercommunication for the physicians of the South was greatly needed, every body admitted; but it was not without much distrust in their abilities to supply such a want, that they entered upon the task. Not being aware that any abler hands would undertake it, they resolved to make the experiment; their chief reliance being upon an earnest desire to do good, and a firm determination to exert themselves to the utmost. There is a solace in *the bare effort to do good*, even though it should not be crowned with success. So far as the undersigned is concerned, this has been almost his only reward for the labor he has spent upon this work. Yet he would by no means undervalue the amount of attendant success. It is gratifying to perceive that the Journal is now established on a firm basis and occupies a respectable rank among the periodicals of the day. The readers of this Journal will recollect that we have ever urged and entreated them to write. We did this from the conviction that if we could persuade them to attempt to write, their *amour propre* would prompt them to study more, so as to qualify them for *writing well*. No one knows what he *can do*, till he *tries*. The Medical Profession of the South commands no inconsiderable portion of the finest talent of the Country, but it needs to be stimulated into active operation. There are now four Southern Medical Journals, which we hope will exercise a most beneficial influence in this way. With my parting words I call upon the physicians of the South to foster and support their Medical journals, not only with their pens, but with their purses also.

In relinquishing my Editorial position, which has thrown me in contact with so many members of the Profession, I must express my gratitude for the uniform kindness with which my humble efforts have been viewed, and for the great pleasure I have derived from my extended acquaintance. I now join your ranks as a labourer in the common field of medical science, and trust I may yet be able to contribute something towards the elevation and improvement of our noble Profession. Adieu!

E. D. FENNER.

It will be seen from the above that one of the Editors of this Journal has retired. It will be permitted to his colleagues to say, that in all their intercourse he has ever borne himself as an enthusiastic lover of his profession, working willingly and zealously. We fear that his retirement is but a prelude to that of others. The Journal is not supported as it should be by the Medical Profession of the South and Southwest. Three volumes of the Journal have been published and nearly a fourth is ready, but it has been up-hill work with the Editors. Of pecuniary compensation there has been none;—on the contrary, some of them have expended sums for which they have received no reimbursement. The Editors cannot be expected to do this:—they cannot be expected to work and pay for their work. The Profession must support the Journal not only with their subscriptions, but by their pens, or it must fail.

In taking leave of our friend we wish him all health, happiness and prosperity.

EDS.

HEALTH OF THE CITY.

Since our last issue but little has transpired in the medical world to interest either our city or country readers. The winter thus far has been unusually mild, and the changes have been so gradual as to induce but little serious disease. As usual during the winter season our city has been visited by sporadic cases of Scarlatina; in some instances, it proved fatal in a few hours after the attack; again, it was so mild as to attract but little attention and required scarcely any medication. Nevertheless in certain localities it proved to be quite unmanageable, and at one period excited considerable alarm among the timid.

Recently the disease seems in the decline, and now we hear but little said on the subject. A few cases of variola have been reported from time to time during the winter, and we learn that the cases are increasing in different parts of the city. Vaccination, that invaluable discovery, has, however, robbed this disease of more than half its terrors. This preventive is too much neglected by the poor, and we think it the duty of the city authorities to look to this matter—to compel the careless and ignorant to submit to vaccination, and thus limit the extension of this loathsome disease. Another disease has been introduced into our city during the fall and winter, which has excited considerable apprehension in the public mind—we allude to the Ship, Typhoid or Typhus Fever, (we give the reader choice of names) which developes itself among the Irish immigrants during their voyage across the Atlantic. Several British ships have recently arrived in the Mississippi river with hundreds of poor immigrants on board in a deplorable condition. Those who escaped (and there were few) the Ship Fever, suffered for want of proper food and good water, and when they reached our Levee they were much reduced and many of them utterly prostrated.

This disease, Ship Fever, is produced by crowding two or three hundred steerage passengers into a small space, where filth and debris of every kind—where the exhalations from these half-starved creatures are allowed to accumulate, thus creating a *focus* of infection, from which the poison extends to all who breathe an atmosphere thus contaminated; hence, the disease often assails even the cabin passengers, as we recently witnessed, although less crowded, better nourished and better provided with all the comforts and conveniences for a sea-voyage.

We regard want of *personal cleanliness* together with a *short* allowance of provisions, and of *bad quality*, two conditions highly favorable for the generation of this disease. Hence removal into a pure atmosphere, cold ablutions, fresh provision, and but little medication will suffice to restore the great majority of these cases to health.

So great has been the influx of immigrants this winter afflicted with Ship Fever, that the Charity Hospital now contains over 1000 sick-about 800 of whom are afflicted with Ship or Typhoid Fever. In some instances the disease has been communicated to the nurses, students, physicians and other attendants; this has not been of very frequent occurrence, and it seems to affect those only, who are rather predisposed to low forms of fever, and who are long and frequently exposed to the exciting causes of the disease. The crowded state of the Hospital, and the consequent deplorable condition of its inmates have at length aroused the attention of both the Legislature now in session, and the city authorities. By the former body a Committee was recently appointed to examine into and report upon the condition of the sick at the Hospital. The report was made to that body, in which it was advised to establish a branch of the Hospital in the suburbs of the city, where the sick immigrant, in reaching our shores, should be received and treated.

Acting npon this suggestion, his Honor A. D. Crossman, Mayor, recommended the Councils of the three Municipalities, and the Board of Health to appoint Committees, who should confer together upon the subject, and devise some measures for the disposal and accommodation out of the limits of the city of those arriving here afflicted with Ship Fever or other infectious diseases. The result of this conference is not yet known.

We think the remedy for all these evils very simple. Let the city authorities lease or construct cheap and commodious buildings in the lower part of the third Municipality, nearly opposite Slaughter-house-point; compel all vessels with sick immigrants on board to anchor off this point, transport the sick to this temporary Hospital, and detain the vessel, until she shall be cleansed, fumigated with some disinfectant, and otherwise purified to the satisfaction of the boarding officer. This building could at the same time receive many from the Charity Hospital, and thus diminish materially the crowded state of that great institution. Charity to those already in the Hospital as well as to those, who may be doomed to apply there for relief, demands of the proper authorities some such plan to ameliorate this condition. The mode already suggested will be an important step in this matter, and if this or something similar is adopted, no fear need be entertained that the Ship Fever will spread through the city and become an epidemic. Remove these *foci* of infection, created by crowding a large number of sick together, beyond the thoroughfares of the city, and our citizens will remain exempt from fever.

The disease, as found in our Hospitals, is not malignant or unmanageable; in many cases cleanliness, pure air, an appropriate diet, with general attention to the hygienic condition of the patients, are all that is required to establish convalescence.

Since the memorable epidemic of 1847 our city authorities and citizens generally, with a fractional part of the profession, have advocated the necessity of adopting quarantine regulations for the city of New Orleans. With this view several bills have been presented to the Legislature, asking for authority to enforce quarantine.

We can only say that these propositions have already received the courteous attention of our representatives, and we doubt not that many will advocate and vote for the measure.

Aware of the wishes of our citizens, and anxious to do every thing to promote public health—a Committee from the Board of Health recently petitioned the Legislature to grant power to that body to establish quarantine regulations, whenever such should be deemed necessary for the public security by the Board. What will be the final action of our Legislature on this important subject it is impossible to predict.

Although somewhat sceptical in this subject, yet we are willing to have the quarantine system enforced until experience shall decide for or against it. It will have more than one good effect—viz. it will inspire the public mind with confidence in the sanitary condition of the city—encourage families to remain here the greater part of the year—deter captains of ships from bringing vessels into our ports, infected with disease and loaded with pestilence and death,—there are a few of the beneficial effects that will flow from a proper and rigidly enforced quarantine.

HEALTH OF THE COUNTRY.

MONTGOMERY, ALA., February 15th, 1848.

GENTLEMEN :—I send you a list of the cases,—(from the same case-books,)—which have occurred between the 10th of December, and the 9th of February inclusive.

Abortion, 3; Amaurosis, 1; Anus, Fistula of 2; do. Fissure of 2; Abscess, 4; Asphyxia, 1; Bronchitis, acute, 8; do. chronic, 1; Constipation, 3; Carcinoma (of the liver,) 1; Cholera morbus, 24; Colic, 5; Catarrh, 1; Caries, 1; Diarrhœa, (acute) 7; do. (chronic) 2; Dysentery, 2; Dyspepsia, 4; Delirium Tremens, 2; Enteritis, (acute) 1; Erysipelas, 1; Fever, (Intermittent) 33; do. (Remittent) 21; do. (Irritative) 1; do. (Catarrhal) 6; do. (Typhoid) 1; do. (Ephemeral) 1; Fracture (of astragalus) 1; do. (of forearm) 1; Gastritis, 1; Ganglion, 1; Gonorrhœa, 3; Gleet, 1; Hernia, 3; Hemorrhoids, 2; Hemiphlegia, 1; Hysteria, 2; Jaundice, 4; Laryngitis, (acute) 1; Leucorrhœa, 3; Menorrhagia, 2; Neuralgia, 4; Opthalmia, 1; Pneumonia, (acute) 16; Parotitis, 3; Poisoning (with Fr. opii. suicide,) 1; Pericarditis, 1; Parturition, 1; Paronychea, 1; Phymosis, 1; Rheumatism, (acute) 2; Rubeola, 16; Splenitis, 1; Symblepharon, 1; Syphilis, (primary) 2; do. (secondary) 2; Spinal Irritation, 2; Seminal weakness, 1; Trismus Nascent. 1; Tumour, (fatty) 1; Tonsilitis, 3; Ulcer, 1; Varicella, 1; Wounds, (incised) 5; do. (contused) 2; do. (Punctured) 2; do. (Lacerated) 1. Making in all 240 cases,—of which 11 proved fatal, viz:—five of acute Pneumonia, one of Pericarditis—one of Trismus Nascent.—one of chronic Diarrhœa,—one of Typhoid Fever,—one of Poisoning with F. opii.—and one asphyxiated in birth.

The cases of Cholera Morbus on the list—all occurred in the space of a few days following a sudden change in the weather, from warm to cold.

Very respectfully,
W. M. B.

MEMPHIS, February 23th, 1848.

GENTLEMEN :—Below you will find the report of my cases from the tenth of December to the fifteenth of February.

From cases, which it is unnecessary to state my reports were not made for the two previous numbers of the Journal.

Abscess (Lumbar) 1, Asthma 1, Ascitis 1, Abortion 2, Amaurosis 2, Bronchitis (acute) 4, do. (chronic) 1, Burn 1, Cerebro-spinal Meningitis 3, Convulsions (Infantile) 2, Colica-peritonum 1, Colic 11, Cancer (of the face) 1, Cynanche Tonsillaris 17, Dysentery 3, Dentition 3, Diabetes 1, Diarrhœa 8, Dysmenorrhea 1, Dislocation (shoulder) 1, Dyspepsia 4, Erysipilas 2, Empyema 1, Enteritis (with intussusception) 1, Fever (Catarrhal) 5, (Intermittent) 5, (Remittent 32, (Typhoid) 26, Gonorrhœa 3, Gastro-Enteritis 2, Fracture (Femoris) 1, (Tibia and Fibula) 1, (Ulna) 1, (Clavicle) 1, Hemoptysis 1, Hernia (inguinal) 1, Hysteria 1, Hemorrhoids 3, Jaundice 2, Leucorrhea 3, Menorrhagia 7, Metritis 5, Opthalmia 3, Pleurisy 2, Pleurodynia 1, Pneumonia 4, Pertusis 2, Phthisis 2, Pruritis Pudenda 1, Phlegmasia Dolens 1, Parturition 10, Prolapsus Uteri 3, Parotitis 16, Psora 3, Orchitis 9, Rubeola 15, Syphilis 4, Stricture (Urethea) 1, Spinal irritation 2, Tabes-mesenterica 1, Urticaria 1, Wounds (incised) 2, (lacerated) 2, (contused) 2.

Making 143 cases. Out of these cases 6 deaths occured—2 of Cerebro-spinal meningitis—1 of Cancer—1 of Dysentery—1 of Enteritis accompanied with obstruction of the bowels, and one of Typhoid fever, upon which Pneumonia supervened, the third week of the disease.

The three cases of Cerebro-spinal meningitis were well marked. They all occured in negroes recently brought here by traders. The two cases that proved fatal occured in a camp near town in a very cold spell of weather in December. On account of their exposure to the cold air, a satisfactory course of treatment could not be carried out.

In one of the cases there was blindness on the second evening after the attack.

The next morning the blindness was partially relieved; but was followed by deafness. Death occured that night. She was 14 years old. The other was 24 years old.

The boy that recovered was 12 years old. He was in a comfortable house and had every advantage of good nursing.

The first thirty hours he had convulsions, gradually subsiding into slight spasmodic action of the muscles—total absence of consciousness, with a cool surface, and a depressed state of the muscular system.

By the use of powerful external stimulants toward the close of this period more decided reaction occured, when 16 to 20 ounces of blood was abstracted, after which the calomel, he had taken in large-doses, produced dark mercurial evacuations—these were followed by evidences of partial consciousness; when he was found to be blind. The blindness gradually went off in two or three days—during which time, decided mercurial action was produced, and convalescence established.

Though a few well marked cases of Typhoid fever have occurred here every year, for the past five or six, it has never been so prevalent as during the present winter.

A few cases have presented themselves in Irish emigrants, who arrived here sick, or sickened immediately after their arrival. The symptoms and progress of the disease in them have been strikingly similar to the cases that have occurred among our citizens. The cases this winter have been less protracted generally than they were in previous years when fever occurred.

The case of Empyema resulted from badly treated pleurisy. Paracentesis was performed about three weeks after the attack, and within the first four days near a gallon of yellow pus was discharged. Some discharge of pus still continues from the external opening, and a small quantity is expectorated.

It is now about four weeks since the operation. The general health of the patient is very much improved. The upper portion of the chest is sonorous. The respiratory movement partially reestablished.

Very respectfully,

L. S.

HOSPITAL REPORTS.

CHARITY HOSPITAL.

JANUARY, 1848.

Admitted :	Males,	936,	1182.
"	Females,	246,	
Discharged :	Males,	811,	992.
"	Females,	181,	
Died :	Males,	129,	160.
"	Females,	31,	

Number of patients remaining in the Charity Hospital (Main Building) on the 1st of February. - 862.

In the Lunatic Asylum. - - - - - 125.

Total. - - - - - - - - 987.

FEBRUARY.

Admitted :	1046.
Discharged :	1071.
Died :	134.

LUNATIC ASSYLUM.

Admitted :	61.
Discharged :	56.
Died :	8.

Number of patients in the Charity Hospital Main Building, 1st of March. - - - - - - - 688.

Lunatic Asylum. - - - - - - - 121.

Total. - - - - - - - - - 809.

List of interments in the City of New Orleans from the 18th December, 1847, to the 12th February, 1848, being eight weeks or two months.

Abortion, 1 ; Accidental, 6 ; Adynamia, 1 ; Anasarca, 1 ; Apoplexy, 11 ; Ascites, 3 ; Asphyxia, 1 ; Bowels, chronic inflam. of 1 ; Bowels, inflam. of 8 ; Bowels, ulceration of 4 ; Brain, dropsy of 2 ; do. congestion of 16 ; do. inflam. of 4 ; do. softening of 2 ; Bronchitis, 6 ; do. chronic, 1 ; Burn, 5 ; Catarrh, 12 ; do. chronic, 1 ; do. pulmonary, 2 ; Cerebritis, 8 ; Consumption, 110 ; Convulsions, 18 ; Cramp, 3 ; Croup, 12 ; Debility, 31 ; Del. Tremens, 11 ; Dentition, 7 ; Diarrhœa, 12 ; do. chronic, 14 ; Disease (chronic) 1 ; Dropsy, 16 ; Drowned, 2 ; Dysentery, 36 ; do. chronic, 25 ; Eclampsia, 1 ; Encephalitis, 1 ; Enteritis, 4 ; do. chronic, 1 ; Epilepsy, 2 ; Erysipelas, 2 ; Fever, 5 ; do.

Bilious, 1; do. Congestive, 2; do. Malignant, 2; do. Pernicious, 1; do. Putrid, 1; do. Putrid Malig. 1; do. Remit. 1; do. Nervous, 1; do. Scarlet, 13; do. Typhoid, 46; do. Typhus, 95; do. Yellow, 1; Gangrene, 1; Gastro-enteritis, 12; do. chronic, 1; Head, injury of 1; Heart, disease of 3; do. hypertrophy of 7; Hemorrhage, 3; Hepatitis, 6; do. chronic, 2: Hydrocephalus, 1; do. chronic, 1; Hydro-thorax, 1; Hysteria, 1; Indigestion, 1; Influenza, 1; Intemperance, 5; Laryngitis, 1; Liver, abscess of 2; do. disease of 1; do. inflam. of 1; Lungs, abscess in 1; do. congestion of 1; do. inflam. of 5; Marasmus, 2; Measles, 1; Meningitis, 2; Metro-peritonitis, 1; Myelitis, chronic, 1; Old age, 6; Paralysis, 3; Parturition, 1; Pericarditis, 1; Peritonitis, 4; do. chronic, 1; Pertussis, 4; Phrenitis, 1; Pleuritis, 3; Pleuro-Pneumonia, 4; Pneumonia, 27; do. chronic, 1; do. Typhoides, 9; Rheumatism, 6; Scrofula, 2; Scurvy, 1; Skull, fracture of 3; Small-pox, 4; Still Born, 38; Stomach, cancer of 1; Tabes Mesenterica, 1; Tetanus, 15; Trismus Nascentium, 6; Uncertain, 83; Verminose affection, 2; Wound, gun-shot 1. Total 869.

Of these 232 were under ten years of age; 677 were white, and 192 were colored.

(Extracted from the reports of the Board of Health.)

A. HESTER, SECRETARY.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1848.

BY D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain.
1848.	Max.	Min.	Range.	Max.	Min.	Range.				*Inches.*
Jan. - 1	71.0	38.0	33.0	30.34	30.11	0.23	S.E.	3	2	2.881
" - 8	73.0	42.0	31.0	30.36	29.90	0.46	S.E.	2¾	0	0.000
" - 15	73.5	34.5	39.0	30.48	30.14	0.34	E.	3	0	0.000
" - 22	67.0	49.5	17.5	30.24	30.01	0.23	N.E.	2¾	0	0.000
" - 29	66.5	51.5	15.0	30.22	30.02	0.20	N.E.	3½	4	12.625
Feb. - 5	72.0	45.0	27.0	30.19	29.84	0.35	S.E.	3¼	4	4.500
" - 12	72.5	40.0	32.5	30.30	30.20	0.10	N.W.	3	0	0.000
" - 19	76.5	52.0	24.5	30.35	29.94	0.41	S.	3	0	0.000
" - 26	79.0	48.5	30.5	30.30	29.87	0.43	N.W.	3½	5	7.390
March - 4	78.7	46.0	32.7	30.42	29.98	0.44	N.W.	3	3	2.015

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

Last Notice to Delinquent Subscribers.

With this number we discontinue our Journal to all those Subscribers who may be in arrears for the past or preceding volumes.

We have repeatedly and respectfully urged our claims without success, and consider it but due to those who have generously supported the work by promptly remitting their subscriptions, as well as to ourselves that we should no longer supply the Journal to those who are not disposed to contribute to its support.

In order that no mistake may arise from misapprehension, we enclose in this (the last number of the present year) a bill against such as are indebted to us, and hope they will respond freely. To all who do so, we shall gladly renew our salutations upon the issue of the first number of the forthcoming volume.

All Remittances to be addressed to

S. WOODALL,

Publisher of the New Orleans Medical and Surgical Journal,

49 Camp Street, New Orleans.

JOSEPH COHN, PRINTER, 31 POYDRAS STREET.

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE

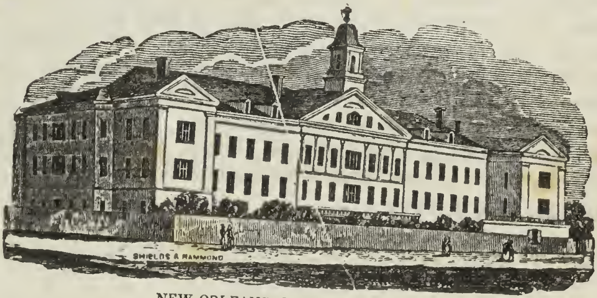
AND

THE COLLATERAL SCIENCES.

EDITED BY

J. HARRISON, M. D.
W. M. CARPENTER, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

MAY, 1848.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1848.

LIST OF AUTHORIZED AGENTS.

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NOTICE TO SUBSCRIBERS.

☞ The above constitute at present the only authorized Agents for this Journal. All monies or remittances may be paid or sent to either of the above as may be preferred—and to them only. We mention this fact for the benefit of our Subscribers many of whom have sent monies to other persons not authorized to receive the same and in many cases the amounts have failed to reach their destination. We therefore repeat that we will consider no payment as valid, should it fail to reach us, if addressed to any other persons than those mentioned above.

New Orleans, May 1, 1847.

TO READERS AND CORRESPONDENTS.

Communications have been received from Drs. Love, C. S. Fenner, Wm. A. Booth and Lewis Shanks.

NOTE.—These articles have been left out of our last number, because received too late. We request our Correspondents to send in their articles before the beginning of the months in which the Journal appears.)—EDS.

Our usual exchanges have been received.

We have received from the publishers the following:

" SOLLY, *on the Brain.*"

" WHITEHEAD, *on Abortion and Sterility.*"

" STILLE's *Pathology.*"

" MATTEUCI's *Lectures on Living Beings.*"

BOWERS' *Memoranda of Surgery and Anatomy;—The Young Stethocopist, or the Students aid to Auscultation.* By HENRY BOWDITCH, M. D.; LATHAM, *on Auscultation and Semiology.*

A Lecture by Professor HARD, *on Atresia Vaginæ;* DUNGLISON, *on the Practice of Medicine*, 2 *vol.;* MULLER's *Physics and Meteorology.*

TO SUBSCRIBERS.

This number completes the Fourth volume of the *New Orleans Medical and Surgical Journal.* For four years we have labored to establish the work, and although many have been the difficulties and obstacles that opposed our progress, yet, thanks to the kind friends and patrons of the enterprize, we think we have finally succeeded. We again request the friends of and subscribers to, the Journal, to send in their communications and subscriptions.

We shall enter upon the new year with fresh zeal and a firm determination to do all we can to advance the interest of the profession.

EDRS.

LIST OF RECEIPTS

ON ACCOUNT OF SUBSCRIPTIONS, SINCE THE PUBLICATION OF THE LAST NUMBER.

Dr. Arnett, G. W., to July 1849 $5
" Atkinson, W. H., Jan'y 1848 to 1849 5
" Arnold, H. H. " 1848 to 1849 5
" Burt, W. D., Jan'y 1848 to 1849 5
" Bates, F. A., to July 1849 5
" Beck, J. F. " 1848 to 1849 5
" Burns, G. H. " 1848 to 1849 5
" Booth, March 1848 to 1849 5
" Barnett, W. B. vol. 3 5
" Battle, F. W. vols. 3 and 4 10
" Calderwood, Jno. vol. 5 5
" Calhoun, D. P. " 4 5
" Colgin, G. S. " 4 5
" Childers, E. R., Jan'y 1848 to 1849 5
" Campbell, S. D. " 1848 to 1849 5
" Cross, Richard " 1847 to 1848 5
" Crumpton, U. J. vol. 4 5
" Catching, J. B. March 1848 to 1849 5
" Cottman, May 1848 to 1849 5
" Day, R. H. Jan'y 1847 to 1848 5
" Dabbs, C. H. " 1848 to 1849 5
" Ellis, T. O. March 1848 to 1849 5
" Fenner, C. S. " 1848 to 1849 5
" French, Wm. Sept. 1846 to 1848 10
" Green, A. L. vol. 5 5
" George, W. W. Jan'y 1847 to 1848 5
" Hall, R. B. " 1848 to 1849 5
" Henrison, M. P. " 1848 to 1849 5
" Hornsby, J. vol. 4 5
" Hubert, R. W. " 4 5
" Howard, J. G. " 4 5
" Holmes, R. R. " 4 5
" Jones, E. P., Jan'y 1848 to 1849 5

Dr. Krouse, T. J. vol. 4 $5
" Leslie, N. K. " 4 5
" Larche, N. E. " 4 5
" Lipscomb & Love, " 4 5
" Liddell, J. R. " 4 5
" Lewis, J. E., Jan'y 1848 to 1849 5
" Lyne, W. H. " 1848 to 1849 5
" Lewis, Dan. " 1848 to 1849 5
" Matthews, Alex., Nov. 1848 to 1849 5
" Myles, Wm. March 1848 to 1849 5
Mitchell, Wm. Esq, Jan. 1848 to 1849 5
Dr. McKee & Smith, March 1848 to 1849 5
" Moore, Thos. E., Sept. 1847 to 1848 5
" Murph, Jas. M. vol. 4 5
" Malory, " 4 5
" Massey, E. J. " 4 5
" Oliver, T. P., Jan'y 1848 to 1849 5
" Pye, E. A. " 1848 to 1849 5
" Quinn, D. H. " 1848 to 1849 5
" Ryan, Hillary, " 1848 to 1849 5
" Rainey, W. W., to July 1849 5
" Rowland, J. A., March 1848 to 1849 5
" Stone, C. H. vol. 4 5
" Sutton, David " 4 5
" Shanks & Frasier, " 3 5
" Sanders, J., March 1848 to 5
1849 5
" Terrell, Joel., " 1847 to 1848 5
" Thomas, N. L. vol. 4 5
" Wilson, A. L. " 4 5
" Washington, W. B. " 4 5
" Wilson, H. R. " 4 5
" Williams, J. A. " 5 5

CONTENTS

OF

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

Vol. IV. No. VI. —— FOR MAY, 1848.

PART FIRST.

ORIGINAL COMMUNICATIONS.

PAGE

Art. I.—A History of the Epidemic which prevailed in Vicksburg during the Fall of 1847. By A. L. C. Magruder, M. D. - - - - 689

Art. II.—Case of Urinary Fistula, reported by F. M. Fitzhugh, M. D., of Madison County, Miss. - . - - - - - - - - 694

Art. III.—Operation for Strangulated Hernia. By Alex. Ewing, M. D., Dexter, Michigan. - - - - - - - - - 695

PART SECOND.

REVIEWS AND NOTICES OF NEW WORKS.

Art. I.—A Treatise on the Practice of Medicine. By George B. Wood, M. D., Professor of Materia Medica and Pharmacy in the University of Pennsylvania; one of the Physicians to the Pennsylvania Hospital; one of the authors of the United States Dispensary, &c. &c. &c. - - - - - - - - - - - - - - 698

Art. II.—On Disorders of the Cerebral Circulation; and on the connection between Affections of the Brain and Diseases of the Heart. By George Burrows, M. D., Late Fellow of Coin's College, Cambridge; Fellow of the Royal College of Physicians, London; Physician and Lecturer on the Principles and Practice of Medicine, at St. Bartholomew's Hospital. With Colored Plates. Lea & Blanchard. Philadelphia. 1848. - - - - - - - - - - - - 753

Art. III.—Adulterations of various substances used in Medicine and the Arts, with the means of detecting them; intended as a Manual for the Physician, the Apothecary and the Artisan. By Lewis C. Beck, M. D., Professor of Chemistry in Rutger's College, New Jersey, and in the Albany Medical College, &c., &c., New York. Samuel S. and W. Wood. 1846. pp. 332. - - - - - - - - 757

Art. IV.—The History, Diagnosis and Treatment of the Fevers of the United States. By Elisha Bartlett, M. D., Professor of the Theory and Practice of Physic in the Medical Department of Transylvania University, &c. Lea & Blanchard. Philadelphia. 1847. pp. 534. 759

Art. V.—Tracts on Generation, No. 1. Proofs that the Periodic Maturation and Discharge of Ova, are in the Mammalia and the Human Female Independent of Coition, as a first condition of their propagation. By T. L. G. Bischoff, M. D., Professor of Physiology, &c., Giessen. Translated from the German, by Professor Gilman and Tellkampf, of New York. - - - - - - - - 764

PAGE

ART. VI.—On the Theory and Practice of Midwifery. By FLEETWOOD CHURCHILL, M. D., M. R. I. A., Professor of Materia Medica and general Therapeutics; Physician to the Western Lying-in-Hospital; Hon. member of the American National Institute and of the Philadelphia Medical Society. With notes and additions. By ROBERT M. HUSTON, M. D., &c., &c. Third American Edition, Revised and improved by the Author. With one hundred and twenty-eight illustrations. Philadelphia. Lea & Blanchard. 1848. pp. 525. - - - - - 766

PART THIRD.

EXCERPTA.

ART. I.—The Cholera. - - - - - - - - - - 768

PART FOURTH.

MEDICAL INTELLIGENCE.

FOREIGN.

ART. I.—Case in which a Foreign Body became lodged in the Trachea—value of the Stethoscopic Diagnosis—its removal by Operation and Inversion. - - - - - - - - - - - - - - 785

ART. II.—On a Function of the Red Corpuscles of the Blood, and on the Process of Arterialization. By GEORGE OWEN REES, M. D., F. R. S., &c. - - - - - - - - - - - - - 787

ART. III.—On the Urine in Typhoid Fever. By M. MARTIN-SOLON. - 787

ART. IV.—On the Presence of Sugar of Milk in the Milk of the Carnivora. By Dr. A. BENSCH. - - - - - - - - - - 788

ART. V.—New Test for Prussic Acid. By J. LIEBIG. - - - 789

AMERICAN MEDICAL INTELLIGENCE.

ART. I.—Discovery and Application of the New Liquid Adhesive Plaster. [A Communication addressed to John D. Fisher, M. D., of Dedham Mass., and read before the Boston Society for Medical Improvement, March 27, 1848.] - - - - - - - - - - - - 790

ART. II.—Chloroform, an Anæsthetic Agent as a Substitute for Sulphuric Ether. - - - - - - - - - - - - - - 794

ART. III.—Communication. - - - - - - - - - 796

EDITORIAL.

Health of the City - - - - - - - - - - - - - 797

An Act to establish a Board of Health in and for the Parish of Orleans 798

Members of the Board of Health - - - - - - - - - 800

University of Louisiana—Medical Department - - - - - 800

Hospital Reports - - - - - - - - - - - - - 801

Charity Hospital - - - - - - - - - - - - - 801

Health of the Country - - - - - - - - - - - 805

Meteorological Table. By D. T. LILLIE - - - - - - - 807

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

MAY, 1848.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*A History of the Epidemic which prevailed in Vicksburg during the Fall of* 1847. By A. L. C. MAGRUDER, M. D.

To the Editors of the New-Orleans Medical and Surgical Journal.

GENTLEMEN: — During the months of September and October last, our city was visited by a disease, with regard to the character and nature of which, a great contrariety of opinion was entertained by the medical faculty.

Some of its members, fully convinced in their own minds (*ab initio*), called the disease *yellow fever*—in which opinion I fully concurred. Others appeared to be very unsettled and fluctuating in their opinions. A most esteemed and worthy physician, on visiting one of the first cases, exclaimed that no medical man, who had ever seen a case of yellow fever, could, for a moment, doubt its identity in the one before him; so potent and unequivocal was the influence addressed to his olfactory nerves; yet to show the Protean forms which the disease must have assumed, or the tergiversations the physician's mind must have undergone, the same gentleman, at a subsequent period of the epidemic, declared emphatically that the disease was *not yellow fever* and that there had not been a case of yellow fever in the city. One of the physicians called it a "mongrel disease;" saying that it possessed all the prominent and characteristic symptoms of yellow fever, and that he would have no hesitation in pronouncing it such, were it not for the fact that many of the cases were attended with a cutaneous efflorescence which induced him to believe that it must be a disease of a mixed character.

Some called it the "*dengué*," and others believing not even the dengué to be a sufficiently fanciful name, or that the terms were or ought to be synonymous, called is the "*devil fever.*" Others discoursed very learnedly, looked wise, but in the whole vocabulary of medical diseases could find no suitable name.

One physician contended that there was no evidence of yellow fever, but *black-vomit*;—and another fully determined, that no such disease *should* prevail, could not be convinced by the presence of the symptom to which so much importance was attached. Others declared it was impossible it *could be* yellow fever, for the reason that *so few died*, in proportion to the number attacked.

I have introduced the above remarks, not so much to condemn or criticise the different views advanced, as by giving them, to present the whole subject in its true light and thereby elicit useful information from those whose position and locality have enabled them to investigate the disease in every form and variety, from the mildest to the most malignant type. In the first place, I assume the position, that the disease, (call it by whatever name you may) was of *endemic origin;* in confirmation of which I will state the facts in the case. So soon as the board of health declared yellow fever to be epidemic in New Orleans, the city council of Vicksburg, in order to prevent the importation of the disease, established *a quarantine*, which went into operation forthwith, and was rigidly enforced, from that time until long after the appearance of the disease as an epidemic; although a the time of the establishment of the *cordon sanitaire*, the city was unusually healthy. The first well marked case that occurred, was a man who had been employed as a labourer in digging and grading the streets in a part of the city remote from any imported influence, and upon close investigation, it was ascertained that he had had no communication with the river or boats whatever. He was removed to the Hospital, where he died:—a post-mortem examination was made by several of the most learned and experienced medical gentlemen of the place, at which I was invited to be present. So indubitable were the evidences presented, that not a doubt was expressed by any gentleman who witnessed it, and the merest tyro in medicine, it seems to me, ought to have been ridiculed who would have given it any other name than *yellow fever*—the stomach and abdomen were completely filled with black-vomit and every mark of violent gastro-enteric inflammation—the tunica albuginea and skin were very yellow, and the patient previous to his death had ejected large quantities of a grumous, coffee-ground looking substance. Simultaneously with the case above, some eight or ten persons were attacked on the same square, directly West of the market-house, in the upper part of the city and far distant from the quarantine ground and steamboat landing. This square was composed almost entirely of old wooden buildings in a decayed and dilapidated condition, and the putrid effluvia, which was perceptible in every part of it, was almost intolerable—besides which, a great deal of grading and filling up had been going on for several weeks on all the streets around this square, which presented an immense deposit of fresh earth, from which a constant, most unwholesome exhalation must have issued, produced by the intense heat which prevailed at the time. The cases alluded to, assumed the most malignant and unmanageable form, placing medical effort and skill at defiance, and proving mortal almost without an exception. It may perhaps be said that these cases may have contracted the disease from the boats, which however could not have been the case, as there had been no such communication, and several of them were females, who had not been absent from their habitations for weeks, and moreover, to this period, there had not a case originated in the lower part of the city, adjacent to the quarantine ground, although several acknowledged cases of yellow fever had been received there, from the boats, and provided for by the city authorities. At this stage of the epidemic, from the mortality attending the first cases, the citizens, taking alarm, fled in every direction to the country; there was also a sudden change in the weather, the wind blowing fresh from the North; it became very cold, which proved disastrous to those who were sick, but beneficial in modifying and changing the subsequent character and type of the disease, presenting for the future a mild and manageable form, still however retaining all the original, leading and prominent symptoms. I will now proceed, in as brief and succinct a manner as possible, to give you a history of the leading features, and also of the treatment adopted.

The disease was ushered in, without the least premonitory symptom in many cases, by a cold shivering sensation, first passing down the spinal region, thence radiating to every part of the body. This chilly sensation, (I may say indescribable feeling of cold,) lasted from one or two, to seven or eight hours, according to the violence of the attack, and was followed by the most intense reaction;—hot skin, bounding pulse, excruciating pain in the occipital and whole spinal region, the eyes red, pain in the eyeballs, the vessels of the conjunctivæ very much enlarged and swollen, cramp in the gastrocnemii muscles with an utter inability to assume any attitude or position of the body from which one moment of ease could be obtained. There was but one paroxysm of fever followed by the sweating stage, when the pulse sank to about a natural standard, and great prostration ensued in every case, whether the paroxysm was long or short;—hurried respiration—tenderness of the epigastric region—excessive nausea and gastric irritability—the tongue, which at first was nearly natural or white, soon became covered with a thick white, viscid, tenacious fur, which it was almost impossible to expel from the fauces. Beneath this fur the tongue was found to be as red almost as scarlet, which gradually assumed a natural colour if the case resulted favourably; if otherwise, it not only continued very red, but became dry and fissured, with a dark sordes adhering to the teeth. The bowels were usually found costive—some of the cases complained of great pain in the right hypochondriac region. In the worst cases, as the disease progressed, the gastric irritability increased, about the third or fourth day the skin and tunica albuginea became very yellow, and as the disease went on to a fatal result, which in most cases occurred from the fifth to the ninth day, (though in some instances protracted to a much later period), the body would be found covered with petechiæ resembling a flea-bite, with hemorrhage from the gums and bowels and the sudden and forcible ejection of a coffee-ground looking substance from the stomach. In the mild and modified form of the disease, in place of the petechiæ, the skin was covered with a rash similar to prickly heat, and in some instances so much like rubeola as with difficulty to be distinguished therefrom. So soon as this eruption made its appearance on the skin, a speedy convalescence ensued. This was the symptom which confused some of the medical gentlemen and led them to believe that it could not be *genuine, unmixed yellow fever*—yet I find in the epidemics which have prevailed in different parts of the world, as laid down by many very learned authorities, such as Mosely, Warren, Blane, Chapman and others, that this very symptom, when corroborated by other circumstances, was a certain diagnostic sign of yellow fever.

With regard to the treatment in the first cases that appeared, almost every variety of practice was adopted with equally poor success, for the result was the same, whether an heroic or a mild course was pursued, or if the patient was left to the unassisted efforts of nature; some were treated with the hot mustard bath—and venesection and mild purgative enemata—others with local blood-letting by cups from the spine and epigastric region, and where the gastric irritability was so great as to render medicine inadmissible by the mouth, they were introduced into the system by the endermic method. Some were treated by sedative doses of the sulphate of quinine—others were brought under a moderate influence of mercury as speedily as possible, and others were treated with warm baths, warm teas, &c., without any medicine. The result was the same, manage the case as you might; for, of the cases, that occurred during the first week of the appearance of the epidemic, they all died.

Subsequently the disease presented a very mild and manageable type—the course which I adopted, and which I found universally successful, was the following: in the cold stage, as soon as called, I had the patient immersed in a mustard bath as hot as the patient could bear it and allowed to remain for fifteen or twenty minutes—where the bath could not be obtained, which was sometimes the case, the hot pediluvium was substituted—the patient was then rubbed perfectly dry, and wrapped up warm in bed. Where reaction ensued and the violent pain in the head and back came on with great gastric distress, I applied cups over the epigastric region and down the whole spine, extracting a few ounces of blood, which appeared to act almost like magic. Where the symptoms were not so urgent, I found a mustard cataplasm extending from the nape of the neck to the sacrum to answer a very good purpose.

The liver being a little torpid and the bowels inactive in most instances—I gave the following prescription:

℞. Hydrarg. chloridi Mitis, gr. v.
Pilulae Hydrarg. gr. x. M.
Make three pills—
S.—Take at one dose.

If no operation was produced by this dose in the course of six or eight hours, I ordered the following enema:

℞. Mucilag. Acac. Gum. Oj.
Olei Ricini, f ℥j.
Olei Terebinthinæ, f ℥ss. M. S.

Administer warm—and repeat if necessary in the course of an hour or two until two or three evacuations are obtained.

If the pills alone acted too much, and seemed disposed to run off by watery operations from the bowels, which sometimes was the case, owing to the excessive gastro-enteric irritability—to obviate this difficulty I gave the following prescription which answered well and quieted the uneasiness—

℞. Tinct. Opii camphoratæ—q. s.
S.—Give a teaspoonful and repeat the dose in an hour, if necessary.

During the fever, I allowed the free use of gum-water or cold lemonade, and where the thirst was great, small pieces of ice to be held and dissolved slowly in the mouth, which allayed the thirst and relieved the nausea and heat of the stomach. As soon as the fever subsided, in cases that would bear it, I found the cure expedited by the following prescription.

℞. Quinine Sulphatis, gr. xij.
Pulv. Acac. Gum., q. s.—M.
Make six pills.
S.—Give a pill every two hours.

As convalescence advanced, I allowed as a diet the free use of arrow root well cooked—and as a tonic, good London porter, or the following prescription:

℞. Tinct. Gentianæ compos., q. s.
S.—Take a teaspoonful three or four times a day.

In some instances, quinine, tonics and stimulants of any kind proved entirely inadmissible. Such cases I managed by using great prudence and particularly in diet, mucilaginous drinks, and allowed the *vis medicatrix naturæ* to accomplish the cure—which, although a little tedious in some cases, was nevertheless the only safe course to be adopted. I regret that it is out of my power to furnish the thermometrical and barometrical condition of the atmosphere which prevailed previous to the appearance of the epidemic, as I have no correct data convenient from which to derive the information—suffice it to say, that for several weeks anterior to its commencement, the weather was extremely hot, quiet and sultry. Had the disease commenced two weeks sooner, and the fortunate occurrence of the cold, together with the sudden dispersion of the inhabitants to the country, been postponed two weeks later, no doubt that in proportion to the population, the disease would have prevailed in this city with equal malignity with the epidemic which ravaged New-Orleans.

I feel fully justified in my own mind from irresistible facts, not only to draw the conclusion that the disease *was yellow fever*, but that it originated in Vicksburg without foreign aid—produced by the peculiar condition of the atmosphere which existed at the time, combined with local causes, and that by the timely and early removal of those causes the city might perhaps have escaped the epidemic entirely. It is not to be wondered at, that such a difference of opinion should have prevailed with regard to the disease. Though not present at the time, I have been credibly informed that during the epidemic of 1841, which prevailed with such malignity, there were physicians of eminent standing in this city, who either from preconceived prejudices or hastily expressed opinions—being unwilling to retire from their original position—maintained to the very last moment, in defiance of facts and their own senses, when there were seven or eight deaths a day with black-vomit, that the disease was *not* yellow fever, and only a high grade of bilious remittent fever.

In every community, there will always be found men, who, willing to cater to popular will, can advocate any notion, or ridicule any idea which conflicts with their own wishes; and others who, regardless of human life, would " sell their birth-right for a mess of pottage" or oppose any thing, however correct, which came in contact with selfish or pecuniary considerations. In the views which I have advanced I do not wish to be understood as conveying the impression, that yellow fever may not be conveyed from one port to another—facts would not sustain me in such a position.

When the atmosphere is in a condition for the reception of the disease, the introduction of infection at such a time will act as the explosive principle to ignite the combustible material. On the other hand a few cases of yellow fever introduced into a pure and healthful atmosphere will have no effect to spread the disease;—again, let ths atmospheric condition and local causes be of the proper kind to generate the disease, and quarantine regulations will be found wholly inefficient. But, by attention to cleanliness, the removal of all local causes, and the rigid enforcement of a quarantine, epidemics may sometimes be averted, which, without the use of such precautions, would prevail.

II.—*Case of Urinary Fistula, reported by* F. M. Fitzhugh, M. D., of Madison County, Miss.

Messrs. Editors:—If you consider the following case worthy an insertion in your Journal, it is at your disposal.

Negro man Isom, aged 60 years; tight stricture of the urethra, with five fistulous openings, one of which, was anterior to the scrotum. The fœtid pus and urine, which passed constantly the openings—his great emaciation and haggard appearance, made him an object truly loathsome. Not more than a teaspoonful of urine passed the natural way. The fistula anterior to the scrotum, he informed me, was produced by lancing an abscess, thrusting the instrument in too deeply, by which an opening was made into the urethra.

When he urinated, which was very often, the pain produced was most excruciating, shaking as one in the paroxysm of an ague. The caustic bougie had been used to relieve the stricture, but as might have been anticipated, only aggravated the case as the fistulous openings in the perineum made their appearance after its use. A catheter had never been introduced into his bladder, and in this miserable condition he was abandoned.

I informed his owner that I was of the opinion the only chance would be to introduce a sound down to the stricture, cut in upon it, in the perineum, and pass the catheter into the bladder; as recommended by Dr. Jameson of Baltimore, (Am. Med. Recorder, 1824.)

I came to this conclusion, believing that the talented Physician who had had him under treatment for sometime had exhausted every other means.

Placing my patient as in the operation for Lithotomy, having a two fold intention in view,—that if I could not pass the small sized silver catheter recommended by Liston, to adopt Prof. Jameson's plan.

After oiling and warming the catheter I proceeded to introduce it—with some considerable difficulty it passed to the fistulous opening in front of the scrotum, at which place it met with much resistance ; by slightly withdrawing the instrument and then rotating it onward, this difficulty was overcome ; I now introduced the fore finger of the left hand into the rectum, felt the end of the catheter, and also that the prostrate gland was much enlarged ; by placing my finger on the end of the instrument, raising it over the enlarged gland and pressing it gently onward, had the good fortune to pass it into the bladder. The time consumed was about 40 minutes. The silver catheter was retained three days, when it was withdrawn, and a larger sized gum-elastic one was introduced, which passed with ease. Several of the latter kind of catheters were used in the case; they were frequently retained from 6 to 8 days, and I am confident the retention for that length of time produced no bad consequences, (the caution of authors upon the subject to the contrary notwithstanding) but greatly expedited the healing of the fistulous openings by preventing the escape of urine through them. The fistula in the perineum healed in about six weeks ; the one in front of the scrotum I had to use the actual cautery three or four times ere it closed, which remained open three months. His health now improved rapidly and in 3 or 4 weeks was quite strong and hearty, experiences no difficulty whatever in urinating, a pleasure he says he has not enjoyed in 20 years before. I was informed by his owner a short time since that his health was excellent and that he had been an efficient hand during the past year.

III.—*Operation for Strangulated Hernia.* By Alex. Ewing, M. D., Dexter, Michigan.

To the Editors of the New-Orleans Medical and Surgical Journal.

Gentlemen :—A young Surgeon of great promise residing at Dexter, Washtenaw Co., Michigan, has just forwarded to me the following account of his operation for Strangulated Hernia—it is written in the familiar style of epistolary correspondence and I transcribe it verbatim, &c.

I would respectfully submit it to your consideration, and should you deem it worthy, I would present it to the medical profession through the columns of your valuable Journal.

Very Respectfully, &c.

A. W. SEARS.

On the 4th December instant, I was called in consultation with Drs. Millman and Hollywood, to a patient who was in an adjoining town, ten miles from his residence, on (what he called by the expressive term) a "lark."

Captain P., 57 years of age, of intemperate habits, has worn a truss during 40 years of his life. Three days previous being "on a frolic," his hernia came down—for 55 hours he vainly attempted to return it himself. On the evening of the 3d he sent for Dr. Hollywood. I saw him on the 4th, when his intestine was down to the size of a child's head or more—very hard and sensitive to the touch,—and of a deep red colour. Dr. Millman, our senior, who is an English Surgeon of considerable experience, was of opinion that it was entirely too late to save the patient, and that sudden death was his inevitable doom, with or without the operation—venous congestion was evident to the most casual observer, and had probably existed for many hours. We explained the case to the old Colonel (who by the way is, "game all over")—he was willing to undergo the operation. Being fresh in my readings, they selected me as operator. The patient was placed upon the table and the operation commenced "according to Mott"—keeping in view his maxim that "when an operation is *well* done it is done quickly enough" —making a free opening through the fascia transversalis. We commenced returning the contents of the sack with great caution, examining every portion of the intestine as it passed up through the opening—the stench was intolerable, the gut presenting every appearance of advanced mortification. We then discovered portions which had entirely lost their integrity, and would give way before the slightest touch of the fingers. What was now to be done? We were of course too late in the operation, and the intestine would burst as it passed up opposite the opening—we emptied nearly a pint of the contents of the intestine into a vessel.

I now carried the director once more up through the internal ring and made a very free division of the stricture through the entire ring, and yet there was about one half of the original contents of the sack in the scrotum. I next passed the director into the scrotum and made a free division of all the parts, pressing upon the intestine, over Poupart's ligament. We here found old and firm adhesion along the whole floor of the inguinal canal, caused by previous inflammation and a bad truss. I placed those portions of the intestine which were broken and further advanced in mortification opposite the wound, which was about 4½ inches in length—brought the edges of the wound in contact with about five sutures and adhesion straps—placed the man on his bed and *left him to die*, believing that he would breathe his last within 24 hours.

Dec. 5th. No pain in the wound—jactitation of which he was very much troubled before the operation, had entirely ceased—Pulse 128, small and hard—slightly delirious—fœces oozing out the wound—the patient possesses great determination and says he will be d——d if he will die—'twould be too great a gratification to his relations—they might go to the Devil, he was determined to live long enough to spend the few thousands he had left.

I gave him my most solemn warning upon his dangerous position, and advised him to arrange his spiritual and temporal affairs without delay—by the way a very disagreeable part of a surgeon's duty.

Dec. 6th. Drove up to the house expecting to find my patient dead—"but it was different!"—Pulse 112, more soft and full. As he was in the habit of drinking nearly one quart of whiskey daily, I allowed him a moderate ration, but the old scamp crawled out of bed and helped himself and when I saw him yesterday he was pretty mellow—contents of the intestines still oozing from the wound, with some suppuration—ordered him an injection of soap-suds, which he would not allow them to administer, considering such a performance as undignified and unbecoming a gentleman—but on being close pressed and rather than surrender, he called in an authoratative tone for a *pot*, and to the astonishment of all present, had a passage through the natural channel.

Dec. 10th. My old Captain is doing remarkably well—appetite very good and general health rapidly improving—stools pass off regularly through the natural channel. Extensive strangulating occurred, but the wound is now doing well,—healthy granulation shooting out and I feel quite confident of his recovery.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*A Treatise on the Practice of Medicine.* By GEORGE B. WOOD, M. D., Professor of Materia Medica and Pharmacy in the University of Pennsylvania; one of the Physicians to the Pennsylvania Hospital; one of the authors of the United States Dispensary, &c., &c., &c.

Although the Medical press of the United States is at the present time teeming, as it were, in the most prolific manner,—scarcely a day being permitted to pass without a corresponding announcement of some new work,—a large proportion of the issue can scarcely be considered entirely legitimate, emanating as it does originally from other sources, and presenting itself before us under the guardianship of American sponsors merely, who take it upon themselves to adapt it to the new circumstances under which it is placed; in other words, "to render the practice applicable to the climate of the United States,"—"a safe guide for American practitioners,"—or, "to point out such modifications of treatment, as experience has shown to be best suited to the cure of diseases as they occur in this country." The appearance of a new work on the practice of medicine of native origin, therefore, especially one proceeding from the pen of a gentleman, as a teacher and practitioner so accomplished as Professor Wood, cannot, we are sure, be otherwise than gratifying to the profession. The work is, seemingly, of a somewhat elaborate character, and considering that the two volumes contain upwards of 1600 closely printed pages, it is but reasonable to suppose that every topic of interest, properly pertaining to the practice of medicine, is treated of sufficiently in detail, for a book of immediate practical reference. Prepossessed as we are, from the reputation of the Author, in favor of the work, we feel but little inclined to enter upon a formal analysis, or extended review; believing, that, in taking up and noticing its parts *seriatim*, but little else than a *series* of complimentary remarks would be the probable result. Without any very strict regard to order then, we will make a few selections, giving the preference to such parts as present the Author's style in its happiest vein; to any important and not generally disseminated points of practice, more especially such as are drawn from the Author's "own stores;" and also, should we meet with such, to parts in which we may deem the opinions presented of a questionable or debateable character.

The following we extract from the preface:—

"Having been engaged for nearly thirty years in private and public practice, and, during that time, devoted an almost exclusive attention to the study of diseases and their remedies, he has accumulated facts, and formed opinions, which have long been soliciting expression, with an urgency to which he has at length yielded, though unfeignedly distrustful of their sufficient value." * * * "The Author claims no indulgence on the score of haste. His leisure has for several years been devoted to the preparation of the work, and there was no urgent necessity for giving it prematurely to the world."

Among the sources to which the Author acknowledges his indebtedness, he refers in respectful terms to the late venerable Doctor Joseph Parrish, and to Doctor Chapman, the present Professor of the Theory and Practice of Medicine, in the University of Pennsylvania.

The work is divided into two parts; the first,—"general pathology and therapeutics," occupying a little over one eighth of it; and the second, "special pathology and therapeutics," the remainder. Each part, besides the division into chapters, is also marked off into sections, subsections, and articles.

The first chapter of the first part is on the "constituent forms of disease."—In the prefatory remarks, the Author observes—

"Efforts have been made to reach the elements of disease; but not very successfully; because we have not yet learned the essential nature of the healthy actions, and cannot therefore understand their derangements. But, though we cannot push analysis satisfactorily to the elements, we are able to appreciate to a great extent their less complex combinations, forming the proximate ingredients of those numerous associations of morbid states or actions, usually called diseases. It may be admitted as a self-evident proposition, that all diseases have their seat in the fluids or solids of the body, or in both."

In the first section, (on disease of the fluids) of the first chapter, Doctor Wood, judiciously evincing, we think, a non-adherence exclusively to either the solid or humoral pathology of disease, remarks:

"In relation to the fluids of the body, our pathological knowledge is very deficient. There can be no doubt that all of them occasionally, are very unhealthy in their condition; and there can be as little doubt, that, in this condition, many of them may become the sources of serious disease. But the question is, whether the vitiated state of the fluids is original with them, or whether it areses from some disease of the solids by which they are generated. During the prevalence of the Humoral Pathology, it was customary to ascribe most complaints to a morbid state of the liquids of the body; and when this system was overthrown, medical sentiment turned with equal exclusiveness to the solids. But at present an intermediate opinion is gaining ground; and the truth probably is, that, while the greater number of diseases have their origin in derangements, either of the functions or structure of the organs, others consist essentially in a disordered state of the liquids, though even these usually find expression in complaints of the fixed structures.

As all the fluids, with the exception of the lymph and chyle, are derived from the blood, and as the two former enter directly into the constitution of the latter, and convey into it all the deleterious principles which they may contain, the blood may be considered as the only fluid subject to original morbid changes, or at least the only one to which we are to look as the primary seat of disease requiring our attention."

Among the means by which the blood may become contaminated or morbidly altered in quality, are mentioned the absorption of extraneous poisonous substances, as of miasmatic and contagious effluvia,—the absorption of deleterious agents generated or produced in the body,—as in some instances the long retained fetid contents of the intestines,—the putrid results of the process of mortification,—pus,—the sanies from unhealthy ulcers, bile and urine, or some of their characteristic principles, which in a healthy state of the excertory functions are thrown off; as for instance urea and the coloring principle of the bile. "But it must also be considered as in a morbid state, when any of its ordinary and essential constitutents become greatly redundant or greatly deficient" as, for example, an increase in the quantity of the fibrine and red globules, especially under the use of a rich diet, and a vigorous condition of the digestive organs, with a comparative deficiency of the nutritive process,"—or, a deficiency of these principles, with an excess of serum, under reversed circumstances, or after profuse losses of blood, &c.

The second section is on disease of the solids, and in the first subsection mention is made of disease from mechanical or chemical causes, as from external violence, gravitation, mechanical obstruction to the course of the circulating fluids, to the escape of secreted fluids, the results of the application of the more powerful chemical agents, &c. A line is of course properly drawn between the immediate results of these causes, and the consequent vital derangements. "It is only the immediate effects of such causes that belong to this division. The secondary effects are consequent upon a disturbance of the functions, produced by the injury already received. * * * Thus by external violence, the flesh is lacerated or a bone broken in the living exactly as in the dead body, and the resulting wound or fracture is a mere mechanical effect: but the inflammation and fever which follow the injury, are exclusively vital phenomena, and obedient to vital laws."

The second subsection is on disease from influences upon the vital properties, and contains articles on irritation, inflammation, depression, congestion, fever in a general sense, and peculiar morbid products. These all bear marks, in every line almost, of the most discriminating judgment, and are models of correct composition; not a redundant word appears, and nothing scarcely seems wanting to render each subject treated of, as explicit as its own intrinsic nature, or the present lights of science will admit. We are perplexed in attempting to select a part to present the reader, where all appears of such equal interest. As with every thing else that has been published on the subject since the days of Hunter, the article on *inflammation* owes somewhat of its interest to the researches of this author; and we observe that the valuable monograph of Thompson has also been freely used in its composition.

With the exceptions of the state of pregnancy, and anæmia, the author considers the presence of the buffy coat as always indicative of the existence of inflammation, though not a necessary evidence of it; for, he observes, that, "though very generally presented by the blood of patients laboring under this affection, it is in some instances wanting." We are strongly inclined to the belief, that this is much more frequently the case than authors generally, since the Treatise of Andral on the blood appeared, seem to think; and that many are governed in opinion, as to the non-existence of inflammation by the absence of the buffy coat, to an extent which the facts and researches of this author by no means warrant. We give below a brief extract from Doctor Wood, bearing in favor of the opinion that inflammation may exist under certain circumstances (exclusive of the exceptions already made) without the buffy coat being present on the blood drawn.

"In cases of indirect prostration, consequent on *great intensity* of inflammation, the blood first drawn may be without the buffy coat, though it afterwards becomes sizy, when the pulse and the heat of the surface rise, as they often do in such cases, under the use of the lancet."

It is a well known fact, too, that the buffy coat, or quantity of febrine in the blood, is by no means in strict proportion in all cases, to the degree of inflammation present; a very slight attack of Rheumatism with fever, being productive of a more fibrinous state of the blood, than the most violent and extensive gastro-enteritis perhaps.

We do not think the following observation, given on the authority of Andral, but seemingly sanctioned by Doctor Wood, is entirely reconcileable with the sentence just quoted. It is made in reference to the symptomatic fever resulting from inflammation.

"It may in part also be ascribed to the changed condition of the blood; and it has been observed that the excess of fibrine uniformly appears, when the inflammation is sufficient to produce fever, and disappears when the fever ceases.

Indeed the alteration in the blood would seem to be rather a consequence than a cause of the fever.

Of the two theories in regard to the condition of the capillary vessels in inflammation, Doctor Wood is in favor of that of Van Helmont and Stahl; viewing it as "a direct result of increased vital action of the part affected."

"The nature of the causes from which inflammation proceeds, would appear to indicate its active character. These are almost always such as, if applied in a moderate degree, produce merely a healthy excitement of the part, or an exaltation of its ordinary functions. * * * * * * We see various degrees of excitement in proportion to the amount of the cause applied, from the slightest increase of the healthy function up to inflammation; nor is it possible to say where, in the ascending scale, merely healthy excitement ends in irritation, or this in inflammation."

The objection to the theory of capillary excitement, based upon the retardation or stagnation of blood in a portion of the vessels of an inflamed part, Doctor Wood shows to be susceptible of removal by recent microscopic researches. These, as enumerated, remind us forcibly of the theory of Boerhaave, in regard to viscidity of the blood, and *error loci*, as causes of inflammation; his error consisting in mistaking a result of the primary cause—which however in the progress of the action becomes a cause of some of the attendant circumstances—for the primary cause itself.

"The rapid development of the white fibrinous or lymphy globules in the vessels of the inflamed part, their slow movement and partial adhesion to the inner surface of the vessels and to one another, and their accumulation at last in such quantities as to arrest the movements of the red corpuscles, fully account for the stagnation alluded to."

We give Dr. Wood's definition of the term "Depression" as distinguishing it from *debility* and *diminished excitability*.

"It must be borne in mind that action, the power to act, and the susceptibility to the influence of excitant agents, which is here denominated excitability, are different conditions or qualities of the system, and may each be reduced without a necessary reduction of the others. A morbid diminution of action is *depression*, a similar diminution of power is *debility*, and the term *diminished excitability* explains itself."

Of the articles named, as composing the second subsection, perhaps the one on *Congestion* will be considered the least satisfactory, whether from the inherent obscurity of the subject itself, or that the present matured judgment even of the author is still more or less influenced by early imbibed views on the subject, we will not pretend to decide. The reader, on comparison, will find the views of Dr. Wood on this subject very nearly identical, we think, with those published by Dr. Isaac Parrish in the American Journal of Medical Sciences for April 1845. The latter gentleman acknowledges his indebtedness to his father, Dr. Joseph Parrish, of whom Dr. Wood, in his preface it will be recollected, also makes mention as his preceptor, and among those to whom he is more especially under obligations. It is but reasonable then to suppose, that the opinions of Dr Wood on the subject were also derived from the teachings of the elder Dr. Parrish.

Of the mere accumulation of blood, Dr. Wood very correctly remarks that "it is always an effect of some preexisting morbid state of action; and it is a partial view, which is directed to this effect alone, without embracing the other elements that enter into the complex phenomena presented by the part "congested." And again, in regard to the same, —"this is undoubtedly an important circumstance, and may be the means through which much injury may be inflicted; but it is only a circumstance, and the judicious practitioner will look beyond it to the true pathological condition."

Two forms of Congestion are spoken of, to wit: active or arterial Congestion—the result of irritation or inflammation—and passive or venous Congestion—"resulting from depression or some pure physical agency."

"The depression giving rise to Congestion, may be general, or confined to a particular organ. Nothing is more common than the occurrence of this condition in diseases attended with sudden and great prostration. The heart, participating in this prostration, is unable to transmit the blood so rapidly, as it is conveyed towards it by the continued action of the capillaries and by the forces which move the blood in the veins. This fluid, therefore, necessarily accumulates in the right side of the heart and the great venous trunks, and consequently in those organs with which these trunks more immediately communicate, viz. in the brain, liver, and through this latter organ in the abdominal viscera in general. Instances of Congestion from this cause are constantly occurring. A blow upon the head, or any severe shock, temporarily paralizing the cerebral actions, certain mental emotions which tend to produce syncope, the chill of fevers, especially those of a typhous or malignant character, and the prostration of violent internal and stomachic pains, all occasion internal Congestions consequent on depression in the movements of the heart."

We are inclined to think that the author has here presented conditions having but the most remote analogy, if any, to each other, and some of them also to the state of Congestion, of which they are adduced apparently in illustration. Is there sufficient ground for the assertion that the veins and capillaries do not participate in the prostration of syncope to as great an extent as the heart and arteries;—or, is it not a mere assumption to say that the former act under these circumstances with greater proportionate force than the latter. The movement of dilatation of the heart, which is considered by some as one of the "forces which move the blood in the veins" is of course as much weakened as its contractile power. So with regard to another of the supposed forces which move the blood in the veins—the suction power of the chest;—this must also be diminished in a state of syncope, in consequence of the feebleness of the respiratory movements at such time. Thus, then, as it would appear, certain of the powers moving the blood in the veins are weakened;—the condition, too, presenting such manifestations of a *general* prostrate state of the system, both as regards the animal and organic functions,—are we not warranted in the supposition, that the less appreciable forces moving the blood in the veins, equally participate in this prostration? We think there is reason to doubt, whether morbid Congestion, at least, is a common attendant or result of syncope, and feel satisfied that but few of those who are familiar with the condition termed Congestion, as manifested in some of the pernicious fevers of the South, will be able to discover any very close resemblance between it and a state of syncope, or the phenomena characterizing concussion of the brain, from a blow on the head. Between it, however, and the chills of malarious fevers, a more close analogy will be found to exist; but in the latter state, the action of the heart, it is our impression, is not as a general rule proportionately weak with the pulse, as in syncope. The one condition seems a state of temporary *general* prostration,—of almost entire suspension of innervation, and is consequently attended with a *general* diminution of the action of the circulating apparatus;—the other, a state of partial prostration, an irregular distribution of nervous influence in other words, and is consequently attended with an irregular action of the circulatory apparatus; the power of which in one part seems increased in proportion to its diminution in another. In several cases of intermittent fever we have recently examined, during the period of the chill, the condition of the heart, and have seldom found it acting otherwise than with more than healthy vigour, judging from the sounds and impulse. It is probably, however, far otherwise in the precursory chills of Typhus. We will not deny either, for we cannot reasonably perceive why it should not be so, that the heart may participate at times in the diminished nervous influence, as well as other parts of the circulatory apparatus, in the chills of our autumnal fevers, and more especially in certain of the more pernicious cases; but under such circumstances the phenomena presented, we are inclined to believe, would be those of a general state of prostration or collapse, rather than of the condition recognised by practitioners as *Congestion.*

In speaking of the different theories of *fever*, the first distinct denial of its essential nature is attributed to Clutterbuck, who viewed all fevers which had hitherto been considered of this character, as dependent on inflammation of the brain,—but the assertion of the absolute and invariable connection of each variety of fever with local affection of some particular organ, by the author of the "*nosographie philosophique*" we are inclined to think entitles the latter to the credit, such as it may be deemed, of priority in this particular. To Broussais, who considered fever to be but the external manifestation of gastritis, or gastro-enteritis, Doctor Wood seems to accord the credit of being the second to deny the essential nature of fever, and to give it a local origin. A claim however to priority has been set up by Professor Giacomini, in favor of his distinguished countryman Tommasini.* Thus it is asserted that in a work denominated "Pathological researches on the fever of Livorno, the yellow fever, and other analogous diseases" published by Tommasini in 1805—"were discussed and resolved the most important questions in relation to fever, and the true pathology of gastric and bilious fevers, and the doctrine of the diffusion of local inflammation established. He there signalized, too, the true material condition of continuous fever, and liberated for the first time the numerous family of fevers from the empire of the abstraction and the essential nature." * * * * * While this work was in the hands of all practitioners, and justly appreciated by them, the celebrated Broussais was with the French Armies in Italy, and sojourned for a long period with us. Returning to France, this illustrious physician published in 1808 his first work, on the *History of the chronic phlegmasiæ.* This work is but the echo and copy of the doctrines of Tommasini concerning chronic inflammation and the nature of gastric fever, and of fever in general; with this sole difference, that while the great Italian physician placed the seat of fever, according to the symptoms, in a phlogosis of this or that viscus, and frequently even in a diffused inflammation of the entire sanguineous system, (anticipating from this, it would seem, Bouillaud, also in his Theory, Rev.) the celebrated Frenchman located it exclusively in the gastro-intestinal canal." From this it would seem, that, should we deem the doctrine of the essential *connection* of each variety of fever, as promulgated by Pinel, as not entitling him to priority of claim, as regards the local *origin* of *fever*, the credit of originality is due to the Italian, rather than to Broussais or Clutterbuck.

Dr. Wood is not an advocate, it is proper to mention, of the doctrine of the invariable dependance of fever on local lesion, but admits its essential or idiopathic nature—and that it may occur "without any necessary dependance upon disease in one particular part.

"Theorists have failed in endeavoring to trace the complicated disorders of fever to some common source, and to point out a particular succession, a particular and necessary line of march, in the progress of the affection. The universal disorder of function which constitutes the dis-

* See Medico-Churingical Review, for July, 1847.

ease may be brought about in various ways; and the starting point may be entirely different in different cases."

The second chapter,—*on the causes of disease*, as well as the third,—on symptomatology, though exceedingly interesting, we must pass without selection or comment.

From the opening remarks to the fourth chapter, which is on *general Therapeutics*, we select the following passages, replete with good sense, and evincive of the spirit of eclecticism so necessary to constitute a safe and judicious practitioner.

"In the treatment of disease, we should endeavor to be guided by certain rules or principles, and not surrender ourselves to the accidental suggestions of the moment. The attempt should always be made, by a careful examination into the nature, seat, causes, &c., of the disease, to deduce indications of treatment; in other words, reasons for the employment of certain influences calculated to prove remedial. The character of these influences being known, it then only remains to fulfil the indications which may have been deduced. * * * * * It often, however, happens, that the nature of the disease is so obscure, as to offer no clear indications of treatment. Under such circumstances, we may obey the dictates of experience alone and employ measures which have repeatedly succeeded in similar cases, though not suggested by any rational view of the disease. The science of medicine is yet very imperfect; and we must often content ourselves with means which we know to be useful, without understanding fully their mode of action.

In doubtful cases, when both reason and experience fail us, the best rule is to adopt the *expectant plan:* that is, to do little or nothing which can strongly impress the system, and wait further developments, trusting in the mean time to nature."

From a brief section on "*general indications*," in which is condensed much valuable information, we extract the following:—

"*Climate* has a modifying influence over the effect of remedies. Neither bleeding nor general stimulation is so well borne by the inhabitants of hot countries, as by those of temperate and cold latitudes; while the influence of calomel is generally better borne, probably in consequence of the less susceptibility of the liver."

Though there can be no doubt whatever we think in regard to the truth of the general proposition above presented, we are strongly inclined to doubt the correctness of the exemplification contained in the latter clause in regard to the action of calomel, so far at least as intended to apply to the Southern sections of the United States. We are confident that this once popular doctrine, is fast losing ground in the South, though still believed and taught at the North; and of those who come among us, impressed with the idea of its accuracy, many of the more discriminating at least, are early led, from the results of practice, to prescribe the article in moderate doses only. In regard to the ability to bear, or the susceptibiltiy to any peculiar or specific action of mercury, however, it is altogether probable that no decided difference exists between the people of the North and those of the South, and it is to its action as a purgative simply that we refer, in presenting our belief, that in large doses it is more injurious to Southern than to Northern constitutions.

"Epidemics are notorious for their quality of imparting something of their own nature to other coexisting diseases. Now, sometimes the epidemic influence conduces to a sthenic, sometimes to an asthenic, feeble or typhoid condition. In the former case, diseases ordinarily of a feeble character, assume a degree of energy and elevation, which requires depletion; in the latter, inflammatory complaints, which usually yield most readily to copious bleeding, sometimes become so prostrate, as to forbid evacuation, and even to require the support of active stimulation."

In the second section of this chapter, the different *general therapeutic processes*, included under the heads *depletion, repletion, dilution, stimulation, sedation, revulsion, supersession, alteration, chemical action and mechanical action*, are treated of.

The means of direct depletion enumerated, are *general and local bleeding* and the promotion of *increased secretion*, by diuretics, diaphoretics, cathartics, &c. *Indirect Depletion*, "is affected by whatever prevents the usual amount of solid organic material from entering the circulation"—as emetics and cathartics,—by discharging the partially digested food before it has entered the lacteals; abstinence, low diet, &c. The remarks relative to the *antiphlogistic diet*, though judicious upon the whole, are not entirely, we think, unexceptionable; we quote a passage.

"If the bloodvessels receive nothing from the alimentary canal, they draw upon the whole system for a supply, and the detritus of the tissues, conveyed into the circulation by the lymphatics, becomes a substitute for food. The blood is thus recruited by the most highly animalized products, and instead of requiring antiphlogistic properties, passes into the state most calculated to excite inflammation. It is always, therefore, safest, where the stomach will receive nutriment, even in cases of the highest excitement, to permit entire abstinence only for one or two days."

Now, in cases of inflammatory diseases, of the highest excitement, food in any shape, we are disposed to believe, may not only safely, but beneficially, be dispensed with, for a much longer period, than is here named, while the probable injury above suggested may be obviated by providing the bloodvessels through the absorbents of the intestinal canal, with such a supply of water, as seems to be required by the system in such conditions.

The term *repletion*, has heretofore, we believe, been understood to mean a superabundant fulness; but as used by Dr. Wood, is intended to signify the therapeutic process, by which a deficiency of the circulating fluid may be supplied,—in short, the reverse of depletion. The means are, a nutritious diet, aided by such measures as are calculated to invigorate digestion and sanguification; as tonics, stimulants, exercise, &c.

There is perhaps no therapeutic process, relative to the precise indications for which more obscurity or uncertainty exists, than that of *revulsion, derivation, counterirritation*, (the terms are used synonymously by the Author,) and consequently, none which are more frequently employed in an empirical manner.

"There is in the human system only a certain capacity of nervous action, and a certain amount of blood. When either the former or the latter is strongly directed to a particular part of the body, there is a tendency to its diminution elsewhere. This is absolutely necessary of the blood, and is true to a great extent in relation to nervous action. Such a direction is given by the application of irritants of any kind. Hence, in order to relieve inflammation, any of the forms of vascular irritation, or mere nervous excitement, as indicated by pain or spasm, in any particular part of the body, we apply irritants, which under these circumstances are called revulsives to some other part."

It is important to recollect, however, in the establishment of a point of irritation, by a counterirritant, that, though this will have a tendency to diminish the flow of blood or nervous action generally, elsewhere, it may and often does have the effect of increasing the determination to particular parts; to those, for instance, which are already the seat of high morbid excitement, adding consequently to the already existing mischief; and it is in deciding when this will probably occur, and when not, that the principal difficulty exists, in regard to the rational indications, for the more active and permanent counterirritants. One rule, urged by Dr. Wood, and recommended by almost every author, who has treated upon the subject,—"not to employ a highly irritant revulsive agent, in inflammatory cases, during the greatest violence of the disease," should be strictly adhered to, and may be considered as designating the limits of the applicability of such remedies in one direction; but even after the violence of the disease is considerably reduced, or in cases of but moderate excitement, it is often questionable, whether a revulsive agent will "unseat the inflammation," or add to it, through the additional excitement induced by its action. Dr. Wood is not more explicit on this uncertain point of practice, than his predecessors have been.

The following paragraph, though correct no doubt in regard to practice in the North, would never, we feel assured, have been penned by a physician practically familiar with the use of purgatives, in the diseases of the South.

"When the revulsive impression is conjoined with copious depletion, as in the case of the hydrogogue cathartics, which produce a revulsion towards the whole lining membrane of the bowels, while they evacuate the contents of the blood-vessels, it may be resorted to in the greatest height of the inflammation. The copious secretion prevents the excitement of an irritation in the bowels, sufficient to bring the constitution into sympathy."

Now, this will not hold good in a vast majority of instances, in regard to the use of purgatives in the South, where, from slight causes, high irritation of the intestinal canal is so readily induced, and the practice inculcated, we feel confident, is calculated to have a dangerous tendency. Indeed, almost invariably, whenever anything like a copious secretion is excited from the lining membrane of the intestinal canal, by purgatives, in our febrile and inflammatory diseases, if a high degree of inflammation, sufficient of itself to prove dangerous, is not the immediate result, the intestinal irritation, at least, produced will be such as to increase the general excitement, and react thus upon any previously existing local disease. Judicious Southern practitioners, in the use of cathartics, select those articles only, and in such doses, as they think are calculated to quicken the peristaltic motions of the canal, and procure the evacuation of its consistent contents merely; and look for mischief, more or less, as an almost invariable consequence of *thin or watery discharges* from the bowels. Differ as they may on other points, but one sentiment seems to exist in regard to this.

We do not recollect to have met heretofore with the term *supersession*, as used in the work before us, and presume therefore that it is to Dr. Wood we are indebted for its present application. It is perhaps appropriate.

"By this process is meant the displacement or prevention of one affection, by the establishment of another in the seat of it. It is a general though not universal pathological law, that two powerful diseases cannot exist in the whole system or any portion of it at the same time. If, therefore, we can produce a new disease, in the exact position of one that may be existing or expected, we may possibly supersede the latter; and if the new disease subside spontaneously without injury, we cure our patient. The operation of numerous remedial agents is explained in this way. It is thus, for instance, that mercury is supposed to cure syphilis. But we have better examples in the powerful influence of certain antiperiodic remedies, such as quinine and arsenic, in the cure of intermittent diseases. They establish their own morbid impression in the absence of the paroxysm; and the system, being thus occupied, at the moment when the disease was to return, is incapable of admitting it."

Whatever opinion may be formed of the principal position set forth, we are inclined to think that the examples here given in illustration of the process, will be considered anything but apposite. It does seem, that for such an *explanation* to be received as at all plausible or satisfactory, the *disease curing* should bear some proportion in violence to the *disease cured.* And yet the paroxysms of a most pernicious intermittent or remittent,—a disease almost invariably fatal when left to nature,—may be arrested by a remedy, which may give no other evidence whatever of its action on the system, than the removal of the disease; while an attack of so evident and dangerous a disease as small-pox, supervening and producing during the absence of the paroxysm, a powerful impression of its own on the system, we have reason to believe would have no such effect, aggravating, perhaps, rather than curing the original affection. It would appear equally as reasonable to attribute every cure, resulting from the action of any remedial agent whatever, to the induction of a new disease by that agent, as the instances named by Dr. Wood. Hence every remedial impression would have to be considered a disease. A disposition to offer explanations of obscure phenomena, by more obscure speculations or hypotheses, is not however an error of frequent repetition with Dr. Wood. Perhaps in all such instances, it might be as well, to admit the imperfection of our knowledge.

We leave unnoticed portions of this section, finding nothing in them of sufficient interest to require attention, and pass to an examination of the second part of the work.

From the prefatory remarks we extract some observations, briefly expressive of the author's opinion in regard to systems of nosology—and his own intention as to the arrangement or classification of diseases.

"A vast amount of time and industry have been expended in the formation of systems of nosology. It is not the intention of the Author to discuss their merits. Imperfect they all necessarily are; because diseases are not yet sufficiently understood to permit us to see clearly their mutual relations; and systems founded on this basis must be constantly changing with new discoveries and the adoption of new views. In this uncertainty, that plan of arrangement appears to the Author to be best, which is most convenient, and which may tend to direct rather to what is positively known, than to the conjectures and peculiar opinions of Authors. Such a plan is the one based upon the seat of the disease, and this it is here proposed to adopt.

Diseases will be placed together, which are situated in the same parts; and no other attention in the mere arrangement will be paid to their mutual relations, than to form distinct groups, in each division, of such as may have the closest analogy. Upon comparing diseases, we find occasion to divide them into three great classes, having reference to their seat. The *first* class includes those diseases which occupy the whole system at the same time, and in which all the functions are simultaneously deranged. To the *second* belong constitutional affections, which may display themselves in local disease in any part of the system, but not in all parts at the same time. The *third* class embraces all the proper local diseases, or those which essentially affect some particular structure or function, and in which any general phenomena that may be presented, are only secondary. This portion of the work will accordingly be distributed into three divisions, corresponding with the classes mentioned."

In his first class, Dr. Wood includes first, Irritative fever. Second, Miasmatic or Bilious fever, (embracing Intermittent, Remittent and Pernicious fever)—third, Yellow fever,—fourth, Enteric or Typhoid fever,—fifth, Typhus fever,—sixth, Plague,—seventh, Variola,—eighth, Vaccina,—ninth, Varicella,—tenth, Rubeola,—eleventh, Scarlatina,—and twelth, Erysipelas.

The *second class, constitutional diseases*, includes Rheumatism and Gout only,—and in the *third class* are included the *local diseases*, arranged in anatomical order, in sections: as for instance, Section 1st. Diseases of the digestive system,—Section 2d. Diseases of the absorbent system,—Section 3d. Diseases of the respiratory system,—Section 4th. Diseases of the circulatory system,—Section 5th. Diseases of the organs of secretion,—and Section 6th. Diseases of the nervous system, These general divisions are again separated into *subsections*; and these again into articles on the individual diseases. For instance, subsection first, of section first, treats of diseases of the mouth,—and then there are various *articles* in this, as for example, Article 1st. Inflammation of the mouth,—Article 2d. Inflammation of the tongue, &c., &c.

Dr. Wood discusses and condemns the practice of naming fevers from the form, grade or type they may assume, or in consequence of a predominance of disease in any particular part, from mere accidental complication, urging in regard to the former, that any one disease, under different circumstances, and in different cases, may present different characters; the same fever for instance in different individuals, being in type, intermittent or remittent,—and in grade Typhous, Synochus, or

Inflammatory;—and in regard to the latter, that the same disease, merely diversified by the occurrence of inflammation or irritation in one organ rather than another, has been made into distinct fevers;—as gastric,—hepatic, &c.—implying some essential difference. He prefers, as the basis of arrangement, the peculiarity of the cause; stating, that all will admit, we think—that "the cases produced by the same cause may very properly be treated as belonging to the same disease; and any incidental peculiarities of form, type, &c.—should serve only as the ground of varieties;"—and it appears to us, that, though not universally, it has generally been in the latter sense, that the terms alluded to, have been applied.

Under the article *Irritative Fever* are included a number of cases which we have deemed of somewhat difficult classification, resulting as they do from causes of irritation having nothing in common, "nothing peculiar or specific in their mode of operation." Among the causes, the Author enumerates, principally, exposure to cold, especially in combination with moisture,—exposure to intense heat,—over-exertion in hot weather,—errors of diet,—teething, worms, &c.,—but he also considers a pre-existing disposition to the febrile movement necessary to the production of the disease. As will be inferred from the causes named he includes under this head *Infantile remittent.*

In regard to *treatment*, we find nothing particularly requiring notice, but must admit that our curiosity has been somewhat awakened to discover, what are the *peculiar* remedial virtues possessed by *Garlic*, sufficient to induce a gentleman of Dr. Woods good taste generally, to advise its use in poultices to the feet,—and, mixed with brandy,— in frictions along the spine, in convulsions attending the Infantile cases,—in preference to mustard, cayenne and other articles of the kind, so much less offensive to the olfactories.

The article on miasmatic fever includes, "all the forms of fever resulting from the influence of marsh miasmata." They are arranged under three heads, to wit, Intermittent fever, Remittent fever, and Pernicious or congestive Intermittent and Remittent fever. Notwithstanding the numerous Types and the differences of Grade, in miasmatic as in other fevers, Dr. Wood considers them all, "as much one disease, as are the different varieties of small-pox, measles, or scarlet fever." Yellow fever, it will be perceived, he does not consider one of the miasmatic family.

The following paragraph is not devoid of interest, offering as it does a somewhat ingenious explanation of the occurrence of Dropsy after intermittents; and also from the fact, mentioned by the Author of the presence of albumen in the urine in such cases,—adding to our already existing evidences that organic disease of the kidneys is probably not a necessary condition, as was once supposed, to the secretion of this substance,—seeing that "the Dropsy is in general easily and often permanently cured;" which we believe is rarely the case when resulting from the morbus Brightii.

"A very frequent result of protracted intermittent is dropsy. This affection sometimes occurs during the continuance of the disease, and as before stated, has been ascribed, in part, to a vicarious secretion into the cellular tissue and serous cavities. But it is more frequent after the intermittent has been interrupted. Even *in such cases*, it may possibly be owing, in part, to the *substitution of a serous discharge for the perspiration to which the system was accustomed.* I have found the urine, in these cases, so far as I have examined it, to be albuminous, and sometimes highly so, yet the dropsy is in general easily and permanently cured."

The explanation, however, which attributes the production of dropsy, following intermittents, to an impediment to the return of the blood, in consequence of obstruction in certain of the abdominal viscera seems sufficient; is the more plausible, and probably the true one, seeing, that, in such cases, some of the viscera alluded to, may almost invariably be discovered to be more or less enlarged or indurated.

Dr. Wood rather favors the opinion,—more current in times past, when the saying was, that

"An ague in the spring
Is Physic for a King,"

than in the present day,—that intermittents prove useful, by "*superseding* other diseases, and removing morbid tendencies which had before resisted treatment." Now, we are inclined to believe, that, where another disease has been cured seemingly, by the supervention of or during the progress of an intermittent, it has been rather in consequence of the treatment instituted for the removal of the latter, than, as Dr. Wood supposes, by substituting a "safe and temporary for an unsafe or obstinate affection." The results of our own observation at least during a practice of several years in a highly malarious section, are in decided opposition to the doctrine; for not only have we never seen a case, in which the occurrence of an intermittent during the progress of another disease seemed of itself to prove beneficial to that disease; but, on the contrary, we can call to mind at this time not a single instance, in which it did not of itself seem to have a marked prejudicial effect on the original disease, proportionate to its own violence and duration. Dr. Wood himself, however, thinks that the opinion, that it has a counteracting influence, where a tendency to pulmonary consumption exists, is based upon insufficient grounds. That consumption may be a less common disease, as a general rule, in some malarious countries at least, than in colder parts, it would seem might be explained by other influences, without the necessity of invoking the aid of counteracting agency from the malaria itself, or an *antagonism* between ague and consumption. It is not true, that in malarious sections,—perhaps with more propriety we might say in warm climates,—consumption, when fairly developed, is more rapid in its progress than in cooler latitudes. But we are deviating somewhat from our proper subject.

The proper anatomical character of intermittent fever, the author is of opinion, has scarcely been ascertained.

Dr. Wood enters at some length into the treatment of intermittent fever, considering in order the remedies proper during the three different stages. He is opposed to bleeding in the cold stage, not so much it would seem because the general result of the testimony on the subject is against it, as because "it has always appeared to be opposed to a sound pathology." In other words, according to Dr. Wood's theory of congestion, a feeble action of the heart is necessary to a morbid accumulation of blood in this organ and the larger vessels, which probably takes place during the cold stage, and as it is difficult to understand how the abstraction of blood can have the effect of correcting this enfeebled action, he is on *principle* opposed to it. It must be admitted that, admitting the state called congestion to be a mere accumulation of blood in the larger veins,—the supposition of an enfeebled action of the heart (from whatever cause resulting) offers such a plausible and seemingly satisfactory explanation of the production, that in the absence of positive observation to the contrary, it is difficult to resist the impression, that such is really the case. Listen to Dr. Wood's explanation.

"The blood, imperfectly transmitted through the vessels of the lungs, and imperfectly forwarded by the heart, accumulates necessarily in the venous system behind these points of obstruction. It is not the accumulation of blood that produces the prostration, but this that causes the accumulation."

Now, whatever may be the immediate cause of the accumulation (and we have no theory to offer at this time explanatory of the phenomenon) it cannot, we feel assured, with propriety be attributed to an enfeebled action of the heart, in diseases at least of malarious origin ; for, in this respect, differing from some other diseases, as for instance Typhus fever, generally in these, the force of the heart's action will in almost every stage be found equal to that of health, and almost invariably disproportionately strong, when considered in reference to the condition of the system in other respects, and in reference to other diseases. If this were really the cause, should we not of course look with propriety for the phenomena of congestion, in the most strongly marked shades, in Typhus fever, in which, as we are informed by Dr. Stokes,* the impulse of the heart is often imperceptible, frequently one of the sounds of the organ entirely extinct and the other with difficulty heard, and that, too, not only in cases of the worst character, or during the period of impending dissolution, but in many in which recovery takes place. Here we have general prostration, in which the heart participates. Nothing like this, we feel assured, will be observed as a common occurrence in fevers of malarious origin, in which, in many instances up to the last hour, in articulo mortis even, the impulse and sounds of the organ may be felt and heard distinctly.

Under ordinary circumstances Dr. Wood advises that, in the intermissions, as soon after the bowels have been evacuated as possible, to commence with the Peruvian bark or one of its preparations. From twelve to twenty-four grains of quinine he considers about the proper quantity, as a general rule to be given in an intermission ; but has not that dread of somewhat larger quantities entertained by many, and does not hesitate to advise their administration, in cases in which the more moderate doses have failed. Subsequently he *gravely* discusses the propriety of its administration in intermittents complicated with local inflammation, and arrives at the following conclusion, which we feel assured cannot fail to elicit a smile from many of those whose opportunities of practically testing the value of the two different methods of treatment have perhaps been more numerous than have those of Dr. Wood.

* On the use of wine in Typhus fever.

"Whenever the intermission is quite complete, in other words when it is quite exempt from fever, quinia may be given without hesitation, if the stomach will support it. If any existing inflammation is of so low a grade, as not to induce symptomatic fever, it will scarcely oppose an obstacle to the anti-periodic action of quinia, and will be much more likely to yield after the paroxysms have ceased. Indeed such inflammation is often supported by, if it do not originate in the fever of the paroxysm.—When the inflammation is so severe or extensive as to induce fever, though the disease may have the paroxysmal form, yet it will present rather the aspect of a remittent than an intermittent, as there will be fever steadily throughout the interval. In such cases, the use of quinia should be preceded by depletion and other measures calculated to reduce the inflammation; but as soon as a distinct intermission has been obtained, there should no longer be any delay in resorting to the anti-periodic remedy."

Those only who have had numerous opportunities of observing the results of various methods of treatment in malarious diseases, are aware, how difficult it is, and of the uncertainty of the attempt, to subdue local inflammation, existing in combination with miasmatic fever, by what is generally considered a pure antiphlogistic treatment. In spite of bleeding, local and general, purgatives, antimonials, &c., so long as the paroxysms of fever are permitted to continue, with each exacerbation, an extension of the inflammation is produced. On the other hand, how little are those aware, whose practice, as regards the use of quinia, is the result of theoretical notions, based upon the more commonly received doctrines in relation to its modus operandi, how readily miasmatic febrile exacerbations may be moderated or broken up completely by it, even during the continuance of local inflammation, or how powerful an adjuvant for the latter itself, judiciously used, it proves.

Dr. Wood prefers the plan of administering it in small doses at short intervals, so that the desired quantity may be equally distributed through the apyrexia; omitting as a general rule the time of sleep. The advantage of this method of administration is, according to the author, that a better opportunity is given for the absorption of the medicine,—there being but "little doubt that it operates by entering into the circulation, as it can be detected in the urine by chemical tests." Now, though we think there are reasons for believing, (we have not time to discuss them at present,) that the curative operation of quinia is in part effected through an impression on the nerves of the stomach and other parts with which it comes in direct contact prior to its absorption, we also believe that its remedial influence is likewise measurably effected subsequent to its entrance into the circulating current, and that this is necessary for its full and more perfect action, still we do not consider, as seems to be the opinion of Dr. Wood, that the mere fact of its detection in the urine by chemical tests, is of itself sufficient and conclusive proof, that its entrance into the circulation is necessary to its curative operation, for we see no reason why a remedy may not exert its curative agency entirely on the nerves of the stomach, and yet subsequently come under the action of the absorbents. Because a medicine may "operate by entering the circulation," it by no means necessarily follows, that all that are absorbed act in this way.

Among the means of *prevention*, to those who reside in miasmatic regions, it is recommended by Dr. Wood, as well as most of his predecessors, to avoid during the sickly season the morning and evening air, the intense heat of the sun in the middle of the day, &c. But, "a much more effectual measure is to keep the system steadily under the influence of quinia or Peruvian bark. "My friend (continues Dr. Wood,) Dr. Samuel Jackson, late of Northumberland, has informed me, that he *kept himself* in this way perfectly well during a season in which miasmatic disease was exceedingly prevalent, and he was compelled to undergo all kinds of exposure, at all hours of the day and night." We are acquainted with a professional gentleman who has *remained* well during a residence of some ten or twelve years duration in a highly malarious locality, undergoing all that time such exposure and fatigue by day and night, as necessarily result from a tolerably extensive practice, and who, nevertheless, has never yet taken a single grain of quinia or Peruvian bark.

Dr. Wood calls the attention of his readers especially to the distinguishing characters of remittent fever and typhoid fever (the one, the autumnal, the other, the winter epidemic of our country)—"as the two diseases have been, and still are, not unfrequently confounded by practitioners." The only conditions, we conceive, under which an error of the kind could easily occur, would be in cases in which the cause or causes of Typhoid fever might be brought in action on patients previously exposed to miasmatic influence, and the latter circumstance impressing upon the disease something of a remittent type; this latter being one of the principal circumstances upon which to base a diagnosis, in the few cases of protracted remittent fever, in which, as in all other febrile diseases of long duration, symptoms denominated *typhoid* frequently supervene. Under other circumstances we think the early history of the case might in most instances serve to designate its true character, whatever the phenomena present in the more advanced stages.

The following contains Dr. Wood's description of the cases of remittent likely to be confounded with Typhoid fever.

"In not a few cases, however, instead of following either of the courses above indicated, the disease somewhere from the 9th to the 12th day takes on a new character, very much resembling that so often met with in enteric fever. All regularity in the recurrence of the paroxysms now generally ceases. The pulse becomes very frequent, often rising to 120 in the minute, and sometimes reaching or even exceeding 140, while it is small and rather feeble. The skin is dry and either universally hot, or cold in some places and hot in others. The tongue is dry or dryish, often contracted and of a brown or blackish color. Sordes often collect about the teeth, tongue and lips. The sufferings from nausea, vomiting and headache, diminish or cease. The bowels, though in some cases costive, are in others loose, with unhealthy discharges, dark, bloody or dysenteric. The urine is scanty or suppressed, or is retained, producing sometimes great distension of the bladder. Stupor or low delirium, with subsultus tendinum, picking at the bed-clothes, slipping down in the bed, supersede the former cephalic symptoms.—Not unfrequently the patient thinks himself in a strange place, and, insisting on returning home, sometimes rises from his bed, and sinks exhausted on the floor. At length, if relief is not obtained, profound coma sets in, the pulse sinks to nothing, the surface becomes cold, the countenance assumes the Hippocratic expression, and death speedily closes the scene."

We think the above description will be found applicable in its principal features, in many instances, to other febrile affections and diseases of a protracted character attended with fever, as well as to remittent fever, as descriptive in short of *the* condition to which protracted febrile and inflammatory diseases so frequently tend in their advanced stages. As certainly however identifying it as remittent fever, Dr. Wood remarks, that "even this form of the fever sometimes ends in intermittent, and thus proves that it was not what it might otherwise be thought to be, a pure case of enteric or Typhoid fever."

The following is Dr. Wood's account of Remittent fever occurring in subjects previously exposed to the operation of causes calculated to depress the vital powers and to deprave the blood. The cases of this character probably more frequently arise, he observes, "from the co-operation of an epidemic typhoid influence with miasmata."

"In such cases, connected with more or fewer of the characteristic symptoms of bilious fever before enumerated, are, at a comparatively early period in the disease, a dark and dryish tongue, with sordes upon the teeth and gums; dark alvine evacuations, becoming in the end involuntary; flatulent distension of the abdomen; irregularity of respiration; a pulse either frequent or slow, slender or full, regular or irregular, but always feeble and readily compressible, and sometimes almost fluttering; a strong tendency to passive hemorrhage, as shown by oozing of blood from the gums, discharges of dark blood from the bowels, and petechiæ and vibices upon the skin; a dusky, livid or purplish hue of the skin, often combined with the yellow of the bilious disease; irregular distribution of heat upon the surface, and the early occurrence of low delirium, stupor or coma, or, in their absence, of great restlesness, jactitation, anxiety and mental depression."

Dr. Wood speaks of several *modifications* of remittent fever, most of them dependent apparently upon the organ principally affected. In all of them, he says, "there are evident, in some of the cases, certain signs, either full blown or, as it were, embryotic, which mark the disease as miasmatic or bilious fever; more especially gastric irritation, yellowness of the skin and a tendency to the regular paroxysmal form."

Among the symptoms of *remittent fever* enumerated by the author is one which we do not recollect to have heard named by others, and have rarely observed in our own circle of practice.

"Sometimes the patient is troubled with uneasy sensations on the back part of the tongue or in the fauces, which causes an almost constant hawking, with the discharge of glairy mucus. The author well remembers, that in an attack of this kind one of his most uncomfortable sensaions, in a certain stage of the disease, was a feeling as if there were a loose hair in the fauces."

Having as often proportionately observed the above symptom in other febrile affections, as in remittent fever, we have not considered it therefore *as specially pertaining to the latter disease*, but have considered it rather the result of the increased viscidity of the mucus of the mouth and fauces, and its consequent more difficult expulsion, arising from the deficient secretion attending generally a febrile state of the system from whatever cause induced. It might, we conceive, have been omitted very properly in the enumeration of the symptoms of a disease with so many striking and peculiar features as the one under consideration.

"The average *duration* of bilious fever" (observes Dr. Wood) "in all its forms, may be stated at about fourteen or fifteen days. It sometimes ends as early as the fifth or seventh, often about the ninth or eleventh day; and is sometimes greatly protracted, even to four weeks or more."

We are persuaded that the above estimate of the average duration of remittent fever, as applied to many sections of our country at least, is incorrect; unless, indeed, it has reference to the period to which the cases extend, or would extend, if uninterrupted by treatment. Somewhat, of course, depends upon the epidemic, and much upon the practice pursued. The average duration of 124 cases treated between the 1st of June and the 20th of November, 1847, to the records of which we have had access, was a fraction less than 7 days, including the day of the attack and that of the discharge in the calculation; this period, too, being very considerably augmented by a few complicated and protracted cases. But 24 cases only of the number extended beyond the 7th day. The average duration of treatment was about 3½ days, including the day of its commencement and the day of the patient's discharge; such pernicious cases as occurred as well as the more mild are included in the calculation.

Among the *post mortem* appearances Dr. Wood mentions inflammation of the mucous membrane of the stomach, as also of the bowels; enlargement of the mucous glands of Brunner in the duodenum; signs of inflammation of the membranes of the brain and congestion of that organ; enlargement and softening of the spleen; enlargement and softtening of the liver;—but, in regard to this organ, states that "the most striking phenomenon revealed upon dissection" is the alteration of color, first distinctly described by Dr. Stewardson in the American Journal of Medical Sciences for April 1841. Of this, Dr. Wood remarks that "it may be regarded as one of the characteristics of the disease" and considers it the only post mortem "peculiarity which has been discovered;" but, in another place observes, "how far this may be connected with the peculiar and distinctive pathology of the disease, it would not be easy to determine." Of the other lesions enumerated he observes, that, they "are the same as those frequently met with in other febrile diseases." The genuineness of those cases in which inflammation and ulceration of the elliptic patches of Peyer's glands, such as characterize the enteric or typhoid fever, is questioned by Dr. Wood.

The truth of the following statement is admitted by all, we believe, at present, whose opportunities for observation in regard to the circumstance, have been at all considerable.

"It is a singular fact, that the negro, though not entirely exempt from miasmatic fever, is much less liable to it, and, when attacked, suffers less from it, as a general rule, than the white. Hence the coast of Africa, which is so fatal to persons of our color, is favorable to the negro; and the latter lives and works in the rice fields of Carolina, at seasons when a single night spent among them would be fatal to his master."

As bearing upon this singular fact, we make the following brief extracts from a valuable paper on the Medical History of Alabama, containing much interesting information, recently published in this Journal, by Dr. P. H. Lewis.

Speaking of *Congestive fever*, he observes, "the negroes, who are much more exposed than were the young men of the white population are seldom attacked. In 1835 and 1836, the writer's circle of practice embraced a population of 1500,—one thousand of whom were negroes, and the balance whites. During the Autumn of these two years, he treated 88 cases of grave congestive fever; and among the whole number there were but three cases occurring in negroes. * * * * During the warm weather, this class of persons enjoy the best of health; but after the approach of cold nights and mornings, such as we have in October and November, disease, * * * is rife among them. Further—" To the negro whose organization is such as to endure the heat of summer with impunity, those diseases which come on the chilly blast, and are nourished by cold and moisture, are peculiarly noxious and alarmingly fatal."

The *nature* of remittent fever is discussed briefly, and the view which considers it merely owing to the superaddition of acute inflammation of one or more of the organs, to intermittent fever, clearly refuted. The opinion which referred all the phenomena of the disease to gastritis, as it no longer meets with supporters, is not combatted.

An over production of bilious matter in the system—Dr. Wood considers "one of the direct and peculiar effects of the cause" of remittant fever. * * * "It is not in bilious fever as in jaundice, that the liver does not act, and that the biliary principles being thus prevented from escaping by their usual outlet, accumulate in the blood, and are then thrown off by the skin. On the contrary, though there may be cases in which the liver is congested beyond the power of secretion, yet in the great majority of cases, that organ acts even more vigorously than in health, as is evinced by the bilious vomiting, bilious stools, and and abundance of bile found in the gall bladder after death. Whence then proceeds the yellow color of the surface, and the jaundiced condition of the urine? Undoubtedly from an excessive production of the biliary principles in the blood. These principles have been detected in the blood by chemical examination. They are produced in it probably through the agency of the cause, and, being injurious in this excess, seek an escape through all the emunctories, not the liver only, but the skin, kidneys, and possibly also the mucous membranes. Still, the excess of bile in the blood is only one of the peculiarities of the disease. It cannot be the only one; for such an excess frequently exists, without producing remittent fever. Can the elimination of this excess of bilious matter have anything to do with the cure or prevention of the disease? Can it be in this way that calomel acts? Very often an attack of remittent fever is preceded by symptoms of epigastric uneasiness, which indicate portal congestion. There is some reason to think that a spontaneous attack of cholera morbus at this period, or the somewhat similar operation of a full dose of calomel, occasionally prevents the development of the fever. Can it be by carrying out of the system the excess of the biliary principles, which may have been accumulating in the blood, and which may have been stimulating the liver for a time beyond the secreting point, that the agents alluded to produce the effects ascribed to them?"

Dr. Wood, though generally cautious, occasionally, we think, on mere speculative questions, too intent upon some particular or favorite notion, loses sight of what would seem the most obvious objections to the establishment of his position.

As shewing that an answer in the affirmative to the question just propounded, would probably be incorrect, the fact may be mentioned, that it very often happens, even after the full development of the disease, that yellowness of the skin, (in the cases in which this does occur) is not observed, till an excessive secretion of bile is produced. In many instances then it is but reasonable to suppose that it is from excessive action of the organ, rather than from an excessive production of the principles of bile primarily in the blood, that the yellowness of the skin is to be attributed as a result. Thus cases not unfrequently occur, in which the disease may continue for many days, with a moderately augmented secretion of bile only, without the occurrence of jaundice; but let the liver be excited to excessive secretion, by the too free administration of calomel, or other irritants of the gastro-duodenal mucous membrane, and in all probability we will have jaundice immediately produced.

Dr. Wood tells us that the biliary principles have been detected in the blood by chemical examination,—and according to his view, (if we do not mistake it) that these already exist in the blood, and that they are produced in it in excess, through the agency of the cause of remittent fever, independent of the liver, the function of the latter is reduced to one of mere mechanical separation or filtration, and not one of formation; and admitting this, it would be no unreasonable conclusion, we think, that the bile, like the urine, is entirely excrementitious—serving no useful purpose in the economy.

Undoubtedly some of the biliary principles, (as the biliphæin for instance) have been found in the blood in disease; but that the views of Dr. Wood in regard to the bile should be correct, (it may be that we have misunderstood his remarks, for they are not characterized by his usual clearness,) it would be necessary that these should all not only be found in it in disease, but should also exist in it in health. According to Simon,* in healthy blood we find neither bilin nor biliphæin, though in icterus, biliphæin exists in the serum. He is not aware that bilin or bilifellinic acid has ever been observed in the blood; and he remarks that should the former constitute one-thousandth part of the blood, it would be easily detected.

Should it be, however, that by others not only all the biliary principles, but these united so as to form that fluid itself, have been detected in the blood in disease, this would not preclude the idea of the formative action of the liver, as their presence might be the result of absorption from the intestinal canal. That jaundice frequently occurs in connection with

* Chemistry of man.

a suspension of the biliary discharge, would seem to imply the existence —or favor the doctrine of the previous existence or formation of the biliary principles in the blood itself; these being permitted to accumulate in consequence of the cessation of the function, (as a mere organ of separation,) of thel iver. But is it not possible that certain derangements of the organ may exist, not incompatible with the secretion of bile in the acini, but yet incompatible with its excretion through the finer commencing ramifications of the ducts—and thus absorption of the fluid take place from the liver itself. Any impairment of the function of the organ, from excessive irritation extended continuously from the duodenal mucous membrane along the biliary ducts, would reach of course and affect the minute commencing *excretory* ramifications—before reaching the secretory portions of the organ. In these remarks of course we do not have reference to any palpable mechanical obstruction.

Where jaundice occurs in remittent fever, we think it will be found invariably in connection, either with an excessive secretion of bile, or a total suspension of the discharge. That explanation, which refers the excessive secretion of bile, in miasmatic diseases, and sometimes in yellow fever, to excitement of the liver from sympathy with the irritated gastro-duodenal mucous membrane, and the jaundice of such cases to absorption from the intestines, has seemed to us the most satisfactory. The suspension of the discharge of the secretion which sometimes takes place, is probably the result of an excessive degree of sympathetic irritation of the organ; a moderate degree of stimulation, it being known, increasing the activity of the secretory organs, while excessive stimulation has the effect of diminishing their activity or suspending entirely their functions. Dr. Wood, we are inclined to think, did not bear this principle in view, when he asks, can it be that calomel acts, "by carrying out of the system the excess of the biliary principles, which may have been accumulating in the blood, *and which may have been stimulating the liver for a time beyond the secreting point*?" An additional stimulant to excite to secretion an organ, already stimulated beyond the secreting point?

Is it really known that the "epigastric uneasiness" alluded to in the quotation, is indicative of "portal congestion?"

We are surprised somewhat at the questions of Dr. Wood; "can the elimination of this excess of bilious matter have any thing to do with the cure or prevention of the disease? Can it be in this way that calomel acts?"—presented as they are doubtingly, and as if containing suggestions entirely new, when the practice of so many, and for so long a time, has been based upon the assumption of the truth of both; when, in giving calomel to "carry off the bile" has for so long consisted the popular practice and theory of extensive sections of the country.

Within the last ten or twelve years a very material change has taken place in certain sections, in the treatment of remittent fever; the more general use and more liberal administration of quinia having taken the place of, or rendered unnecessary to a considerable extent the repeated administration of mercurials and other purgatives, and the irritating nauseants,—diaphoretics as they are termed,—which constituted so important a part of the treatment in times past. Those practitioners, who remember the results of the past practice in the sections alluded to, cannot but be struck with the comparatively rare occurrence of jaundice, as also the diminished quantity of bile generally discharged in remittent fever, as treated at present. Indeed, under any circumstances this symptom (jaundice) is rarely present in the early period of the disease; and this, with the fact just named, we consider as bearing strongly against the opinion of Dr. Wood, that the over production of bilious matter in the system, is one of the *direct* and peculiar effects of the cause of remittent fever; but would seem to favor the belief, that it is rather an indirect effect,—the result of gastro-enteric irritation; seeing that generally both the jaundice (when it occurs) and the quantity of bile eliminated, bear a tolerably exact relation to the degree and period of continuance of the gastro-intestinal irritation attending the disease, or, induced during the treatment, and the quantity of purgatives and nauseants administered. This relation, however, is destroyed in those cases in which, from excessive gastro-duodenal irritation,—or whatever the cause,—the discharge of bile is suspended.

In regard to the *prognosis*, all must concur in opinion with Dr. Wood, that "there is perhaps no disease in which the resources of our profession are more happily displayed than in the worst forms of bilious fever. Fearfully fatal under neglect or mismanagement, they may, in a great majority of cases, be conducted by proper treatment to a favorable issue."

Dr. Wood considers in succession emetics, cathartics, venesection, diaphoretics, cold water externally applied, mercury and quinia, in relation to treatment.

Emetics are limited to cases in which there are irritating substances in the stomach.

"The offending matters are undigested substances that may have been swallowed, or acrid accumulations in the stomach from chemical change, perverted secretion or regurgitation. The former may be supposed to exist when the attack has come on very shortly after a full meal, or after indulgence in unwholesome food or drink. The presence of the latter is indicated by a feeling of oppression, severe nausea, and frequent but ineffectual attempts to vomit, the patient now and then discharging a mouthful of very sour or sharp colorless fluid, or of bitter and acrid bile."

Emetics are no doubt admissible and even beneficial in certain cases, as for instance, where an attack has supervened soon after a meal, which is not thrown off spontaneously, as is most generally the case, however, at the commencement of the paroxysm, and remaining upon the stomach, oppresses, and is itself the cause of additional irritation in that organ. There can scarcely be a question, however, that in almost every severe case of remittent fever there are produced in the stomach depraved secretions, in consequence of the already existing irritation or inflammation, and as emetics would rather tend to increase than to allay this, they would in all probability add to the evil they were intended to remove. The results of experience are strikingly confirmatory of this opinion; for, how few are there, who have had opportunities of observing their effects, without condemning their administration; and, for the reason, that the very symptoms named by Dr. Wood as indicating their use, are invariably almost, aggravated by their operation. Indeed, were the practitioner to be guided by the instructions of Dr. Wood on this point, emetics would be remedies of every day administration—instead of being reserved for exceptional cases merely, and the indications for their administration would be strengthened by each repetition; for we think, that the symptoms named by Dr. Wood, as pointing to their use for the removal of acid accumulations, will be found present in a vast majority of severe or grave cases of remittent fever, that are permitted to reach a third or fourth exacerbation. They are: "a feeling of epigastric oppression, severe nausea, and frequent and ineffectual attempts to vomit; the patient now and then discharging a mouthful of very sour sharp colorless fluid, or of bitter and acrid bile."

The remarks upon cathartics are in the main judicious, though perhaps a somewhat more free administration is advised, than in many instances would be necessary or even safe. We are satisfied from the result of observation, that from half an ounce to an ounce of the sulphate of magnesia, given six or eight hours after a medium dose of calomel, as recommended by the author, would as a general rule operate too actively; at least upon the easily irritated bowels of Southern patients. "After the bowels have been thoroughly evacuated," observes the author, "it will be sufficient as a general rule, during the remainder of the complaint, to keep them open once or twice daily." To this, none can object we think; but, in the South, the judicious practitioner would often hesitate to administer the quantity of purgative medicine recommended daily for this purpose; for instance ʒ i of magnesia, a half an ounce or less of sulphate of magnesia, or an equivalent of some other article, though no doubt necessary in Philadelphia.

The remarks on venesection will meet, if we are not mistaken, with general approval. Indeed had Dr. Wood spent years of his life in the treatment of remittent fever, they could not have been more to the purpose. We copy a few passages.

"It (bleeding) is wholly powerless in the eradication, or even in the control of the febrile movement. The force of the pulse may be reduced, and the strength of the body exhausted, and yet the fever shall not have abated one iota of its violence, or its duration. The only legitimate object of venesection in remittent fever, is the prevention of organic injury from inflammation, or local determination of blood." * * *

"Blood may be taken from the arm in patients previously healthy and of vigorous constitutions, when the pulse is full, strong and tense, the face flushed, and the pain in the head considerable. * * * * * From 12 to 20 ounces may be taken at once; the caution being always observed, to stop the flow as soon as a decided impression is made on the pulse or any marks of faintness appear."

* * * "In doubtful cases, it may be proper to bleed cautiously, stopping the orifice after taking a few ounces to ascertain the effect, and, if the pulse be found to flag, and the apparent prostration to increase, then to abandon the measure altogether; but, under opposite circumstances, to resume it, and continue till the requisite quantity has been lost."

Diaphoretics, such as Dover's powder, spiritus mindereri, antimonials and effervescing drafts, are favorite remedies with our author, in the treatment of remittent fever. He remarks:

"Some authors speak slightingly of these remedies. But when we consider that nature very often brings about a partial or complete solution of the paroxysm of fever by sweating, the inference appears very reasonable, that we should favor this result, by promoting her own favorite process."

To this there can be no objection; but, is it certain that the articles enumerated by the author are the best calculated to effect the object proposed. When we consider, that in a vast majority of cases of remittent fever, if not indeed in every case, a high degree of inflammation of the stomach is present, either from the beginning, or is developed early in the attack; when we consider that this very inflammation must ,in proportion to its extent, increase the difficulty of producing a favorable diaphoresis,—that upon it indeed must in many instances depend to an extent the continuance of the febrile heat, the imperfect state of the remissions, and consequently the lack of perspiration; and when we also consider that the articles named are more or less calculated to augment the existing inflammation, either from their inherent properties, or from the irritation resulting from their mere presence in the quantity in which they were given—we think there are legitimate grounds for doubt. And when we consider further, that, in remittent fever, one of the most distressing attendants of the disease, generally, is such excessive nausea with irritability of the stomach, that often a few teaspoonfuls of water, even, will not be retained over a few minutes; and that the greatest tact and nicest management are required, and often unsuccessfully employed, to insure the retention of even the most important agents of cure; and when we consider further, that the articles named are all calculated to increase this nausea and vomiting, and tend to render the retention of really curative agents impossible, other cogent reasons are presented for their administration with a cautious and sparing hand. Dr. Wood's favorite effervescing draught, prepared, as he directs, with fresh lemon juice, and the carbonate of potassa, is perhaps less mischievous than most of the other articles named,—and its substitution for them, where the propensity to be administering cannot be resisted, is of course an advantage to the patient. It is curious to observe the young practitioner, settled in a malarious locality, prepared with numerous recipes for diaphoretic combinations, in the utility of which he has, in the treatment of remittent fever, the most unbounded confidence, ceasing to use them, one by one, as successively the utter inefficiency, to say the least of some, and the probable mischievous tendency of others, become manifest in the course of practice.

In regard to the use of mercury in the treatment of remittent fever, the observations of the author are highly valuable and judicious, and we feel persuaded that the young practitioner, who takes them for his guidance in the administration of the remedy, will seldom have cause to regret its use. While warmly advocating its administration, in moderate doses, he objects to the larger quantities, as, at least useless, if not hurtful. He admits of course that, however carefully employed, owing to idiosyncrasies, patients are sometimes violently affected by it. "But this is no reason for abandoning its use altogether. There is no efficient remedy of which the same may not be said."

"Mercury," observes the author, "is after the lancet infinitely the most powerful antiphlogistic remedy in our possession. Now, most fatal cases of bilious fever probably become so, in consequence of the disorganizing effects of the inflammations that attend them. Yet such is the state of the system, that bleeding very frequently cannot be pushed to the point necessary for the eradication of the inflammation. * * * * Whatever may be the explanation of its action, the fact of its beneficial influence rests upon experience.

* * * * * "When the disease is violent from the outset, does not show a disposition to yield to the remedies employed—or, when it assumes a dangerous aspect in its course, there will always be a propriety in administering it in reference to its constitutional effects. * * * * * The immense quantities in which it has been sometimes employed are altogether unnecessary. Only a certain amount of the medicine can find access into the system from the alimentary canal, and all the rest is either inert or a source of irritation. * * * * * From half a grain to two grains may be given every hour, two, or three hours, according to the urgency of the symptoms, and the known susceptibility of the patient. * * * * * When the stomach is very irritable, doses of only one eighth of a grain, given every half hour or hour, and regularly persevered in, will often have a more decided constitutional effect, than fifty times the quantity."

The results of our own experience are in accordance with the observations of Dr. Wood;—indeed we are of opinion, that in many instances we have seen the best effects from the administration of small doses, when larger ones have seemed to have a most mischievous tendency; and one principal rule by which we are governed, in its use, is, that the quantity should be diminished, in proportion as the gastro-intestinal irritation is increased; the increase of the irritation from the large doses, rendering its constitutional effect more difficult and uncertain.

Even in mild cases,—recollecting the tendency of malarious diseases to assume suddenly and unexpectedly a pernicious character,—we prefer the calomel in most instances as a laxative, where opening medicines are necessary, administered with the view, however, to its incipient constitutional effect, and of ultimately profiting by this, should the case prove obstinate or protracted. It is true, however, that under a judicious treatment, in a very great majority of instances, the disease will be arrested before the constitutional effect of mercury can be induced; but now and then we do meet with cases, in which the main or only hope, is in the mercurial impression.

Dr. Woods opinion as to the condition of the system, necessary for the safe administration of quinia,—principally influenced it would seem by a disinclination to relinquish the old opinion, as to the excitant and tonic action of the medicine,—is much the same, as that which has been long taught, and even at present entertained by many, who highly adorn the profession, but whose experience with its use, in most instances, owing to the circumstance of position, has been necessarily somewhat limited. He says—

* * * * "In ordinary cases it will often shorten the duration of the disease, if given in the remission, after this has become very decided, so as almost to amount to an intermission."

It is its action upon the brain that he fears principally, "because it is upon that organ that it operates most powerfully;" and under certain circumstances, in which otherwise he would admit the urgent necessity for its use; "symptoms of cerebral inflammation, or strong determination," are viewed as positive contra-indications. Perhaps there are no circumstances in which the beneficial influence of quinia is more strongly manifested, or in which it is better borne in full doses, than in a certain class of cases in which there is often considerable determination to the brain. Witness its almost *talismanic* influence over the violent and pernicious cases, with coma, stupor, &c.; in some of which, at least, there are the most reliable and certain evidences of determinations to this organ present.

In those cases to which it is admitted to be applicable, from 18 to 24 grains, the author thinks, will generally be sufficient for cases not falling under the title of malignant or pernicious. In a vast majority of instances, more we think is unnecessary, but where the paroxysms occur at long intervals, as in the tertian type, it may be prudent to prescribe a larger quantity.

"The doses," says the Author, "must be regulated by the length of the remission. If this be short, they must be very large, and if of a few hours duration only, the whole quantity must be given in two or three doses. If the remission be long, the medicine should be equally distributed through it, care being taken, that the whole shall have been administered two or three hours before the expected paroxysm."

Bearing in mind the *insidious* character of miasmatic disease and the tendency of the paroxysms to anticipate, more especially in the more violent cases, we think a better plan is, to administer two or three pretty full doses in quick succession in the early part of remission; and with smaller quantities at longer intervals, retain the patient under influence of the remedy the requisite period.

But there are circumstances, in bilious remittent fever, which render quinia of the utmost value. When a paroxysm of great virulence has occurred, from which the patient has been saved only by the most strenueous exertions, and there is every reason to fear that a similar one will prove fatal, recourse should be had to the sulphate of quinia in the remission, however imperfect or short it may be."

In this recommendation we presume all will concur. The treatment of miasmatic fevers, so far as the influence of medicine extends, is one of prevention, in a measure, rather than of removal. How little the control that we are able to exert over a paroxysm already formed, and how trifling the influence of medicine in its removal, or in preventing its disorganizing influences, compared with the power we possess of preventing a recurrence?

The conditions, it will be remembered, commonly urged as contra-indicating the use of quinia, generally in remittent fever, and concurred in by Dr. Wood,—by those who oppose the practice,—are the existence of local lesions, and the continuance of considerable febrile excitement during the remissions. Now, it will, we think, be admitted as a general rule, liable to exceptions perhaps, that the remissions are less perfect, and the local lesions more profound and extensive, in proportion to the violence of the previous paroxysm. With what consistency then can those who so strenuously oppose the general practice of the administration of quinia in the ordinary milder cases, or early in the disease, before the development of great violence in the paroxysms, merely because there may exist some slight local lesions, or that the remissions are not so decided as almost to amount to intermissions—advise it in the more violent cases, such as above referred to by Doctor Wood, in which the remissions must be less perfect, and the local lesions more profound and extensive? Could we reasonably expect else, consistently, to be advised by them under the circumstances named, than a redoubled diligence merely in the administration of the effervescing draughts, diaphoretics, and other favorite *antiphlogistics*, for the purpose of bringing the remission to the requisite approach to an intermission?

We present briefly Doctor Wood's remarks on the treatment of the more protracted cases.

"It was stated in the description of the disease that, if a favorable change did not take place from the ninth to the twelfth day, it was apt to alter its form, and assume many of the symptoms which characterize the advanced stages of the common enteric or typhoid fever. In such cases the tongue being dry, the skin dry, and the secretions generally deficient, it is always advisable, if the patient has not already been salivated —(if he has, the symptoms enumerated, we think, will rarely present themselves. Rev.) to give the mercurial pill, in the dose of a grain every hour or two through the day, until the gums begin to exhibit some signs of its effect. Should diarrhœa exist at the same time, as often happens, about a sixth, or a quarter of a grain of opium, with the same quantity of Ipecacuanha may be given with each pill. Should any tendency to the paroxysmal form be observed, the sulphate of quinia should be administered."

The oil of turpentine, which "acts as an alterative upon the inflamed and probably ulcerated mucous membrane of the bowels," in the dose of from ten to twenty drops every two hours, is a favourite remedy with the author, in these protracted cases; and he also suggests the use of the nitrate of silver, for a similar purpose under the same circumstances. "Should pressure upon the abdomen detect a tender spot, a few leeches may sometimes be applied advantageously; and in all cases where there is diarrhœa and a tympanitic abdomen, this should be kept constantly covered with a large emollient cataplasm to which a little mustard may be added, sufficient to sustain a slight feeling of warmth." When the system sinks into a very prostrate condition, serpentaria, ammonia, wine whey, mulled wine, milk punch, &c., with external stimulation, by means of the hot bath, sinapisms, cayenne, &c., are named as appropriate remedies.

The author thinks, "that many attacks of bilious fever might be avoided, by taking a mercurial cathartic, when the epigastric uneasiness, which so often precedes the disease, is experienced, and afterwards following the purgative with quinia, in the quantity which would be necessary to prevent a paroxysm." We are inclined to believe, however, from some observation in regard to the subject, that any perturbative measures will as often cause the immediate development of the disease, where it might otherwise have been avoided, as prevent its occurrence. If the prophylactic course suggested by Dr. Wood, however, could always be so timed, that after the calomel, the necessary quantity of quinia might be administered prior to any febrile movement, the desired effect would probably be gained. Entire abstinence from food, the avoidance of all exposure, cooling drinks, rest and quiet, where the premonitory symptoms are slightly felt, are the prophylactic measures, upon which we are inclined to believe that reliance should chiefly be placed.

Dr. Wood prefers the term *pernicious*, to that of *malignant* or congestive, "because not having been generally applied to other diseases it may be received as designative of a particular morbid state of great danger, to the exclusion of others, which may be equally dangerous, but in a different way."

"It may be asked why make a distinct affection of what is nothing more than a modification of an ordinary disease? The answer simply is, that its extreme danger, and yet often easy curability, when early recognised, render it desirable that the practitioner should have a vivid impression of its character and importance, which may be best given by treating of it distinctly; while the danger of false pathological views may be readily guarded against, by due explanation."

However inappropriate the term congestive, (and we see no objection to it, used in connection with such explanations as are proper to show that it is not intended to designate a *primary* morbid condition, but a state resulting from some previous derangement, obscure in its nature, of the nervous system probably,) we do not think that the term *pernicious* can in strict propriety be substituted for it; for the reason, that there are many cases comprehended under this latter term, as used by the writers of the continent of Europe, in which not one of the phenomena characterizing, what in the South is understood as the state of congestion, is present. For instance, many of the cases of the comatose remittent are of this character; for though classed among the pernicious fevers, instead of the pulse being small and frequent, as in congestion, it may be full and slow; instead of the extremities being shrunken, clammy and cold, they may be warm, dry, and the capillary circulation active. So indeed with all the other symptoms. Dr. Wood himself says, in regard to these cases, that the pulse is full, and though generally somewhat accelerated, is much less so than in other cases, and occassionally is even slower than in health. It often too has considerable strength." Again, "the organic functions are at first comparatively unaffected, the heart often continuing to act with energy, and the surface to retain its warmth, when the patient is quite insensible. That the affection is chiefly nervous is to be inferred from its periodicity. There *may* often be congestion; there may sometimes be inflammation of the brain."

The pernicious miasmatic fevers have been considered as presenting both the intermitting and remitting types, and in addition Dr. Wood supposes the disease to be occasionally continued. This latter is in all probability an exceedingly rare form; and *strictly speaking*, the former is also, we are disposed to think, of much less frequent occurrence, than is generally supposed. We have seen but few such cases, and indeed, after the occurrence of one *pernicious paroxysm*, it would seem improbable that there should occur, in the short space between it and the time for a recurrence, such an entire restoration of the functions to a healthy state, as to admit of that normal condition of the surface, pulse, &c., constituting strictly an intermission.

From the symptoms of pernicious fever, as given by Dr. Wood, we extract a few passages.

"The disease exhibits different phenomena, according to the direction of the morbid innervation. Thus, in some cases, the organic functions are especially affected; in others, the animal. In the former, the evidences of disease are presented chiefly in the organs of digestion, respiration, calorification, circulation and secretion; in the latter, most prominently in the brain."

In the first, "there is almost invariably a feeling of epigastric weight and oppression, with tenderness upon strong pressure; and often of intense internal heat, with excessive and unquenchable thirst. This sense of burning heat and thirst is among the most striking symptoms, when taken in connection with the positive reduction of temperature over the greater portion of the surface, and sometimes even within, as indicated by the tongue and breath."

Our experience in some respects does not accord with that of Dr. Wood. In a very considerable proportion at least, if not all the cases of congestive fever which have fallen under our observation, we may observe, that, though the extremities have been exceedingly cold, clammy and shrivelled, the surface of the chest and abdomen has generally been very considerably above the healthy standard; sometimes exceedingly hot. Indeed, in many instances we think it may be observed, that the heat of the surface of the body and the complaint of internal heat and thirst are in a measure proportionate to the coldness of the extremities. It is difficult to separate the cold stage proper, in many instances, from the period of febrile exacerbation, for it is during the latter, that the coldness of the extremities is frequently the greatest; the development of heat, often so excessive, being confined to the abdomen and chest. It is during this time, too, that the sensation of internal heat and the thirst are most distressing, and the action of the heart most forcible. The period of remission, we are inclined to think, has by some writers been mistaken for that of the exacerbation. In the remissions, as the *coldness* of the extremities diminishes somewhat, the *heat* of the body also diminishes in the same proportion; the sensation of internal heat and thirst becomes less distressing, and the force of the heart's action abates. We do not recollect at this time to have observed, as a general law, anything in the state of the tongue or breath, indicating a reduction of temperature internally, as stated by Dr. Wood to be the case, while we have had such evidence of the existence of great internal heat during life, as its presence discovered in examinations a few hours after death, may be supposed to give. That the patient does not complain of coldness of the extremities, considering their real temperature, may seem strange, but impressed, as we have heretofore been, with the belief that a great degree of morbid internal heat existed, we have thought that this, and the other distressing sensations, as thirst, nausea, epigastric oppression, &c., have been so overpowering, and the attention so concentrated upon them, that the coldness of the extremities has merely not been noted. We remember no case, in which the patient has actually complained of heat of the extremities themselves, when they were cold. We proceed with our quotations.

"The state of the respiration is often highly characteristic. The breathing seems like a succession of deep sighs, and occasionally each respiration is interrupted in its progress, and effected as if by a double effort. * * * * * Occasionally, however, the respiration is panting, hurried and irregular. The patient complains that he cannot get his breath, and desires to be fanned, or longs for fresh air."

The action of the heart, according to the experience of Dr. Wood, is feeble; indeed, according to the mechanism of the formation of congestion, as he has explained it, this would seem the necessary commencement, the first step, so far as the mere accumulation of blood is concerned. He says: "generally the heart, though contracting very frequently, for reasons explained elsewhere, has appeared to me to be feeble, rather than energetic." On turning to page 172, to which the author refers us, for the *reasons why* the action of the heart has appeared to be feeble rather than energetic, we find the following remarks, to which we presume he alludes as embracing the "reasons."

"In diseases of depression and debility, the pulse is sometimes slower than natural; but very often it is more frequent. This generally arises from the existence of some source of irritation, in the midst of the depression or debility. Indeed, it often happens that debility is one cause of extreme frequency of pulse. A certain supply of blood is demanded by the functions, and the heart, being too feeble to act forcibly, is driven to excessive frequency of contraction in order to compensate for its want of strength."

We may be mistaken in supposing the above paragraph to contain the *reasons why, in congestive fever, the action of the heart has seemed to be feeble rather than energetic, to Dr. Wood,* but we find nothing else on the page to which he has referred, having any bearing seemingly on the subject.

Perhaps the most satisfactory *reasons* which the author could have given, would have been a mere statement, that in certain cases, which he had carefully examined with reference to this question, such had actually been the condition of the heart's action. In that case, there would have been no necessity for supporting his position by mere theoretical reasoning, based upon what we must designate false analogies; and by those who entertain a different opinion, it would remain only to question the genuineness of the cases, or to admit that in congestive fever the action of the heart may, at least in some instances, be feeble rather than energetic.

Dr. Wood's "reasons" which we have just copied, it will be observed, imply the existence of real and considerable debility. But, do the circumstances connected with, and the phenomena attendant on congestive fever, indicate a state of general prostration, or is this consistent with the fact, mentioned by Dr. Wood himself, that "the patient will sometimes walk about his room, hours after the pulse has ceased to be felt at the wrist," or with the circumstance that the disease is one frequently of sudden occurrence, in patients with whom no debilitating treatment has been adopted, and in persons not previously exposed or subjected to debilitating influences? The author also remarks that "the weakness of old age, or previous disease, does not appear to constitute a predisposition, for" (he continues) "according to Dr. Parry, the greater number of fatal cases is between 25 and 35, and among the plethoric and robust." We may here remark also, that the author elsewhere, attributing, very correctly we have no doubt, the occurrence of congestion to "the peculiar state of the innervation," admits that this "does not consist in a universal prostration of the nervous power. On the contrary, while defective in relation to certain functions, it may be unimpaired in others." Still more correctly we believe he might have said, *augmented in others.*

We have alluded to the explanation given by Dr. Wood of the mechanism of congestion, and we will now present a few extracts in which it is exhibited.

"The congestion *necessarily follows the prostration* of the active circulating forces. The pulmonary capillaries, the *heart* and the systemic capillaries are all enfeebled; the blood, therefore collects in the veins and in the great internal organs." * * * * Nothing is more common than the occurrence of this condition, in diseases attended with great and sudden prostration. *The heart participating in this prostration, is unable to transmit the blood so rapidly as it is conveyed towards it*, by the continued action of the capillaries, and by the forces which move the blood in the veins. *This fluid therefore necessarily accumulates in the right side of the heart and in the great venous trunks.*" * * * * * "We see it" (congestion) "in syncope when all the blood deserts the capillaries, and becomes concentrated in the veins and great organs. We see it in concussion of the brain in no less degree than in pernicious fever." (?) "We behold it in all cases of violent shock upon the nervous system—*prostrating the powers of that system and consequently of the heart*, as in severe surgical operations and violent injuries."

The rationale above given, of the mode of the occurrence of congestion, or internal accumulation of blood, is exceedingly pretty and plausible; but it is of course only applicable to the congestion of syncope, and the analogous conditions mentioned by the author, including such cases of congestive fever as he has seen, in which the action of the heart was feeble rather than energetic; but it is obviously inapplicable to cases in which the action of the heart is not enfeebled, and to certain cases, the existence of which is implied, in the admission of Dr. Wood, that this condition * of the capillaries may co-exist with considerable power of the heart; for the want of innervation is not necessarily equal in the whole circulation;" and in this—"it is possible that the heart may have its natural share and even more than its natural share

* Here is the condition referred to. "In the pernicious fevers, the innervation of the extreme vessels fails, and they cannot therefore perform their part effectually in the circulation. *The blood enters them with difficulty in their enfeebled state, and is carried through them very slowly.* Hence the paleness; and hence also the lividness of the surface, owing to the stagnation of the blood. From the same approach to nervous death in these vessels, they allow the watery portions of the blood to ooze through them, almost as through dead membrane. Hence the profuse sweats. The coldness obviously arises from the languid circulation and deficient change of blood." In a former quotation it is stated that, "the *heart participating in this prostration* is unable to transmit the blood so rapidly as it is conveyed towards it, *by the continued action of the capillaries*," &c. Page 290.

of nervous energy, while the innervation of the extreme arteries and capillaries may be defective." Now, as the cases of congestive fever which we have observed, have been connected with the latter condition, (we could show too that this is not contrary to the experience of others) it would have been most gratifying had the author given us an explanation also, of the mechanism of the formation of congestion, consistent with the existence of this state of the heart's action; admitting for ourselves, that as yet we have not been able to fashion out one in accordance with this fact, at all satisfactory.

The discrepancy of opinion, existing among different observers in regard to the degree of force with which the heart acts in congestive fever, may perhaps be reconcilable by the supposition, that in different instances, very opposite states of the system have served for the accounts given. We admit of course that we have met with cases of miasmatic fever, in which the action of the heart was feeble; but, if we recollect aright, invariably it was in connexion with a state of *general prostration* of the system, from age, feebleness of constitution, various debilitating causes, and especially, an active evacuating and debilitating treatment, adopted for the cure of cases, at first of but very moderate severity. In such instances, the phenomena presented must of course be different from those characterizing the state of congestion, as occurring in congestive fever,—attacking as it does, as is shown by the quotations of Dr. Wood, generally the young and plethoric; considering too, that "the weakness of old age or previous disease does not appear to constitute a predisposition." In the one instance, the peculiar phenomena are mainly the result merely of real prostration or debility, and may bear in reality no inconsiderable analogy to a state of approaching syncope; in the other they are, it would seem, owing chiefly to the concentrated action of the one morbific agent. The one appears to be a state of real prostration, a general defect of nervous influence; the other, a redundant supply of nervous influence to some one part or parts, and a proportionate defect to others. There are, unquestionably, cases in which the phenomena of congestion are present, in connection with those of a state of more or less real prostration, but even in these instances, the action of the heart will be found *comparatively strong*.

Dr. Wood gives no detail of the symptoms of the particular cases observed by himself, in which the action of the heart was feeble rather than energetic, and it is necessary to the formation of a proper judgement, that the course and symptoms should be minutely recorded. There is reason to believe that mere prostration, in cases of fever, has sometimes been mistaken for the state in the South and Southwest termed *congestive*; and phenomena, principally dependent on the former, placed to the account of the latter. We cannot resist the impression, nor yet its utterance, in spite of the high estimate we are compelled to place upon the attainments and experience of Dr. Wood, and the profound respect which these, together with his position, inspire—that, notwithstanding the "*reasons*" which he has given *why* the action of the heart has appeared feeble in the disease under consideration, his opinions, in regard to the congestion of pernicious fever, in so far as they are based upon the observation of cases of syncope and other instances of pure nervous prostration,—are really of little value.

Dr. Wood tells us that he "never saw a case in Philadelphia or the neighbourhood, except in the hospitals, among sailors recently from the Southern coast, or among medical students from the Southern and Southwestern States."—It is one of the peculiarities of the miasmatic poison, that in many instances it remains dormant, as it were, in the system for a length of time, and its effects become developed, perhaps, some time subsequent to, and in situations remote from the place of its reception. It will, however, we think, be considered highly improbable by those generally most familiar with its effects, that in many of the cases, in which the quantity and degree of concentration of the poison received are sufficient for the production of well marked congestion, it would remain dormant in the system the length of time implied above; though, where merely sufficient for the production of ordinary intermittents and the less pernicious remittents has been received, this might reasonably be expected to be of more frequent occurrence. Now, if in connection with this, the antiphlogistic and evacuating propensities of our Northern confreres generally, in regard to this disease, be borne in mind; their fondness for purgatives, antimonials and other debilitating measures, for the removal of local lesions (which are almost invariably aggravated in each recurring paroxysm,) and to make the "remission almost amount to an intermission," and the consequent protraction of their cases, before venturing upon the administration of the anti-periodic, we think it will be scarcely doubted, that the opportunities of Dr. Wood, for the observation of cases of remittent fever, with general collapse or prostration, have been much more numerous than for those attended with decided congestion.

But this digression has become too much extended, and we return to the *symptoms* for further extracts.

* * * * * "There are often great restlessness, general uneasiness and jactitation. The patient, not aware of his extreme danger, and sometimes wondering at the anxiety exhibited by his attendants, often attempts to rise from his bed, and, if not prevented, will walk to the window or door of the apartment. The direct cerebral functions, including the action of the will upon the muscles, are singularly undisturbed, in the midst of the wreck of organic life. The patient will sometimes walk about the room hours after the pulse has ceased to be felt at the wrist."

After the detail of symptoms, from which the preceding extracts have been made, marking the disease, the author notices "some of the more prominent diversities," in one of which particular allusion is made to a weakened state of the heart's action, as especially characterising it; though from the general tenor of his previous remarks on the subject, it will, we think, be the opinion of many, that this very state of the heart is considered by him a *sine qua non* to the existence of congestion at all; necessarily the first step in the mechanism of its production.

"It may be proper," observes the author, "to notice some of the more prominent diversities, in the variety of the disease above described; that, to wit, in which the organic functions are chiefly concerned. *Sometimes, the force of the disease appears to be directed especially to the heart, and the prominent phenomena are those of excessive prostration of the circulation.*" * * * *

„ In other cases, the coldness is the most prominent symptom, gradually deepening as the paroxysm advances, and at length occupying almost the whole surface, without any primary extraordinary reduction of the pulse, and without disorder in the digestive functions. The heart at length gives way and the patient perishes in the first or the second paroxysm."

The following remarks are well calculated to impress upon the mind of the practitioner the importance of a correct diagnosis. Of course, an error of the kind mentioned by Dr. Wood, could only occur where the disease is of very rare occurrence, and under the care of one but very little accustomed to the treatment of miasmatic diseases.

" It is of the utmost importance to be able to distinguish this from the ordinary forms of miasmatic fever; because, the safety of the patient depends upon the adoption of prompt and vigorous measures, which are not deemed necessary in other cases. When fully formed, the affection can scarcely be mistaken if the practitioner be on his guard. Its possible occurrence in any instance of bilious fever should be borne in mind. I have no doubt whatever, that death frequently arises from a want of this caution. Cases of the kind have, indeed, fallen under my own observation. In the course of a bilious remittent, symptoms of a pernicious character appear, and yield, perhaps, after a time, either to the remedies employed, or in the regular course of the disease. The amendment is very naturally supposed to be the commencement of convalescence, and no extraordinary measures of safety are resorted to. But the apparent retreat of the disease is only the preparation for a more vigorous onset, and, when the onset comes, in an unguarded state of the defences, it is irresistible."

The morbid appearances mentioned by Dr. Wood, are given from the examination of thirteen cases, by M. Maillot. We discover nothing of special interest. The condition however of the liver, it may be as well to name, as the " anatomical characteristic" of remittent fever, described by Dr. Stewardson, does not appear to have been present, or if so, not noted. "The liver was variously affected, being either red and soft like the tissue of the spleen, or enlarged, *yellowish, dry and brittle*; or of enormous size, and engorged with blood; or softened to the consistence of paste; or finally quite healthy." We call attention to the appearance alluded to in the words which we have italicised, as being probably the same as that noted by M. Louis, in the yellow fever of Gibraltar.

The remarks of Dr. Wood generally, relative to the *nature* of the disease, are exceedingly interesting; and are as satisfactory as such explanations commonly are, or, as the obscurity of the subject will perhaps admit. We extract a few paragraphs.

" What is it, that imparts its peculiar character to pernicious fever? Can it be inflammation? Is it possible that this process can be extinguished and relighted again so suddenly? It has been suggested, that, the general excitability being exhausted by the violence of the paroxysm, the inflammation may continue to exist in the interval, and yet be unable to bring the system under its influence. But who can conceive of an acute and fatal inflammation of the mucous membrane of the stomach with a healthy appetite, or of the brain, with a perfectly sound intellect, and without headache or other uneasiness? Inflammation may in a certain degree co-exist with the other phenomena; but it constitutes in most cases no portion of the danger; for all the difference between death and a speedy restoration to health, lies in a few grains of quinia. Who has ever administered quinia in ordinary cases of gastritis, or any other inflammation with similar results?

It is in the peculiar state of the innervation, that we are to look for the source at once of the symptoms and the danger. This does not consist in a universal prostration of the nervous power. On the contrary, while defective in relation to certain functions, it may be unimpaired in others. Let us apply this view to the explanation of the symptoms. In the first place, in relation to the cases of collapse, in which the organic functions are especially concerned. This is prominently characterized by a want of action in the capillaries and extreme arteries. Some suppose that these vessels are spasmodically contracted. There is no evidence whatever of the existence of such spasm. They collapse simply because they contain no blood, just as they collapse in death. All parts of the organism receive a certain supply of nervous influence which is essential to the due performance of their functions. The extreme vessels are probably not less under that influence than other parts. * * * * In the pernicious fever *the innervation of the extreme vessels* fails, and they cannot, therefore, perform their part effectually in the circulation. The blood enters them with difficulty, in their enfeebled state, and is carried through them very slowly."

We decidedly concur in opinion with Dr. Wood, that it cannot be inflammation which stamps the peculiar character upon pernicious fever; though we believe, that there are few cases indeed, in which, in a greater or less degree, it is not present in some one organ or other; but we are tempted almost to doubt, whether in any case deserving the term pernicious, "a healthy appetite" is ever present between the paroxysms. Further,—we feel certain almost that the question, "who has ever administered quinia in ordinary cases of gastritis, *or any other inflammation* with like results?" was written by the author in a fit of momentary forgetfulness,—at least, if by the expression "similar results" is meant, a decided and marked curative influence; for, we cannot for a moment suppose that Dr. Wood is not aware of the extensive application of this remedy, which is made in the treatment of inflammations. Has it not become a common remedy in the treatment of acute inflammatory rheumatism? Indeed, is it not *the* remedy principally relied on in this disease by some? Innumerable instances of its successful application, to the treatment of the *common* inflammatory diseases might be cited. We will simply refer to two cases of *meningitis* successfully treated with it, mentioned in a valuable paper, "on the poisonous properties of the sulphate of quinine," published in the American Journal of Medical Sciences for April, 1847, by Dr. W. O. Baldwin, of Montgomery, Ala.

If fault can possibly be found with the author's observations on the *treatment* of congestive fever, it is in his recommendation of certain remedies, which, many we are assured, of the most extended experience in the management of the disease, will deem of very questionable utility, as for instance the acetate of lead, kino, oil of turpentine, &c.,—at a time, (for in all cases of congestive fever, this is generally the case) when it is with the greatest difficulty that the really important remedies can be retained on the stomach :—All this however is outweighed by his recommendation of the liberal use of opium,—and the following:

"As soon as a remission or intermission has been obtained, there is but one course of treatment, and that is all important. There should be no delay for previous treatment; no waiting for a more perfect relief from this, that, or the other symptom. Such dallying has but too often been fatal. No matter whether the patient has been under treatment or not during the paroxysm, no matter how partial the remission, provided it be a remission, no matter at what period of the interval the practitioner may have been called; his first, his last, almost his only thought should be sulphate of quinia."

Perhaps in consequence often of the brevity of the remissions, the possibility of anticipation, and the difficulty now and then of detecting the remissions, it might not be safe to make it an invariable rule to wait for a remission before commencing the administration of quinia.

The third article of the first class, is devoted to *yellow fever* of which we have an excellent account, corresponding pretty well,—a few points excepted,—with the details given by many of our best and most experienced authors.

"It is an interesting fact" observes Dr. Wood, "that it does not occur in all countries where the circumstances apparently favorable to its production, exist in an equal degree. Thus, while it is exceedingly common in the sea-port towns of intertropical America, and not unfrequently in those of South-Western Europe, it is almost unknown in those of Asia, Eastern Africa and South-Eastern Europe. We never hear of it in Canton, Calcutta, Alexandria, Smyrna, or Constantinople, while every one is familiar with its ravages in Vera Cruz, Havana, New Orleans, Gibraltar and Barcelona."

"It seldom or never shows itself as a prevailing disease in a scattered population, or at a distance from navigable waters," but is confined almost exclusively to places where human beings congregate; as to ships, garrisoned forts, and towns upon the sea-coast, or upon streams emptying into the ocean. To these statements of the author, however, a few rare exceptions might be named.

The initiatory symptoms, as given, present nothing clearly distinguishing the commencement of the attack, from that of several other febrile affections. They are chilliness, more or less marked; subsequently a hot and dry skin, flushed face, thirst, nausea and vomiting, furred tongue, oppression at the epigastrium, headache, and perhaps delirium; and severe pains in the back and limbs. The latter, Dr. Wood considers "among the most characteristic symptoms of the incipient stage." In a circumstance mentioned by the author, to wit: the frequent commencement of the attack in the night, it would seem to differ from the miasmatic fevers; the latter, more frequently it is believed, being ushered in during the day.

We quote at some length the symptoms given, as being present after the perfect development of the disease.

"These febrile symptoms continue usually with little or no remission, for a period varying from a few hours to three days, and sometimes even longer. The duration is shorter in the more violent cases, and longer in the mild; and in the latter, is sometimes extended to four or five days, with a more decided tendency to remission. Having run its course, the fever subsides, and a great apparent amelioration of the disease is experienced. The skin becomes cooler and softer, the pulse nearly or quite natural, the respiration calm, and the stomach comparatively quiet. The headache and excruciating pains in the back, if not previously relieved, disappear; and the patient, freed from the distress of body and mind, becomes comparatively cheerful and hopeful, and not unfrequently confident of recovery. It is not unusual to find him sitting up, either in or out of bed, and to be told by him that he is quite well. But this is a delusive calm. Sometimes, indeed, convalescence dates from the subsidence of the fever in mild cases; but generally the great struggle is yet to come. This apparent amelioration is not in any respect comparable to the remission or intermission of miasmatic fever. The disease still continues unabated. It is only that the febrile phenomena have disappeared under the failing powers of the system. The struggle against the noxious influence has ceased for a time. The continuance of the fever would be a favorable rather than an unfavorable sign; as it would evince a greater ability of the system to cope with its ferocious adversary.

There are phenomena even during this temporary calm, which evince the existence of undiminished danger. Upon pressure in the epigastrium, the tenderness, instead of being diminished, is found to be greater even than before. The redness of the conjunctiva, and the flush of the face may be gone; but in their place is often a yellowish or orange color, which gradually extends itself from the forehead and eyes to the face, neck and chest, and ultimately in a greater or less degree over the whole body. The urine also has a yellow tinge, which even though the discharge may appear in mass of a dark brown color, may be detected when it is in thin layers. The pulse is sometimes even slower than in health, and has been known to descend to 40 in a minute. In bad cases, there is sometimes a little heaviness or stupor. This period of apparent abatement may continue but a few hours, or may be protracted for twenty-four hours.

Another class of phenomena now ensue; those, namely, of debility or prostration. In severe cases, the weakness is extreme. The pulse is quick, irregular and feeble; the skin is yellow, orange, or of a bronzed appearance; the blood appears to be often nearly stagnant in the capillaries, so that when removed by pressure with the finger from the portion of the skin, the color returns very slowly; the dependent and extreme parts of the body, as the fingers, toes, scrotum and back, become of a dark purplish hue; the tongue is now often brown and dryish in the centre, or smooth, red and chapped; and sordes occasionally collect about the gums and teeth. The stomach resumes its former irritability; every thing swallowed is thrown up again, and a new matter is ejected, consisting of brown or blackish flakes or particles, diffused in a colorless liquid, which may be at first slightly tinged by them, but ultimately becomes black and opaque. In very malignant cases, the condition of system above described, may come even as early as the first day; and occasionally the extreme capillary prostration, with the purplish skin, and a pulse scarcely perceptible at the wrist ensues, while the heart and large vessels are still beating tumultuously. The urine, often scanty and high-colored during the fever, is now sometimes nearly natural—sometimes almost or quite suppressed, and occasionally, though rarely, retained. At this stage of the disease, hemorrhage occasionally takes place from various parts of the body, especially from the mucous membranes. Blood oozes from the gums, the fissures in the tongue, the fauces and the nostrils. It is sometimes also vomited or discharged by stool, or with the urine; and petechiæ and vibices, arising from its extravasation, appear upon the skin. The irritability and extreme distress of the febrile stage are now replaced by an extraordinary apathy, and the countenance expresses a quiet resignation or gloomy indifference. The pulse at length almost ceases; the respiration becomes slow, sighing, and occasionally interrupted by hiccough; the skin assumes a cold and clammy feel; the bowels often give way and discharge large quantities of black matter, similar to that ejected by the stomach; low delirium sets in; an offensive odour sometimes exhales from the whole body; the eyes become sunken and the countenance collapsed: and death takes place, often quietly, but sometimes in the midst of convulsions. Black vomit, yellowness of the skin and hemorrhage, have been mentioned as attendants upon the last stage; but patients often die without them.

Instead of pursuing this fatal course, the system very often reacts after the period of abatement, and a secondary fever sets in, which may be of various grades of violence, but may always be regarded as a salutary effort of nature, or at least a sign that the vital energies are not yet exhausted." * * * *

"From a review of the course of yellow fever as above described, it will appear that it has usually three distinct stages. The first is that of the primary febrile action, which continues from a few hours to several days, on the average, perhaps, from thirty-six to sixty hours. The second is that of subsidence or abatement, in which the exhaustion of the excitability leaves the system in a state of temporary repose, and which may continue, in a greater or less degree, from twelve to twenty-four hours. The third stage is that of secondary fever or collapse, according as the system has or has not the strength to rally under the depressing influences to which it is subjected."

"When convalescence takes place from severe cases of yellow fever, it is commonly tedious, in consequence of the amount of repair which is necessary to restore the dilapidated organs."

It may not be irrelevant here, to observe, that in the Medical History of Alabama, a paper recently published by Dr. Lewis, of Mobile, who has unquestionably seen much of yellow fever, we find it stated that, "as a general rule recovery is very rapid, frequently resulting in a condition of health greatly better than that which existed previous to attack." This is merely one of the innumerable discrepancies found between the statements of different authors who have treated of yellow fever.

The preceding account is descriptive of the more common course of the disease; certain "diversities" are enumerated of which we can only spare room for one, which the author observes is "in the fashionable language of the day, denominated congestive."

* * * * "The state of system is asthenic from the commencement; the pulse being very frequent and feeble during the febrile excitement, or the general strength being inadequate to rally from beneath the force of the first blow, and the symptoms at the beginning being those of great prostration, especially of the whole capillary system. * * * * * Universal weakness, with obscure and, as it were, paralytic pains in the back and lower extremeties, and a sense of weight or stupefaction in the head; the skin dry, unctuous or perspiring, without tone and without heat, unless near the centre of the body; the pulse sometimes frequent and sometimes full, but always feeble and occasionally almost wanting at the wrist, while the heart and carotids may be throbbing tumultuously; the face, pale or purplish, with an expression of countenance either stolid or apathetic, or such as usually indicates a feeling of horror or intense agony."

By the way, how does Dr. Wood reconcile a tumultuous throbbing of the heart in this congestive form of yellow fever, with his reason, why, in the congestive cases of miasmatic fever, the heart is feeble rather than energetic in its action; seeing that the *reasons*, when examined, may be resolved into this, that feebleness of the heart's action is, according to his theory of its formation, necessarily the first step in the mechanism of congestion?

The *yellowness of the skin*, in this disease, is attributed by Dr. Wood to the same cause, as that to which he attributes its occurrence in remittent fever, to wit, a change in the blood, giving rise to an excessive production of the same coloring principle that imparts yellowness to the bile. When the color is deeper, presenting a hue of bronze, it is "ascribable to its admixture with the dark red color of the stagnant blood."

The black vomit "is not generally observed before the second or third stage. The matters ejected from the stomach are at first such as have been swallowed, and afterwards usually a little bile, probably from the gall-bladder, and the vitiated secretions of the stomach itself." * * * "The mode in which it is thrown up is different from ordinary vomiting. It appears to gush forth apparently without effort on the part of the patient, and sometimes almost without his consciousness; being not unfrequently discharged upon the bedclothes. Sometimes it is ejected by mouthfuls, by a sort of regurgitation, and portions of it come up occasionally with hiccough."

It is the opinion of the author, that "the vessels of the stomach really modify the blood in its passage through them," and that it is in this manner that the black vomit is produced. That this matter is however produced merely by the admixture of blood discharged into the stomach with the acid secretions of this organ, we are almost inclined to consider an established fact, from the results of certain experiments performed by Dr. Nott of Mobile; an account of which appeared in the American Journal of Medical Sciences for April 1845. In these experiments the matter was found to possess an acid reaction, and this fact had been also previously observed by others;—but by the admixture of muriatic acid (the acid principally secreted by the stomach) with blood, a substance, identical in appearance, and seemingly in every respect, with black vomit, was produced. Professor Harrison, too, of New-Orleans (New-Orleans Medical and Surgical Journal, Sept. 1845) entertains a similar opinion, the result, it would seem, of somewhat similar experiments. "In some cases," he remarks, "the vomit can be distinguished in nothing from blood in an uncoagulated dissolved state." * * * * "A fluid, so like it as to deceive the most experienced persons, can be artificially formed by pouring a little hydro-chloric acid upon blood." * * * * "I once," he further observes, "with Dr. Thomas Hunt of this city, performed the following experiment. A man was brought into the dead-house, while we were there. Upon examination, there was no black vomit in the stomach, but a whitish, acid smelling liquid, amounting to about half a pint. Into the stomach, containing this liquid, some blood from the vena cava was poured. At first, we thought the experiment had failed, and returned to other investigations. Upon examining the fluid, however, after the lapse of ten or fifteen minutes, it was impossible to distinguish it from specimens of black vomit, with which it was contrasted." It is further stated by the same gentleman, that by the black vomit, litmus paper is turned red, and tumeric paper which has been changed by an alkali, is restored to its original color. These facts, taken in connection, can leave scarcely a doubt, we think, as to the nature and mode of formation of black vomit.

In regard to *post mortem* appearances, it may be observed, that many of them are similar to those found in some other febrile affections. It was supposed, after the observations of Louis on the yellow fever of Gibraltar in 1828, that an appearance, anatomically characteristic of the disease, had been discovered, in the peculiar, anemic, dry condition and yellowish color of the liver, which he describes as being present in all the cases which he examined. Subsequent examinations however prove, that this appearance is far from being invariably present.

Dr. Wood discusses, at some length, the long and warmly mooted question of the identity of yellow fever and remittent fever—the negative of which, as the reader is already aware, he espouses; and indeed the weight of testimony, resulting from the observation of the more recent authors, decidedly inclines to this view of the matter. Disposed to the same opinion, we cannot however consider the question by any means entirely and satisfactorily settled; and it must be admitted that many of the arguments presented will by no means bear examination.

There is, perhaps, no disease respecting which, the accounts of authors are more discrepant than of yellow fever; the statements of one writer flatly contradicting frequently those of another; and symptoms mentioned by one, as principally characterizing the disease, are said by others rarely to be present, or perhaps to be characteristic of some other disease. Nor, is this discrepancy confined respectively to the closet book-maker and the experienced and practical observer, but may not unfrequently be observed between the statements of writers of equal attainments and of equal opportunities for observation. Why is this? Is it that *different diseases* have actually been described by different observers, under the one name? or, is it that the modifications and shades of *one disease* of varying features, have, on different occasions, supplied the outlines of the accounts given.

"A very prevalent hypothesis has been that the causes of yellow and bilious fevers are identical,—in other words that the former disease, as well as the latter, proceeds from marsh miasmata. The chief arguments in favor of this notion are, that the two diseases prevail in hot countries and at the same season of the year; that, when the yellow fever becomes epidemic, the bilious fever is also apt to be peculiarly rife; that acclimated individuals are less liable to the attacks of both, than the unacclimated; and finally, that no essential difference exists between the symptoms and course of the two diseases."

"It is true that the two fevers prevail in hot countries and in hot weather. So do cholera, dysentery and hepatitis; yet no one considers these as identical with yellow fever. But though heat is essential to both, their localities and other circumstances in relation to their prevalence are very different. Bilious fever occurs abundantly, and even malignantly, in many situations, where yellow fever is never seen. Thus, the former disease is not less prevalent nor less fatal in Asia, Eastern Europe, and Eastern Africa, where the latter is seldom heard of, than in the West Indies and other parts of tropical America, where it is very common. Bilious fever is quite as prevalent and quite as violent in the interior of miasmatic countries as near the sea; yellow fever seldom or never occurs at any considerable distance from the sea-coast, or the borders of navigable streams. The latter disease is almost always confined (there are however exceptions to this. Rev.) to a dense population, as for example to cities and garrisons; one of the most striking circumstances in relation to the former is, that its cause appears to be neutralized by the atmosphere of cities."

Now, by very much such reasoning as the above it might, we think, be made equally apparent, that the causes of intermittent and remittent fevers, as also, of the pernicious forms of these diseases, were different; for, it is well known, that one of these may affect chiefly one locality; another, another locality, owing perhaps to differences in the quantity and concentration of the poisonous agent, and certain modifying influences. Thus, it is a well known fact that intermittents may prevail extensively in certain localities and seasons, where and when intermittents are rarely seen, and it is stated by Dr. Wood himself, on the authority of Dr. Parry, that, "while ordinary bilious fever occupies the table lands, the pernicious form has been observed to prevail especially in the low grounds skirting the rivers. If *the cause* of remittent fever be neutralized by the atmosphere of cities, it is well known that the *disease itself* is vastly prevalent in some of them; and though yellow fever most generally prevails in cities near the sea-coast, as remarked by the author, there are not wanting well authenticated accounts of its occurrence in very different situations. We think the epidemic of 1844, in the village of Woodville, Miss., which also affected the residents of the plantations of the country around, may be presented as an example.—Dr. Hort, in the New Orleans Medical and Surgical Journal, for July 1845, makes the following statement. "I have seen the yellow fever in Florida, *fifty miles from the Gulf,* * * * * * where the black-vomit was forcibly ejected five or six feet, and where it could be traced *to a local cause on the plantation.*"

"One attack of bilious fever,"—proceeds the author,—"so far from giving any future exemption, is well known to dispose to a second attack. The case is exactly the reverse with yellow fever. This disease seldom occurs more than once in the same person. It is true that this fact is denied by some; but it appears to me, that no impartial person can read the testimony upon this subject, and refuse credence to the statement. Second attacks of yellow fever are probably less frequent than second attacks of small-pox."

The circumstance of the different degrees of exemption conferred by attacks of the two diseases, does not appear to be entirely irreconcileable, we think, with the supposition that yellow fever is but a higher grade of miasmatic disease; for, though several mild attacks of remittent fever frequently follow each other in quick succession, it is a fact, that one violent attack of this disease confers generally a long immunity; and the *testimony* examined by Dr. Wood, must certainly have been all on one side. which led him to the conclusion, that, "secondary attacks of yellow fever are probably less frequent than second attacks of small-pox." Professor Harrison of New Orleans, in an article already alluded to, says he has known several who have been attacked a second time; while Dr. Lewis of Mobile, (Medical History of Alabama, page 58) says: "there can be no question that some persons have the disease the second, and some the third or fourth time." The opportunities of these gentlemen for observation, in regard to this question, have been, we are assured, of the very best character. The latter, for some seven or eight years, has had an extensive practice in Mobile, while the opinions of the former have the sanction of thirteen years practice in the city of New Orleans,—during ten of which he was connected with the Charity Hospital,—and of several hundred post-mortem examinations.†

It is in the type, we are inclined to think, that the most striking difference, between yellow fever and the miasmatic fever, is to be found; for, while in the latter there are almost invariably intermissions or perceptible remissions, the latter is generally now spoken of as being of a continued character, or at least lacking anything like a regular periodicity. It may be remarked, however, that there is even some discrepancy among writers on the subject of yellow fever, in regard to the latter statement; and Dr. Wood himself alludes to milder cases of yellow fever, in which "the febrile stage is extended to four or five days, with a more decided tendency to remission." A few cases of miasmatic fever unquestionably occur, in which it is exceedingly difficult to detect the remissions.

"It is not true,"—again we quote from Dr. Wood,—"that the symptoms of the two diseases are the same. Though perfectly familiar with bilious fever, when I first saw a case of yellow fever, I was at once struck with the latter as something I had never seen before. The febrile stage of yellow fever is continuous, like that of small pox or measles, for one, two, or three days, and then ceases, while bilious fever has a tendency to remission or intermission, every day or every other day, to the end of the disease. In the latter, the secretion of bile is usually increased, in the former diminished."

But is it not a fact, that in a large proportion of the cases of bilious fever of the higher grades, the secretion of bile is diminished, or entirely suspended? We have seen in this disease the most violent efforts to vomit, continued for several days, with scarcely an interval of rest of an

† New Orleans Medical and Surgical Journal—September, 1845. Page 129.

hour's duration, without the evacuation of one particle of bile. Though the secretion of bile may in yellow fever also be diminished or suspended in many cases, is this of sufficiently general occurrence, to be deserving of any consideration in the diagnosis, or as establishing a distinction between bilious fever and yellow fever? On the contrary, there is frequently in yellow fever a profuse secretion of bile, so much so, that it has actually been considered one of the distinguishing characteristics of the disease. In the New-Orleans Medical and Surgical Journal, for November 1845, we find it stated by Professor Harrison, that the passage of bilious stools, during the first days of yellow fever, is a common occurrence. In the same Journal, for July 1845, Dr. Kilpatrick, in an account of the yellow fever which occurred in Woodville in 1844, speaks of "great quantities of bile" being discharged; and in the September number of the same Journal, in his account of the same epidemic, Dr. C. H. Stone actually mentions "the profuse secretion of bile" as one of the circumstances, in which the yellow fever differs from our ordinary fevers; and further, in allusion to the yellow fever, states, that it appears to him "to be *the only truly bilious fever.*" He also mentions particularly the case of a patient who "discharged immense quantities of bile from the bowels for a week." We continue Dr. Wood's account of the chief points of difference between the two diseases.

"Though it is possible that the turbid conjunctiva and purplish flush of the upper part of the face, so common in yellow fever, may occur in some cases of the bilious fever, they are uncommon in the latter, and I have never seen them in an equal degree."

The fact, that in bilious fever, the symptoms just named are not present, "in an equal degree," as in yellow fever, will be considered of course very natural by those who have adopted the view of the identity of the two diseases, seeing that with them the latter is looked upon as merely a *higher grade of disease than the former.*

"Gastric inflammation, though common to the two diseases, is much more striking in the yellow fever."

Let us examine the testimony on this point. Of eight cases of yellow fever examined in 1843, remarks Doctor Nott, (American Journal of Medical Sciences, April, 1845,) the stomach, "in four, presented no appreciable change." "Of eight cases, in 1844, the mucous coats of the stomach in three were perfectly healthy." "Though," observes Professor Harrison, (New Orleans Medical and Surgical Journal, September, 1845,) "in a great majority of cases, the stomach is finely injected with blood," * * * * * "in some other cases, though these are far more rare, the stomach, duodenum and other intestines, present us with an almost entire absence of appreciable lesions." Dr. P. H. Lewis, (Medical History of Alabama,) says, that he has not, "with a few exceptions, met with such lesions as would warrant the conclusion, that the stomach had been inflamed." Now, we know of no author, who, in speaking of the condition of the stomach after death from *bilious fever*, says, that the evidences of inflammation of this organ have been wanting, in a larger proportion than seven out of sixteen cases, which was the proportion in the examinations of Dr. Nott. Indeed, there are in all, or nearly all of the more severe cases, during life, the most reliable evidences of its presence.

"The black-vomit of this disease is entirely different from the black discharges of bilious fever, which are homogeneous, and consist of altered bile, not altered blood."

This is a somewhat difficult point to handle; the ejection of a black matter from the stomach, being, in most instances, of itself considered decisive as to the character of the disease. It is then yellow fever.—Frequently, in bilious fever, matters of various shades, but evidently altered bile, are thrown up, and such, we are informed by various authors, is also now and then the case in yellow fever; but, in certain instances of fever, occurring in places remote from situations where yellow fever prevails, a matter is occasionally thrown from the stomach in the manner precisely in which it is said the black-vomit of yellow fever is ejected, in appearance corresponding exactly with the descriptions of this matter, as given by the best writers on the subject of yellow fever. Now we know of no one who has actually ascertained that this is altered bile, instead of altered blood. It is but recently that, in a remote country place, we saw a patient in an attack of fever, who had not within a year past been within two hundred miles of any locality where yellow fever is known to prevail, throw from the stomach before death about a pint of matter, answering precisely to the description of black-vomit, and in the manner, too, in which it is said that this matter is generally ejected in yellow fever. There was no effort to vomit, seemingly no nausea, but the fluid was suddenly forced up into the mouth, and then spit out carelessly upon the bed, the patient lying indifferently on the side or back during the time. Hemorrhage from the bowels had also been present in the progress of the case. We proceed.

"Uncomplicated yellow fever never ends in regular intermittent fever; while this is an exceedingly frequent termination of bilious fever. It has been said that the yellow fever is but an aggravated form of the bilious; but this is not so: many cases of the former are as mild as the mildest of the latter; and bilious fever is sometimes quite as malignant as the worst form of the yellow; yet, in both instances, the characteristic difference of symptoms is observable."

But might it not be urged also, that intermittent fever and remittent fever are distinct diseases, seeing that the former is sometimes as malignant as the worst form of the latter; and that the latter is in some instances as mild as the mildest cases of the former.

"Dissection,"—continues our author,—"presents somewhat different phenomena in yellow and bilious fevers. The stomach is inflamed in both, but in the latter seldom, if ever, contains the true black-vomit.—The liver in yellow fever is often bright, yellow, dry and anæmic; in the bilious it presents wholly different phenomena."

Let us see what others say on this point. In but 6 of the 16 cases of yellow fever examined by Dr. Nott, in 1843 and 1844—to which we have already alluded, did the livers present anything like the appearance named. Two were olive—two normal, and six darker than natural and much engorged. Professor Harrison, who it will be remembered has witnessed several hundred post-mortem examinations of patients who have died of this disease,—in the paper to which we have already had occasion to refer, observes: "There is no organ in the body which presents such various appearances as this,—at times being very dark; in other cases presenting a pale yellow aspect." This latter appearance, it may not be uninteresting to state, he seems to attribute principally to loss of blood, and remarks that "in cases in which the lancet has been freely used, we shall generally find a pale yellow liver." A somewhat similar opinion has been expressed by Dr. Lewis of Mobile, who says, (Medical History of Alabama, page, 57,) that it is confined to those cases of yellow fever, in which the hemorrhage, "either in the shape of black-vomit, or unchanged blood, has been excessive." However, to return to our subject. It may also be stated, that M. Chervin, (quoted by Dr. Nott, in the American Journal of Medical Sciences, for April, 1845,) who made 500 post-mortem examinations of persons who had died of yellow fever, states that this appearance of the liver is often wanting, and that it is not rare to meet the liver in its normal condition in this disease."

This appearance then cannot, it would seem, with propriety be considered distinctive, as it is present only in a limited proportion of cases of yellow fever, and being present also at times in remittent fever, as we will show, might with more propriety be presented as a circumstance in favor of the identity of the two diseases, rather than an argument against it. Thus, it is also stated by M. Chervin that the liver "shows not unfrequently a yellow color in the remittent and intermittent fevers of hot climates." Dr. Lewis, (page 24, of his Medical History of Alabama,) found the liver, in one of 4 fatal cases of bilious fever, which he examined, "dry and brittle, like those dying of yellow fever, and of a pale straw color." Dr. Nott, (American Journal of Medical Sciences, April, 1845, p. 279,) speaks of finding this appearance of the liver in a patient who had died "of protracted bilious fever." In the post-mortem appearances of pernicious fever, detailed by M. Maillot and quoted by Dr. Wood himself,—page 289,—we find it stated, that "the liver was variously affected, being either red and soft like the tissue of the spleen, or enlarged, *yellowish, dry and brittle.*"

"The gall bladder in the latter,"—(bilious fever,)—"is usually distended with bile; in the former,"—(yellow fever,)—"it is seldom distended and often contains less than in health. The spleen in bilious fever is almost always enlarged and softened, in the yellow it is often little, if at all, changed."

The result of some observation inclines us to the belief, that enlargement of the spleen is seldom found in patients affected with remittent fever for the first time, who have but recently settled in a malarious section, and that it is a consequence rather of a protracted and somewhat latent action of malaria upon the system, than of an open attack of fever. Bilious fever is not uncommon among the older residents of malarious districts, in whom enlargement of the spleen already exists, while the grade of disease, called yellow fever, seldom occurs except in the unacclimated. This appearance of the spleen in bilious fever,—as in the few cases of yellow fever in which it may be noted,—we are disposed to believe in most instances, if not in all, to be pre-existent.

In regard to the state of the gall-bladder, the diversity of opinion among authors is not less marked, than as to the condition of other parts.

That it would frequently be found distended,—contrary to what Dr. Wood says is the case generally in yellow fever,—is rendered probable by the profuse secretion of bile which we have seen is so often present in the disease. In 15 of Dr. Nott's 16 cases, the gall-bladder was found to contain bile, varying in quantity from half an ounce to four ounces.

Dr. Wood thus sums up the diagnosis; admitting however, that for the first day or two it is difficult to establish, owing to the initial state of most fevers having so many symptoms in common.

"The severe pains in the loins and lower extremities, the turbid conjunctiva and the darkish red suffusion of the upper portion of the face; * * * * * at a more advanced period, the excessive irritability of the stomach and epigastric tenderness; the regular continuance of the fever, and its subsidence, after a duration of one, two or three days; the supervention of yellowness of the skin and eyes, when the fever subsides; the great prostration, or the febrile reaction, which follows the subsidence; and finally the occurrence of black-vomit, are among the most characteristic symptoms."

As we have already observed, it is the type of the fever, we are inclined to think, in which consists the most constant and marked feature of difference between this disease and remittent fever; and the reader will not fail to observe, that the greater number of the symptoms above enumerated may with propriety also be considered equally among the most characteristic symptoms of the latter disease. We may mention especially the pains in the loins and lower extremities, the yellowness of the skin and eyes, and the epigastric tenderness and excessive irritability of the stomach. In regard to this latter symptom, indeed ,it may be stated, that Dr. Lewis says, that there "are less nausea and retching than in bilious fever,"—and again,—"as a general rule there are far less vomiting and irritability of stomach during the first stage of yellow fever, than in that of bilious fever."*

The question of the contagiousness or non-contagiousness of yellow fever is ably discussed, and impartially too, by Dr. Wood, and testimony pro. and con. fairly presented. It would seem, that, notwithstanding the conclusion of Mr. Chervin to the contrary, after the most laborious and indefatigable research upon the subject,—the number of those who believe the disease to be contagious have recently increased. The author himself is of opinion, that the doctrine of contagiousness of yellow fever, in the ordinary meaning of that term, is quite untenable.

In regard to the *treatment* of yellow fever, as given by Dr. Wood, we need say but little, as it is almost identically the same as that advised by him in remittent fever, of which we have already presented to the reader some of the more interesting portions. We can discover a few points of difference only, and these are unimportant. The sulphate of quinia, we may observe, is not recommended in the latter by Dr. Wood, as in remittent fever, with special reference to its antiperiodic properties, though enumerated among the appropriate tonics, when the system shows signs of sinking; but it will be remembered that in remittent fever this remedy is prohibited by Dr. Wood prior to the reduc-

* New Orleans Medical and Surgical Journal—Vol. I. Page 209.

tion of the excitement, so far that the *remissions shall almost amount to intermissions*, by which time, in most instances, in the latter under the general treatment recommended by Dr. Wood, tonics also become necessary, so that the circumstances, accordingly, under which its use becomes proper, are in the end very much the same in the two diseases. The acetate of lead, too, we believe mentioned in the treatment of yellow fever, is not spoken of in the treatment of remittent fever. As, however, in the former it is merely advised in consequence of the presence of gastritis, and for the cure of this complication; and as this is one of the most frequent complications also of remittent fever, it might of course with equal propriety be prescribed in the latter disease. Nitre and antimonials he considers less appropriate in this disease than in remittent fever, "because more apt to produce gastric irritation;" but when we consider, that in remittent fever this is one of the most troublesome symptoms generally present, and that, with every care and precaution for its prevention, it frequently supervenes to an extent, beyond which, any augmentation is scarcely conceivable, the restrictions of their use, it must be admitted, would seem as appropriate to one as the other. With these exceptions, the remedies advised are precisely the same as those recommended in remittent fever; under the same indications, in the same order and with the same restrictions. They are, it will be remembered,—emetics; the lancet; mercurial cathartics, followed in the course of the disease with saline and other laxatives; mercury for its constitutional action; cold water externally applied; various diaphoretics, as Dover's powder, serpentaria, spirit of nitric ether and the effervescing draft; and blisters and stimulants. Indeed, without any injury that would probably result therefrom, that we can perceive, the position of the entire remarks of the author on the treatment of the two diseases might be reversed. This similarity, or rather identity, as regards the practice recommended, will probably suggest itself to those who are inclined to this view of the question, as another circumstance that might be urged in favor of the identity of the two diseases.

The fourth article in the class under consideration is on *typhoid fever;* to which term, however,—though sanctioned now by pretty general usage,—the author objects, (because, as he states, "independently of the fact, that the complaint is not essentially typhoid, and that it very often runs its whole course without any symptoms analagous to those which characterize typhus fever, there is the strong objection that any other febrile disease may equally assume the typhoid form; so that a mere epithet, applicable to a common condition of disease, is thus appropriated to a distinct complaint and must inevitably lead to misconceptions;" and prefers the term *enteric fever*, which is merely intended to express the fact, "that this fever is distinguished from all other idiopathic fevers, by the frequency and extent of intestinal disease. Other fevers are attended occasionally with disease of the bowels; this almost always, if not essentially. The intestinal affection is as characteristic of this disease as the eruption is of small-pox."

The progress and symptoms together, of the disease, are well described, after which, the more prominent phenomena, as, for instance, diarrhœa, tympanitis, the rose colored eruption, &c., are seperately considered, and the value of each in diagnoses discussed.

We copy a few remarks, descriptive of a peculiar appearance of the tongue which occurs in some cases, which, from a special treatment, based principally upon it by the author, it is well to remember.

"Instead of cleaning gradually from the edges, the tongue throws off its fur in flakes, generally at first from the centre or towards the base, leaving the surface smooth, red and somewhat shining, as if the papillary structure had been partially destroyed. This state of the tongue is sometimes preceded by soreness of the fauces; and the velum pendulum and half arches will, if examined, be found covered with an exudation, which they are beginning to part with. This is usually a sign of an approaching amelioration of the symptoms. If the tongue when thus cleaned remain moist, convalescence may pretty confidently be expected, though it is always tedious. In some instances the tongue coats itself over again, and again it becomes clean, and this change may take place more than once. Occasionally, too, an apthous eruption appears upon its surface. But still, if the moisture continue, the progress is ultimately favourable.

If, however at any time, during the above cleaning process, or even after it has been completed, the tongue should become permanently dry, the symptoms are again aggravated, and the patient again thrown into danger, I have ascribed this result to an increase in the intestinal disease, which is a prominent feature in the complaint, and have found it to yield most happily to a treatment addressed to that affection."

Though in all cases in which "the tongue remains dry and the abdominal distension undiminished," the author thinks the *oil of turpentine* in the dose of from five to twenty drops, every hour or two, in an emulsion of gum arabic and sugar, will prove an excellent remedy, he has found it more especially beneficial in cases in which the tongue presented the appearance just described. In no one instance hitherto in which he has used it, has he known it to fail. "In the course of 24 or at most 48 hours, some amelioration of the symptoms may be observed. The tongue becomes gradually moister and covers itself with a whitish fur; the tympanitic distension ceases to augment, and after a time diminishes; the pulse becomes less frequent, and the skin less dry and harsh; and the patient enters slowly but regularly into convalescence, often without any other remedy."

In a few cases of acute disease, in which we have had reason to suspect the existence of inflammation of the ilium, as a complication, the tongue presenting the appearance above described, we have seen the most prompt and decided benefit follow the administration of Bals. Copavia, in capsules, to the number of five or six in the course of twenty-four hours.

"The *diarrhœa*," observes the author, "undoubtedly depends upon the inflammation or irritation of the intestinal mucous membrane; but has not been found to bear any fixed relation to the characteristic disease of the mucous follicles."

Anatomical Characters. "There is scarcely a single organ of the body, in which signs of inflammation are not sometimes found after death, from enteric fever; for it is one of the peculiarities of this affection, or possibly of the febrile movement, which, in this affection, is of unusual duration, to develope local diseases of an inflammatory nature.

But there are certain anatomical changes, which are especially characteristic of enteric fever, and which are so seldom wanting that they may be considered as almost essential. Such is the affection of the elliptic patches of the aggregated mucous follicles in the ilium, denominated the glands of Peyer. This is quite as characteristic of the disease in question, as the peculiar pustular eruption is of small pox. It has in fact come to be regarded almost as a necessary post mortem test of the existence of the disease."

"Nothing precisely is known of the *cause* of enteric fever. The circumstances of its production are very diversified. It is often certainly generated in situations where human beings are crowded together with insufficient or unwholesome food, and in confined and vitiated air. Hence it appears to originate especially in prisons, badly ventilated hospitals, large cities and ships. Many of the patients brought every year with this disease to the Pennsylvania Hospital, are poor emigrants from on board ships. I have repeatedly known the disease to occur in young men serving as resident physicians in hospitals. It is a well known fact that young persons coming from the country into large cities to reside, are very apt to be attacked with it."

Though many strong facts, as we find it stated by the author himself, have been adduced in support of the doctrine of contagiousness of enteric fever, he has never known an instance "in which it could be clearly shown to be the result of contagion," and believes that, "if contagious at all, it must be so only feebly and under peculiar circumstances."

It would appear from the observations of Louis, Chomel and others, that this disease rarely occurs in individuals above the age of forty, or under that of puberty. In regard to the latter statement, however, the author observes that he has repeatedly seen it in children under ten, and accounts for the opinion by the supposition, that the observations upon which it is based were made in hospitals, into which children are not admitted. That it is seldom observed in the aged, Dr. Wood supposes to be owing to this, (seeing that it is generally admitted not to occur twice in the same person,) that, "those who are susceptible to the disease, will be likely to have it before they have attained middle life." This explanation, however, we cannot consider as entirely satisfactory, for we see numerous instances of other diseases, which attack an individual but once, even those more generally confined to infancy and youth, as for instance scarlatina and measles, attacking the aged. In many sections, too, typhoid fever is almost entirely unknown; and were the explanation of Dr. Wood correct, indeed, had not advanced age a considerable influence in some way, in diminishing the susceptibility to the disease, is it not probable that it would frequently attack those of mature age of such sections, when visiting or having removed to the places of its more general prevalence?—as, for instance, from country places to cities.

Nature. That the disease is not, as has been supposed, a mere gastro-enteritis, has been proved by dissection. "Another opinion considers the disease, though differing from ordinary enteritis, as consisting essentially in the peculiar state of these glands; all the other phenomena resulting from this affection, just as the fever in pleuritis results from the inflammation of the pleura. But this is scarcely more tenable than the other. The general symptoms bear no certain relation in intensity to the degree of the local affection. It has indeed never been proved that this begins with the disease ; and there is reason to believe that, in many cases at least, it may not commence till several days after the fever. Nor does there seem to be any necessary connection between the intestinal affection, and various other symptoms, such as the frequent occurrence of epistaxis at the commencement, the great tendency to stupor, the rose colored eruption, &c. * * * * * It is not indeed certain that this follicular affection is absolutely essential. As to the real nature of the fever, we are in the dark, as we are in fact in relation to all the essential fevers."

The fifth, sixth, seventh, eighth, ninth, tenth, eleventh and twefth articles of the first class of Dr. Wood's book, are respectively on typhus fever, plague, small-pox, vaccine disease, chicken-pox, measles, scarlet fever and erysipelas. Considering, however, the length to which our notice has already been extended, and finding but little in the articles named of sufficient interest, or rather sufficiently peculiar to the author to arrest attention, we will pass them without comment. Though we had marked off much interesting matter, with the intention of presenting it to the reader, we are also compelled to omit any notice, or for the present at least to defer a notice, of the other two classes, embracing though they do much the larger portion of the volumes. In drawing our notice of the work of Dr. Wood to a conclusion,—at least for the present,—we would be doing injustice to the accomplished and industrious author, were we to neglect an acknowledgment of the high gratification, as well as instruction, which we have derived from its perusal. It would perhaps, savor somewhat of presumption, were we to venture to recommend it to the more industrious and reading portion of the elder members of the profession ; and for these, extended and elaborate monographs are indeed more appropriately adopted ; but to the student, and more especially to the American student, we unhesitatingly recommend it as peculiarly suited, and as being the best epitome of what is valuable in our knowledge on the subject of practical medicine now extant. The style, which is simple, chaste, concise, to the point, and entirely devoid of anything like an ostentatious display of words,—bears evidence of the utmost clearness of thought on the part of the author. The work really is, as is claimed for it in the preface, something more than a mere compilation. Much is given as the result of the author's own observation and experience, and in the application even of the labors of others to his use, there is a thorough revision, a perfect and yet concise elaboration, an appropriateness of selection and nice adaptation of parts, evincive of a degree of labor and care, far beyond that ordinarily bestowed on works of a similar character. In this respect, how favorably does it contrast with some that have recently preceded it, which, instead of giving the views and experience of the (soi disant) authors, to any useful extent, are almost entirely composed of verbatim extracts from standard works of the day, and from the various medical periodicals, strung together until a sufficient mass has been accumulated to form a book, the only original part of which is perhaps the title page, on which appears conspicuous the name of one as author, whose share in the production has been confined principally to an industrious use of the editorial scissors, *and a careful correction of the proof sheets.* Perhaps in most instances, in such productions as we have allusion to, it will be found that the sources from whence the materials have been obtained, may be acknowledged by quotation marks, or by appending the names of the authors borrowed from, to the parts respectively for which each has been laid under contribution; but to this even, there are exceptions, and it must be noted with regret, that the gatherers of the mingled heterogeneous masses to which we have now and then so far forgotten the rights of others, as to copy from their labors, page after page, without acknowledgement in any shape whatever. That the unwary reader, too, may be deluded into the belief, that there is at least some originality in the production for which he has expended his money, and on which he may waste his time, the device is now and then adopted, of modifying slightly the phraseology of some of the *borrowed* paragraphs, by the substitution of one word occasionally for another, or slightly varying the arrangement of the sentences. That our imagination is not at fault in regard to this matter, should any reader ask, "can such things be?"—we would refer to a work, (the first edition of which was published in Philadelphia in 1842, and which has passed through a second edition since,) entitled, "The Practice of Medicine, or a Treatise on Special Pathology and Therapeutics," as an example, liable in no slight degree to the objections to which we have made allusion. In regard to the first objection which we have urged, a general glance only is necessary to shew to what extent this production is a mere hasty and unelaborated gathering from other works. For an instance of the second objection, to wit, the appropriation without any acknowledgment whatever of the labors of others, we would refer the reader to the diagnosis of Iritis, on page 417 of the second volume, and to the symptoms of the same disease by Dr. Taylor, in the second volume of Twedie's Library of Medicine, page 498, from whence the spoliation is made. For examples of the *original* material entering into the composition of the book, manufactured as we have already observed, by a transposition of sentences which others have written, and the occasional substitution of one word for another, we need not search; open the book and let chance determine the page, as we will now do, and lo! they are present. Accident has opened before us the article on "Purulent inflammation of the conjunctiva," and we will turn to the corresponding part of Twedie's Library of Medicine, headed "Purulent Opthalmia."

From "the Practice of Medicine, or a Treatise on Special Pathology and Therepeutics." First Edition published in 1842.

"Purulent opthalmia occurs under three forms,—the two first, however, are in reality the same disease; and the third is merely produced by a specific cause;—*first*, the purulent opthalmia of the adult; secondly, that of the new born child; and thirdly, the gonnorrhœal."

From Twedie's Library of Medicine. First American Edition, published in 1840.

"Purulent opthalmia occurs under three different forms, which are yet essentially the same disesse; the two distinguished only by circumstances arising out of the age of the patient, the last (gonorrhœal opthalmia) presenting some peculiarities, which are perhaps referable to the specific nature of the cause."

Speaking of the Purulent Opthalmia of the adult,

The author of "the Practice of Medicine," &c., observes:—

"Whilst the inflammation is confined to the conjunctiva, the pain may not be great; but as soon as it involves the deeper seated parts, which do not readily admit of distension, it is at times excessive, generally it is felt chiefly in the orbit, and is of an aching pulsative character, subject to occasional exacerbations."

"Under such severe irritation the constitution sympathises greatly and there is often much fever; if the disease, too, persists for any length of time, the health often suffers. It is very liable to relapse,—and even if resolution takes place the inflammation may give occasion to various morbid conditions of the eye and its appendages—as vascular thickening of the conjunctiva lining the eye lids, with enlargement of its follicles, commonly called *granular conjunctiva*, opacity, sloughing, staphyloma of the cornea or prolapse of the iris, or suppuration and collapse of the eyeball.

Rupture of the cornea sometimes takes place during the violence of the pain. This may occur at an uncertain period from the commencement and afford some relief; but at other times it does not even seem to check the progress of the disease."

In Twedie's Library of Medicine we find:—

"The pain varies in degree according to the textures which are involved; if the conjunctiva alone suffers it is inconsiderable, but when the deeper and firmer tissues are attached it becomes almost insupportable. It is felt chiefly in the neighborhood of the orbit, and is of an aching pulsative character, subject to remissions and exacerbations of variable duration." * * *

"With local suffering of such severity, we find some degree of constitutional spmpathy in the form of fever, and the general health is at times much impaired by the prolonged irritation.

A characteristic of the disease is its tendency to relapse. * * * * Rupture of the cornea, which sometimes occurs during a paroxysm of pain, at variable periods from the commencement of the disease, may afford a temporary relief to suffering, but this is not always the case, as sometimes it does not put a termination to the disease, and scarcely even checks its progress. If resolution takes place the inflammation may issue in the production of various morbid conditions of the eye and its appendages, as vascular thickening of the palpebral conjunctiva with enlargement of the mucous papillæ, commonly called "granular conjunctiva," opacity, ulceration, sloughing or staphyloma of the cornea, or prolapse of the iris."

Turning over a page or two we come to Strumous Inflammation of the Conjunctiva, from which in the appropriate columns we will place a short paragraph from each book.

From "the Practice of Medicine," &c.

"The degree of pain is not often great whilst the eyes are shaded from the light, but should the inflammatory phenomena be considerable, it is frequently urgent especially during the night. The secretion from the eyes in passing over the cheeks gives occasion to redness of the integuments, and the nostrils are often greatly irritated."

From Twedie's Library of Medicine.

"The degree of pain is generally moderate while the eyes are shaded from the light. When the inflammatory symptoms are active it is considerable, especially during the night.—The secretion from the eyes is evidently of an acrid nature, from the irritation it occasions in the nostrils and upon the integuments of the face."

We turn to the next subject,—inflammation of the corea,—for our next extracts.

From "the Practice of Medicine," &c.

"The disease generally commences slowly, and insidiously, and the cornea loses its natural brilliancy, and becomes dull, shaggy,—the surface appearing as if covered with fine dust, or resembling glass that has been breathed upon. * * * *

The fine vessels of the conjunctiva and sclerotica, which is the principal seat of vascularity, being arranged in radii round the cornea and presenting a carmine hue."

From Twedie's Library of Medicine.

* * * "It generally commences slowly and insidiously, the cornea loses its brilliancy, and becomes dull and hazy, its surface appearing as if covered with fine dust, or resembling glass that has been breathed upon. * * * * The fine vessels of the conjunctiva and sclerotica become injected with red blood; those of the latter membrane, which is the principal seat of increased vascular action, are arranged in radii round the cornea, and present a carmine hue."

But why should we weary the reader by multiplying such examples here, when a simple glance through the volumes will serve to show most incontestibly, that there is scarcely a single chapter in the entire work, in which specimens of what appear at least to be the most gross and flagant plagiarism, such as we have presented, may not be found. "There is, it really would seem, in this wholesale, but varied method of appropriating for ones own especial profit, the productions of other men's brains, a something, for which it is difficult to account. Is it in consequence of an "Auri * * * * * fames?"—Most certainly," authors of books of the stamp of which we are speaking, cannot delude themselves into the belief that they are conferring a benefit on the members of the profession, by giving them, under a new name, a crude and undigested mass, collected from this source and that, the different parts of which have already appeared before them in a less questionable shape," and served the purposes intended, in their proper position and connections; nor yet do we think it probable that they can so far deceive themselves as to believe for a moment that they are acquiring, in the way of reputation and character, by such means ,anything which the good and wise would not look upon as most unenviable. There is in a course like this, it does seem, we repeat, a double deviation from that strict and rigid regard for the rights of others, by which some at least would desire to be guided, in their intercourse with the world,—first, in the seizing upon that which properly belongs to another, and using it to one's own profit, and consequently more or less to his detriment; and second, in inducing, by the fascination of a new name to the purchase of that which may already be possessed in its original shape and place. Does not the law recognize, as an offence against it, the obtaining of money under "false pretences?" And can there be a stronger exemplification than is here presented? We have glanced at the matter here for illustration in a pecuniary view, but it might be placed in other lights.

For the purpose of obviating charges of a similar character hereafter, we would with all due deference suggest to the author of "the Practice of Medicine, or a Treatise on General Pathology and Therapeutics," a slight modification in the title page, of any future editions of the work, which might with propriety read thus:—

"*Extracts from Twedie's Library of Medicine, and other Standard Authorities, selected and arranged by Robley Dunglison, M. D., &c.*"

But perhaps we have been over hasty, and have made an inconsiderate use of the word "plagiarism," when all might be explained on the score of mere *coincidence* ; and, as an instance in point now occurs to our mind, where an author satisfactorily exculpated himself from a similar insinuation on something like the grounds we have named, it is but justice to Professor Dunglison, in the fear that we have done him wrong to mention it, that he may have the benefit of the precedent. We allude to the case of *Mr. Puff*, author of a tragedy called "*the Spanish Armada.*" The reader will please remember that Messrs. Sneer and Dangle are witnessing, with Mr. Puff, in front of the curtain, the first rehearsal of his play.

"Enter (on the stage) a Beefeater."

Beefeater. "*Perdition catch my soul, but I do love thee.*"

Sneer. Hav'nt I heard that line before ?

Puff. No, I fancy not—where pray ?

Dangle. Yes, I think there is something like it in Othello.

Puff. Gad ; now you put me in mind on'it, I believe there is,—but that's of no consequence—*all that can be said is, that two people happened to hit on the same thought—and Shakespear made use of it first—that's all.*"

We know not whether it should be looked upon as a fortunate circumstance, or a subject of regret, that in so many instances other writers hit upon the thoughts of the author of "The Practice of Medicine," &c., and made use of them first. In one point of view, at least, the circumstance may be looked upon rather unpleasant,—or, would be so to most men,—since it may lay him under the imputation of plagiarism ; but most fortunate for him at least, (having an eye to lucre,) if not for his readers, if so it is, that previous publication, (as some perhaps may think to be the case,) alone could have incited him to the efforts of authorship, and that otherwise all that he has accomplished would have remained latent, still retaining its fluidity within the reservoir of ink from whence the ideas flow.

W. M. B.

NOTE.—The foregoing able Review was sent in early for publication in our last number, but owing to the demand upon the first part of that number, was crowded out.—EDS.

II.—*On Disorders of the Cerebral Circulation ; and on the connection between Affections of the Brain and Diseases of the Heart.* By GEORGE BURROWS, M. D., Late Fellow of Coin's College, Cambridge ; Fellow of the Royal College of Physicians, London ; Physician and Lecturer on the Principles and Practice of Medicine, at St. Bartholomew's Hospital. With Colored Plates. Lea and Blanchard. Philadelphia. 1848.

The anatomy of the brain and its diseases have recently excited a great deal of attention. The science of Phrenology has pushed our inquiries into the most minute structure of this important organ. Were this the only good that the science had produced, it would amply reward those, who had laboured to build it up as a distinct science. With a more thorough knowledge of the anatomy of the brain, we are now enabled to understand, and treat the various diseases of this organ with greater certainty and success. It has been remarked, that in a cultivated and commercial community, when the mind is actively and intently engaged from day to day, cerebral affections are of frequent occurrence and difficult to manage. As a primary disease, we are not prepared to admit the truth of this remark ; but we believe, that in the summer diseases of our city, the brain participates sooner or later in the morbid action going on in the system ;—hence, delirium, stupor and coma, usually characterize the last stages of our summer and autumnal fevers.

With these introductory observations, we leave this interesting subject, and turn now to an analysis of Dr. Burrow's instructive work, on the "*disorders of the cerebral circulation,*" &c.

The main object of Dr. Burrows in writing this book, seems to have been to refute certain theoretical opinions, first advanced by Drs. Kellie and Alex. Monro, and afterwards adopted and defended *in extenso*, by Abercrombie and Clutterbuck.

Deceived and misled by some experiments well conceived, but bunglingly executed, Dr. Kellie was induced to believe, and boldly asserted that the brain contained at all times and under all circumstances both of health and disease, nearly or exactly the same amount of blood. We shall not pause here to reproduce the experiments which led him to such a conclusion ; but we shall state the inferences he deduces from these experiments.

1st. "That a state of bloodlessness is not discovered in the brains of animals which have died by hemorrhage ; but on the contrary, very commonly a state of venous cerebral congestion.

2d. That the quantity of blood in the cerebral vessels is not affected by gravitation or posture of the head.

3d. That congestion of the cerebral vessels is not found in those instances where it might be most expected ; as in persons who die by hanging, strangulation, suffocation, &c.

4th. That if there be repletion, or depletion, of one set of vessels (arteries or veins) in the cranium, there will be an opposite condition of the other set of vessels."

With commendable zeal and industry Dr. Burrows instituted a series of experiments upon inferior animals, in order to test the truth or demonstrate the fallacy of the conclusions drawn by Kellie, from his experiments on the cerebral circulation.

We need scarcely say (for if the reader will follow us up,) he will soon find that Dr. Burrow's experiments triumphantly refute every proposition advanced by Dr. Kellie, and led him to adopt the very opposite conclusions.

So great was the influence of Monro, Kellie, and Abercrombie, that few, until the day of Burrows, were bold enough to question the opinions of this great trio on the subject of the cerebral circulation. Happily for humanity these *false* facts have had but little influence in the treatment of cephalic disease on this side of the Atlantic; for since the days of Rush, we have continued to abstract blood both locally and generally to relieve certain cerebral affections.

The three authors above mentioned, as well as those who adopted their views on the circulation of the blood in the brain, maintained that the cranium was a complete sphere of bone, which was completely filled by its contents, thus excluding the influence of atmospheric pressure, except what might be communicated through the blood-vessels that penetrate the brain. In an organ thus situated, they asserted it to be highly improbable that the quantity of circulating fluid could be materially augmented, unless a rupture of some vessel should take place and thus make room for an additional quantity of blood. They denied that posture had any influence in producing cerebral congestion. Dr. Burrow's experiments and daily post-mortem examinations, demonstrate the contrary to be true. How does apoplexy produce paralysis, hemiplegia and death, if the brain does not admit more blood in its vessels than they can contain? It is matter of surprise that men of sound minds and great research should embrace views so utterly opposed to common sense and the experience of the profession. Admit the doctrine that the amount of blood in the brain is invariably the same, and how meagre would become our means of relieving apoplexies, phrenitis, hydrocephalus, cerebral congestion and all that class of encephalic affections, ending or beginning in a state of hyperæmia. The author whilst on this subject throws out a hint which we deem of so much practical importance, that we shall quote it. He says, "the discovery, (made by his experiments) of the operation made of this force (gravitation) on the blood within the cranium after death, suggests a precaution very essential to be followed when it is desired to ascertain the precise amount of congestion of the cerebral vessels at the time of death. In such cases, he continues, a ligature should be placed around the throat of the corpse, and drawn sufficiently tight to compress the cervical vessels and arrest all flow of blood through them." This precaution will be highly necessary, when from the mode and manner which death has been brought about, we have reason to believe the blood remains fluid in the great vessels.

It has been remarked by able pathologists that we not unfrequently find the sinuses of the brain and other reservoirs for the blood, comparatively empty in persons strangulated by hanging. It was this fact that led Dr. Kellie and others to assert that the cerebral vessels were not found in a state of congestion in those who perished on the gallows. Dr. Burrows, however, explains this fact, by stating that in consequence of the non-compression of the cervical vessels by the cord, the blood, during the suspension of the body, recedes through thin vessels from the brain, and thus leaves that organ comparatively free from congestion.

Besides these vessels, the "vertebral sinuses and spinal plexus of veins" serve likewise to drain the brain of its superflous blood.

It is the usual mode to elevate the head and place it on a block some six or ten inches high in order to remove the calvarium; and whilst in this position, who can doubt but that in many cases of cerebral congestion, when, as is well known, the blood, or that portion of it which remains *fluid*, gravitates from the brain and sinks into the great vessels which are connected directly with the heart. Hence another source of error in post-mortem inspections of the encephalon.

To the reader, these views may appear common place, and so self-evident as not to require even an explanation; but it must be remembered that opinions adverse to these have been advanced and maintained by some writers, eminent for learning and candour. And again, if the brain under every condition of the system contains the same amount of blood, how, we would ask, is syncope produced by copious blood-letting?

It was but a few days since we ordered a robust and healthy male child, aged about seven years, labouring under a violent attack of scarlatina, to be bled from the arm, until incipient syncope manifested itself; when about six ounces of blood had been drawn he was seized with convulsive spasms which excited some alarm among those who did not know that these symptoms were produced by *suddenly* emptying the overloaded vessels of the brain of their contents.

The recumbent posture, and a stimulating enema, soon restored the equilibrium of the cerebral circulation, and the convulsions ceased.

Dr. Burrows, after having given his views on cerebral congestion in his 1st. Section, proceeds, in his 2nd, to speak of *vascular pressure within the cranium, and its influence on the functions of the brain.*

The principle of pressure, he observes, is one of much importance, both in sustaining and destroying the functions of the brain.

When we look at the influence, always reciprocal, which the heart exercises over the functions of the brain, we may cease to wonder that the latter should be subject to such frequent and so great a variety of attacks of diseases. Two causes may operate either separately or together to increase the pressure upon the contents of the cranium;—the one is an increase in the force and frequency of the heart's action, thus driving the blood through the arteries upon the brain faster than the veins can return it to the right side of the heart. Hence constant head-ache, and sometimes apoplexy, result from hypertrophy of the walls of the left ventricle of the heart.

There is a serous fluid, enclosed in the cerebro-spinal arachnoid membrane which has attracted the attention of pathologists; it derives its name from its seat, and is supposed to exert an important—a modifying influence over the functions of the brain and the spinal marrow. It occupies the ventricles of the brain and the entire tract of the spinal canal, travelling freely from one point to another, according to the wants, so to speak, of these organs. May we not reasonably assume that the pressure exerted upon the cerebro-spinal axis, by the cephalo-rachidian fluid, is antagonistic to that produced upon the same organs by the arterial blood, propelled by the action of the heart! If this hypothesis be correct, any material loss of equilibrium between these antagonistic forces may have much to do in producing various head-symptoms, such as cephalalgia, transient delirium, and other nervous phenomena, difficult to explain upon any other supposition.

Dr. Burrows thinks this extra-vascular, or cerebro-spinal fluid or serum, is supplemental to the other contents of the cranium; since it may disappear by pressure or be removed, or taken up by absorption; giving place at one time, he believes, to an increased quantity of blood in the cranium, and at another, supplying any deficit that may chance to exist in the vessels of the head.

We regret that space forbids us following Dr. B. further in his interesting observations upon the "functions of the cerebro-spinal fluid."—He has scrutinized the writings of every author who has commented on the structure and functions of the brain, and his analytical mind clothes the subject with more than ordinary interest.

Speaking of the effects of insufficient vascular pressure on the brain, he observes, that syncope is caused by this want of pressure, and not from the inadequate quantity of blood supplied to the brain and its vessels, as is generally supposed. This seems to us like assuming the effects for the cause, for the pressure must be in direct proportion to the amount of fluids circulating through the brain.

We turn now to a still more interesting and practical part of Dr. Burrows' little work, when he speaks of the "*connection between affections of the brain and diseases of the heart.*" He thinks, and he is correct, that the full extent of the influence of diseases of the heart in disturbing the functions, or producing actual structural changes in other organs, and especially of the brain, has not been thoroughly estimated by writers on the diseases of that organ. True, cardiac affections have, during the last fifteen or twenty years, been carefully studied and pretty generally understood, yet Dr. Burrows is almost the first writer who has called the attention of the profession to the influence exerted by cardiac affections upon the condition of the brain. The first organs which seem most likely to suffer from organic disease of the heart, are the lungs; the next in order and importance, is the brain. Indeed, the nervous and vascular connections existing between the brain and heart might lead us to expect derangement of the latter to produce serious consequences in modifying the functions of the former; such seems to be the case, if the reasoning and facts adduced by our author are entitled to credit. In cases of hypertrophy of the heart, we frequently observe violent head-symptoms, such as cephalalgia, partial deafness, blindness, tinnitus aurium, and other symptoms—the result of the increased momentum of the blood, urged onward through the arterial tubes, by the powerful action of the heart. This increased power of the heart's action sometimes overcomes the tone of the vascular system of the brain, thus producing rupture, extravasation of blood, and death.

Dr. Burrows asserts, that many cases of insanity, if examined with a stethoscope, with a view to ascertain the condition of the heart, will be found, when least expected, to be labouring under some organic disease of the heart;—such an abnormal condition of the central organ of the circulation must necessarily produce disastrous effects upon the brain.

Unfortunately for the interests of this branch of pathology, medical men are usually content to examine the brain alone in cases of death from insanity, neglecting entirely the inspection of the heart and the great vessels directly connected with it.

Should we not be induced to push our post-mortem researches beyond the brain, especially as the lesions, found in this organ after death, are frequently too trifling to account for such an event?

Dr. Burrows has after much labor and research compiled a table, which embraces 132 cases of "apoplexy and sudden hemiplegia, with reference to the co-existence of cardiac disease. From this table he infers that in any given number of cases of "apoplexy and sudden hemiplegia, no less than three-fifths will present unequivocal signs of cardiac disease: either hypertrophy, dilatation, valvular disease, or some combination of these lesions. M. Bricheteau reports the particulars of twelve cases of apoplexy, in which he found unequivocal evidence of hypertrophy, either with, or without dilatation. These facts demonstrate that the existence of these two diseases in the same subject, at the same time, is too frequent to be co-incident, but rather bear the relation of cause and effect to each other. But for the tortuous rout by which the arterial blood reaches the brain, diseases of this organ would necessarily increase, particularly in cases of cardiac affections; such as hypertrophy, &c.

We regret that we cannot follow Dr. Burrows further in his valuable and instructive remarks upon these interesting subjects. To our mind, it is the ablest work we have read on any particular branch of medicine. It is simple in style, clear and concise in reasoning, and breathes, throughout, a spirit ardent in the pursuit of truth and anxious to communicate it to others, when found free from sophistry and mysticism.

The size of the book is another recommendation; embracing but little over 200 pages; we feel sure that every one who has not read it will be instructed and highly pleased with its perusal.

A. H.

III.—*Adulterations of various substances used in Medicine and the Arts, with the means of detecting them; intended as a Manual for the Physician, the Apothecary and the Artisan.* By Lewis C. Beck, M. D., Professor of Chemistry in Rutger's College, New Jersey, and in the Albany Medical College, &c., &c., New York. Samuel S. and W. Wood. 1846. pp. 332.

The object of this work seems to have been, on the part of the author, laudable enough; yet we have scarcely the leisure to examine carefully into its merits. It was designed to exhibit the adulterations of the different substances used in medicine and the arts, and to point out the mode by which they might be detected.

It is candidly acknowledged by Dr. Beck to be a compilation chiefly from the writings of J. Garnier and C. Haril; the U. S. Dispensatory;

Pareira's Materia Medica; Neligan, Ure, Parnell, H. Rose, and Fresenius. As such, it may be useful to the chemist in ordinary, to the practical apothecary and the artisan. For these practical purposes it seems well adopted, as the medicines and the articles used in the arts, are all arranged alphabetically. To make it still further available for extensive practical purposes the author assures us, he has stript the subject as far as possible of technicalities. Dr. Beck does not profess to give accurate information in regard to the exact extent of these adulterations; he simply strives to point out those substances most commonly employed for the purpose of adulteration, and also the means of detecting them. Few who are conversant with chemical science need consult this little work, although it contains information that may be useful to the younger part of the profession—and the pharmaceutics.

To give the reader some more distinct idea of the plan upon which Dr. Beck proceeds, we will begin as he does, with *acidum aceticum.* This fluid has a specific gravity of 1,063, it dissolves camphor and the essential oils, and its behaviour is that of a powerful vegetable acid, &c.

As acetic acid is extensively used, so it is often adulterated; frequently Dr. Beck tells us with sulphuric, muriatic, tartaric and sometimes with nitric and oxalic acids, and occasionally with metallic salts.

He then details ,with sufficient minuteness, the process by which the merest tyro in chemistry may detect each of these substances when suspected. We have often thought too little attention was paid to this subject; we have our tobacco, beef, lard, pork, flour, corn, hay, &c., inspected by law, articles of the soundness and purity of which the ignorant as well as the learned can judge each for himself by using the sense of taste and smell. But when we come to medicines, we swallow it down, often grasping the nose and closing the eyes, as if fearful these senses might, untaught, detect adulterations, too often through cupidity practised upon the unsuspecting public.

Yet how much of human happiness is involved in this apparently trifling matter?

We have been informed that certain large manufacturing chemists, in Europe and this country, turn out inferior articles, often adulterated, no doubt, in order to undersell or break down competition; and these articles too are thrown into the market; they pass from the wholesale into the hands of the retail apothecary, and are here made into portions, pills, electuaries, &c., and devoured by the unsuspecting invalid.

What is the result? The physician is disappointed, because he is reposing confidence in a spurious medicine, and often doubts the efficacy of therapeutic agents; the sick expects results from the medicine which do not take place, and he begins to question the skill of his choice; all this grows out of the dishonesty of the manufacturer. This evil should be arrested, and had we space, we think we could suggest a remedy, but we leave this subject for the consideration of those more directly interested.

A. H.

IV.—*The History, Diagnosis and Treatment of the Fevers of the United States.* By Elisha Bartlett, M. D., Professor of the Theory and Practice of Physic in the Medical Department of Transylvania University, &c. Lea & Blanchard, Philadelphia, 1847. pp. 534.

We do not know that the "American System" has had any thing to do with the making of books, but most certainly book-wrights have increased of late to an amazing extent among us. It may be that books multiply books, as we think is exemplified in the volume before us; for what Dr. Bartlett has added to what was known before, we are at a loss to discover. In other words, the book shews Dr. Bartlett to be a reader of books, and but little more.

The work is a made-up affair, containing the observations of almost every body that ever wrote, except those of the author himself;—to review it, would be to review the opinions of some hundreds of different writers.

We do not object to this in systematic treatises,—for such is their purpose. They present an historical sketch of the opinions of men eminent in science, and are doubtless valuable to the student as condensations of such opinions.

The present work claims to be one of this class—"a Systematic and Methodical Treatise on the Fevers of the United States."

So far well enough. But when an author writes, he is expected to tell his readers something, and whether he can add to their stock of knowledge or not, depends as much upon nature as upon his own industry. We do not think the author has laid us under heavy obligations.

In truth, to write a valuable work on such a subject as the author has chosen, demands a number of attributes as well as acquirements. There is requisite a command of language—a power of description—a pictorial talent, in fine, which no mere book-reading can give, though the natural gift may be improved by it. A talent for observing is also requisite—for it is not every one who *sees* that *observes*—in other words a talent for analysis. The two, conjoined, make the painter, as distinguished from the copyist. The one pores over trivial things, with a care as great, and attention as unremitted, as over the most essential. Hence a confused intermingling of light and shade, of the small and the great, of the valuable and the worthless. The other's nice tact enables him to discard the accidental and unimportant, and portray only what is of value. In most of these qualifications we think the author of the present work deficient.

In chapter IX, p. 134, we have his speculations on the Theory of Fever. We give a portion of his remarks for what they are worth. They remind us of the floundering of a seal in a tub; and constitute an admirable attempt to theorize against theory:

"The most positive thing that can be said, under this title is, that the materials for a complete and philosophical *theory of fever*, or *theory* of an individual fever, using this phrase in its ordinary acceptation, do not exist. Such a theory presupposes and involves a knowledge of the intimate processes and relations of the living powers, which has not yet been attained. It is very questionable even, whether such knowledge is attainable.

In order to see clearly the truth of these observations, and the extent of this truth, let us inquire, for a moment, what *some* of the elements are, which must go to make up this knowledge; what their nature is, and in what they consist. In the first place, we must know what the actual, efficient, *causes* of any given fever, or form of fever, are. We must know what that agency, or combination of agencies, is, which, being present, brings into existence, originates, sets in motion, that concatenation of disordered actions, that complex combination of morbid processes, which constitutes the fever. We must know in what manner these agencies act; where they make their impression; and in what the modifications consist, which they work in the living organization, and its properties. Of all these things, we are utterly and profoundly ignorant. In the second place, we must know the seat and character of all these processes and modifications themselves; their peculiarities; their tendencies; the differences, which exist between them, in the several forms of febrile disease. We must know their relations to each other. We must know which amongst them are primary and essential; which are secondary and accidental. We must know the parts which they severally play in the production of the integral disease. Of these things, also, as of the causes of fever, and their mode of action, it is not too much to say, that if we were not wholly and profoundly ignorant, we are so to a great extent. They are but very partially and imperfectly known to us. They are known to us, rather analogically, if I may so speak, and by comparison with other morbid processes, than absolutely and positively. We can see wherein they differ, in many respects, from these other processes, and wherein they resemble them. With these limitations, and under the conditions implied by these remarks, there is no reason why we may not attempt to commence the foundation of a *theory of fever*. But, in the present state of science, it can only be an attempt at a commencement. We may endeavor to interpret the connection and relationship, which observation has shown to exist between certain phenomena, or groups of phenomena. We can do nothing more."

Now let the reader observe what is coming forth in the following extract. The italics are the author's own:

"There is no such individual disease as that which has always been expressed, and which is still expressed by the term *fever*. How then can there be any *theory* of fever? There are many separate diseases, to which this generic name is properly enough applied, on account of certain general analogies, which exist between them. *But the disordered actions and processes, which constitute one of these diseases, may differ essentially; and, as far as we can ascertain, in most cases they do so differ, from those which constitute another of these diseases. The theory of one fever, then, must be wholly, or to a great extent, inapplicable to another.* The elements which enter into the composition of one problem are not to be found in the other, or they are present in different proportions. *The word fever, when used, as it commonly is, to designate a disease, has no intelligible signification.* It is wholly a creature of the fancy; the offspring of a false generalization and of a spurious philosophy. What, then, can its *theory* be, but the shadow of a shade?"

If such be the philosophy of the nineteenth century we have been in profound ignorance of it. We were unaware that any recent authors contended that *fever* was an "individual disease;—we thought that such notions had gone by, with the old disputes of the Realists and Nominalists. We thought, in fact, that the word "Fever" was used to designate a generalization—in other words, conditions of the system which agree in certain things, but may differ widely in others. These general terms are necessary for the conveyance of thought, and they constitute most of our nouns substantive. Have we not different *species* of Fevers;— have we not *varieties* in each species;—and of these varieties, have we not differences according to age, sex, temperament, idiosyncrasies and many other circumstances? In short, have not *all*—but that is a broad word—have not a vast majority of modern authors inculcated upon us the precept that there can be no specific treatment of any disease?—that we must treat the sick man and not an abstraction? If such is the case, we agree with the author, that a *theory* based upon an abstraction can but be "the shadow of a shade."

But the author is fighting against a phantom. He has taken pains to refute the celebrated Crambe in Martinus Scriblerus:———"Martin supposed an *universal man* to be like a knight of a shire, or a burgess of a corporation, that represented a great many individuals. His father asked him, if he could not frame the idea of an universal Lord Mayor? Martin told him that, never having seen but one Lord Mayor, the idea of that Lord Mayor always returned to his mind; that he had great difficulty to abstract a Lord Mayor from his fur gown and gold chain; nay, that the horse he saw the Lord Mayor ride upon, not a little disturbed his imagination. On the other hand, Crambe, to show himself of a more penetrating genius, swore he could frame a conception of a Lord Mayor, not only without his horse, gown and gold chain, but even without stature, feature, colour, hands, head, feet, or any body, which he supposed was the abstract of a Lord Mayor."

The universal fever, which Dr. Bartlett contends against, has doubtless as little real existence as the universal Lord Mayor of poor Crambe.

As a specimen of our author's precision of language and clearness of thought, we select the following passage:

Mode of Access.—"There is a good deal of difference, in different cases of typhoid fever, so far as the suddenness of violence of the seizure is concerned. There is no other acute disease, perhaps, in which the attack is more frequently slow and gradual than this. In many cases, it is quite impossible for the patient to fix with any accuracy upon the day when his fever commenced. Neither, in many of these cases, is he able to tell *in what* his sickness consisted. He can only say that, for several days, he has not enjoyed his accustomed degree of health. He may have merely felt a sensation of mental and bodily languor, an indisposition, or an inability to accomplish his usual labor, either of mind or body. He may have had slight and dull pain in the head, or in the back and limbs, with a general feeling of soreness or of fatigue. At the same time he may have experienced some sensations of chilliness, alternating with heat. There may have been, also, diminution, or loss of appetite, and moderate thirst, with a dry or clammy state of the mouth. The expression of the countenance sometimes becomes listless and dull, the eye loses its animation, and the mind is either indifferent or apprehensive. There may have been moderate diarrhœa, with some pains in the abdomen. This obscure and indefinite condition of ill health may continue for more than a week, occasionally for two or three weeks even, with but slight changes from day to day. Oftentimes there is a slow but steady increase in the severity of these morbid *sensations*, with a like gradual but regular appearance of other and more characteristic symptoms of the disease,—these latter *coming out*, day by day, one after another, a complete and successive development of the peculiar and strongly marked phenomena of the disease."

Here are *sensations*, properly speaking,—mixed up in a jumble, with "expression of the countenance,"—with the animation of the eye, and the state of the mind:—and with diarrhœa:—all concluding, with the following remarkable sentence;—"oftentimes there is a slow but steady increase in the severity of *these* morbid *sensations*, with a like gradual but regular appearance of other and more characteristic symptoms of the disease,—these latter *coming out*, (we admire the elegance of this latter phrase ; Rev.) day by day, one after another, a complete and successive development of the peculiar and strongly marked phenomena of the disease."

With regard to Yellow Fever, Dr. Bartlett tells us, that he knows nothing about it. Hear a portion of what he terms his "Preliminary Matters."

"*Introductory.* As I wish to be always on honest and fair terms with my reader, I shall commence this fourth and last part of my book, by informing him that I am not personally familiar with the subject of which it treats. I have never met yellow fever at the bed-side ; I have had no opportunities for direct and clinical study of the disease."

A precious confession !—and yet he has given 124 pages to this disease, criticising at every step the writings of those who have recorded their observations from long experience.

The confession of Dr. Bartlett precludes us from examining his account of Yellow Fever critically ; but there are some remarks we must take notice of. He observes, p. 441, that, "Dr. John Harrison, in his *Remarks on the Yellow Fever of New Orleans*, says :—" The liver sometimes contains less blood than we usually find in the viscus, and, in those cases, it is paler and drier than usual. At other times, however, it is engorged with blood, and bleeds freely when cut ; but these appearances it is subject to in common with all the organs, and the existence of one or the other, appears to depend much upon the condition of the patient at the time of the attack, and the treatment he has undergone. In cases where the lancet has been used freely, we shall generally find a *pale yellow liver*." The author then goes on to remark :

"The interest of science not only justify here, as in all similar circumstances, but they demand a single criticism. Where *results merely* are given,—and these only in general terms,—where cases of disease are observed in a hospital, by extensive practitioners, constantly and busily occupied with their private practice, and where these cases are not reported in detail, there must inevitably occur, and this not unfrequently, errors of diagnosis. That this error was sometimes committed in the Charity Hospital, no one can for a moment doubt who reads the following statement by Dr. Harrison. He says,—" *In some cases of a low typhoid type*, in which there existed before death *a low nervous delirium*, we found, sometimes ulceration, and at others hypertrophy and softening of Peyer's glands."* These were unquestionably cases of true typhoid fever ; such at any rate is the conclusion which, in the absence of any detailed histories of the cases themselves, we are justified in adopting ; and if, under such circumstances, cases of typhoid fever could be confounded with those of yellow fever, how much more readily might this happen with *the more closely allied forms of periodical fever,—bilious remittent, and congestive.*"

"*These were unquestionably cases of true typhoid fever.*" We can only reply to Dr. Bartlett, that we admire his self-complacency, and assure him that we have some acquaintance with typhoid fever, and inform him furthermore, that in our opinion, any person who could suppose yellow fever could be mistaken for typhoid, or vice versa, knows as little about the one, as the Doctor confessedly does about the other.

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* New-Orleans Medical and Surgical Journal, vol. ii. p. 139.

We call the reader's attention to the gratuitous assumptions in the extract last quoted. We tell this writer that our observations were made with great care, and when we were House Surgeon of the Institution he mentions, not being permitted by the rules to practice out of doors. The facts are not only known to us, but to many others who assisted at the examinations. These occurred daily during the epidemics of 1833–34 and 35; when Dr. H. resided within the walls of the Institution, and in every succeeding epidemic up to 1842, during which time he was one of the visiting physicians. We doubt if there be a physician in New Orleans of two years practice, who would confound typhoid with yellow fever.

But when people take a monomania, reasoning with them is useless until they are cured. It is doubtless true, that the *plaques* of Peyer are generally affected in typhoid fever, but it is not true, that the fever and the local lesion are invariably connected. Dr. Bartlett himself admits that the lesion is secondary, or a consequence to the disease. But Dr. B. sees typhoid fever wherever there is found ulceration of the plates. Has he never met with a case of chronic dysentery? We have seen many in the Charity Hospital, going down to the grave month by month and sometimes lasting for years, and after death, ulceration of Peyer's glands were found as well as those of Brunner. His pertinacity in this respect reminds us of a scene in Molière.

M. Tomès. Comment se porte son cocher?
Lisette. Fort bien. Il est mort.
M. Tomès. Mort?
Lisette. Oui.
M. Tomès. Cela ne se peut.
Lisette. Je ne sais pas si cela se peut, mais je sait bien quo cela est.
M. Tomès. Il ne peut pas être mort, vous dis-je.
Lisette. Et moi, je vous dis qu'il est mort et enterré.
M. Tomès. Vous vous trompez.
Lisette. Je l'ai vu.
M. Tomès. Cela est impossible. Hippocrate dit que ces sortes de maladies ne se terminent qu'au quatorze, ou au vingt-un; et il n'y a que six jours qu'il est tombé malade.
Lisette. Hippocrate dira ce qu'il lui plaira; mais le cocher est mort.

M. Louis seems to be the Hippocrates of our author.

In his bibliographical notices, the author remarks of Dr. Harrison, that he "praises, almost extravagantly, the sulphate of quinine."

What Dr. H. said of quinine consists of two things; viz: 1st. Certain facts which he himself witnessed, and hundreds similar to which are now well known to the physicians of New Orleans:—the last epidemic giving ample opportunity to acquire experience. 2ndly. Certain opinions concerning the value of quinine in the treatment of yellow fever. We here copy those opinions and leave the reader to judge of the charge of extravagance.

"From the above observations, concerning the effects of quinine, it will not, I hope, be understood, that I advocate its administration in all cases whatever. As I have before remarked, there can be no specific treatment for yellow fever, or any other disease. When, in the commencement, there is great congestion of blood in the brain, or any other important organ ; or, where the fever supervenes upon chronic inflammatory diseases, I would most certainly resort to other means. Nor would I be understood as speaking of quinine as an infallible remedy. The practitioner to whom the disease is a new one, will soon discover that in certain cases, particularly those of the congestive and ataxic types, that quinine is as inefficacious as any other remedy. In cases, in which the fever is well and fully developed, it will, unquestionably, cut that fever short, and thus prevent the formation of those local congestions which are produced by the febrile action. In this consists its value, and assuredly, it is a great one."

We fancy the value of quinine in yellow fever is confined within somewhat narrow limits in the above paragraph ; but if our author still thinks our opinions *extravagant*, he had better ask those of most of the practitioners of any standing in this city. Most of them, we suspect, will tell him that Dr. H. has not done justice to the virtues of quinine.

But, in fact, the paragraph was penned with caution, and after mature consideration ; and although another epidemic has occurred since the passage quoted was written, we have met with nothing to change our views, therein expressed.

A considerable portion of our author's work is devoted to bibliographical notices. The spirit in which they are conceived, and the taste with which they are executed, may be gathered from the following :

" *Results of an Investigation respecting Epidemic and Pestilential Diseases, etc. By Charles Maclean, M. D. London,* 1817. 2 *vols., pp.* 1016. My only motive for including this work in my bibliography of yellow fever is to guard my readers against buying or attempting to read it. In all medical literature, it would be difficult finding a noisier, emptier, or more arrogant, egotistical, and puppyish book than this."

Now we know nothing of Dr. Maclean's book, and all that is written against it may be true, but it even so, we " hold it not honesty to have it thus set down."

But we must bring our notice of this work to a close,—adding this advice to the author, that when he next writes a book, to let us have less of the thoughts of others, and more of his own.

J. H.

V.—*Tracts on Generation, No.* 1. *Proofs that the Periodic Maturation and Discharge of Ova, are in the Mammalia and the Human Female Independent of Coition, as a first condition of their propagation.* By T. L. G. Bischoff, M. D., Professor of Physiology, &c., Giessen. Translated from the German, by Professor Gilman and Tellkampf, of New York.

We believe it was, until within the last few years, maintained by physiologists that the ova were not matured and discharged without coition ; and furthermore, that the *corpora lutea* were invariably caused by a rupture of the Graafian vesicle, and the discharge of ova, leaving behind cicatrices, designated the corpora lutea. Bischoff has proved in this tract, by direct experiments, that the ova may mature and be discharged independent of coition. He has demonstrated that the ova are formed, matured and discharged, from the female organism usually at regular monthly periods, and this too totally independent of the presence of the male semen.

The menstrual evacuation is nothing but the discharge of these matured ova.

Every one knows that in fish, birds, and the amphibia, the discharge of ova takes place without copulation; but until recent experiments were instituted on this subject, no one imagined that the same thing took place in man and the mammalia. The formation of the germ was considered as resulting alone from copulation.

Dr. Bischoff has, by direct experiments, ascertained that the "male semen comes in material—actual contact with the ovum, and found by unquestionable observation that the semen penetrates through the uterus and the tubes (fallopian,) to, and is found upon the ovary." He moreover says that the fecundation of the ovum takes place on the ovary, at various periods after coition in different animals.

By his experiments and observations, M. Bischoff was enabled to announce the following law as governing generation in all organic beings: In mammalia, including the human species, the ova in the ovary advance through regular stages of development to maturity, quite independent of any agency of the male semen.

The period of menstruation in woman corresponds with the season of heat in the lower animals, during which the ova are detached and discharged from the ovary. It is during this period, the sexual appetite is strongest both in the human female and the lower order of animals.

If, at this time, the semen of the male comes in contact, through coition, with the ovum of the ovary, impregnation takes place; but if not, the ovum is discharged and perishes in the tubes.

The process of development begins in the tubes, to be afterwards completed in the uterus. M. Bischoff endeavors to prove "that in the mammalia at the period of heat ova are discharged from the ovary, and enter the Fallopian tubes, whether coition takes place or not, and whether by its instrumentality the semen be brought in contact with them or not."

We must confess that our author has made some important additions to this interesting branch of Physiology. He has, by direct experiments on bitches, decided certain questions in relation to the maturation and discharge of ova from the ovary, which have puzzled philosophers from Aristotle down to the 19th century. Much credit is therefore due M. Bischoff, for the sagacity and patience with which he has prosecuted this interesting inquiry.

Writers on Medical Jurisprudence, until M. Bischoff wrote, regarded the corpora lutea as unequivocal proofs of previous impregnation or, at all events, of copulation. His experiments have overthrown this theory, and decided this question in the negative for all future time; for when questions, growing out of the operations of nature, or organic life, are determined by an appeal to positive facts and observation, no authority based upon speculation, however eloquently put forth, can avail.

In conclusion, we recommend this Tract to every individual who takes an interest in the progress of physiological science.

A. H.

VI.—*On the Theory and Practice of Midwifery.* By FLEETWOOD CHURCHILL, M. D., M. R. J. A., Professor of Materia Medica and general Therapeutics; Physician to the Western Lying-in-Hospital; Hon. member of the American National Institute and of the Philadelphia Medical Society. With notes and additions. By ROBERT M. HUSTON, M. D., &c., &c., &c. Third American Edition, Revised and improved by the Author. With one hundred and twenty-eight illustrations. Philadelphia. Lea & Blanchard. 1848. pp. 525.

It has been said, (and we believe we can endorse it, without the fear of a protest,) that the Dublin Accoucheurs are the most able in Europe. They have assuredly written well and voluminously on the subject, and their writings are highly esteemed on this side of the Atlantic. Among the first obstetricians, may be mentioned the author of the Treatise before us.

This is the third American edition of his work on Obstetrics—no trifling evidence of the high estimation in which it is held by the profession of this country.

As this is a branch of medicine for which, we confess, we have no great partiality, we shall simply give Dr. Churchill's classification of labor and refer the reader to the text. It is as follows?—

Class I. Natural labour.
Class II. Unnatural labour.
A. *From abnormal condition of the expulsive forces.*
Order 1. Tedious labour.
" 2. Powerless labour.
B. *From abnormal condition of the passages.*
Order 3. Obstructed labour.
" 4. Distortion of the pelvis.
C. *From abnormal condition of the child.*
Order 5. Malposition and Malpresentation.
" 6. Plural births. Monsters.
Class III. Complex labour.
Order 1. Prolapse of funis.
" 2. Retention of placenta.
" 3. Flooding.
" 4. Convulsions.
" 5. Laceration.
" 6. Inversion of the uterus.

Such is the arrangement adopted by Churchill, in imitation of Merriman, in his treatise on parturition. He includes the presentations under four different heads; but we think the number might be extended to four times four, just as well.

The plates with which the work is illustrated, are good and will greatly assist the reader in understanding the mechanism of labour.

The use of Chloroform in mitigating the throes of the parturient female, will doubtless create an important era in obstetrical medicine. By Dr. Simpson of Edinburg, it has already been successfully applied in a great number of cases; and promises to remove, or at least to mitigate the curse pronounced against the daughters of Eve!

Churchill's "System of Midwifery" should occupy a place in the Library of every intelligent practitioner. The work is brought out in superior style, for which the publishers are entitled to the thanks of the profession.

A. H.

Part Third.

EXCERPTA.

I.—*The Cholera.*

The anticipated approach of this fatal pestilence has, as might be expected, attracted the attention of the press and of the medical profession, and has induced the recently-appointed Metropolitan Sanatary Commission to review the history of its first attack in 1831–2, to reconsider the measures at that time adopted to prevent its approach, and check its progress, to examine the predisposing causes which determined the places and class of persons attacked, and to point out the course to be adopted, with a view, not to prevent its appearance among us, (for that would appear to be a hopeless undertaking,) but should it arrive in England, to disarm it, as far as may be, of its terrors. An abstract of the important evidence collected by the commission, preceded by a brief reminder of the particulars of its first visit, and a short summary of its continental progress, up to the end of November of this year, cannot but prove acceptable to the reader.*

The cholera appears to have been unknown in Europe prior to the year 1831. It broke out near Calcutta, in the year 1817, when it not only committed fearful ravages in India, but carried off 400,000 persons, in Java and Malacca. In the succeeding year, China, the Birman Empire, the Malaccas and the Mauritius suffered severely, and assuming a more northern course it passed through Persia and Arabia in 1821; appearing, in 1823, at the foot of the Caucasus, and the margin of the Caspian Sea; 1826 witnessed its advent in Siberia, whence it advanced with hasty strides into the interior of Russia. Africa was invaded in the next year, and the disease also was raging at the same time in Egypt. Poland, Gallicia, Austria, Bohemia and Hungary suffered in their turn; it reached Prussia in 1831; thence it rapidly traversed the sea to England, passed over to France, and was next seen in the New World. It also passed from Asia Minor to the south of Europe. The number of cases in England and Wales, during the years 1831–2, including London, amounted to 61,051, and of these 40,473 recovered, and 20,578 died; 33 per cent., therefore, or about 1 case in 3, proved fatal. In the metropolis there were 11,020 cases, of which 5,745 recovered, and no less than 5,275 deaths, being little short of 50 per cent.

The present epidemic, after raging with great violence for two years in Persia, where it was propagated in a direction from S. E. to N. W., towards the end of the summer of 1846 broke out at Tauris and Teheran, and during the

* For more full particulars in reference to the epidemic of 1831–2, and the course of the present epidemic, the reader is referred to a paper by Dr. J. C. Hall, in the Journal of Public Health, No. ii. p. 32, to which we are chiefly indebted for this abstract.

autumn advanced within a short distance of the Russian frontiers. On the 16th of November, 1846, cases occurred at the village of Saliany, and also in the same month at Leukoran, *and it is worthy of remark, that these were the places first attacked in* 1830. The disease also appeared at Bakrou; and advanced in December to Schêmakha, Derbent, and in the month of February, 1847, to the town of Kouba. Its appearance at Saliany and in the district of Talysch was marked with great malignity. Selecting for its victims those who had but recently recovered from the fever of the country, the cholera almost invariably carried off every patient; nearly 9-10ths dying. After a few weeks, the cases were less violent, and the number of deaths, as compared with that of the patients, was in the ratio of four to five. In the other localities of the Trans-Caucasian provinces, the attacks became less violent, and, without the towns, the disease no longer presented a malignant type. Towards the end of February, all traces of the disease were lost, and hopes began to be entertained that the country was once more free. In the following month, however, it broke out with increased violence, and in April, it began to spread with fearful rapidity, traversing simultaneously three districts, passing to the north, along the shores of the Caspian Sea; to the north-west, in the direction of the mountains; and on the west, towards Tiflis, which it reached on the 17th of May. It appeared on the other side of the Caucasus, on the 24th of May, at Kizliar, whence, re-ascending the Terek, it penetrated to Mozdok; afterwards, at the end of June, to Piatigorsk and to Georgierk, and entered Stavropol in the first week of July.

From the 16th October, 1846, to the 14th of June, 1847, the Caucasus and Trans-Caucasian provinces reckoned no less than 17,055 cases of cholera, of which 6318 died.

During the first week of July, the cholera made its appearance also in the government of Astrakan. The first patients were attacked on the 3d, in the quarantine of Astrakan, situate about 100 wersts to the south of the city, on an island named Birutchiakossa; on the 4th, cases occurred in the military district, and on the next day, in the third quarter of the city, a Tartar was attacked, and died on the 6th in the hospital. The malady now sensibly spread into the city. Its progress was at first slow, and some difference of opinion seems to have existed as to the true nature of the disease; the majority of the physicians looking upon it as a severe form of the sporadic cholera that annually prevails during the summer months. The number of cases reported from the 4th to the 13th, was 23, and of these no less than 19 died. The majority of those attacked belonged to the lower orders, and it made no distinction of age or sex: the males attacked, however, exceeded the females in the proportion of 5 to 1; adults were more frequently affected than children, and in general, the Mahometans suffered much less than the Russians, the former being much more cleanly in their habits, and very sober and careful in their diet.

It appears that in Astrakan the disease was at first most violent, death frequently ending the sufferings of the victim in a very few hours; in many cases, so rapid was the complaint, that no time for medical assistance was afforded; the powers of life sinking from the first. During the first three days (from the 13th to the 16th or 17th of July) more than one-half died; after this, the disease gradually assumed a more favourable aspect, and the recoveries were more numerous. On the 19th of July, the number of deaths was 137, which, gradually declining, were reduced on the 2d of August to 14.

From an official return of the number of deaths from cholera in Astrakan, from the 4th of July to the 2d of August, it appears that out of a total of 2071 cases, 1223 died, and 848 recovered. Towards the end of October, the cholera reached Moscow, in which city, according to official accounts from St. Petersburgh, out of the first 140 cases, 40 had proved fatal; and between Oct. 25th and Nov. 1st, 641 persons had been attacked, of whom 233 had died, being a mortality of little more than one-third. The grand total up to Nov. 1st, was 1197 attacks, and 402 deaths. According to the latest accounts, which, however, may require confirmation, cases had occurred in Vienna and Hamburgh, and at Malta.

The Metropolitan Sanatary Commission have very properly made the Asiatic cholera their first subject of inquiry.* To this course they were impelled by a consideration of its high mortality when it made its attack in 1831–2, by its ascertained connection with the defective structural arrangements involved in their investigations, by the probability of its again visiting these islands, and by information received from Sir William Pym, who, at an early stage of their proceedings, attended from the Council Office, and informed them of the advices which had been received from the English Consuls, of the steady progress of this pestilence, precisely upon its former track in 1832.

They immediately called before them medical witnesses who had been in practice in the metropolis when the disease prevailed there, and who were most extensively engaged in attending on the sick, with the view of obtaining information as to the past and present sanatary condition of the people, of the localities in which they reside, of their dwellings, and of other circumstances which appeared to favour the spread of the disease; also as to the effects of the measures both of prevention and alleviation which were then adopted, and the modifications suggested by the experience then obtained.

Concurrently with these inquiries the Commission endeavoured to ascertain the state of information and the practical skill and competence, as exemplified in their works, of the authorities charged with the direction of what all previous inquirers had agreed in representing as the chief means of prevention; namely, the works of draining and cleansing. With this view they examined the chief paid officers of all the Commissions of Sewers, with the exception of that for the city of London.

The first inquiry into which the Commissioners enter has reference to the measures which had been adopted to prevent the introduction and extension of that disease in 1831.

The first act of the government was to appoint, by order of the Privy Council, a Central Board of Health in London; and to issue an order in Council, dated the 20th day of October, 1831, in which they proclaimed the presumed efficacy of the measures of extreme precaution adopted for preventing the introduction of the cholera morbus by a rigorous quarantine, but evincing a well-grounded misgiving as to their ultimate success. This document then went on to speak of strict regulations for ensuing non-intercourse of infected with healthy districts, hinting at the possible necessity of military and police *cordon sanitaries*, to order the setting apart one or more houses in each town or its neighbourhood, as places to which every case of the disease, as soon as detected, might with consent of friends, be removed; and in a word, adopting most of the precautions so rigidly enforced during the prevalence of the plague.

In the meantime the removal of filth of every description, extreme cleanliness and free ventilation, burning of decayed articles, such as rags, cordage, papers, old clothes and hangings; and the purification of clothes and furniture by copious effusions of water, and boiling in a strong ley, were enjoined.

The Central Board of Health, however, had been in existence less than one month when, in consequence of information transmitted to them relative to the progress of the cholera in various parts of Europe, but more especially guided by the conclusions to which Drs. Russell and Barry had arrived, after a five months' careful and laborious observation of the character of the disease in those parts of Russia which they visited, issued a circular, dated Nov. 14, 1831, in which they strongly deprecate all measures of coercion for the purpose of ensuring non-intercourse, adding, "that, under proper observations of cleanliness

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* First Report of the Commissioners appointed to inquire whether any and what special means may be requisite for the improvement of the health of the metropolis.

and ventilation, this disease seldom spreads in families, and rarely passes to those about the sick, unless they happen to be particularly predisposed," so that it will not be necessary, "where there is space, and where due attention is paid to cleanliness and purity of air," "to seperate members of families actually affected by the disease, nor to insultate individual houses, unless in cases of crowded, filthy, badly-ventilated habitations, and other contingencies which involve the health and safety of all."

The circular then went on to prescribe the formation of District Boards of Health, each to consist, if possible, of a resident clergyman, and a number of substantial householders, and of one medical man at least. These Boards to be charged with the following duties in their respective districts, viz.:

"1st. To appoint inspectors. Each inspector to visit daily, and to inquire carefully after the health, means of subsistence, cleanliness, and comforts of the inmates of, say 100 houses (more or less), according to local circumstances. 2d. To receive and examine the reports of these inspectors, which should be made up to a given hour on each day. 3d. To endeavour to remedy, by every means which individual and public charitable exertion can supply, such deficiency as may be found to exist in their repective districts, in the following primary elements of public health, viz. the food of the poor, clothing, bedding, ventilation, space, cleanliness, outlets for domestic filth, habits of temperance, prevention of panic. 4th. To report to their principal Boards respectfully on the above heads, as well as on the actual state of health in their districts."

The Board further recommended that when a family was reported to be in an unhealthy state, and the disease was confirmed to be cholera by a medical member of the District Board, that the head of such family, if unable to afford proper accommodation at home, should be advised to send the sick person forthwith to the temporary hospital, and that the other members of the family should be supplied with such additional means and comforts as their state might require, to enable them to resist the influence of the infected atmosphere in which they lived.

In a circular issued on the 13th December, 1831, it was further recommended that a number of steady men should be appointed to lime-wash and purify such apartments as might be pointed out by the inspectors of the local Board.

Minute directions were added as to diet, clothing, and the general regimen to be adopted, with the view of obtaining and preserving a sound state of health.

Such were the measures at first proposed to protect the country against the introduction and spread of Asiatic cholera. The notorious failure of quarantine regulations, and the advent and fatal career of the cholera, in spite of all the precautions adopted in 1831, by the Central Board of Health, have induced the Metropolitan Commission to turn their attention to the condition of the localities in which cholera first made its appearance, and which it was generally found to select. This portion of the Report is prefaced by the proposition that—

"1. The manner of the introduction and extension of this pestilence in the various cities of Europe which it invaded was every where the same;" and that "the statements are strikingly uniform to the effect that it commonly made its first outbreak in the lowest and dampest part of the city it attacked, generally in the immediate neighborhood of the river, and often on board of some ship lying near the shore." Thus Drs. Russell and Barry, the Commissioners appointed to examine on the spot the introduction and spread of the disease at St. Petersburg, state that the first case that occurred in that city "was that of a merchant who had arrived from Witagan on board a decked boat;" the second was that of a journeyman house-painter, "resident in the quarter where the barks lie, and who was taken ill about the same time as the merchant;" and the third "an invalid soldier, on duty in the same quarter, not far from the barks." They further state that "no direct personal intercourse could be traced between any two of the first five or six cases, but that it is certain that the first three were from the same district, that in which the suspected barks are stationed. This quarter is the easternmost of the whole city, the first you arrive at coming down the stream, and during the late and present perseverance of easterly winds, the very spot from whence effluvia of any kind might be most conveniently blown over the town." They add, "We are informed by Dr. Rehman that many have been taken ill on board the barks themselves."—From the Report of Dr. Hamett on the cholera at Dantzic, it appears that the first two acknowledged cases of epidemic cholera occurred in the Harbour Canal, one German mile from Dantzic, in two *mud barges*, that these were followed by two others, apparently in the same locality, the next day; and that these cases occurred previously to the first arrival of vessels from Russian ports. It is stated by Dr. Becker, of Berlin, that the first cases of cholera in that city occurred among the skippers in the boats lying on the river Spree, which flows through the town, and in houses in the immediate neighborhood of the river; and that the disease prevailed to a considerable extent in all those streets which lie along the navigated branch of the river. In Moscow, the place in which it principally prevailed, and was most mortal, was a low quarter, surrounded by a bend of the river Moskwa. At Breslau, it first attacked and principally ravaged that part of the town which is low and marshy, and which is the constant seat of intermittent fever. It is stated by Dr. Automarchi, that the condition of the houses in which cholera prevailed at Warsaw was little better than that of sewers.

The Report of the Central Commission of Paris states that the disease first appeared and subsequently spread, above all other places, in the greater number of the *quartiers* situated upon the borders of the Seine, that it was most prevalent and most fatal in the low, close, undrained, and uncleansed localities.

In England the cholera first broke out in the port of Sunderland, and on board of vessels which were supposed to have brought the disease from some infected place on the Continent; but on a close examination of the facts, not only could no evidence be adduced to justify this suspicion, but, on the contrary, it is declared in the most positive manner that the suspected vessels had neither come from diseased ports, nor had any cases of cholera on board.

In its subsequent progress through the country, it generally first appeared in the neighborhood of rivers or marshes, and principally raged in low and damp localities, particularly where these were also the outlets of filth. In Carlisle, for example, it is stated that it first broke out "near a mill, and raged down the damside, few cases occurring in any other part of the town."

Mr. Roberton, in his Report on the sanatary condition of Manchester, after stating that cholera "appears to have generally, in Europe, followed the track of rivers and water-courses, and in cities and towns kept in a remarkable manner to the neighborhood of sewer-mouths," adds, "as far as my knowledge of cholera extends, in our Lancanshire towns it manifested itself more than elsewhere, along the water courses (including docks, wharfs, districts occasionally flooded, &c.,) and with peculiar virulence near the outlets of drains. The progress of the disease in Manchester, from first to last, furnishes a comment on this remark. For example, in the New Bailey Prison, which stands within a few yards of the Irwell, there were no fewer than 60 cases; in Allen's court, situated between Long Mill gate and the Irk, and near the mouth of a large sewer, out of 17 seizures, in four houses, 14 died; in Back Irk street, in which a number of cases occurred, the only houses visited by the disease were close by the main sewer, which there burst into day, and ran above ground; the same remark applies in reference to cases in Little Ireland, on the river Medlock. Where the cholera broke out (as often happened) in places apart from the canals and streams, it was noticed that this, in most instances, was in yards, courts, and narrow streets, polluted by offensive cesspools, pigsties, and other sources of malaria, (some of which were too disgusting to be described,) or by open or obstructed sewers. In Warrington, where the disease raged destructively, it located itself principally in Bank street and other neighboring low streets running into or near the Mersey—the whole quarter so notorious for its filthy sewers, as to receive the name of 'Sewer Island.' In Bolton, the number of cases did not exceed 50; but nearly all of them occurred in closes and entries adjoining a stream, into which a number of offensive sewers discharged themselves. Perhaps, however, the most striking illustration on record of the influence of ill-contrived sewerage on the origination of cholera, supposing the epidemic once prevailing, occurring in Liverpool. One morning it was discovered that several men had been seized with cholera, during the preceding night, on board a vessel lying in one of the docks. The men were sent to hospital; and the vessel having been immediately warped into the river, another ship with a healthy crew took up her station. The next morning all the hands on board were ill of cholera. On examining the dock, it was found that a large sewer discharged its contents under the spot where the vessel was placed. I give this most instructive fact on the authority of Dr. Gaulter, an accomplished physician (since deceased,) the author of a valuable work on the 'Origin and Progress of Malignant Cholera in Manchester,' published in 1833."

In a Report on the sanatary condition of the laboring classes of Tain and Easter-Ross, by Mr. James Cameron, it is stated that in 1832, cholera appeared in Easter-Ross, during the fishing season; that it was, with few exceptions, confined to the fishing villages; that in the remote village of Inver, situated on the low sandy shore of the Tain Frith, and notorious for its malaria, its ravages were fearfully rapid, having cut off nearly one-half of the inhabitants; while the town of Tain and most of the rural districts escaped.

To the same effect is the observation of Mr. George Sheward, in the west of England, who states that he was parish surgeon at the time that cholera made its appearance in Upton-upon-Severn; that this town is situated upon the bank of a large navigable river, and is liable to a constantly changing population, many of the lower orders depending on the river for support; that nearly three per cent. of the gross population fell victims to the disease within the short space of three weeks, but that its ravages were entirely confined to the lower classes; that every case fell under his notice; that the most diligent inquiries led him to believe that the disease takes generally the course of navigable rivers, and that it was so in the present instance, though its march was erratic; one case breaking out near the river, another more in the town; but in almost every one in the houses of persons who worked by the water.

Mr. Bowie, who appears to have had the first case of cholera that occurred in the metropolis, gives evidence to the same effect. He "was practising near the river, in East Smithfield, when the cholera arrived in the metropolis in 1832. Thinks he had the first case of it. It was that of a seaman, named Daniel Barber, mate of the 'Felicity,' of Limerick, which had come to London direct from that port, and had lain in the river three weeks prior to his being attacked. There was no cholera in the place from whence this ship had sailed; there was nothing particular in the condition of the ship itself; she had lain in the river three weeks before cholera broke out." "I am not quite certain," continues this witness, "but I think the second case was a seaman named Thomas Skowkes, of the 'Evander' of Aberdeen; and the third was the mate of a Scotch vessel, lying likewise at the Hermitage." He further states that the cholera, having spread from Wapping along that side of the shore, including Limehouse, crossed to the opposite side of the river, namely, Rotherhithe and Bermondsey; that it then attacked the lower parts of the borough of Lambeth; next, the lower parts of Westminster; then it extended along the Fleet ditch; and thence passed into the City.

Other witnesses confirm the correctness of these statements. Thus Mr. Wagstaffe, who is practising in Bermondsey, Southwark, and Lambeth, and who saw very much of cholera, observed its course to be along the side of the river, and principally in low and damp situations.

This is entirely in accordance with the history of the origin and progress of the cholera in the country which may be considered as its birth-place; for all accounts from India agree in stating that it first breaks out and principally prevails in low and marshy situations, and particularly near the banks of rivers; that whenever a village or military station lies upon or near low, marshy, or damp ground, the occupants suffer in direct proportion to their proximity to such a situation; and that when a regiment has been encamped, one part on high and dry land and the other part on a morass, or on the bank of a river, it is constantly observed that the former has remained healthy, while the latter has suffered severely from this disease.

The history of cholera in this respect seems to bear a close analogy to that of fever. Thus Sir John Pringle, in his account of the diseases of the army during the campaign in Flanders, states, that when the army were encamped along a canal, or on damp and marshy ground, the effect was seen in the sudden seizure of the men with fever.

2. The second proposition of the Report is to the effect, that "there is no evidence that cholera spreads by the communication of the infected with the healthy." This proposition is supported by the following evidence:

When cholera broke out in Cairo in 1831, two *cordons sanitaires* were established between Cairo and Alexandria, but they did not prevent the disease from extending to Alexandria. On the 21st of August, two or three of the soldiers were seized with the disease; on the morning of the following day the cases had increased to 22, and by the afternoon of that same day they amounted to 45. Among these, one which proved rapidly fatal, occurred in the palace of the Pacha. Within the space of five days after the disease broke out in Cairo, it had spread over the whole of Lower Egypt, making everywhere nearly equal ravages, and nearly, at the same time, infecting Mansoora, Fua, Alexandria, Rosetta, Burlos, Damietta, and all the towns and villages of the Delta. Again, it is stated by the late Sir John Lefevre, physician to the English embassy at St. Petersburgh, that within a few days of the cholera breaking out in that city, it had spread so widely and so generally as to preclude all idea of its propagation by infection, and this is confirmed by the reports of the English Commissioners, Drs. Russell and Barry. A similar account is given of the manner of its spread in Dantzic. At Vienna the disease first broke out on the 13th of September; on the 14th it had extended to six quarters of the city, and on the following day it had spread through all the rest. In Paris it was rumored that a case had occurred in the Rue des Lombards as early as the middle of February, but this was doubted. Four cases, however, were observed in the interval between the 13th of February and the 26th of March, all of which occurred in the neighborhood of the Seine, in the quarter de la Cité and in the quarter de l' Hôtel de Ville. On the 27th, six persons were attacked simultaneously; on the 28th, 22 more were seized; on the 31st, the number had increased to 300, and out of the 48 quarters of Paris, the disease had invaded 35. In 18 days after the first invasion of this plague, namely, on the 14th of April, there were from 12,000 to 13,000 sick, and 7000 persons had already perished. At this terrible period of the epidemic, 1000 persons sometimes perished in a single day, and to be struck with the pestilence was, in general, to be dead in a few hours.

The Report goes on to argue, that "while the manner of the invasion and extension of this disease thus precludes all thought of its propagation by the communication of the infected with the healthy, there is another fact which is altogether irreconcilable with the notion of contagion, namely, that as no human means have succeeded in excluding it from particular spots, so no extent of communication with the sick has been able to carry it into other places."

In support of this proposition the Commissioners quote M. Londe, the author of a French work on Hygiene, who says, "In the North, while three lines of troops have been unable to arrest its progress, it has often passed over large tracts without infecting any intermediate place, and without following collateral lines. At other times it has been concentrated on a population which has continued to keep up free communication with the neighborhood without at all extending itself to that neighborhood. In France, for instance, where communication with the infected has been everywhere entirely free, there are departments, parishes, towns, and even villages, which have never had a single case of cholera, though these different localities have been sometimes inundated with persons who had fled from places devastated by the disease. Thus, according to M. Monfalcon, during the prevalence of cholera at Marseilles in 1835, Lyons alone received upwards of 10,000 immigrants from that town, and Lyons has never been attacked by cholera. Sometimes, also, portions of certain towns, though they maintained an unrestrained communication with surrounding districts decimated by cholera, were never affected in the slightest degree. At St. Petersburgh, one of the islands in the Neva enjoyed this complete exemption from the invasion of the disease, and this was also the case with the faubourg Leopoldstadt at Vienna. Hence the non-transmissibility of cholera, in any manner whatsoever, appears to us to be a demonstrated fact."

In like manner, Mr. Greenhow, after refuting the notion that it was imported into Sunderland by shipping, and stating that the strictest inquiries respecting the origin of the first cases, have failed to obtain the slightest evidence of their having arisen from any infected source, insists upon "the broad fact which is totally irreconcilable with contagion, that numerous cases have occurred simultaneously at distant points, where no communication could by possibility have taken place;" and goes on to state, "that when several members of one family have been attacked, it has usually been either so precisely or nearly at the same point of time as to forbid the belief of one having communicated the disease to another." "That in the hospitals at Newcastle and Gateshead, no case has occurred of illness arising from attendance on the sick, either in the persons of the nurses, the resident apothecaries, or the attending or numerous succession of visiting members of the medical profession;" and that "those most exposed to contact with the dead, as medical men, in pursuing post-mortem examinations, have not, in any instance, suffered."

With reference to the first appearance of the disease in Great Britain, Dr. Ferguson, Inspector-general of Hospitals, observes,—"Amateur physicians from the Continent, and from every part of the United Kingdom, eager and keen for cholera, and more numerous than the patients themselves, beset and surrounded the sick in Sunderland with all the fearless self-exposing zeal of the missionary character, yet no one could contrive, even in the foulest dens of the sea-port, to produce the disease in his own person, or to carry it in his saturated clothing to the healthier quarters of the town where he himself had his lodging." And he proceeds to point out the fact so strongly at variance with the idea of contagion, that though the first appearance of cholera in England presented a fair *primâ facie* case of imported contagion, nevertheless, at the very period of its thus breaking out in Sunderland, "a case equally as fatal and severe showed itself in the upper part of Newcastle, ten miles off; another equally well marked, in a healthy quarter in Edinburgh; a third not long before in Rugby, in the very centre of the kingdom, and a fourth in Sunderland itself, as far back as the month of August, as well as many others in different parts of the country."

In the two remarkable cases already stated, that of the isolated village of Inver, where it swept away nearly one half of the inhabitants, not a single case occurred in the town of Tain, in the immediate neighbourhood; and in that of Upton-upon-Severn, not a single case of infection could be traced, either to contact with the living or dead body or with the clothing.

But, perhaps, the most striking and important demonstration in England of the fact that cholera does not spread by contagion, was afforded by the town of Birmingham. In its near vicinity, Bilston, seven or eight miles distant, with daily and hourly communication going on by the road and canal between the two places, cholera prevailed more virulently than in any town of the kingdom. The people at Bilston were obliged to send over to Birmingham for coffins, as they could not be made fast enough in that town. The disease also prevailed in the townships around Bilston, but in Birmingham, with its 160,000 inhabitants, there was scarcely a single case of cholera originating in the town; the few cases that occurred there having been imported, especially along the canal, from Bilston; the persons having been clearly attacked by the disease in those localities, and then going to Birmingham, where the symptoms broke forth: but we are informed that upon a close inquiry not a single instance could be found where the disease had spread from the infected to the healthy.

The Commissioners sum up this part of their report by stating that, "every witness examined by them appears to have arrived at the most clear and decided conviction from what was uniformly observed of its progress in the metropolis, that it did not spread from the communication of the infected with the healthy." And they add a very remarkable fact, on the authority of Mr. Bowie, to the effect that a crew of a vessel, all of whom had assisted in waiting upon a boy, escaped the disease, while the captain, who showed extreme apprehension, but had never gone near him, was attacked on shore.

3. The third proposition established in the report is to the effect that "cholera observes in its progress the laws of ordinary epidemics, being influenced by the same physical conditions, and attacking similar classes of persons."

These conditions may, according to the commissioners, be comprised in impure and humid air, and unsuitable or insufficient food and clothing, ill-constructed dwellings, and defective appliances for the regulation of warmth or protection against cold. The want of sufficient and proper food, it is argued with justice, is an agent of very inferior power to the habitual respiration of impure air; and it is justly affirmed, that "in the present state of most towns and cities, the number of persons whose constitution is enfeebled by want of food, compared with the number whose vital energy is depressed by want of pure air, is found to be an exceedingly small minority," an assertion which is borne out to a certain extent at least, by the fact that the population contrives to spend 24,000,000*l.* per annum on ardent spirits, and nearly an equal amount on tobacco and fermented liquors.

The Commissioners proceed to state that typhus fever may be taken as the type of the entire class of epidemic disease that infest this country, that the *habitat* of typhus is that of the class; and that the conditions which favour its spread, and convert it into a pestilence, are equally favourable to all other pestilences. Those conditions being in the metropolis, as in every town and city, defective house and street drainage and cleansing, involving a scanty and insufficient supply of water. The evidence that the tract of typhus is everywhere, the domain of filth being taken for granted, the report proceeds to prove that this was also everywhere the precise track of cholera.

It has been already seen that while cholera generally followed the track of rivers and water-courses, it had a marked preference for those portions of this track which were at the same time the outlets of filth, being remarkably prevalent and fatal in the neighbourhood of sewer mouths. Mr. Robertson's evidence in reference to the large towns in Lancashire, already cited, was decidedly to this effect; so also is that of Mr. Bowie, who, speaking of the first case of cholera on board the "Felicity," says, "The neighbourhood where this case occurred was one of the dirtiest along the river. What were called the 'bone vessels,' vessels employed to carry old bones for manure, usually lay there, and some of them lay there at the time. The stench was exceedingly sickening, and was perceptible at a great distance. Such was the recklessness of the crews of these vessels, that I have frequently seen them using bones as fuel, and cooking their provisions with them, the most offensive smoke penetrating, meantime, into the houses along the shore. Putrid carcasses of dogs and cats, and other inferior animals, likewise the refuse from the shipping in the neighbourhood, thrown into the river, or left on the muddy beach by the tide, were allowed to remain there, deteriorating the atmosphere. The whole of the coast, extending from St. Katharine Docks to Wapping, was very bad, with the exception of a few houses at and near the entrance of the London Docks."

In this same locality occurred the next cases of cholera, one of the sufferers stating that he had got up early one morning and gone on deck; that the smell from the bone vessels lying a-head was so bad that it made him feel sick, and that he had never been well since.

"The condition of the houses of the labouring population in this district, through which cholera spread with great rapidity, was," continues this witness, "extremely bad. It was the practice to pump the water out of the cellars, which had got up into the houses by infiltration from the river, or more frequently flowing in through the house drains from the sewers when the tides forced back the water into the house. The stench from the water pumped out from the cellars was often intolerable; so much so that I was accustomed to go out of the way to avoid it. Cesspools were general, the contents of which percolated through the substratum, and the river water percolating through the substratum carried with it the matter of the cesspools."

This witness adds, that in his opinion "cholera took the place of typhus, affecting the same class of persons, and being influenced by the same class of circumstances."

Dr. Murdoch gives a similar account of the state of Rotherhithe: "In this district," he says, "open ditches received the contents of the privies, the privies hanging over the ditches; the paths in front of the houses were unpaved and filthy; some of the dwellings were wretched hovels. Typhus fever is always most prevalent in these filthy places, when in the neighbourhood at all; and were cholera to reappear, it would follow the law of typhus and typhoid fever, and first visit such neighbourhoods."

Of Southwark, Mr. Leadam says: "This was certainly one of the districts the most severely visited by cholera. The disease prevailed chiefly in the filthy dens which we have about us, in the close courts and alleys. The cholera track and the typhus track in this district were identical."

Mr. Hooper states, that in the parts of this district attended by him, the majority of those attacked were the inhabitants of the narrow streets and the close courts and alleys of the parish, living in filth, and breathing a confined and impure air. Many of their habitations had not even cesspools; the soil was seen oozing through the pavements of the courts. Where there were cesspools they were in a very bad condition, seldom or never emptied. Within the dwellings there was no boarding to the floors of many of the houses, the inmates sleeping on the earth on a few shavings. "I will mention," continues this witness, "Three Tuns court, in White street, in which there are about 15 houses, and probably 150 inhabitants. There is but one privy, and that without covering. The fluid soil is running down the court in front of all the houses. Several of the houses are entirely without windows or floors, that is, without boards on the floors. I could adduce examples of other courts not quite in so bad a condition, but still deplorable. These are the constant abodes of typhus, and these were the places where cholera prevailed. Few or no cases of typhus were observed while cholera was at its highest; this disease taking the place of typhus, attacking the same description of persons, and prevailing in the same localities."

"In the parish of Christ-church, in this same district," says Mr. Doubleday, "and in the neighborhood of Broadwall, there are open sewers. At Brunswick Place there is another. In these neighborhoods the cholera was unusually severe; in one row of houses, within two yards of the sewer, houses which are very miserable as regards size, ventilation, and means of cleanliness, the mortality was excessive; as many as five died in one house. When certain atmospheric conditions prevail and typhus arises, it is always found much more in these districts, and the result is more fatal. If cholera should revisit the metropolis, it would certainly be that the cases would there be more numerous and fatal."

"In Lambeth," says Mr. Wagstaffe, "in the streets, courts and alleys, in which cholera principally prevailed, the drainage was extremely bad; the privies were often in the cellars. I have myself passed through two feet of water to get to the houses, being obliged to walk along planks. . . . Cesspools are general in the district, and I have often seen the soil from these cesspools swimming about in the water. Whenever typhus is prevalent in the metropolis, it is invariably found in these localities, and common fever is very apt in these places to assume a typhoid type. This is the case at the present time with several cases now under my care. Scarlet fever, measles, and small-pox also are very apt to become malignant here under certain atmospheric conditions. These localities, in which typhus is constantly present are the very localities in which cholera chiefly raged. I have at the present moment many cases of fever in the very places in which cholera was most prevalent. This autumn, diarrhœa and dysentry have also been prevalent there, and some cases were so similar to Asiatic cholera that I have asked some of my professional brethren to go and see them; two of these cases were fatal. They had, in fact, all the characteristic symptoms—vomiting, diarrhœa, with rice-colored evacuations, cramps, suppression of urine, the particular sunken countenance, giving the expression of age to the patient, with a livid or even blue color. If cholera were again to reappear, these would be the places which it would first visit, and in which it would be most prevalent and fatal."

Mr. Simpson, of Bloomsbury, being asked, with reference to St. Gile's, among what description of persons, and in what localities, were the chief attacks of cholera? answers, "Precisely the same description of persons and in the same localities where typhus, influenza and scarlatina, assume the putrid type."

The water supply of these districts would appear, from the evidence of the same gentleman, to be on a par with the drainage and cleansing. "Very bad;" "pumped into many of the houses from the parts of the river where the most abominable impurities abounded." "The filthy state of the water in the rooms"—"exceedingly filthy water, which has been used over and over again, the odour from which is most offensive"—these are some of the expressions made use of by the witnesses.

With regard to the existing state of these districts, the uniform testimony of the witnesses is to the effect "that although some old open sewers have been arched over, and some additional common sewers have been made, no real improvement to any considerable extent has been effected in their respective districts; and that almost invariably the additional sewers that have been constructed, not being supplied with a quantity of water sufficient to carry off their contents, and keep them clean, they not only do not accomplish any sanatary purpose, but, on the contrary, act as extended cesspools."

The Commissioners then proceed to embody in a series of tables some valuable reports from Sir William Pym, relative to the total number of cholera cases in the metropolis, reported to the Board of Health in 1832; a return of the number of deaths from fever in 1838, and a return from the Fifth Annual Report of the Poor Law Commissioners of the number of fever cases for the same year (1838) among the pauper population of the same 20 districts of the metropolis, from which the deaths from fever for the whole of the population has been taken. These tables establish to demonstration the general coincidence of the cholera track with the track of typhus, as attested by the witnesses from particular cases within their own observation. On comparing, in the 30 metropolitan districts, the proportion of deaths to the population from fever and from cholera, it appears that in the districts where the deaths from fever were the highest, as St. George's-in-the East, Bermondsey, Southwark, Lambeth, Whitechapel, Stepney, and Bethnal Green, cholera was the most prevalent and fatal. In some of these places the deaths from fever and cholera were nearly equal, as in Whitechapel. In others, as in Bermondsey, Southwark, and Lambeth, there was an excess on the side of cholera. But there were places in which the deaths from fever absolutely exceeded those from cholera, as in Holborn, where the deaths from fever were 1 in 227, whereas from cholera they were only 1 in 594. In St. Pancras they were still further in excess, the deaths in this district from fever being 1 in 269, from cholera 1 in 933, and in Shoreditch from fever 1 in 256, and from cholera 1 in 1203 of the population, so that in the first case the deaths from fever were more than double the deaths from cholera, in the second case more than treble, and in the latter case nearly five-fold.

"Comparing the total of deaths from fever with that from cholera, in the two groups of districts, it appears that in the 15 districts in which the mortality was greatest, the deaths from fever were 1 in 237, and from cholera 1 in 253, whilst in the 15 districts of lowest mortality, from fever there were 1 in 494, and from cholera 1 in 358; the general average of the whole of the districts being from fever 1 in 319, and from cholera 1 in 296; so that the whole difference between the mortality produced by cholera and that produced by fever, is the difference between 296, the average deaths from cholera, and 319, the average deaths from fever."

With regard to the proportion which *attacks* of fever bear to those of cholera, it appears, that in a population of 851,229, there were of in-door and out-door paupers 77,186; and that out of this number of paupers, 13,972 were attacked with fever; whereas from the Cholera Return, it appears that out of a population of 1,486,020, only 11,020 were the subjects of cholera, being the total number of the registered cases of cholera occurring in the metropolis during the year 1832.

While the total number of attacks of fever is thus enormously in excess of the total number of attacks of cholera, the *absolute* mortality from cholera is not very materially in excess of that from fever, the proportion, as already stated, being as 296 to 319; but the *comparative* mortality of cholera is terrific; nearly one half of those that are attacked by this dreadful disease inevitably perishing, the utmost range, between its lowest and highest mortality, being that between 1 in 3.6 and 1 in 1.1.

Typhus fever, which since 1838 has been epidemic in the metropolis, has for the last three years been constantly on the increase. The admissions into the London Fever Hospital since April, have exceeded by several hundreds those of any corresponding period, and, as is clearly shown by a table, has been increasing during 1845, 1846, and 1847, for corresponding periods of time in proportions represented by the average numbers 28, 32, and 34; 24, 28, and 44; 21, 31, and 66; and for the six weeks ending the middle of November, by 26, 48, and 80.

From these startling facts the commissioners draw the very reasonable inference that "the causes of epidemic disease continue to operate in the metropolis with unabated and even with increased force at the present time;" and "that were cholera to revisit it at the present time, with the existing predisposition to epidemic disease, it would come at a period peculiarly favorable to its extension."

The Commissioners conclude that portion of their Report which relates directly to cholera, by some suggestions respecting the measures of alleviation which may be adopted in anticipation of those permanent works of drainage and cleansing, which offer the best hope of preventing the advent and spread of the disease.

Reverting to the recommendation of the Central Board of Health in 1832, that a number of steady men, proportionate to the districts in which they are to act, should be appointed to lime-wash and purify, under the direction of medical authority, such apartments as may be pointed out by inspectors of the local Boards, they give it as their opinion that, by some modification of this plan, an efficient agency might be formed for the thorough cleansing, both of particular localities and of individual houses; but of the nature and extent of the additional arrangements which may be necessary, they are unable to judge until they shall have received a sufficient number of returns to the circulars addressed to the medical officers and the Boards of Guardians of the metropolis.

With regard, however, to the measure of alleviation chiefly relied on during its last visitation, viz., the establishment of district cholera hospitals, they state that experience is by no means favorable to their re-adoption, except under particular circumstances and modifications. The prostration of all the vital powers in a severe attack of cholera is often so great, that the mere assumption of the erect position for a few minutes appeared often to deprive the patient of the slightest chance of recovery. "The medical testimony is uniform in representing the fatigue of removal as highly injurious in great numbers of instances. It is often strikingly so in the advanced stage even of typhus. It not unfrequently happens that when a patient is removed to the fever hospital in an advanced stage of this disease, on opening the door of the carriage in which he has been conveyed he is found dead; and still more frequently it occurs that when he has not actually expired before he reaches the ward, and is placed in bed, he is cold, pulseless, and insensible, and never rallies, notwithstanding all that can be done to restore animation. In typhus this extreme debility does not take place for many days; often not until the end of the second or third week; but in a severe attack of cholera it occurs in two or three hours, and is sometimes present, in its highest degree, before there is time for the medical attendant to reach the bedside of the patient. This circumstance places the extensive employment of any remedy which involves exertion, or even slight motion, out of the question." This statement is fully borne out by the evidence of the medical witnesses who have had the greatest experience on this subject, which evidence is given at length at pp. 19 and 20 of the Report.

Experience having thus shown that cholera hospitals failed in accomplishing their object, the Commissioners recommend that the best provision practicable should be made for rendering effectual assistance to the individuals who may need it at their own houses. This, in their opinion, would be best effected by the selection of proper persons, who may be instructed as nurses, and engaged to devote their whole time to attendance on the sick at their own habitations, under the direction of the medical officer. Prompt assistance might thus be given to the patient without subjecting him to any risk from bodily fatigue, and without anything being done calculated to excite apprehension or alarm; at the same time that the curative measures employed by the medical attendant would be administered under circumstances peculiarly adapted to ensure their success.

The adoption of the principle here indicated, that of sending competent persons to attend the sick, under medical direction at their own abodes, would be attended with this further advantage--that all the means recommended for cleansing the interior of the house, and for maintaining the atmosphere of the sick room in the highest attainable state of purity, might be most efficiently carried out by the same agency.

Though, for the reasons just assigned, the Commissioners deprecate the removal of cholera patients to seperate cholera hospitals, they recommended that, in cases of extreme destitution, the cholera patients should be sent to the fever wards of the new union-houses, after those establishments have been inspected by officers specially conversant with warming, ventilation, and other structural arrangements.

The Commissioners then state it as their opinion that there is but one safeguard against the cholera, as against other diseases of the same class, viz., such sanatary arrangements as will secure the purity of the atmosphere, particularly by the immediate and complete removal of all filth and refuse, and that not only from the principal squares and thoroughfares, but also from the streets, courts, and alleys of the lowest portion of the population.

The chief measures of prevention are cleansing and ventilation, carried out concurrently and skilfully performed; or if the external atmosphere is not pure, the ventilation of houses may be the very means of producing and aggravating disease.

The prevention, as far as may be of overcrowding is also insisted on as a sanatary measure, and the opinion of the Cholera Commission at Paris and the experience of Breslau are cited in confirmation of this view. The Commissioners, however, in reference to this great evil, say, "There appear to be no available legal means for the immediate prevention of overcrowding; all we can do is to point it out, as a source of evil to be dealt with hereafter."

As an evidence of the preventive efficiency of cleanliness, it is stated that the German colonists in Gallicia, who were distinguished by habits of regularity and cleanliness from the Sclavonic population, were distinguished amidst that population by an immunity from cholera.

The measures of prevention thus pointed out, if not warranted by the presence of cholera, would, as Mr. Bowie justly observes, exercise a most beneficial influence in the prevention of typhus fever; and the same witness gives a striking confirmation of his views in the case of the model lodging-house in Glasshouse yard, where means have been adopted to secure cleanliness and effective ventilation, so that "whilst fever has been prevailing to a very great extent in Glasshouse street and its adjacent courts and alleys, and the verdict of a coroner's jury has been given that disease and death have been the consequence of breathing impure air, there is not at present an individual under medical treatment in the building, nor has there been a single case of fever there for upwards of four months. The only deaths which ever occurred among the lodgers were two children, labouring under hydrocephalus internus when they were admitted, and an aged mutilated seaman, who had long been affected with hydrothorax and disease of the heart." Such facts as these certainly warrant Mr. Bowie in believing that typhus fever, "might be as completely put an end to in houses, villages, and towns, as the ague has been in many parts of the country." Mr. Liddle, another witness, the medical officer of Whitechapel Union, who has already contributed one or two very striking statements as to the efficacy of cleansing, drainage, and ventilation in banishing fever, and reducing the amount of disease, adds the following fact: "That Hairbrain court consists of 13 houses; that in this court he has attended 22 cases of typhus within the last six months; that during the prevalence of fever this place was without drainage, and without water, and very badly paved and cleansed; but that recently it has been drained into the new sewer in Blue Anchor yard, and that since this was done not a single case of fever has occurred in this court. In like manner in Cooper's court, which consists of 12 houses, he has attended 29 cases of fever within the last six months; the condition of this court was precisely similar to that of Hairbrain court, but Cooper's court having been drained, fever has taken its departure from this place also."

The Commissioners suggest that the principle of flushing may be immediately applied to the draining of courts and alleys, and the rapid and safe removal of decomposing refuse, but that this can only be effectually done by a single body, with which voluntary associations of district visitors or local boards, might co-operate with effect in carrying out measures for the removal of nuisances, and which might make early agreements with the several water companies for the necessary supplies of water; and the Commissioners state that they have had detailed measures placed before them for the safe and prompt removal, by the free use of water, of the soil of cesspools and privies in the worst localities, and the collections of filth in their dirtiest courts and alleys.

The following are the general conclusions at which the Commissioners arrive:

"That amidst the town populations the cholera visits with most severity the same classes of persons and the same places, and is governed nearly by the same circumstances as typhus.

"That it has been proved by experience that those circumstances are generally removable by proper sanatary arrangements, and that typhus is to a great extent preventible; and we have every reason to believe that the spread of cholera is preventible by the like means, namely, by general and combined sanatary arrangements.

"That these arrangements, instead of being incidental and collateral to other measures, are paramount, and principal, and effective, not only against cholera, but also against other epidemics.

"That when cholera first appeared in this country the general belief was that the disease spreads principally, if not entirely, by communication of the infected with the healthy, and that therefore the main security of nations, cities, and individuals consists in the isolation of the infected from the uninfected—a doctrine which naturally led to the enforcement of rigorous quarantine regulations; the establishment of military and police cordons; the excitement of panic; and the neglect and often the abandonment of the sick even by relations and friends.

"That since opportunities have been obtained of a closer observation of the character of this disease, and of the mode in which it spreads through continents, nations, cities, towns, and families, facts have been ascertained which are incompatible with the foregoing view of its mode of dissemination and of its prevention.

"That the disease is not, as it was then generally supposed to be, contagious, and that the practical application of that doctrine did no good, but was fraught with much evil.

"That when it previously visited this country it was believed that the most powerful predisposition to this disease is induced by improper or deficient food, and that, for this season, its chief victims are found among the poor; but it is now universally admitted that a far more powerful predisponent is the habitual respiration of an impure atmosphere; that the highest degree of susceptibility is produced where both these conditions are combined, that is, where people live irregularly, or on unsuitable diet, and at the same time filthily; and that, in places in which a great degree of cleanliness is maintained, the poor as well as the rich enjoy exemption from this disease.

"That on an examination of the actual state of the back streets, lanes, courts, and alleys of the metropolis, it is found that in general little or no improvement has taken place in their sanatary condition since the prevalence of cholera in 1832; and that were this disease again to break out in the present state of these localities, there is no reasonable ground to suppose that the pestilence would not spread as extensively and prove as fatal as on its former visitation.

"In regard to this disease, we fear that complete measures of prevention must be eventual on the combination of works, which must be the subject of further investigations; but in respect to the immediate and special measures available for the prevention of the cholera, we find that such would be measures of cleansing of whole lines of sewers, from their commencement, through the several districts to the outfalls; the cleansing of cesspools (wheresoever it may be affected into the sewers), and the removal of whatsoever may be removed in suspension in water in the various modes of flushing by the use of additional and abundant supplies of water, and we find—

"That it is expedient that a Commission for the entire drainage of the whole of the metropolis should be appointed, with a special view to such measures, and with aid to carry them out.

[Such a Commission, embracing all the districts, with the exception of the city of London, has been lately appointed, and is now actively engaged in carrying out the recommendation of the Report.]

"With respect to the measures of alleviation of cholera we find—

"That it is one of the peculiar characteristics of this disease, that it sets at defiance, to a great degree, the resources of medical art and science, as is too fully proved by fact that, under the most favourable circumstances, of those whom it attacks there perish one out of three, or nearly one out of four, and under the most unfavourable circumstances nine out of ten.

"That still there can be no doubt that individuals are saved, who would otherwise perish, that are early placed under favourable circumstances and judicious medical treatment.

"That although the removal to cholera hospitals, unless at a very early period of the attack, and unless the situation of the hospital happened to be highly favourable, was proved by experience to be injurious rather than beneficial, yet among the classes most subject to this disease, there must be individuals in a state of such utter destitution as to render some provision absolutely necessary.

"That it is desirable that existing establishments for the reception and treatment of the sick be immediately inspected by officers especially conversant with warming, ventilation, and other structural arrangements, to advise on the alterations and adaptations necessary to afford effectual aid to the individuals who may require it.

"That where there is at present adequate accommodation by proper hospitals for fever cases, such accommodation will in general suffice for cholera, fever not being prevalent when cholera is epidemic, and fever cases being in general more numerous than those of cholera."

The remainder of this valuable Report consists of an examination of the practical working of the sewer commissioners of the metropolis. The general conclusions are all that our limited space will enable us to notice. They are as follows:

"That unnecessary expense and inconvenience to the public is consequent on the division of the natural drainage areas among several district authorities, and that it is impossible that improved works of systematic drainage can be carried out under arrangements that geographically divide the lines of watershed and the outfalls between seperate and conflicting authorities.

"That the works which the present district commissioners execute, and propose to execute, are uncertain, erroneous, and defective in their general principles of construction, injurious in their actions, and unduly expensive.

"That, after the authentic expositions which have been given of the principles of construction and management of improved works, the extension of sewers or drains, accumulative of decomposing refuse, are acts of injury to the public health and of waste of the public money.

"That the execution by the district courts of commissioners of large works of drainage or sewerage, without reference to any general plan or survey, involves great risk of erroneous and imperfect works and waste of the rates they are empowered to levy.

"For the prevention of disease and the saving of health and life, by early carrying out efficient works of drainage, and diminishing the mass of atmospheric impurities, by which the public health is depressed, and, for the prevention of expenditure upon inefficient works, we feel it our duty to recommend an immediate exercise of the powers of the Crown, and

"That the several commissioners appointed under its authority, in the metropolis, be recalled with the least possible delay.

"That the law of sewers, now administered by numerous persons in these seperate districts, be confided to one body of commissioners for the whole of the metropolis.

"That to ensure executive dispatch, and obviate that weakening of responsibility which arises from its present division amongst large bodies, the commissioners should be limited in number; and competent, through their known attention to sanatary improvement, to select and sustain the labors of paid officers and the execution of works in the attainment of this their proper object."

This consolidation is suggested as a measure of immediate urgency to abate epidemic disease and to stay waste, and as essentially preparatory to further alterations which the Commissioners propose hereafter to submit for consideration.

The Commissioners wisely anticipate that the first work of the consolidated commission will be a general survey by the officers of the Royal Engineers, under the direction of the Board of Ordnance, as a measure of paramount and most pressing importance.

The expediency of making this survey of the metropolis, in the first instance is very properly urged, on the ground that the metropolis serves as an example and guide to the provinces. "The errors of the works of this class in the metropolis are literally copied and exaggerated in the provincial towns, where it is rare and accidental to meet with any improvements upon them. In the provincial towns, which had abundant sources of water-supply within reach, the pernicious system of intermittent supplies have been copied from the metropolis, to the injury of trading companies, the deterioration of the supplies, and double expense of works to the consumers."

The Commissioners, aware of the ignorant objection often advanced to the creation of new bodies on the score of expense, cite the remarkable example of consolidation afforded by the Metropolitan Road Commission for the management of the roads formerly administered by a number of local trusts comprehending the suburban parishes in the metropolis. Under that commission the roads have been improved, the tolls and the debts reduced, and the business of 100 miles of road transacted satisfactorily with less attendance and consumption of time on the part of the honorary members of the board than was previously required, by the defective dispatch of business, by any one of the numerous seperate boards under which important improvements were found to be impracticable.

The commissioners have yet to report on the commission of sewers for the City of London, on the water-supply, and surface cleansing and paving of the metropolis, on the assessment and collection of rates, and on other important matters. These labours of the commission will have to be noticed in a future report. In the meantime the progress of the cholera on the Continent, and its now confidently reported presence in London, lends to this first Report of the Metropolitan Sanatary Commission an importance which will fully justify the details into which we have entered, at the same time it must serve as an explanation and excuse for the omission of much important and interesting matter which would otherwise have found a place in this Report.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*Case in which a Foreign Body became lodged in the Trachea—value of the Stethoscopic Diagnosis—its removal by Operation and Inversion.*

H. W———, æt. 13, was admitted into Guy's Hospital on Friday morning, at 2 A. M., 30th of July, 1847, with the following history:—On Thursday, the 29th of July, he was running a race with another boy, and had placed two pebbles in his mouth for the purpose of keeping it moist (a common custom on such occasions); and, while urging his speed to the utmost, on taking a full inspiration, one of the pebbles slipped from his mouth, and it seems passed into the larynx down into the trachea, as he was immediately seized with a fit of coughing, and some dyspnœa, which, however, soon subsided; but the boy immediately expressed his conviction that the pebble was in his "windpipe," as in the act of coughing he could distinctly feel it moving up and down the passage; but, at the same time, said that it never passed above a point to which he directed attention (the lower edge of the cricoid cartilage), which would account for the absence of the violent paroxysms that attend the presence of a foreign body in the glottis.

Mr. Pritchard, of Foot's Cray, was sent for, and immediately concluded, from the history and symptoms, that the boy was correct in his suspicions that a foreign substance was in the trachea; and therefore proceeded to place him with his head downwards, in which position he remained for some time, but without the desired object obtained. This experiment was three times unsuccessfully repeated, producing each time violent coughing and difficulty of breathing; and ecchymosis of the conjunctivæ resulted from his forcible straining.

During the whole period of this ordeal, the boy said that he could feel the pebble move up and down in his windpipe. Mr. Pritchard, concluding from the circumstances of the case, from the lad's clearly-expressed feelings, and from the effects of inversion, that the stone was in the trachea, sent him at once to Guy's Hospital, where he was placed under the care of Mr. Bransby Cooper.

On his admission, the patient was free from cough or dyspnœa, his respirations were 20 in a minute, and he slept composedly in the usual position, but still stating his conviction that the stone was in the windpipe, and indicated its position to be about an inch above the sternum.

FRIDAY, 2 P.M.—At this time he was first seen by Mr. Bransby Cooper, who, by auricular exploration, could not obtain sufficient evidence to convince him that any foreign body was still in the trachea; and argued the probability of the abnormal sounds being produced by the substance having irritated the glottis, or that, if actually admitted into the windpipe, it might have been expelled. Mr.

Cooper requested Dr. Hughes to examine the patient, who found roughened tracheal breathing, with wheezing on the right side, and to a less degree on the left, but acknowledged the physical signs were unsatisfactory as to the positive proof of a foreign body being still in the trachea, probably in consequence of the general irritation of the trachea and larger bronchial tubes.

On consultation, it was determined not to interfere at present, but to wait for more certain indications of the actual presence of the foreign body, to keep the patient perfectly quiet, and to have him constantly watched.

In the evening, it was observed by Mr. Hilton that the left lung was nearly inactive, scarcely any murmur being audible. The breathing, however, was not attended with any distress.

31st.—Passed a comfortable night in a sitting posture, with occasional cough, but without any violent dyspnœa. The left lung remains in the same condition; but, in the course of the day, after a slight fit of coughing, the respiration became quite audible in it.

On Sunday, August 1st, he remained much in the same state.

On Monday, 2d, he was examined by Dr. Addison, who found diminished supply of air to the left lung; but as the boy had undergone much examination, he agreed with Mr. Cooper, as he was not suffering from urgent symptoms, that he should be kept perfectly quiet until the next day, when a more complete exploration might be made, and the propriety of an operation determined on.

3d.—Dr. Addison examined him to-day. Breathing had returned completely in the left lung, and was puerile there, but at the apex of the right lung it was more full, and accompanied with a slight roughness. This was the state of the breathing found both at the anterior and posterior aspects of the chest. In all the lower part of the right lung the breathing was pure and loud. The percussion over the apex of the right lung was less clear than on the left side. When the patient coughed a movement was heard and felt as of a foreign body impelled by the air at each operation.

Dr. Todd who happened to visit the hospital to-day, examined the patient with Dr. Addison, and concurred with him, and indeed with Dr. Barlow and Dr. Hughes, in the opinion that from the present signs and the previous history of the case, no doubt could now exist of the presence of a foreign body in the upper branch of the right bronchus that impeded the passage of air into the upper lobe of the lung. The shifting of the impediment from the left to the right side, as first noticed by Mr. Hilton, was much in favour of this opinion. A consultation was now held, and it was resolved that the operation of tracheotomy should immediately be performed, in order to allow of the inversion of the patient with comparative safety.

The operation was performed in the usual way by Dr. Bransby Cooper: a free opening was made into the trachea, and four of its rings divided by a bistoury.

During the operation, both before and after the opening had been made in the trachea, the patient coughed violently, and stated that he felt the pebble move, but he thought it was expelled through the opening. A probe was passed into the trachea with a view to feel the pebble, but without any other effect than that of making the patient cough violently. He was now inverted, and struck forcibly on the back, when he said he felt the stone move above the wound in the trachea, and while in this position, during the act of inspiration, the pebble fell through the wound made in the trachea into Mr. Hilton's hand.

The outline of the pebble will show its size and form; its thickness may be about four lines.

Since the operation the boy has been free from any constitutional disturbance; he breathes freely, no air passes through the wound, and he may now be considered uite convalescent.—*London Medical Gazette.*

2.—*On a Function of the Red Corpuscles of the Blood, and on the Process of Arterialization.* By GEORGE OWEN REES, M. D., F. R. S., &c.

The author states that he was first led to the new theory he has formed for the explanation of the chemical phenomena of respiration, and more especially of the change in the color of the blood which occurs in that process by having observed that a garlic odour, similar to that evolved from phosphorus, was produced by agitating in distilled water the clot obtained from some specimens of venous blood. His attention was consequently directed to the investigation of the state in which the phosphorus exists in the blood; and the result of that investigation was the theory, of which the following is a succint outline.

The venous corpuscles are known to contain fat in combination with phosphorus. This compound ingredient of the corpuscles on coming into contact with atmospheric oxygen during the respiratory act, is consumed, and combining with that oxygen, forms the carbonic acid and water which are expired, and also phosphoric acid, which, uniting with the alkali of the liquor sanguinis, forms a tribasic phosphate of soda. This salt, like many others, acts upon hæmatosine in such a manner as to produce the well-known bright arterial tint.

The analyses which the author has performed in order to test the correctness of this theory were made upon the blood, both of the veins and of the arteries of the same animal; and also upon separated portions of the same venous blood, one of which portions had been artificially arterialized by having been brought into contact with air, while the other portions had not been so exposed. These comparative experiments showed that arterial blood, both when obtained from the vessels, and when artificially produced, contains in its serum a larger proportion of tribasic phosphate of soda than that obtained from the veins. The venous corpuscles, as they are contained in the clot, yield a fatty matter combined with phosphorus; while those from arterial blood yield a fat, the ashes of which manifest an alkaline reaction. Thus the venous corpuscles are shown to be acted upon, both by respiration and by the artificial arterialization of the blood, in such a manner as to lead to the formation of tribasic phosphate of soda at the expense of the phosphorus they contain.

No exact quantitative analyses were attempted by the author, the comparative experiments having been performed on small portions only of serum (from 25 to 40 grains); sufficiently large, however, to furnish satisfactory evidence of the actual presence of the phosphate in arterial blood, and also in those portions of venous blood which had been arterialized out of the body; while no such indications were obtained from similar portions of the blood contained in the veins.

At the conclusion of the paper, the author notices the experiments of Enderlin, in which no alkaline carbonate could be detected in the ashes of blood; and shows that this is the natural consequence of the phosphates of the clot being oxidized during combustion, and that supplying a quantity of phosphoric acid sufficient to decompose completely the alkaline carbonate produced by the incineration of the lactate and albuminate of the serum. Most specimens of serum, even as obtained from arterial blood, yield an alkaline carbonate when incinerated; and this is always the case with the serum of venous blood. The author therefore thinks himself warranted in regarding the conclusion founded on Enderlin's experiments, that the blood contains no lactate, as being erroneous. *Proceedings of the Royal Society, June* 3, 1847.—*British and Foreign Medico-Chirurgical Review.*

3.—*On the Urine in Typhoid Fever.* By M. MARTIN-SOLON.

M. Martin-Solon recently (Nov. 1847) read at the Académie an interesting paper upon the condition of the urine in typhoid fever, the principal points of which are thus summed up: 1. The urine in typhoid fever is less abundant, higher coloured, aud generally more dense, than in health. 2. It is as acid as in the normal state, and sometimes more so. 3. It is rarely alkaline, but in consequence of its large proportion of urea, it rarely passes into the condition of alkalescence. So abundant is the urea, that sometimes a nitrate may be at once formed by the addition of nitric acid without any preliminary evaporation, a density of from 1·030 to 1·036 indicating this condition. 4. Generally transparent, the urine is sometimes clouded with mucus (the *enæorema* of the ancients,) or by an excess of too sparingly soluble salts, which give it a "*jumenteux*" appearance, and which give rise to sediments, formed especially of uric acid and the urates with colouring matter. 5. That critical signs deduced from these appearances are not to be depended upon. 6. That in transparent urine, nitric acid sometimes gives rise to no reaction, but in other cases produces a *cloudiness* immediately. 7. The same thing is observed in "jumentous" urine, when rendered clear by filtering. 8. That this cloud, of a peculiar tomentous aspect, formed by a bi-urate of ammonia, but the nature of which is perhaps not vet entirely known, is seen especially at the period of the resolution of typhoid fever and acute diseases, which it precedes and announces; and according to our clinical observations (in 54 cases) possesses a *critical* value which deserves attention. 9. That the bile undergoes a notable alteration during typhoid fever, which is doubtless the cause of the appearance of *biliverdine* in the urine. 10. That the urine sometimes becomes temporarily albuminous during the course of acute diseases; but that the congestion of various organs, especially partaken of by the kidneys, and the especial tenuity of the blood in typhoid fever, render such temporary albuminuria far more common in this than in other affections. 11. Temporary albuminuria is especially seen in severe cases of typhoid, and generally gives rise to the most unfavorable prognosis. 12. Temporary albuminuria may sometimes become continuous, and the kidneys then exhibit the usual pathological characteristics of confirmed albuminuria. 13. The inspection of the urine throws light upon the progress of a case of typhoid fever, and may serve as a means for the direction of its treatment.—*Bulletin de l' Académie*, tom. xiii, p. 398.—*British and Foreign Medico-Chirurgical Review.*

4.—*On the Presence of Sugar of Milk in the Milk of the Carnivora.** By Dr. A. Bensch.

In Dumas' experiments upon the milk of a bitch, it was shown that the sugar completely disappeared when the food consisted solely of flesh, whilst it always existed, and in considerable quantities, when the diet was vegetable. The following experiments, however, prove that this is not the case, and that the sugar is always present in the milk; also, that it frequently undergoes a change owing to the method of analysis, which prevents its crystallisation.

Two large dogs, which had pupped four days before, were fed entirely upon meat; the pups were separated from them six hours before the milk was drawn. The milk reddened litmus-paper at the moment it left the teat. When heated it coagulated, forming a dense mass; on dilution with water it lost this property, and coagulated imperfectly. When diluted, and treated with a little acetic acid, the caseïn readily separated on boiling; the fluid is easily filtered when a small quantity of a mixture of ether and alcohol is added to it. After the evaporation of the ether and alcohol, the whey reduced persalts of copper on the addition of potassa, which indicates the presence of sugar.

In analysing it, I adopted Haidlen's method, increasing the quantity of sulphate of lime to half the weight of the milk, so as to obtain a residue capable

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* Liebig's *Annalen*, bd. lxi., heft 2.

of being completely dried. On drying the residue, unless especial care be taken, it frequently occurs that it suddenly increases in weight, probably from a powerful oxidation of the fatty matter; this amounted in one case to 30 milligrammes in the space of six hours. The milk of the first dog had a specific gravity of 1·036, and was acid. It yielded on analysis—

Water, - - - - - -	75·54
Butter, - - - - - -	10·75
Sugar and soluble salts, - - -	3·47
Caseïn and insoluble salts, - - -	10·24
	100·00

The solution obtained by exhausting with alcohol the residue left after extracting fatty matter with ether was evaporated, the residue treated with absolute alcohol, the insoluble portion dissolved in water and set aside; no crystals of sugar were formed, although oxide of copper was readily reduced when potassa was present. Another portion of the same milk was diluted with three parts water, a few drops of acetic acid were added, the mixture heated and filtered, the filtered liquor evaporated with carbonate of magnesia, and the dried mass exhausted with alcohol until the reaction of the magnesia discontinued; the residue on the filter exhausted with hot water, and when thus separated from the carbonate of magnesia, on the evaporation to consistence of a syrup, yielded perfect transparent and colorless crystals of sugar of milk. Therefore it is certain that during the course of the first analysis the sugar had undergone a change; that probably at the long-continued temperature of 212° it had become converted, by the free acid (phosphoric acid) present, into grape-sugar, and remained as such with the extractive matters in a syrupy form.

In the milk of the same dog, examined after continuing diet of flesh, for 12 and 27 days, crystals of sugar of milk were also found in both cases. In the case of the second dog, after five days' animal diet, the alcoholic solution reduced the oxide of copper with potassa, although it did not yield crystals of sugar.

Dr. Bensch considers that the acidity of the milk arises from the presence of acid phosphate of lime, which salt, with the phosphate of magnesia, constitutes the greater part of the ash of this fluid. He thinks it probable, as previously stated, that the inability to procure milk-sugar from the milk of dogs fed exclusively upon animal diet arises from its conversion into grape-sugar by the acid phosphate of lime; whilst in the milk of dogs fed upon vegetable food, which has a neutral or alkaline reaction, this transformation is prevented.

From the above experiments, it is evident that even with long-continued exclusively animal diet the sugar does not disappear from the milk; consequently the organism must possess the power of forming sugar of milk from the fatty nitrogenous matters.—*The Chemist—July*, 1847.

5.—*New Test for Prussic Acid.** By J. Liebig.

The nature of the test will be immediately understood from the following short extract:—

"A couple of drops of prussic acid, which has been diluted with so much water that it no longer gives any certain reaction with salts of iron by the formation of Prussian blue, when mixed with a drop of sulphuret of ammonia and heated upon a watch-glass until the mixture has become colorless, yields a liquid containing sulphocyanide of ammonium, which produces with the persalts of iron a very deep blood-red color."

This statement is fully confirmed, and the great value of the test firmly established by Mr. Alfred Taylor, who proposes a modification of it similar to that already recommended by him in the use of the silver and Prussian-blue test—

* Liebig's *Annalen.*

viz., exposing the hydrosulphuret of ammonia to the vapors of the acid, evaporating the resulting sulphocyanide of ammonium to dryness, and then adding to the residue a persalt of iron. The test so applied is both prompt and delicate; it acts in a few seconds, and succeeds where both the nitrate of silver and the Prussian-blue test fail. Thus, in a comparative experiment, with an acid of the same strength, the Prussian-blue and nitrate of silver tests failed entirely to detect the $\frac{1}{786}$th of a grain in ten minims of water, while the new test detected the $\frac{1}{3930}$th of a grain.

The great advantages of this test, as modified by Mr. Taylor, are the avoidance of heat, and the objections to which its employment gives rise, and its applicability to organic substances even in a state of putrefaction. The test was also applied with success to laurel water, bitter-almond water, essential oil of bitter almonds, decomposed prussic acid, cyanide of potassium moistened with water, cyanides of silver and mercury moistened with strong muriatic acid, ferrocyanide of potassium mixed with dilute sulphuric acid, and prussic acid mixed with decomposed organic liquids. The result of all these experiments is "that the process is more delicate, more speedily and universally applicable, and more certain and unobjectionable in its results, than any of those yet suggested for the detection of this powerful poison." So that Liebig has "here done for prussic acid what Reinsch has recently done for arsenic." It is scarcely necessary to state that the test will not act characteristically unless the hydrosulphuret of ammonia, after exposure to the vapor of the acid, be evaporated to dryness, as the uncombined hydrosulphuret gives a black precipitate with the persalt of iron. The red color of the sulphocyanide of iron is removed by a few drops of a solution of bichloride of mercury. Mr. Taylor recommends, as a simple way of applying the Prussian-blue test, that we should precipitate the mixed oxide from the green sulphate in a gelatinous form, and expose them to the vapor of the acid in a watch-glass.—*The Chemist—October*, 1847.

AMERICAN MEDICAL INTELLIGENCE.

1.—*Discovery and Application of the New Liquid Adhesive Plaster.*

[A Communication addressed to John D. Fisher, M. D., of this City, and read before the Boston Society for Medical Improvement, March 27, 1848.]

Dear Sir,—Some time last summer, when you were at Dedham, you requested me, as you may recollect, to furnish you with some account of a liquid adhesive plaster, which I had been using in surgical operations, with permission for you to read it before the Boston Society for Medical Improvement.—Although I had at that time made many experiments with the new adhesive substance, and had formed a very favorable opinion of its properties, still I did not feel willing to express this opinion in a paper to be read before the above learned Society, until I had perfected the manufacture of the substance itself, and employed it in surgical cases sufficiently numerous and various, to determine its true adhesive qualities and real importance to surgical and medical science. Consequently time passed on, and I had really forgotten the request you had made, until I was reminded of it by reading in some Journal, a day or two ago, the announcement that my friend and fellow student, Mr. Samuel L. Bigelow, had written a paper on the subject of the new adhesive material, and that his paper was read before the Society for Medical Improvement by one of its members at its last meeting. This circumstance has induced me to address you this communication, in compliance with the suggestion you made to me last summer, which communication I submit to your disposal. As I shall in this letter speak of the nature and the history of the application of this new adhesive fluid, it is very possible that I may repeat some things that have already been said on the subject by Mr. Bigelow. Should this be the case, my total ignorance of the contents of Mr. B.'s paper must be my apology.

While attending the medical lectures in Boston the winter before the last, Mr. Bigelow showed me a liquid which he was using as a varnish, and informed me, that it was made by dissolving "gun-cotton" in sulphuric ether, and that he obtained the directions for making it from Dr. Charles T. Jackson. Having at this time occasion to use some varnish for a purpose to which the common varnishes of the shops were found by experiment not to be applicable, and noticing that this "gun-cotton" varnish dried suddenly and became hard, transparent and glossy, it occurred to me that it might answer the purpose I had in view. I therefore requested Mr. Bigelow to furnish me with a small quantity of the liquid. The quantity he kindly gave me. I made experiments with it as a varnish, but soon discovered that it would not answer the object I had in view. For instead of improving and protecting the gilded surface, as I had hoped it would, it destroyed it, probably by the action of the acid it contained. While making this application of the varnish, my fingers became covered with it, and I noticed that my index and middle fingers were so firmly glued together by the varnish, that it required a considerable degree of force to separate them.

This accidental occurrence at once suggested to me the idea that this fluid, as it suddenly became solid, and seemed to possess an adhesive tenacity unequalled by any known gum, might be made use of as an elegant and effective substitute for the common adhesive plaster, and become an important agent in surgery. Impressed with this idea, I made experiments with it on my own person; first, by spreading the fluid over the surfaces of two of my fingers with a small brush, and allowing it to dry while the fingers were in contact; and second, by moistening straps of cotton cloth and of sheep-skin with the liquid, and applying them on the back of my hand. The fingers were soon found to be glued together somewhat firmly, and the cotton and sheep-skin straps to adhere strongly to the parts on which they were applied. These simple experiments convinced me, that the substance would answer as an adhesive plaster in incised wounds, and I used it as such on a little neice of mine, who had cut her finger, and then on my own hand which had been accidentally wounded. In both of these instances it proved perfectly successful, keeping the incised surfaces together until they healed. The fluid was used in these cases in the following manner:—It was spread by means of a brush over the approximated edges of the wound, and also over the sound skin, on each side, and a thin strap of cotton cloth was pressed upon it, which soon became firmly united to the surface, by the evaporation of the ether, retaining the cut edges immovably together. The wounds in these cases healed by the first intention, and the straps were not removed until perfect and solid union had taken place.

These I believe to be the first surgical applications that were ever made with this new adhesive mixture. Feeling somewhat elated by the success of the experiments, and by the idea that I had made a discovery that might prove of value in operative surgery, I informed Mr. Bigelow, that I had discovered a new and important application for his "gun-cotton varnish," and related the experiments I had made. Some time after this, he told me that he had made use of his varnish, as I had previously done, in surgical operations and with success. The experiments I had made exhausted the small quantity of the varnish that had been given me, and to obtain more I was obliged to attempt the manufacture of it by dissolving the gun-cotton in ether, according to the formula furnished me by Mr. Bigelow. But on trial I found that gun-cotton dissolved in ether would not produce the desired gum. Being in Dedham at the time, I wrote a note to Mr. Bigelow, mentioning the failure of my attempts to re-produce the article, and requested him to give me particular directions how to make it. In answer to my note, Mr. B. stated that he, like myself, had been unsuccessful in his efforts to make "gun-cotton" yield a gum such as he had before used and given me. I now determined to make experiments with the view of effecting the re-production of the adhesive solution. I accordingly obtained, from the city, a large quantity of acids, and commenced the preparation of the raw cotton, and after many trials and many failures I finally succeeded in preparing a cotton, which would dissolve in ether and form a gum of greater adhesive qualities than that I had been using.

In this connection I may observe that in attempting, at a subsequent period, to make more of the article, I failed, having mislaid my notes specifying the exact proportions of the acids previously used, and the length of time required for them to act upon the raw cotton. Consequently I was compelled to repeat my experiments in order to re-produce "the ethereal solution of prepared cotton." I call the adhesive liquid by this name, rather than by that of solution of gun-cotton, for the reason that I have never been able to produce the article from gun-cotton. Pure gun-cotton will readily dissolve in ether, but the solution possesses no, or only very slight, adhesive properties.

Having now at command any desirable amount of this new adhesive preparation, I made it a business to investigate its usefulness in the healing art, by employing it myself, and engaging others to experiment with it. Dr. Whitney, of Dedham, was furnished with some of the solution soon after I had prepared it; and Dr. Fisher of Boston, Dr. Warren of Waltham, Dr. Clarke of East Cambridge, Dr. Comstock (now residing in Wrentham,) and a few others, were supplied with it, some eight or ten months ago. In July or August last, Dr. John C. Warren was informed of its nature and properties by Dr. Fisher, and recently I gave some of it to Dr. J. Mason Warren, who used it in his private practice, and afterwards in the Mass. Gen. Hospital. Previous to the commencement of the last course of medical lectures in Boston, I had used it and seen it used by my instructor, Dr. Whitney, in more than a hundred cases of surgery, some of which were of a serious nature; and in these cases it was most successfully employed, and was found to possess great advantages over the common adhesive plaster of the shops. On a future occasion I intend to draw up a detailed report of the cases in which the liquid adhesive plaster has been used by Dr. Whitney, myself, and some other practitioners who have employed it in their surgical and medical practice. To do this now, would require more time than I have at command. I will, therefore, at present, merely state that the preparation has been employed by Dr. Whitney and myself, with the most gratifying results, in cases of incised wounds; in fractures of the fingers, in which it performed the office of an immovable bandage; in a case of hernia occuring in a child; in cases of deep ulcers, in which it was desired to approximate the surfaces of the sores for the purpose of hastening the process of granulation; in four cases of amputation of fingers, accidentally caused by a circular saw, and other cutting instruments; in cases of burns, attended by loss of substance; in two cases of enlarged testicle, accompanied by an effusion into the scrotum; in the case of an operation on the face of a young lady, for the cure of a deformity resulting from a severe burn; in the case of a wound in the scalp, made by extirpating a wen from the head. These are some of the surgical cases in which I have witnessed the successful application of the cotton plaster.

The mode in which it was used as a dressing in these cases, varied according to the nature, size and and situation of the wound. In slight cuts, a moderately thick coating of the solution laid over the incised parts was, on becoming dry, sufficient to keep the lips of the wound in position till union took place; but in most instances it was employed in conjunction with straps of cotton and sheep-skin, and with raw cotton, forming with them strong, unyielding, adhesive straps, bandages and encasements; and after many experiments, I am convinced that this is the best and most effectual way in which it can be employed as an adhesive agent in surgery. The solution dries rapidly, and in a few seconds, by the evaporation of the ether it contains, it becomes solid and impermeable to water—and a strap moistened with it and glued to any part of the cutaneous surface, adheres to it with a tenacity that is truly surprising.

In proof of this, I will mention the following facts. A strap of sheep-skin, glued to the hand by a thin layer of the solution, nine lines long and one and a half wide, sustained a weight of two pounds. A second strap, attached to the hand by a layer of the substance, nine lines in length and three in width, sustained a weight of three pounds. A third strap, fixed to the hand by a layer of the liquid, twelve lines square, resisted the force of ten pounds without giving way; and a fourth strap of the leather, glued to the hand by a stratum of the solution, measuring one and three fourths of an inch in length and one in width, was not separated from its attachment by the gravity of twenty pounds! These statements may appear incredible; but they are founded on exact and carefully performed experiments, and are true. No other known gum possesses such adhesive power as these experiments show this cotton gum to be endowed with. No adhesive plaster hitherto used in surgery is to be compared to it in this respect. It therefore can be made use of in cases in which the common adhesive plaster would be useless.

The wonderful adhesive properties which my experiments proved it to possess, suggested the thought that it might answer the purpose of sutures in surgery. And an opportunity soon occurred to enable me to decide the fact that it would. I allude to the operation performed by Dr. Whitney, for the removal of a wen from the head. Fearing that an erysipelatous inflammation might arise in the scalp, in case he united the divided parts by sutures, Dr. W. shaved the hair from the raised scalp, and by means of the cotton solution he glued some short and narrow straps of sheep-skin on each flap, a short distance from their edge. These straps were then drawn towards each other until the edges of the wound were brought into close and exact union, and the free ends of the straps were fastened together by sutures. In this case the needle and thread were passed through inanimate leather instead of living flesh, causing no pain to the patient and no interruption of the process of healing. The wound healed favorably, and without the usual accidents necessarily occasioned by the presence of sutures in, and the operation for their removal from the parts. The happy result of this case convinced me that a means was now discovered which would enable the surgeon to do away with sutures, pins and needles, in most of the cases in which these are at present considered indispensable.

Although unauthorized to do so, I must take the liberty, in this place, to mention the interesting fact that Dr. Comstock, of Wrentham, has recently employed this liquid as a dressing in a case of extensive laceration of the perineum, with a success that he thinks never attended any other mode of management. The dressings remained firmly attached and solid during the process of healing, notwithstanding they were for a time almost constantly covered by urine and mucus, and subject to being displaced by the movements of the patient. This case, I trust, will be communicated to the profession, as it supports the opinion I have advanced that this new adhesive solution will be used as a substitute for sutures and needles.

From the success that attended these two last-mentioned operations, every surgeon and practitioner will readily imagine how effectual and valuable this new dressing must be, in cases where there is great loss of substance—in operations for hare-lip, artificial nose, &c. But I will not attempt to predict the cases in which this new adhesive substance may hereafter be successfully employed. I prefer to speak of it only in connection with cases in which its value has been tested. Future experiments must determine the applications that can be made of it in surgery, and its true value to medical science. As a varnish, it may be useful in the arts—and has been found to afford protection to the fingers and hands while engaged in dissections and autopsic examinations. It was used for such a purpose last summer by Dr. Whitney and myself. I might also speak of the applications that have been made with it in medical practice, as in cases of burns, of eruptive diseases, of sore nipples, &c.; but I must bring this long and hastily-written letter to a close. In it I have given you a true and faithful history of this new adhesive agent, so far as I am connected with or have any knowledge of it.

Had I not heard to-day, while visiting the Hospital, that I had no claim to the credit of having originally applied this new agent to surgery, I should have signed my name to this letter without alluding to the subject. But since my pretensions are disputed, I will remark that the grounds on which I rest my claim are the following:—1st. That I used it in the first instance on my own person—then on the body of another—again upon a wound on my own hand, and that these cases were the first instances, as I believe, in which it had been surgically applied. 2nd. I afterwards communicated the fact of my having surgically used it to my friend and fellow student, Samuel L. Bigelow, upon whose veracity and memory I must depend for the corroboration of the statement. 3d. Public announcement was made last summer, in the journals of the day, that it had been applied most successfully in a surgical operation performed by Dr. S. S. Whitney of Dedham, upon the face of a female for the cure of a horrible deformity caused by a burn in childhood. 4th. I have used and superintended its use for more than a year, in over a hundred cases of surgery. For proof of this I refer to Dr. Whitney of Dedham, Dr. Fisher of Boston, Dr. Mason of Lowell, and the patients who were the recipients of its benefits. Notwithstanding all this, it will not be inconsistent with human nature should many *post-facto* claims be set up for the credit of first applying a *solution of cotton to surgical uses.* If, however, any person can establish a clearer right than I have to this credit, I shall be content. Yours, &c.

Dedham, March 18, 1848. JNO. P. MAYNARD.

2.—*Chloroform an Anæsthetic Agent as a Substitute for Sulphuric Ether.*

Dr. Simpson gives the following account of the chemical constitution of Chloroform:

"Formyle is the hypothetical radical of formic acid. In the red ant (*Formica rufa*) formic acid was first discovered, and hence its name. Gehlen pointed it out as a peculiar acid; and it was afterwards first artificially prepared by Doebereiner. Chemists have now devised a variety of processes, by which formic acid may be obtained from starch, sugar, and, indeed, most other vegetable susbtances.

A series of chlorides of formyle are produced when chlorine and the hypochlorites are brought to act on the chloride, oxide, and hydrated oxide of methyle (pyroxylic or wood spirit.) In the same way as formic acid may be artificially procured from substances which do not contain formyle ready formed, —so also are the chlorides of this radical capable of being procured from substances which do not originally contain it.

Chloroform, chloroformyle, or the perchloride of formyle, may be made and obtained artificially by various processes,—as by making milk of lime, or an aqueous solution of caustic alkali act upon chloral,—by distilling alcohol, pyroxylic spirit, or acetone, with chloride of lime,—by leading a stream of chlorine gas into a solution of caustic potass in spirit of wine, &c. The preparation which I have employed was made according to the following formula of Dumas:

' ℞	Chloride of lime in powder, - -	*lbs.* iv.
	Water, - - - - - -	*lbs.* xii.
	Rectified spirit, - - - -	f. ℥ xii.

Mix in a capacious retort or still, and distil as long as a dense liquid, which sinks in the water with which it comes over, is produced.' (Gray's *Supplement to the Pharmacopœ* 1846*ia*, p. 633.)

The resulting perchloride of formyle consists of two atoms of carbon, one of hydrogen, and three of chlorine. Its specific gravity is much greater than that of water, being as high as 1.480. It boils at 141°. The density of its vapor is 4.2. It is not inflammable, nor changed by distillation with potassium, potash, sulphuric, or other acids.

It is now well ascertained that three compound chemical bodies possess, when inhaled into the lungs, the power of superinducing a state of anæsthesia, or insensibility to pain in surgical operations, &c., namely, nitrous oxide, sulphuric ether, and perchloride of formyle. The following tabular view shows that these agents are entirely different from each other in their chemical constitution, and hence that their elementary composition affords no apparent clue to the explanation of their anæsthetic properties:—

	Propor. of Nitrogen.	Propor. of Oxygen.	Propor. of Carbon.	Propor. of Hydrogen.	Propor. of Chlorine.
Nitrous Oxide,	1 Atom.	1 Atom.	....	....	....
Sulphuric Ether,	....	1 Atom.	4 Atoms.	5 Atoms.	....
Chloroform,	....	....	2 Atoms.	1 Atom.	3 Atoms.

It is, perhaps, not unworthy of remark, that when Soubeiran, Liebig, and Dumas engaged, a few years back, in those inquiries and experiments by which the formation and composition of chloroform was first discovered, their sole and only object was the investigation of a point in philosophical chemistry. They labored for the pure love and extension of knowledge. They had no idea that the substance to which they called the attention of their chemical brethren could or would be turned to any *practical* purpose, or that it possessed any physiological or therapeutic effects upon the animal economy. I mention this to show, that the *cui bono* argument against philosophical investigations, on the ground that there may be at first no apparent practical benefit to be derived from them, has been amply refuted in this, as it has been in many other instances. For I feel assured, that the use of chloroform will soon entirely supersede the use of ether; and, from the facility and rapidity of its exhibition, it will be employed as an anæsthetic agent in many cases, and under many circumstances, in which ether would never have been had recourse to. Here then we have a substance which, in the first instance, was merely interesting as a matter of scientific curiosity and research, becoming rapidly an object of intense importance, as an agent by which human suffering and agony may be annulled and abolished, under some of the most trying circumstances in which human nature is ever placed."

[In confirmation of the remark that the elementary constitution of the three known anæsthetic agents affords no explanation of the theory of their action, we may mention another circumstance, namely, that while it is currently stated and we believe on good grounds, that the presence of a very small proportion of alcohol in sulphuric ether occasions so much irritation as to render it improper for inhalation, the choloric ether which consists of chloroform, with above 80 per cent. of alcohol, produces little or no irritation, and was originally preferred to sulphuric ether partly on this account.

As it is likely that chloroform will be extensively used, it is particularly desirable that uniformity in its constitution should be observed. Time and experience will probably bring it to a moderate cost, and insure to the public the advantage of a uniform preparation. We have received a communication from Mr. Morson, and another from Mr. Hooper, each of which contains a caution on this subject, pointing out the importance of distinguishing between chloroform and chloric ether. The great specific gravity of chloroform, and its insolubility in water, are mentioned as characteristic tests, to which we may add, that it is a body very difficult of ignition, which will readily distinguish it from chloric ether.—*Ed. Pharm. Journal.*]

In a paper on the chlorides of hydro carbon, published in 1833, in the 4th vol. of this Journal, its author, Daniel B. Smith, uses the following significant language: "The action of this ether (chloroform) on the living system is interesting, and may hereafter render it an object of importance in commerce. Its flavour is delicious, and its intoxicating qualities equal to or surpassing those of alcohol. It is a strong, diffusible stimulant, similar to the hydrated ether, but more grateful to the taste."

Several of our manufacturers now furnish chloroform of good quality, and we shall, ere long, have its powers fairly tested, as a strong interest is felt in reference to it by the medical public.

W. P., Jr.

[COMMUNICATED.]

New Orleans, January 20th, 1848.

To the Editors of the Medical and Surgical Journal.

Gentlemen:—In your November number, public attention is drawn to the "adulteration of medicines" and long Extracts from the New York Journal of Medicine and Buffalo Medical Journal, denunciatory of the practice, are furnished. Whether the parties nefariously engaged in importing, or the domestic houses, who knowingly and dishonestly vend spurious articles; or the public who appear indifferent to the quality, provided the price be low, be most worthy of the reproach of the faculty and the consequent odium, I will not now enquire, leaving the matter as it stands a mooted question. My present purpose is simply to show, that the chemicals manufactured by Mr. William Bailey, Wolverhampton, are as pure, as handsome, and as much entitled to the confidence and favor of the medical profession as the products of any other chemist on this or the other side of the Atlantic. I have imported his products for some years, and still import them, and at the present moment am perfectly willing to submit such as I have on hand to the most careful chemical analysis, and share with him all the responsibility that attaches to unprincipled dealers in sophisticated preparations, should any deception in quality be found to exist.

Mr. Bailey like every other large operative chemist in Europe and America, will, of course, *manufacture on order to meet price*, but in such cases the article so made is never sold under his name or by his authority, although his labels, like those of others, may be counterfeited, or surreptitiously obtained; as well might Pelletier or other distinguished names be held up to public execration, because in their vocation they unwittingly become in the hands of unscruplous and dishonest men parties to a most wicked, atrocious and inhuman deception.

Relying on your sense of justice for publication of this in your next number, I am, gentlemen,

Respectfully, your obd'nt servant,

JOSEPH TICKELL.

Remarks:—Our correspondent seems quite indignant, because we copied, with some comments, articles from some of our respectable Northern cotemporaries, reflecting rather severely upon certain wholesale manufacturing druggists.

In his attempt to vindicate the character of Mr. Bailey's house from such a charge, he is candid enough to admit, (strange enough!) that Mr. B. "like every other large operative chemist in Europe and America, will, of course, *manufacture on order to meet price.*" This is pleading guilty; and he is a poor advocate, who advises his client to put in such a plea, without the most positive and direct proof against him.

But, says our correspondent, "Mr. Bailey does not affix his name to these spurious and adulterated medicines; this might affect the reputation of his house, and damage his credit."

This is really too bad; further comment on such conduct is unnecessary, as this admission bears directly on the point at issue.

Our correspondent in his efforts (honest no doubt,) to save Mr. Bailey from the jaws of Charybdis, has stranded him upon the rocks of Scylla.—Edtrs.

NEW ORLEANS, MAY 1, 1848.

HEALTH OF THE CITY.

This is the season of the year, when the diseases of winter and spring are rapidly disappearing to give place to those of summer and fall.—The ship-fever and the acute affections of the thoracic organs, with a large class of exanthematous diseases, are yielding to gastro-enteric affections and obstinate intermittents. Although the middle and latter part of April was characterized by unusually cool weather, yet it did not add materially to the list of deaths, notwithstanding the prevalence of slight catarrhal symptoms. The question is now almost daily asked us "shall we have the yellow fever this season?" To answer this question at this time, would be to untie the "*gordian knot.*"

If the new Board of Health, recently organized, under the State Legislature and the different Municipal Councils, with his Honor the Mayor as President, possessed sufficient power to regulate the entire Hygiene of the city, we believe that much might and would be done for the health and prosperity of the city. We do not assert that any body of citizens, however intelligent and well-organized, could combat successfully the elements, regulate the heat of the season, dictate to the winds, or guide the storms, but they might modify or annul their influence upon our population, by recommending a more effectual system of drainage,—by causing currents of water to flow through the streets, from the front to the rear of the city, and by the speedy removal of all offensive or deleterious matters from the public streets and private alleys.

We think the water from the Mississippi might be made available in improving the health, to say nothing of the comfort of the city; as it is admitted that flowing water not only cleanses the gutters and sewers, but likewise purifies the atmosphere, by keeping it in constant motion.

The present high stage of the Mississippi river holds out additional inducements for the authorities, backed by the new Board of Health, to enforce a more thorough and complete system of drainag ethroughout all parts of the city and suburbs.

We still anticipate that in the course of time, as the city extends, and more attention is given to the condition of the streets, the yellow fever may be banished from our midst, and thus destroy one of the greatest obstacles to the growth and prosperity of New Orleans. To accomplish an end so desirable, every good citizen should apply himself with diligence, because all will be mutually benefitted. Nothing but the epidemic can check the onward progress of our metropolis; and economy as well as humanity, calls for large and liberal appropriations from the city authorities, to stay the ravages of the dreaded yellow fever.

It is too late, when the disease is among us, to debate about the best method of staying its course; it is only when we are in the enjoyment of health that we can prepare to ward off the threatened pestilence.—We feel some interest in this subject, and had much to say, but we have too little space to devote more attention to this important question. In our next, at a more advanced period of the season, we shall have more to say.

AN ACT,

To Establish a Board of Health in and for the Parish of Orleans.

SECT. 1. *Be it enacted by the Senate and House of Representatives of the State of Louisiana, in General Assembly convened:* That a Board of Health shall be established in and for the Parish of Orleans; said Board shall consist of twelve members, and be appointed annually, four to be appointed by each Municipal Council of the city of New Orleans; of the four members so appointed, not more than two shall be practising physicians. The Mayor of said city shall be President of said Board—but shall not vote except in case of an equal division of the members on any question.

SECT. 2. *Be it further enacted &c.*: That said Board shall annually elect a Secretary, who shall receive a salary, to be affixed by said Board, not to exceed one thousand Dollars; said Salary to be apportioned by the General Council among the several Municipalities according to existing laws, and to be paid by them. Said Board shall prescribe the duties of said Secretary. A majority of all the members of said Board may remove said Secretary for incompetency or neglect of duty.

SECT. 3. *Be it further enacted &c.*: That five members of said Board shall constitute a quorum for the transaction of business. By-laws shall be made by said Board for their own government.

SECT. 4. *Be it further enacted &c.*: That it shall be the duty of said Board to appoint annually not less than two citizens, to be known as "Health Wardens," for each ward of each Municipality of said city of New Orleans. It shall be the duty of said Health Wardens from time to time to visit and inspect the condition of the houses and lots in their several wards, and should they discover therein any nuisance likely to prove injurious to the public health, it shall be the duty of any Health

Warden of the Ward in which said nuisance may be found, to order its removal; and if within the time designated by such Health Warden for said removal, the order be disobeyed, it shall be the duty of said Health Warden to report the same to any two members of the Board of Health; and if said members approve the order made by the Health Warden, they shall direct the immediate removal of said nuisance at the expense of the tenant or owner of the property. If there be no tenant, the Municipal Council shall promptly advance the money for the removal of the nuisance, and shall institute suit for the recovery of the same, before any court of competent jurisdiction. Such suit shall be tried by preference over all other causes.

Sect. 5. *Be it further enacted &c.:* That it shall be the duty of the Board of Health to designate the hours when offal or other filth shall be deposited in the streets, and the time when the same shall be removed by the contractor for cleaning the streets. In case the regulations of the Board of Health on this subject should be violated, it shall be the duty of any Health Warden of the Ward in which said regulation has been violated, to report the same promptly to the attorney or assistant attorney of his Municipality; said attorney or assistant attorney shall immediately institute suit in the name of his Municipality for the penalty, which is hereby imposed, of not less than twenty, nor more than one hundred Dollars, and in case said penalty be recovered, said attorney shall be entitled to a tax fee of ten Dollars, to be paid by the defendant and the penalty shall be for the use of the Municipality.

Sect. 6. *Be it further enacted &c.:* That the Board of Health shall have power to require the Sextons of the several cemeteries of the Parish of Orleans to make returns to said Board of Health, in the manner and form to be designated by said Board, and to impose penalties for neglect or failure to make said returns.

Sect. 7. *Be it further enacted &c.:* That the regulations of the Board of Health shall be published in the official gazette of the General Council, and the expense of the same shall be apportioned amongst the several Municipalities, according to law. All the expenses of said Board shall be paid in the same manner—but said expenses shall not exceed five hundred Dollars, exclusive of the salary of the Secretary.

Sect. 8. *And be it further enacted &c.:* That it shall be the duty of the Board of Health to make an annual report to the General Council, as to the health of the city for the preceding year, and to suggest means for improving the same.

(Signed,) PRESTON W. FARRAR,

Speaker of the House of Representatives.

(Signed,) TRASIMON LANDRY,

Lieutenant Governor and President of the Senate.

Approved 16th of March, 1848.

ISAAC JOHNSON,

Governor of the State of Louisiana.

MEMBERS OF THE BOARD OF HEALTH.

Conformably to the provisions of the act, establishing a Board of Health in and for the Parish of Orleans, the following gentlemen were appointed from, and by the respective Municipal Councils of this city.

A. D. Crossman, Mayor and President of the Board of Health.

Dr. Warren Stone, " Y. R. Lemonnier, *Lay Members.*—Preston W. Farrar, " H. C. Carmack.	*First Municipality.*
Dr. Geo. E. Harral, " D. J. Rogers, *Lay Members.*—J. P. Freret, " J. W. Andrews.	*Second Municipality.*
Dr. J. J. Ker, " Isard, *Lay Members.*—J. Beebe, " J. E. Holland.	*Third Municipality.*

A. Hester, *Secretary.*

UNIVERSITY OF LOUISIANA—MEDICAL DEPARTMENT.

We see by the last Legislature, an act was passed, appropriating thirty-five thousand dollars to the University of Louisiana. A large portion of this sum is to be expended in completing the Medical Department.

This sum, although not as large an appropriation as the friends of the University desired, yet it is sufficient to show a desire, on the part of the State, to extend the ægis of its fostering care and protecting power over this infant institution.

We are informed that the medical department, constructed upon the plan of that ot the University of Pennsylvania, will be completed in time for the commencement of the lectures next fall.

The building is large, has all the necessary lecture rooms, apartments, &c. &c., for the comfort of the students and Professors; and stands nearly in the centre of a large square, remote from the noise and bustle of the streets. Its isolated situation, and the plan upon which it is constructed, will render it an admirable place for study, and easy of ventilation; two important considerations for the health and advancement of the students.

As a proof of the growing popularity of this institution, over one hundred and sixty students matriculated during the last session, and but for the report of the prevalence of yellow fever in our city, the number must have been much greater. We can assure students, however, that at the commencement of the lectures, nothing is to be apprehended from the epidemic, as the season is too far advanced to endanger the health of strangers. We have been unable to obtain from the Dean of the medical faculty, a list of the graduates for the last session, but we learn that between twenty-five and thirty received the degree of Doctor in Medicine.

A. H.

HOSPITAL REPORTS.

CHARITY HOSPITAL.

March 1848.

Admitted:	Males,	571,	744.
"	Females,	173,	
Discharged:	Males,	537,	712.
"	Females,	175,	
Died:	Males,	78,	104.
"	Females,	26,	

April.

Admitted:	Males,	464,	573.
"	Females,	109,	
Discharged:	Males,	261,	583.
"	Females,	122,	
Died:	Males,	49,	61.
"	Females,	12,	

CHARITY HOSPITAL.

Board of Administrators.

At a meeting of the Board of Administrators held on the first of April, 1848, the following gentlemen were elected to serve for the ensuing six months:—Surgeons: Drs. Nott and Compton:—Physicians: Drs. Moss, Smith, Barnes, Brickell, Cross, McGibbons, Rouannet and Bonzano.

Dr. J. P. C. Weirdestrandt was re-elected Resident Physician and Surgeon. M. Boyens Apothecary.

We are indebted to Dr. D. W. Brickell, one of the visiting Physicians to this institution, for the subjoined Hospital reports. Others connected with the hospital, have promised to communicate, through the Journal, any interesting cases that may come under their observation, to the Profession. From this source we expect a large mass of valuable practical matter, and we shall freely avail ourselves of this kind offer. Practical medicine is progressive, and facts, clinical facts are the foundation upon which it must rest. In our next number we shall be able to lay before the Profession a large amount of practical and interesting matter, taken from the *cliniques* of the visiting Physicians and Surgeons of the Charity Hospital.

The object of the following cases, is first to illustrate the beneficial effects of large doses of the Hydriodate of Potassæ in *Secondary Syphilis*; and secondly the virtues of large doses of the sulphate of Quinine in "Fevers." Dr. Brickell promises to continue his reports.—Edrs.

Large Doses of the Hydriodate Potassæ in "Secondary Syphilis."

Case 1.

"April 23d, 1848."—Wm. K——, æt 26 years—native of Ireland—quite robust—entered Ward 13, Charity Hospital.

Complains of almost universal articular pain, being most acute, however, in the knees, shoulders and lumbar region. Has some soreness of throat, together with red, indurated swelling on either side of exterior of neck, showing some tendency to suppuration. Has a small ulcer between the right "anterior and posterior palatine arches," but has never experienced any inconvenience from it. Says he had Syphilis in January last—both "chancres" and "bubo"—that these "healed up" entirely about a month ago, when he deemed himself *well*; but from that date these pains commenced, and have continued to grow worse each day. Has had pain in his ancles, but does not think that nodes have ever existed on the bones of either leg. Bowels are regular—appetite pretty good—sleeps very little.

"Treat."—Potass. Iodid. grs. xv.
Liq. Morph. Sulph. f. ℥ ss.
Aq. Font. f℥ i.
Ft. Solut.
S. Take tablespoonful every 4 hours.

"24th, 9 A.M."—No change in symptoms as yet. "Treat." Increased Potass. Iodid. to grs. x. three times daily, with same quantity of Morphia as before.

"25th, 9 A. M.—Says he feels some better—rested more comfortably last night than heretofore—pains not so severe—appetite improving.

"26th, 9 A.M.—Rested *well* last night—pains by no means so intense—appetite good.

"Treat."—Same continued.

"27th, 9 A.M.—Says he feels better this morning than he has for months—no pain whatever—merely a little stiffness in his joints—appetite *very* good.

"Treat."—Same continued—Also, lanced two small abscesses on his neck, which discharged a small quantity of thin ichorous fluid—ordered poultice to neck.

"28th, 9 A.M.—Patient says he is improving rapidly.

"Treat."—Same continued.

"29th, 9 A.M.—Still improving—abscesses on neck have ceased to discharge, and the indurations in their vicinity are disappearing—ulcer in his throat has disappeared entirely.

"Treat."—Same continued.

"30th, 9 A. M.—Discharged patient he being entirely well, and quite desirous of returning to his ship.

Case 2.

"April 24th, 1848."—John Power, æt. 28 years, native of Waterford, Ireland, resided in this country twelve years, quite robust. Entered Ward 11, Charity Hospital, this morning—complains of severe Cephalalgia, has been afflicted with it for months—was quite a hard drinker previous to his being afflicted—says he drinks very little now—has good appetite—bowels quite regular—no other untoward symptom that I can see at present.

"Treat."—Blister to nape of neck—purge of Magnes. Sulph.

"6 P. M."—Visited my wards in company with Drs. Fenner and Moss—upon examining this patient again, we found that he had been afflicted with "Syphilis," some eight years since—also found that there exists at present a fœtid discharge (slight) from the nostrils—bones of the nose appear to be somewhat thickened—patient says that he sometimes has cansiderable swelling about the junction of the nasal and frontal bones, with the most excruciating agony. Dr. F. pronounced it a case of "Secondary Syphilis," in which opinion Dr. Moss and I readily concurred.

"25th, 9 A. M."—Patient has experienced very little benefit from the remedies of yesterday—is pretty much "in statu quo."

"Treat."—Ordered Potass. Iodid. grs. xv. three times daily.

"26th, 9 A. M."—Patient is pretty much the same.

"Treat."—Increased Potass. Iodid, to grs. xx, three times daily, with the addition of one fourth gr. Morph. Sulph. at each dose. Half diet of the Hospital.

"27th, 9 A. M."—Patient reports himself very much better—slept pretty well last night—pain in head very much better—appetite improving—discharge from nostrils somewhat increased, and quite fœtid.

"Treat."—Same continued.

"28th, 9 A. M."—Gradual improvement—complains of constant inclination to sleep, the probable effects of the Morphia.

"Treat."—Same quantity of Potass. Iodid. continued, suspending the Morphia.

"29th, 9 A. M."—Gradual improvement—comparatively little headache—discharge from nose is most annoying symptom to patient—appetite good.

"Treat."—Same continued.

"30th, 9 A. M."—Headache very slight—discharge from nose somewhat diminished—complains of stiffness of the neck.

"Treat."—Suspended Potass. Iodid., and gave in stead Syrup. Sarz. comp. Ol. Ricin. f ℥ i.

"May 1st, 9 A. M."—Still improving.

"Treat."—Same continued.

Large doses of Quinine in Fever.

Case 1.

April 1848, æt. 20, entered the Hospital laboring under inveterate Inter. Fever, has been sick several months—paroxysms occurred every other day for some time; but in the last month the disease has assumed the "quotidian" type—paroxysm occurs every day at 12 M.—chill is very severe, and fever lasts four or five hours—patient is pale and feeble—appetite tolerable—bowels in pretty good order.

"Treat."—Ordered grs. xxx. Quinine to be given at one dose just an hour before the expected paroxysm—good diet.

"Patient had no chill at the regular time yesterday—perspired very freely about an hour after taking the quinine. Is perfectly cool this morning, and says he feels better than he has done for two months—appetite good.

"Treat."—Ordered grs. xv. Quinine to be taken an hour before the time for paroxysm—good diet.

"Patient has had no more chills—did not retain the last dose of quinine, however—it made him sick, and he vomited it up very shortly after taking it—is perfectly cool and appears well this morning.

"Treat."—Ordered Quinine, grs. xv. in pil. iij. to be taken as before.

" "—Patient is apparently well this morning—retained last dose Quinine, and perspired freely for some time. Has good appetite, and says he feels *well*—looks very much better. Discontinued the quinine, and put him upon Port wine and full diet.

"16th, 9 A. M."—Discharged this patient, he being perfectly well in every respect.

CASE 2.

"April 25th, 1848, 9 A. M."—Charles Bee, sailor, æt. 19, entered ward 11 of Charity Hospital this morning. "Has had Inter. Fever for five days past, "quotidian" type—paroxysms occur at 7 P. M.; chill is quite severe, and fever lasts nearly all night—patient is quite cool in the morning—he has the general aspect of persons laboring under this disease, is quite pale, eyes pale, spleen is quite large—appetite pretty good, bowels regular.

"Treat."—Quin. Sulph. grs. xxx.
Pulv. Opii.——grs. i.
Mix.——take at $5\frac{1}{2}$ P. M.

"26th, 9 A. M."—Patient had quite a severe chill last evening notwithstanding he took the quinine and opium. Upon examination I find him laboring under "Hydropericardium" to a considerable extent—does not complain of any uneasiness on account of it whatever. He is quite cool this morning, has good appetite.

"Treat."—Repeated the quinine with *two* grs. opium, to be taken at $5\frac{1}{2}$ P. M. Port wine and "full diet."

"27th, 9 A. M."—Had no chill yesterday—took the medicine at 5 P. M., perspired very freely after two or three hours—slept well—is perfectly cool this morning—tongue coated—appetite good.

"Treat."—Quin. Sulph. grs. xv.
Pulv. Opii. .... grs. i. *M.*—take at 5 P. M.
Port wine and full diet.

"28th, 9 A. M."—Had no chill last evening—perspired very freely for several hours after taking the medicine; skin quite cool this morning—tongue still coated white—appetite good.

"Treat."—Ordered Quin. Sulph. grs. x.
Pulv. Opii. gr. i. *M.*—To be taken at 5 P. M.
Full diet.

"29th, 9 A. M."—No recurrence of paroxysm—feels so well this morning, that he insists on leaving the Hospital—discharged him.

CASE 3.

"April 27th, 9 A. M."—Sebastian Reichsle—æt. 38 years; native of Germany, resided in this country———quite robust, entered ward 13 of Charity Hospital yesterday afternoon. Has had Inter. fever for three days—paroxysm occur every day at $12\frac{1}{2}$ P. M., has severe chill, and hot fever after it, lasting altogether some 10 or 12 hours—no appetite, bowels rather costive before coming in—took dose of oil yesterday afternoon, which operated freely—is quite cool this morning, tongue slightly coated.

"Treat."—Quin. Sulph. grs. xxx.
Pulv. Opii. grs. ij. *M.*—Took before I left the ward.
"Half diet.

"28th, 9 A. M."—Paroxysm recurred yesterday at the usual hour though very much mitigated, is quite cool this morning; tongue slightly coated—appetite pretty good.

"Treat."—Quin. Sulph. grs. xxx.
Pulv. Opii. grs. ij. *M.*—To be taken at $10\frac{1}{2}$ A. M. instead of $8\frac{1}{2}$ as yesterday.

"29th, 9 A. M."—No recurrence of paroxysm yesterday: perspired very freely for several hours after taking the medicine, is perfectly cool this morning—tongue nearly clean—appetite good.

"Treat."—Quin. Sulph. grs. xv.
Pulv. Opii. grs. i. *M.*—Take at $10\frac{1}{2}$ A. M. Full diet.

"30th, 9 A. M."—Had no chill yesterday: is quite cool this morning, tongue clean, appetite good—says he feels as well as he ever did in his life.

"Treat."—Suspended the Quinine—ordered Port wine and full diet.

"May 1st, 9 A. M."—Discharged him well in every respect.

HEALTH OF THE COUNTRY.

LOWNDESBORO' ALA., April 1st, 1848.

Editors New Orleans Medical and Surgical Journal.

GENTLEMEN:—As an amateur of "Medical intelligence," I send you a brief sketch of the doings of the "Alabama State Medical Association," which assembled at Selma, on the 8th March. I do not pretend to prepare a report for publication, but merely state the facts which you can dispose of as you think proper; room, convenience, and propriety being considered.

A constitution and by-laws were adopted, and committees appointed in different parts of the State to report to the next session,—to be held in Wetumpka, on the first Tuesday in March, 1849—a medical history of the year for their respective localities. The following officers were elected :

R. LEE FEARN of Mobile, *President.*
B. R. HOGAN—Selma,
S. D. HOLT—Montgomery, } *Vice Presidents.*
R. CLARK—Woodville,
J. MARION SIMS—Montgomery, *Record. Secretary.*
H. V. WOOTEN—Lowndesboro', *Corres. Secretary.*
D. FAIR—Selma, *Treasurer.*

A. G. Mabry, Selma, to deliver the next annual address.

The following delegates were elected to the American Medical Association at Baltimore.

P. H. LEWIS,	W. B. JOHNSON,	H. V. WOOTEN,
A. LOPEZ,	B. R. HOGAN,	J. E. PRESTRIDGE,
B. A. BLAKEY,	D. H. BYTHEWOOD,	F. A. BATES,
	EDWARD GANTE.	

The annual address was delivered by myself.

These facts,—or any portion of them, which you may deem appropriate to your Journal—are at your disposal.

Very respectfully,
H. V. WOOTEN,
Cor. Sec. A. S. M. A.

MEMPHIS, April 15th, 1848.

Editors New Orleans Medical and Surgical Journal.

GENTLEMEN :—The following are the various cases of disease, and the number of deaths that have occurred in my practice in the last two months.

Aptha, 1 ; Abortion, 1 ; Bronchitis, 3 ; Cerebro-spinal meningitis 1 ; Colic, 4 ; Croup, 1; Cynanche-Tonsilluris, 1 ; Dysentery, 2 ; Diarrhœa, 24 ; Erysipelas, 1 ; Dysmenorrhœa, 2 ; Empyema, 1 ; Dispepsia, 3 ; Fever, (catarrhal) 5; Fever, (Intermittent) 8; Fever, (Verminosa) 3; Fever, (remittent) 29 ; Fever, (typhoid) 19 ; Gonorrhœa, 1 ; Gastritis, 2 ; Hysteria, 4 ; Hemorrhoids, 1 ; Leucorrhea, 1 ; Menorrhagia, 10 ; Metritis, 7 ; Neuralgia, 3 ; Opthalmia, 4 ; Pleurisy, 12 ; Pneumonia, 3 ; Purtusis, 4; Phthisis, 4 ; Parturition, 10 ; Twins, 1 ; Prolapsus, uterine 1 ; Psora, 2 ; Parotitis, 4 ; Orchitis, 3 ; Rheumatism, 1 ; Puerperal-Peritonitis, 1 ; Pruritus Pudenda, 1 ; Rubeola, 16 ; Syphilis, 5 ; Stricture, 1 ; Spinal irritation, 1 ; Tubercular meningitis, 1 ; Tinea-capitis, 1 ; Teething, 3 ; Urticaria, 2 ; Wounds, (incised) 1 ; Wounds, (contused) 2 ; Varicella, 1.

Making 203 cases. Out of these there were 6 deaths :—1 of Bronchitis, 1 of Cerebro-spinal Meningitis, 2 of Typhoid fever, 1 of Pneumonia, and 1 of Tubercular meningitis. L. S.

NATCHEZ, April 5th, 1848.

Editors New Orleans Medical and Surgical Journal.

GENTLEMEN :—Observing in the January number the statement that "Quarantine was maintained at Natchez and Vicksburgh, that the former escaped and the latter had the [yellow] fever, and that no correction of an error therein contained has appeared in the March number, I desire you to publish in the May number that *several* cases of yellow fever occurred here during the Fall of 1847.

These cases were of persons having had no intercourse with boats, or with persons ill of, or convalescent from yellow fever—and who had not received goods during the summer or fall—and were therefore clearly from local cause, be that what it may.

In evidence that they were yellow fever cases, I am authorized to cite, in addition to my own, the unhesitating opinions of Drs. Cartwright, Cochrane, Jones, Lyle, and McPheeters.

The Quarantine was established soon after the announcement of the epidemic character of the fever in New Orleans, and maintained, with unusual rigor, till the official declaration that it no longer existed as such in your city. People were restrained from entrance into the city from 1 to 5 days; goods, if from Boston or New York, remained 24 hours at quarantine ground—if from New Orleans, 48 hours—exposed to the sun during 24 hours of this time—the above, I am informed, were the rules with some few exceptions.

Your's respectfully,

C. H. S.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1848.

BY D. T. LILLIE, AT THE CITY OF NEW ORLEANS.
Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1848.		THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — Inches.
		Max.	Min.	Range.	Max.	Min.	Range				
March	- 11	66.0	43.0	23.0	30.49	29.95	0.54	N.	3¼	2	0.675
"	- 18	75.7	52.5	23.2	30.40	30.05	0.35	S.S W.	2½	0	0.000
"	- 25	81.0	65.0	16.0	30.28	30.00	0.28	S.W.	3¼	1	0.015
April	- 1	83.0	60.5	22.5	30.33	29.50	0.23	S.E.	2½	3	0.293
"	- 8	77.0	61.5	15.5	30.47	29.83	0.64	S.	4	3	1.375
"	- 15	82.5	64.0	18.5	30.20	29.94	0.26	S.	3	0	0.000
"	- 22	79.0	58.0	21.0	30.34	30.10	0.24	S.E.	3	1	3.750
"	- 29	84.5	61.5	23.0	30.33	29.73	0.60	S.E.	3¼	1	2.225

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M. and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.