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EDITED BY

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BOOKS AND PAMPHLETS RECEIVED.

- I.—*Woman: her Diseases and Remedies.* A series of Letters to his Class: By CHAS. D. MEIGS, M. D., Professor of Midwifery, and the diseases of women and children, in the Jefferson Medical College at Philadelphia; Member of the American Medical Association; of the American Philosophical Society, and one of the Council; Vice President of the College of Physicians of Philadelphia; late one of the Physicians to the Lying-in department of the Pennsylvania Hospital, &c. &c. Third edition revised and enlarged. Philadelphia: Blanchard & Lea, 1854. pp. 672, 8vo. From the publishers through Mr. J. B. Steel, bookseller, 60 Camp street.
- II.—*Handbook of Chemistry, Theoretical, Practical and Technical:* By F. A. ABEL, Professor of Chemistry at the Royal Military Academy, Woolwich; and Assistant Teacher of Chemistry at St. Bartholomew's Hospital; and C. L. BLOXAM, formerly First Assistant to the Royal College of Chemistry; with a preface by Dr. HOFMANN: and numerous illustrations on wood. Philadelphia: Blanchard & Lea, 1854. Pp. 681, 8vo. From the publishers through Mr. J. B. Steel, bookseller, 60 Camp street.
- III.—*The Science and Art of Surgery; being a Treatise on Surgical Injuries, Diseases, and Operations:* By JOHN ERICHSEN, Professor of Surgery in University College, and Surgeon of University College Hospital: Edited by JOHN H. BRINTON, M. D. Illustrated by 311 engravings on wood. Philadelphia: Blanchard & Lea, 1854. Pp. 908, 8vo. From the publishers through Mr. J. C. Morgan, bookseller, Exchange Place.
- IV.—*Transactions of the Medical Association of the State of Alabama at its Seventh Annual Session, held in the City of Montgomery, Jan'y, 1854.* Mobile: Dade, Thompson & Co.—Pp. 190, 8vo.

- V.—*Quarterly Summary of the Transactions of the College of Physicians of Philadelphia: From February 1, 1854, to April 5, 1854, inclusive.*—Pp. 98.
- VI.—*Principles and Practice adopted in the Orthopædic Institution of Brooklyn: By LOUIS BAUER, M. D., and RICH'D BARTHELMESS, M. D.* New York: L. W. Schmidt, 1854.—Pp. 39.
- VII.—*Remarks on Croup and its Treatment: By HORACE GREEN, M. D.* New York, 1854.—Pp. 21.
- VIII.—*An Essay on Follicular Diseases of the Throat and Air Passages: By J. A. WILSON, M. D., of Memphis, Tenn.*—Pp. 11.
- IX.—*Essay on the Mechanism and Management of Parturition in the Shoulder Presentation: By WM. M. BOLING, M. D., of Montgomery, Ala.* Charleston, 1853.—Pp. 91.
- X.—*Eleventh Annual Report of the Managers of the State Lunatic Asylum, Albany, 1854.*—Pp. 55.
- XI.—*On Spiritual Manifestations: By the Rev. JEROME TWICHELL, A. M., New Orleans, 1854.*—Pp. 15.
- XII.—*Causes of Contagious and Epidemic Diseases, with Hints for their Prevention, and Reform in Medical Police: By M. M. RODGERS, M. D.* Rochester, 1854.—Pp. 31.
- XIII.—*The Transactions of the Iowa State Medical and Chirurgical Society: Third and Fourth Sessions. Held at Fairfield, May, 1852, and Davenport, June, 1853.* Burlington, 1854.—Pp. 48.

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THE NEW ORLEANS
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Part First.  
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ORIGINAL COMMUNICATIONS. ✓

ART. I.—CATARACT IN THE AGED; WITH PHYSIOLOGICAL AND
PATHOLOGICAL REMARKS.

BY BENNET DOWLER, M. D.

Suffusio quoque, quam Græci ἐπίόχουσιν nominant, interdum oculi potentia, qua cernit, se opponit.—Celsus de Medicina, Lib. vi. § 35.

1828, Jan. 15th.—Mrs. Ancram, aged 85, of good constitution, had been blind from cataracts in both eyes, for seven years before I saw and advised her to submit to the only means of relief, namely, surgical operations. This advice was given incidentally during a visit made to a gentleman ill of typhoid fever, in Mrs. A.'s family, in Tyler county, Virginia. Mrs. A. was much rejoiced to learn that there was a probability of regaining her sight, and was anxious for me to operate without delay. As Tyler county was destitute of the necessary surgical apparatus, a messenger was dispatched for mine, a distance of about forty-five miles.

Assisted by my brother, then one of my pupils, now a practitioner of this city, namely, M. M. Dowler, M. D., I proceeded to break up the cataracts in both eyes. It was my wish to operate at first on only one eye, (the left) at the same sitting. During the operation, Mrs. A. exclaimed, "I see a glimmer of light!" and as soon as the almost fluid lens and its capsule of this eye had been disintegrated, she surprised me with an urgent request to operate on the other. To this request I consented the more readily, as it would save me a journey in future of ninety miles should the operation upon the right eye have been deferred.

The operation upon the right eye was a little painful; the pain increased subsequently, and radiated through the head, accompanied with nausea, vomiting, and sleeplessness; symptoms which, however, during the night were relieved by opiates.

Jan. 16th.—Right eye inflamed, though not very painful. Mrs. A. was permitted for an instant to try the visual powers of the left eye; saw objects plainly, and recognized the countenances of her friends instantly.

Jan. 17th.—Could read large print in an almanac, without spectacles.

April 3d.—The pupil of the right eye, irregular, and partly occupied by opaque shreds of the capsule of the lens. The vision of the left eye is as perfect, or rather is more perfect, than is usual with persons of her advanced age, since, without spectacles, she can distinguish printed letters.

1829, March 10th.—The left eye is in the best possible condition for a person of her age; the right is somewhat inflamed, in connection with a recent catarrhal fever. In 1833, and again in 1836, I heard that this lady still could see well with one eye; a period of eight years after the operations above described.

1828, Jan. 16th.—In the same neighborhood, within twenty-four hours after having performed the above operation, I was called to operate on Mr. Smith, a robust farmer, aged 72, who resided in the family of his son. As the final result of this case was learned chiefly by means of letters from the family, it is proper to state that the parties were highly respectable and altogether reliable for veracity.

Mr. Smith has been blind from cataracts for five years. The pupil of the left eye greatly dilated; permanently fixed, and not perfectly circular owing to adhesions between the iris and lens; the latter bulges forward prominently into the pupil. A strong light produces slight oscillations, at intervals, along the iris, some portions of which are free or have but slight adhesions to the capsule.*

In the right eye the lenticular opacity is apparently incomplete—the pupil large—the iris movable, responsive to the light. Although

* Adhesions are not necessarily fatal to the success of an operation. Sir C. Bell says, (*Anat.* 1834,) "I should refuse to operate when the pupil is rugged and irregular, because the disease may be more extensive than it appears to be;" this dictum is contravened by the same author, (see his *Op. Surg.*, vol. 2, p. 44,) who says, with truth, that "irregularity of the pupil is an unfavorable circumstance, but not a bar to the operation."

Mr. S. can distinguish between day and night as is usual in cataractous patients, yet useful vision is wholly lost, a guide being necessary when he attempts to walk the plainest road.

While operating on the left eye, the numerous adhesions between the iris and the lenticular capsule suddenly gave way, the former contracting strongly after having been, as it is believed, permanently dilated and fixed for several years.

Immediately after withdrawing the cataract needle, the patient could discern objects, though it was deemed unsafe to attempt to test the accuracy of vision fully by any particular examination of minute objects; the eye was therefore almost instantly closed.

The capsule and lens of the right eye were tough and hard. During the operation, the iris or pupil dilated enormously, and then suddenly contracted. In this eye pain was felt during the operation. Directed sulph. magn. for aperients. Rested imperfectly—a little pain in the right eye.*

Jan. 17th.—Right eye painful.

April 4th.—Mr. Smith sees a little laterally with the left eye; in the right, the cataractous opacity is increased. On dilating the pupils with an infusion of stramonium, the irises were found to be adherent to the lenses or their capsules in sundry portions, giving the pupils serrated or angular margins. The lens of the left eye had almost wholly disappeared or dissolved; the greater portion of the capsule remained, constituting membranous cataract; this was lacerated and removed from the axis of vision, but owing to some remaining minute threads of attachment to the iris, rose again. During the operation upon the right eye, which contained the hard cataract, an extravasation of blood into the posterior chamber of the aqueous humor occurred, embarrassing to the operator from the obscuration of the aqueous humor and pupil.

Immediately after the operation upon the left eye, vision was in a good degree accurate, although much of the pupil was obscured by shreds of the capsule having filiform attachments to the iris.

The patient was bled. Sulphate of magnesia, as occasion required, with a low diet, was directed.

* Unfortunately, my notes of this case do not mention whether the cataract in the left eye was, at this first operation, soft or not. These notes, taken from my first MS. volume of researches and experiences, now grown to 22 vols. folio, are like most beginnings, defective.

April 30th.—Learn by letter that his sight is restored.

1829, Jan. 1st.—Learn by letter that he can travel, work, and discern small objects without spectacles.

I subsequently learned by letter and the testimony of individuals, that his sight continued good until his death in 1833.

I have operated with various success on several other persons, whose ages ranged from 72 to 79, but it is not necessary to detail their cases for the purpose of illustrating the fundamental aims of this paper—among which are these: the physiological necessity of the lens—the expediency of operating on the aged, and other considerations which the sequel will disclose.

“No one has been known who could read an ordinarily printed book without the aid of a convex glass, after the removal of the crystalline; while such instances as that of the postillion, related by Sir William Adams, confirms the belief, that one eye, if young and otherwise uninjured, may accommodate itself in a very considerable degree to the loss of the crystalline, so far as distant objects are concerned.”
—*Brit. and For. Med. Chir. Rev.*, Oct. 1847.

Mr. M'Kenzie, a very high authority, it must be admitted, says, in his elaborate Treatise on the Eye, “it is an error to suppose that the eye has the power to change its focal distance, after being deprived of its lens. It is necessary to use cataract glasses, double convex or plano-convex.” This statement appears to agree with the mechanism of vision, or rather with the functions of the artificial lenses of optical instruments, yet my own experience does not coincide with it even in the very cases wherein the necessity of cataract-glasses would seem most obvious, namely—among the very old. If in a single case it can be proved that the lens is not essential to vision, this theory must fall, even if it be proved that glasses are commonly necessary.

It is probable that the site of a displaced or absorbed lens, is speedily replaced by the vitreous humor, which adapts itself in such a form as to compensate in refractive power the loss of the crystalline humor. The reproductive and adaptative power of the vitreous humor is remarkable, as is often seen in traumatic lesions, particularly in unsuccessful attempts to extract cataracts. I have seen the protrusion and loss of a large portion of the vitreous humor immediately after the division of the cornea, before the cataract could be extracted; yet, experience shows that such losses in healthy individuals are of temporary duration only.

Whether the lens itself may not be regenerated, is a question that experimenters might investigate in comparative physiology with a fair prospect of arriving at satisfactory results, by the destruction or removal of this organ in the inferior animals, and, then after a lapse of time, ascertaining the condition of the eye. I am not aware that an extended and satisfactory series of experiments have yet been made, illustrative of the reproductive powers of the living economy in relation to the crystalline humor. The following meagre account is taken from Muller's *Physiology* :

“It would appear that, in certain cases where the lens has been removed, it is reproduced by the capsule—its matrix. Leroy d’Etiolle has observed this. In the first case, thirteen days had elapsed since the extraction of the lens when the eye was examined ; in the second case, thirty-three days ; in the third, ninety days ; in the fourth, thirty-one days ; in the fifth, forty-six days ; in the sixth, one hundred and sixty-five days. The experiments were made on rabbits, cats, and dogs. The contents of the capsule were either a crumbly mass, as in the second case ; or a small lenticular body, as in most of the other cases ; but in the sixth case a full-sized lens was found.”—(120.)

Were the physiological theory of Lamarck well founded, nothing would seem more natural than the reproduction of the lens. According to that great naturalist, whenever an animal wants an organ, nature goes to work and supplies the organ needed, and that such organ so supplied, is never permanently lost, but is invariably transmitted in all its integrity to all succeeding generations. Yet, dreamy as this theory appears, there are some facts reported which, if true, would seem to favor the Lamarckian doctrine. If, for example, an animal be placed in a condition where the eye (of all organs the most complex) is not wanted, as in perfect darkness, and, if, as a consequence of this, the annihilation of the organ, now useless, should occur, this fact would seem scarcely more extraordinary than the creation of an organ or any important part of it, as the consequence of a preëxisting want. “This high argument,” though apparently a negation, is in a certain sense an affirmation of the theory of Lamarck. Now, in the Mammoth Cave of Kentucky, this theory becomes (if the expression will be allowed) a concrete fact. For, in that case, as some naturalists affirm from actual dissection, certain animals, as fishes, insects, &c., are completely eyeless. The affirmative of this argument might possibly be illustrated by placing some

of these cycless animals in the light. In such a medium eyeless fishes might need eyes to escape from enemies, &c., and it would then be seen whether conditions of exteriority or internality—whether exigencies and wants, can create the most complicated and noble of all organs—the Eye!—that point of departure for the physiological anatomist, the priest of natural theology, and the teleologist who finds here the doctrine of adaptation—the means and the ends—the purpose, the grand finality.

But to return from this digression: it is worthy of remark that most writers upon the operations for cataract, are silent as to the necessity of cataract-glasses. Doubtlessly some person require glasses while others do not.

If the lens be invariably necessary to vision, an artificial, to replace the natural lens would be necessary for a double reason where one eye only is cataractous—necessary for the eye operated on—necessary to the sound eye in order that both should have the same focal distances.

Physiology can scarcely go so far as Dr. Arnott, who asserts, “that the eye is nothing but a simple Camera Obscura, formed of the parts essential to the Camera Obscura.” (*Elements of Physics.*)

Mr. Samuel Cooper (*Dict. Surg.*) says: “That the patient would never be able to see distinctly after the operation, (upon the cataractous eye,) by reason of the difference of the focus (foci) in the eyes, is (I have grounds for believing) only a gratuitous supposition, transmitted from one writer to another.”

The density of the lens has been supposed to exceed that of the vitreous humor by one-cleventh; the specific gravity of the former being 1.079; of the latter 1.0009. Now, it is probable that the greater power of refraction incidental to the superior density of the lens, may, at least, in some cases be compensated by the altered configuration of the vitreous humor and the adaptative powers of the living economy. Mr. R. Owen, an eminent naturalist, in his *Notes upon Hunter's Animal Economy*, says, “it has been shown in the well-attested case of Henry Miles, that the eye may retain its power of adjustment after the removal of the lens.” (296 *Am. Edit.*)

Artificial dilations of the Pupil:

As all know, stramonium, belladonna, and the like possess the singular property of causing a relaxation of the circular or sphincte-

roid fibres of the iris, leaving the transverse or radiated fibres which converge upon the circular, active, thereby dilating the pupil, which latter, is on the other hand, diminished by the contraction of the circular. These agents are often very useful in breaking up recent adhesions between the uvea or posterior surface and margin of the iris and the capsule of the lens; they are advantageous in expanding the pupil where the operation of breaking up the lens is performed through the cornea; but they are not called for—indeed they have proved inconvenient in their action when the operation is performed through sclerotica, as I have found—because as soon as the lens becomes detached it is apt to slip forward through the pupil into the anterior chamber of the aqueous humor where it can neither be depressed nor divided, and where, if it be hard, it causes inflammation before it dissolves and disappears. Any manipulation of the dislocated lens from behind the iris, may injure the latter and bring on traumatic iritis which is the chief danger in this operation.

Can cataract be cured by medication without a surgical operation?

Recently, M. J. Leport, oculist, at Rouen, in France, obtained the prize proposed by the Medical Institute of Valencia, in Spain, for the best essay upon the cause and cure of cataract. M. Leport maintains that medical treatment never cures cataract without an operation:

Mon opinion est que jamais jusqu'à présent on n'a guéri un seul cas, même au début, de cataracte vraie, à l'aide de moyens médicaux; qu'une grande partie des gens soidisant guéris de la cataracte sans opération ont été guéris d'une toute autre affection par des médecins exploitant le charlatanisme, ou par des praticiens qui avaient commis une erreur de diagnostic.—*De la cataracte mémoire couronné*, 11.

In the Virginia Medical and Surgical Journal, for April, 1854, an article digested from the researches of Dr. Garcia Lopez and others, gives a more hopeful view of the medical treatment of cataract:

The majority of surgeons consider the cure of cataract without an operation a chimera. And yet it is impossible to deny that cataract is sometimes spontaneously cured. Is it then impossible that therapeutics may some day realize what nature occasionally accomplishes by herself? This question is often asked by unprofessional persons. Medical men have uniformly denied the possibility of the cure of complete lenticular cataract by medical treatment, while impudent and shameless charlatans have industriously propagated the opposite opinion among all classes of society. In the clinical reports of Dr.

Beauvais,* are reeorded five *homæopathic* cures of cataract, one of which, treated by Dr. Caspari, was evidently a ease of corneal opacity depending on trichiasis. The offending eilia were removed *surgically*, the patient took one drop of *cannabis* daily, and reeovered entirely.

In the work of Tavignot the reader will find an account of the experiments of Professor Pugliatti, of Messina, on the curability of capsular or capsulo-lenticular cataracts without an operation. This surgeon supposes that a strong solution of ammonia, applied at the internal orbital angle, penetrates the tissues of the eye, and exerts a curative effect upon the opaque crystalline system. He combines with this treatment the administration of five grains of iodide of potassium daily. He reports several cases which were benefitted, and one or two which were supposed to be cured by this means. In every case the treatment lasted many months. In the majority of cases no amelioration was perceived. In the Spanish journal *El Porvenir Medico*, for December, 1853, we find four eases in support of the efficacy of the treatment of Pugliatti, reported by the author, whose name we have placed at the head of this article, Dr. Garcia Lopez :

1.—A man of fifty years; nearly complete capsular cataract had existed for three months: vesication with ammonia, without any internal treatment, procured resolution in two months. 2.—A man of thirty; soft, striated, lenticular cataract, two years old; capsule healthy. After six weeks of treatment the patient demanded an operation. Dr. Lopez found the lens diffuent and lactescent. 3.—A woman of forty; hard, incomplete capsulo-lenticular cataract in both eyes, developed within a year. She could only distinguish very large objects. After seven months of treatment the patient recovered her vision, and could sew and follow her usual avocations; the opacity had disappeared except in a limited central point. 4.—A woman of fifty; capsulo-lenticular cataract had existed for three years on the right side; a similar opacity was forming in the left eye, complicated with amblyopia. In six months, after the constant employment of ammoniaeal vesication and iodide of potassium, a decided amelioration took place in the left eye; the right was unaffected.

Dr. Lopez reports these cases without any desire to attach undue weight to them, and can hardly be supposed to be prejudiced in behalf of a method which he did not originate.

There is another remedy to which much consequence is attached, to which Dr. Lopez does not allude, we refer to the iodine ointment, allowed to dissolve on the conjunctiva. The editor of the *Paris Gazette hebdomidaire*, (Dec. 1853,) reports a case of double capsulo-lenticular cataract cured by this method.

In a succeeding article we have given a full account of the mode of treatment of cataract depending on capsulitis. The reports of the

* Annales d'Oculistique, tom. ii., p. 218.

Medico-Chirurgical Society of Richmond, contained in the present number of the Journal, contain the histories of two cases of traumatic cataract, and one of supposed congenital cataract, in which the advantages of a free use of mercury in opacities of the crystalline system connected with inflammatory action are strikingly exemplified.

In all such cases, and immature cataracts, no harm can arise from instituting medical treatment during a period which would be otherwise lost in awaiting the opportune moment for an operation. It must be confessed, however, that Lebert's researches into the structure of cataract, demonstrate that in the great majority of cases medical treatment must be unavailing. Rognetta was of the opinion that an absolute denial of the efficacy of remedies in cataract was unwise; we concur in this sentiment, and trust that farther researches will be made in this interesting and obscure subject.

Is Cataract Hereditary?

Mr. W. W. Cooper relates the following interesting facts: His late Royal Highness, the Duke of Sussex, informed me that cataract was brought into the present Royal Family by the marriage of one of his ancestors with a princess of Saxe-Coburg Gotha. She became blind from cataract, and the following members of the Royal Family have since been afflicted with that disease: The Duke of Cumberland; George the Third; George the Fourth; the Duke of Gloucester, the Duke of Sussex assured me that such was the case, and that it was one cause of his seclusion. Three years ago a young woman came under my notice with congenital cataract, and she stated that her grand-father, father, uncle, aunt, and three sisters, had all been born with cataracts.—*Lond. Jour. Med.*, 1849—*cited by Dr. Hays in Lawrence on the Eye.*

Do Concussions, Wounds, and Inflammatory Affections of the Eye, produce Cataract?

Although in a great majority of cases of cataract, the causes producing it remain unknown, yet, inasmuch as the cause of one variety of this disease, that is, the traumatic is indisputably traceable to inflammation, the latter might upon the principle of analogy afford presumptive proof that all cataracts may have a similar origin when no other cause can be suggested as probable. The ideas usually associated with the word inflammation are vague and often erroneous: thus, neither redness nor vascularity is an essential element of inflammation; for example—the arachnoid membrane of the brain, when inflamed, is erroneously described by nearly all writers as being red,

vascular and injected; whereas, inflammation of this tissue causes opacity, thickening, increased tenacity in, and fibrinous and watery exudations from, this membrane; the vascularity, redness, and injection being seated in the sub-arachnoid tissue and pia mater and seen through the semi-transparent arachnoid. Even pain is sometimes absent in slow changes of an inflammatory origin and character. Thus, it may happen in chronic capsulitis, lentitis, and crystallino-capsulitis. In fact these known inflammatory products, in the absence of all other ætiological knowledge of cataract in general, should be viewed by the pathologist as the possible or provisional types of even congenital and non-traumatic opacities; instead of concluding dogmatically with M. Leport, (already quoted) that inflammation must be wholly excluded from the causation of cataract, he may take the known cause as a guide to the unknown. M. Leport says:

“ L'opacité de la capsule antérieure survenant pendant une capsulite ou une irido-capsulite, ou bien à la suite d'une lésion traumatique, ne peut jamais constituer une cataracte. Ce genre d'opacité est très souvent curable, et on ne doit lui donner le nom de cataracte que lorsqu'elle persiste longtemps après que tous les symptômes inflammatoires se sont dissipés.”

On the other side of this question, the following extracts taken from “The Virginia Medical and Surgical Journal, for April 1854,” will be read with interest:

Extracts from the Records of the Medico-Chirurgical Society of Richmond, Va. By the Secretary.

Jan. 3, 1854.—*Two cases of Traumatic Cataract.*—Dr. Parker related the following case: A lad of nine or ten years received a blow on the eye from a small pebble, which inflicted a slight wound of the cornea. There was not much vascular excitement of the conjunctiva; the pupil was clear; the movements of the iris were natural, and there was not much pain. Under these circumstances, Dr. P. was satisfied with directing the application of cold compresses to the eye-ball, and the use of a mild astringent collyrium.

One week afterwards, he saw the child again, and was astonished to find a well marked opacity behind the pupil. The capsule, in fact, was of a milky white color throughout its whole extent. The wound of the cornea had been converted into a circular ulcer, but it did not penetrate the interior chamber. The aqueous humor had not escaped; there was no anterior or posterior synechia, nor deformity of the pupil or other symptom of iritis. Vision was greatly impaired, though not entirely lost. There had been considerable pain in the orbit.

The patient was put upon small doses of calomel, and cupping and counter-irritation employed. Under this treatment, the opacity gradually diminished, until, at the end of four or five weeks, the capsule had completely regained its transparency, and vision was entirely restored.

Dr. Gibson reported a somewhat similar case: A boy of twelve years of age fell into a bush, and, in so doing, received a severe concussion of the right eye. When he was seen by Dr. G. several days after the occurrence of this accident, the eye presented the following appearances: The cornea was perfectly clear and free from injury; the iris was inflamed, and the uvea had already contracted adhesions with the anterior surface of the capsule; the pupil was deformed, and the dull opacity characteristic of capsulitis could be perceived behind it. The vessels of the conjunctiva were highly injected. The next day the capsule was completely opaque: its surface was manifestly covered with fibrinous deposits.

Active treatment was at once instituted. The patient was cupped, and mercurials and belladonna were administered internally, while revulsives were applied to the temple and neck. Resolution gradually took place. Absorption was complete at the end of five weeks; the iris was liberated, and resumed its normal movements, and vision was entirely restored.

In these cases inflammation was prominent; but it may in other cases, be masked, local, and attended by whiteness, rather than redness, &c.; and thus cause many, if not all cataracts. At all events if inflammation be not the cause of cataracts the cause is unknown.

In the case of Smith, above mentioned, the cataract in the right eye at the first operation had but a slight opacity, which, however, prevented vision, and which at the second operation was perfectly opaque, doubtlessly owing to the consecutive inflammation from the first. In this case the lens at the first operation was but slightly opaque in the central portion, not the capsule; the cataract at the next operation proved to be altogether opaque and completely membraneous, perforated with several holes by the needle. There can be but little doubt that cataract could be artificially produced, thereby illustrating its ætiology by a series of experiments upon the inferior animals. The puncturing of the capsule and its enclosed lens, with the cataract needle, would probably produce cataract, differing from non-traumatic cataract only in this, namely—the inflammation of the numerous other tissues necessarily wounded in the artificial process. It would appear from actual experiment that the disintegration of the parts involved in ordinary cataract gives no pain. Thus, if this element—pain—be

deducted from the non-traumatic cataractous process, it may analogically speaking be reduced to the vague category called inflammation—so that the cataractous alteration, a peculiar, local, painless, white one—the analogues of which exist in other structures, would be established on a basis more or less satisfactory; whereas, it has now none whatever.

Are there any satisfactory criteria whereby hard and soft milky cataracts can be distinguished anterior to the actual operation, of extraction, depression, or keratonyxis?

This is a fundamental question lost sight of by the partizans who dogmatize upon the superiority of the particular mode of operation to which they adhere; the advocate for extraction admitting that a soft cataract should be couched or broken up, and the coucher that a hard one ought to be extracted; but neither give any satisfactory criterion anterior to the actual operation; and then it is too late to be of any use! This kind of logic affords a good example of that called the *petitio principii* and that termed a bull—as the learning how to swim before venturing into the water.

Ought the operation for cataract be performed on both eyes simultaneously?

The rule laid down by authors answers this question negatively with few exceptions, but the rule may be controverted morally, physiologically, and pathologically. The moral tone of the patient's mind when once made up to undergo the operation in one eye will suffice for the operation on both. When I was going to operate on *one* of his eyes, according to the rule, J. M. said in a firm tone, "I will not make two bites of a cherry."

If only one eye be operated on, and if this first operation should result unsuccessfully, the patient having not only the fear of another unsuccessful operation but the dread of a second course of medical treatment to deter him, will probably refuse to permit the second attempt. The double operation which is pathologically but one, does not materially enhance the dangers of consecutive contingencies.

Therapeutic Retrospection.

The reader will have seen in Smith's case, as above mentioned, that a blood-letting was performed in anticipation of consecutive inflammation which the writer does not now regard as a sound rule of prac-

tice, though twenty years ago it was the fashion to bleed a great deal more than at present. Even at this day systematic writers do not sufficiently guard their readers against the danger of depletion in some cases of pure ophthalmic inflammation in which bleeding, purging, and antimonial nauseants weaken the patient and strengthen the disease—the eyes running into rapid disorganization. Cases of this deplorable character might be given as well as cases in which rapid cures have resulted from the administration of opium, morphia, Dover's powders, quinine, blisters, together with small doses of mercurials, warm baths, &c. Even in idiopathic or spontaneous iritis, these remedies subdue inflammations, in some cases, where the so called intiphlogistics aggravate it. Happy is the patient who has a physician who can make the distinction!

ART. II.—OBSTETRICAL CASES AND PHYSIOLOGICAL REMARKS, &c.

BY BENNET DOWLER, M. D.

Monstrosity.—1838, June 28th, 4 P. M.—Mrs. Keefe, born in Ireland, aged twenty-eight, mother of four children, resident in New Orleans three months—of robust constitution—sick for a week last past—in severe labor for twenty-four hours, under the care of an apparently demented midwife—tongue furred—intense thirst—restlessness—vomiting—skin cold and clammy—hiccuping—almost pulseless—vital powers failing—vagina hot and painful on being touched—two feet and legs of a monster protruded—a third leg of double size completely solid and round having two feet, was brought down—the amniotic waters, completely discharged—the uterus contracted closely on the presenting immovable mass—the three legs proceeded from an enormous unyielding body consisting of two consolidated pelves—a single common anus was discovered with indications of female organs of generation.

Death impended; the friends were apprized of this—the husband was absent up the river. A clergyman was called, while I went for instruments to dissect the monster, to gratify the wishes of the patient, though without the expectation that she could be saved by these means or by the Cæsarian section, as she was evidently sinking from

exhaustion and constitutional irritation. Unavailing parturient pains still continued.

The clerical visit proved the great value which psychological medicine often betows upon the patient, thereby paving the way for the easy execution of the materializing processes of the surgeon and the physician. Before the last rites of the Church were administered, her mind was all perturbation—she was restless—impatient both in conduct and conversation, and unmanageable—after this for twelve or thirteen hours, she presented an example of moral courage, of unmurmuring quietness, of calm, of deliberate and even cheerful conversation alike free from enthusiasm on the one hand, and sullen indifference on the other. The clearness, serenity, and energy of her mind even under the exhibition of opiates which seemed to produce no effect, were to the last hour like the setting sun undiminished, unclouded.

Lest the term psychological medicine should appear to some readers as a neologism, it may be remarked, that several periodicals bearing this name have recently appeared in different languages and countries. A knowledge of the mental frame, including its physiological and pathological laws and reactionary effects upon the body, is at once the highest and most useful kind of knowledge, not excepting that of drugs, in practice, whether the primary disease be mental or corporeal.

But to return: Mrs. K., though aware of her danger, still desired the removal of the monster. This occupied nearly five hours. The legs having been removed—the pelvis or hips strongly fused together, were broken and extracted, as were the apparently single viscera, the double spines and the single or rather double continuous ribs. The anterior spine corresponding with the symphysis pubis and anterior abdomen of the mother was particularly in its upper half, as the anatomist must perceive, in an inaccessible position not being in the axis of the inferior outlet of the pelvis, but rising vertically with and above the pubis, and thus beyond reach. Of the vertebræ of the anterior spine of the monster only about half could be extracted; the residue, were beyond the range of instruments. Of the posterior spine corresponding with that of the mother, all was removed but the cervical portion, together with nearly all of the ribs. The operations had now extended as high as possible; but the head or heads could not be reached;

one arm, however, was at length brought down. Towards midnight all further dissection was abandoned. Mrs. K. was now pulseless. An hour after midnight, on taking my leave, Mrs. K. full of gratitude for my unavailing but well meant attempts, said "that she knew that I had given her up to die," and her manner and language showed that she had the same opinion herself. These facts are alluded to merely to show the great effect of moral or psychological influences in sustaining the mind in the midst of the greatest conceivable trials—the final agony. In an incurable case psychological medicine is the only remedy—in a curable case often paramount.

At 5 A. M., Mrs. K. died. An hour after, the Cæsarian section was performed. The abdomen was already convex from gaseous distention. The uterine walls were injected, dark, and nearly gangrenous in places where the pressure of the monster appeared to have been greatest. The uterus contained two or three pounds of black, grumous, and almost putrid blood; one enormous placenta; one large umbilical cord; and a two headed monster with four fully developed arms. The upper part of the chest still entire had no mesial line showing the place of fusion. In fact it was difficult to say whether it did not partake more of the single than of the duplex form of monstrosity. The chest appeared single though double-ribbed, double-spined, double-necked, double-headed, and furnished with four large symmetrical arms. The necks were long, yet well proportioned; the heads were large and beautiful; the tracheæ and foreheads were directly opposite, facing each other. The arms, except the one that had been brought down, were raised and laid along the sides of the heads and faces.

From the configuration of the monster, it presented an unyielding mass of a wedge-like or conical shape, the apex of which corresponded to the outlet of the maternal pelvis; for, the opposing foreheads and necks with the upraised arms and shoulders, gave the cranial or upper end of the mass in its shortest diameter, a greater diameter than the maternal outlet afforded. Indeed the descent of the monster was arrested by the brim or superior strait of the maternal pelvis; so that delivery was impossible by the natural passage. The continuity of the ribs from spine to spine, together with the double spines, and the two pelvis fused into one, gave the body a rigidity throughout incompatible with its descent through the curving outlet of the maternal

pelvis, as all acquainted with the mechanism of labor must admit. Had the existence of the monstrosity been ascertained in time, the Cæsarian section would have afforded the only chance of life for the mother and the monster.

The mutilated monster was taken to my office, and in a few hours after, several hundred people having seen it, the relatives demanded and received it for burial. The late Mr. Shaw, apothecary, who saw its remains, estimated its weight at seventeen or eighteen pounds, independent of the parts that had been removed. However this may be, its weight may safely be supposed to equal that of two large children at birth.

A gentleman of undoubted veracity who saw this monster, informed me that in the same neighborhood where Mrs. K. lived, he had seen a few months before her death, a wax figure of a double-headed monster which some strolling musicians carried about for exhibition, which resembled very exactly the one he saw in my office above described, so far as the unutilated parts were concerned. His attention had been strongly directed to the wax figure which was brought to his door and which he ordered away, because he was afraid his wife, then *enceinte*, might see it. He inferred that Mrs. K. must have seen the wax figure. Hence the cause of her misfortune, according to his reasoning.

If fright, sorrow, jealousy, and anger, kill the fœtus and cause abortion, as must be admitted, why may not these same moral causes during its early state of growth, disfigure, modify or alter its form?

The following case of monstrosity reported by Mr. W. H. Popham, in the London *Lancet*, for June, 1852, (Am. Ed.) though very different from that above mentioned, is both curious and interesting. The lady, ten or twelve hours before delivery, (which occurred at the end of the sixth month,) expressed to Mr. P. her fears that she carried twins or a monstrosity. It will be seen that the monster was small and weighed but four pounds. Mr. P. says:

“After three hours more in strong labor, she was delivered, and the first question she asked, was, ‘Is the child all right?’ On examining the child, I found it most strangely malformed. There were two heads attached to the neck, and united by integuments all up the side to the vertex. The mouths, noses, and eyes were all perfect, the neck was single, and evidently contained but a single vertebral column, and

which continued single as far as the eighth or tenth dorsal vertebra, where it bifurcated. From this downwards, the body was double, there being two separate and distinct pelves, detached from each other, and containing the organs of generation (female) perfect, as well as the ani. There were four legs, and perfectly formed; in fact, the body was double from the naval down. The navel cord was single, entering the bodies at the point of their junction. The chest presented nothing abnormal, and the arms were well made.

“This extraordinary monster was born alive, and lived as long as the circulation was carried on in the cord; it gasped several times simultaneously with both mouths, and passed meconium from the ani at the same time. It weighed about four pounds.

“From the commencement of Mrs. M——’s pregnancy to the time of her delivery, she had a most morbid desire to see anything that was unnatural, and wherever she heard of a ‘*lusus naturæ*,’ there she would go and see it. For two months previous to her confinement, she continually said she should either be brought to bed with twins or a deformed child. Her recovery was quick, not having had a bad symptom. Of course she was not aware that she was delivered of a monster.”

Monsters are not, as was formerly supposed, mere sports of nature, amenable to no laws, nor beyond the pale of scientific classification.

Human monstrosities limit themselves to, and form themselves after, the normal standard so far as never to lose altogether the unity of the human type. They never degenerate into the inferior forms of the classes of their proper sub-kingdom; that is, they never identify themselves with the other classes of vertebrata, as monkeys, birds, fishes, &c.; much less do they sink into the other sub-kingdoms, as the articulata, mollusca, radiata.

Some malformations become such from deficiency or redundancy of development in a particular organ; others, by supernumerary or supplemental formations which, still, do not violate the oneness of the individual. Compound monsters—in which two individuals are fused or blended into one are rare, and present physiological, psychological, and medico-legal questions not easily answered. A more common variety of duplex monstrosity is characterized by a more or less extensive union of two individuals by means of cutaneous, ligamentous,

fleshy or bony cohesions, as in the Siamese Twins, the Hungarian Twins, (Helen and Judith,) and more recently in the Carolinian Twins, exhibited in New Orleans. The union of these black female twins of North Carolina is very like that of the Hungarian girls, who lived twenty-two years. Judith fell sick and died. Helen died three minutes after. Such is the fate in reserve for the black girls.

Helen and Judith were exhibited for seven years in the principal capitals of Europe. They were distinct persons except where they were united back to back from the loins to the common anus and common vulva, as in the Carolinian Twins. In the Hungarian Twins it was ascertained that "the aorta and venæ cavæ communicated in their lower parts, and thus established a large and direct communication between the two hearts, producing an intimate relation of life and functions between the two beings. Whenever one was ill the other felt so too, and participated in her sister's disease; it was therefore predicted that the death of one would necessarily destroy the other, which proved to be true. Judith, at the age of twenty-two years, was attacked with disease of the lungs and the brain, of which she died. Helen, who, at the commencement of her sister's attack, was in perfect health, soon became ill, and both expired at almost the same instant."—*Penny Cyc.*

The doctrine of final causes or purposes in physiological anatomy is a fruitful principle of research, but is hard to reconcile with great aberrations of structure which interfere with the essential conditions and functions of life and individual well-being. The normal form of organization comprehends both the means and the ends of existence—the abnormal fall short in both and must be regarded as deviations from the general plan of Nature, being no more adapted to the ends of existence than the oyster is to flying.

It is a curious but incontestable fact that a great majority of monstrosities are females. These vices of conformation probably take place from some disturbing cause or accident in the early stage of fetal life, as certain diseases do, such as dropsy of the brain, *nævi materni*, and probably traumatic alterations.

"Direct evidence has been afforded, by experiment, that the natural stages of formation may be altered in the embryo of the chick, during incubation, by external injury. Geoffroy St. Hilaire

injured several eggs, in which the process of incubation had commenced, and had been going on naturally for several days. He shook some of them violently, he perforated the shell of others in different places with a sharp instrument, or kept them in a vertical position, upon either the large or small end, during the whole term of hatching; again, in some he covered part of the shell with wax, or varnish impervious to air. The constant effect of these injuries, was the production of a very considerable number of anomalies, either simple or complicated, among which may be mentioned *cyclopia* and other malformations of the face and head, eventration, and spinal fissure. In no instance was any case of double monstrosity met with, which might, *à priori*, have been supposed."—*Penny Cyc.*

The Cæsarian section successfully performed by a negress while drunk.

The late eminent Judge Waggaman, many years ago a member of the Senate of the United States, informed me that an old drunken negress, who acted as midwife on his plantation, above New Orleans, on being called to a black girl during her first labor, which was natural, took a sharp case-knife, and without any reason to justify her conduct, laid open the abdomen and womb, and took therefrom a living child! The girl speedily recovered, with no other inconvenience except a slight incontinence of urine. The judge vouched for the accuracy of these facts, and pressed me to visit, with him, his plantation that I might examine the girl, which, however, I had not then an opportunity of doing. He fell in a duel soon after.

According to the estimates of Merryman and Blundell, published a few years ago, it appeared that the Cæsarian section had been performed in only twenty-six cases in the British Isles, all of which, two excepted, proved fatal to the mother.

Midwifery in New Orleans.

In his work on the Hospitals of Paris, in 1843, Dr. Stewart, of New York, says "Midwives in France after two years' study at the School of Delivery, and submitting to two satisfactory examinations, receive a diploma to practice, always, however, under certain restrictions, one of which is, that in no case, and under no circumstances whatever, shall they resort to delivery with instruments without the attendance of a physician. They amount, in all France to the number of 450, and practice almost exclusively among the lower classes of the community." This number would give in France, one midwife to

every 75,000 inhabitants. New Orleans has probably one for every fifteen hundred or two thousand people, while, perhaps, not one in twenty of these midwives have received even an elementary anatomical education.

That such uneducated persons should be generally successful is owing to the fact that in a great majority of cases no scientific skill is required, and thus a lucky negress becomes the rival of the most learned obstetrician.

The whole argument which the uneducated midwife offers and the credulous public receives is better expressed by the poet Crabbe than by the professor of midwifery himself; the midwife's opinion of the accoucheur is represented on this wise—

“By proud and learned words his powers are known,
 By healthy boys and handsome girls my own;
 That *I have luck* must friend and foe confess,
 And what's good judgment but a *lucky guess*?
 Can this proud leech, with all his boasted skill,
 Amend the soul or body, wit or will?
 * * * This stranger from your door repel,
 And be content to *be* and to be well.
 So long *successful* in my art she cried,
 And this proud man, so young and so untried!”

An example or two will best illustrate the evils alluded to, and of which society is the victim: on the 27th of November, 1832, I was called to see Mrs. R., fifteen miles distant, whom I had attended in a former labor. Mr. R., the husband, before he came for me had called in the nearest midwife in whose care he left his wife. On arriving at the house, I learned that the child had been delivered. I asked to see it. The midwife with great reluctance retired to the corner of the room and brought forth the child. I found a large lacerated wound of the vertex exposing the brain, from which the blood issued freely. The wound was dressed. The child lingered in great misery for two days before it died. The following is the explanation of the cause of this murder: the midwife offended that a *doctor* should be thought necessary when *she* had been called in, went, as soon as Mr. R. left the house, and got a pair of shoemaker's pincers, and seizing the scalp of the child, which judging from the wound must have presented in the most favorable manner, she tore open the brain—some hours afterwards the child was born.

A midwife of large practice for a quarter of a century, had, as she had often told me, "a falling of the womb" for thirty years. It was, as she asserted, completely external, and gave her much uneasiness. Finally she was confined to bed, whereupon she sent to town for my assistance. I found the womb high up and instead of its prolapsus, a pudenal hernia of large size which had descended within the pelvis opposite the acetabulum, and which pushed forward the right labium and a part of the vagina.* A lucky midwife ignorant of the position, size, structure, and properties of the womb, and her own womb too! Oh most wise public! think of this. But the women are not alone in these blunders. A very amiable young woman, an only child, the wife of a merchant, during her first labor had a convulsion during which she bit her tongue. Her father who was present, held her jaws assunder to prevent further mischief during her struggles, while an old male practitioner of reputation, though not a regular M. D., delivered her. This practitioner and another of the same gender and like reputation, though not an M. D., "doctored" this lady thirty-seven days, as I was assured, because her mouth was widely open and could not be shut—her chin projected forward—her strength and bloom were gone—she was unable to take anything but liquids—whereupon, I was called in—I found ulcers within the cheeks and a complete dislocation of the under jaw; which I at once reduced, on the thirty-seventh day. I will not add to this catalogue by examples nearer home.

There is probably no city of equal population in Christendom where the male practitioner is less employed in child-birth than in New Orleans—none in which the female practitioners are less educated, being chiefly negresses or mulattresses, or foreigners without anatomical, physiological, and obstetrical education, and consequently unacquainted with the mechanics of parturition.

Whatever science, art, skill or manual manœuvres the exigencies of child-birth may demand, physiology and anatomy, not mere luckiness, must guide the accoucheur, just as they guide the surgeon and physician.

* I have met with a similar case of hernia in this city, although writers rarely allude to its existence.


ART. III.—OSTEO SARCOMA OF LOWER JAW.

BY C. S. FENNER, M. D.,

OF MEMPHIS, TENN.

A little girl, eight years of age, was brought to me in July, 1852, at Aberdeen, Miss., with an Osteo Sarcomatous tumor, occupying all the anterior portion of the lower jaw; extending from the bicuspid tooth, on the right side, to the first molar on the left. The tumor had an elastic feel, was about three inches in diameter, distending considerably the under lip, and encroaching on the base of the tongue. Its top presented a circular aspect, with flat ulcerated surface; both incisors, and left cuspid teeth gone; the right cuspid, and left bi-cuspid loosely hanging to the surface of the tumor, and apparently ready to drop out. When the jaws were closed the upper incisors were nearly buried in the tumor below. I advised the immediate removal of all the diseased portion of the bone; to which the child's mother readily assented. After the administration of chloroform, assisted by Drs. Pope, Randle, Haughton, and others, I made an incision through the mesial line of the lower lip, down to a point just below the base of the jaw, continuing the incision on each side along the base of the bone, to the points where the facial arteries cross. The flaps thus formed, were dissected back, leaving the anterior portion of the tumor exposed. The attachments on the inside were divided to admit the forceps; the saw applied to notch the bone, and the separation completed with the bone cutting forceps. The attachments to the tongue were then divided and the entire tumor removed, including a sound tooth on each side. But little hemorrhage followed. The divided parts were brought together and secured by interrupted sutures. The little patient awoke entirely unconscious of having undergone a serious operation. No unpleasant symptoms followed. The pins were removed the second, and the entire dressings on the third day. The external incisions, were entirely healed leaving scarcely a trace of the knife. The patient was removed to her home, on the fourth day, since which time I have not seen her. I heard from her however not long since. There has been no return of the disease and the deform-

ity is scarcely perceptible. On dividing the tumor the bone was found to be completely degenerated to a soft, elastic, grayish substance, of the consistency of the albuminous portion of a hard boiled egg—several small spiculæ of bone were interspersed through this substance—a very thin shell of bone, with the periosteum entire, remained, giving attachment to the hyoid muscles.



ART. IV.—WOUND OF THE SMALL INTESTINE SUCCESSFULLY
TREATED BY THE INTERRUPTED SUTURE.

BY J. C. MCGEE, M. D.,

OF CADDO PARISH, LA.—Reported by C. G. Young, M. D.

Merriman, a stout, middle aged negro man, belonging to the plantation of Thomas Holt, in attempting self-destruction, stabbed himself in the right iliac region. The knife entered the abdomen just above poupart's ligament and about half an inch outside of the iliac artery, making a gash an inch and a half long, and wounding the small intestine in two places. The first opening in the gut was about fourteen lines in length and the second about four; the direction of both wounds was longitudinal, and both were upon the same side of the intestine. This occurred on the 11th of April instant, and the man was seen in half an hour after by Dr. McGee, who informs me, he found a considerable length of the intestine protruding, and wounded as described. Four stitches of the interrupted suture were taken in the longer wound and one stitch in the shorter with fine sewing silk. The ends of the thread were cut short, and the intestine returned within the cavity of the abdomen without difficulty. The external wound was secured by stitches—an anodyne given, warm fomentations applied and strict rest enjoined. After thirty-six hours the belly was moved by small doses of castor oil, and the man recovered without an unfavorable symptom. I have known several instances where farmers have wounded the intestines of their pigs in spaying them, and they have pursued the practice adopted by Dr. McGee. The pigs have been turned loose in a pen and have done as well as if nothing had happened.

ART. V.—ENCEPHALITIS IN A CHILD.

BY R. L. GRAVES, M. D.,

OF SAN ANTONIO, TEXAS.

On April, the 20th, 1854, I was called to see a child two years of age, laboring under encephalitis, with violent convulsions; the pupils of the eyes were greatly contracted with great drowsiness and tendency to coma.

On my arrival, I learned from the family that the patient had been in good health except that it had shown great restlessness a day or two previous to the attack from convulsion which had already taken place before I saw the child. It still showed symptoms of another spasm. I ordered chloroform and antispasmodics, and the feet to be bathed in a warm mustard bath, and cold water to the head until the patient became quite composed, at which time, I prescribed calomel, and James's powders, which were given every two hours until the bowels were freely moved. On the next morning, I found the patient composed and in a mild perspiration. The head continued hot. I then prescribed the following mixture: distilled water, four ounces; tartar emetic, two grains; spirits of nitre, two drachms; a teaspoonfull to be given every two hours, and directed the patient's head to be held over a basin, then poured water on the head from a pitcher in a small continued stream, and at the same time kept the feet in a warm mustard bath. The next morning after I saw the child and found it doing well, having had no convulsion for twenty-four hours previous; the head was cool and the extremities warm, and a gentle perspiration over the body. I neglected to say the child was cupped on the nape of the neck during the above treatment. I then prescribed ten grains of quinine, ten drops of Fowler's solution of arsenic mixed with a little magnesia to be divided into five powders; gave one every three hours during the day. The child continued to improve. On the morning of the 23d, I found the child doing so well that I discontinued my visits.

Four days afterwards I was called to see the child again. On my arrival, I found it quite drowsy and with a tendency to coma; the

head was quite hot. I again applied cold water to the head, and cupped it on the nape of the neck; gave a teaspoonful of castor oil and turpentine, every four hours. The patient being quite thirsty, I gave gum arabic water and applied a poultice to the stomach. The next morning, I found the patient much improved, having had two operations of the bowels. I then directed quinine to be given every two hours; but before it was given, the parents imagined the child worse, and a consulting physician was called in, who approved of the course that had been adopted; we then prescribed quinine, one grain; calomel, one-sixth of a grain; to be given every three hours, and cold water to the head. The child continued to improve, and is now (May, 15th) convalescent.

ART. VI.—AN ESSAY ON THE MEDICAL PROPERTIES OF QUININE.

BY RICHARD H. DAY, M. D.

Read before the Baton Rouge Physico-Medical Society on the 13th of May, 1854; and, by order of the Society, furnished the "New Orleans Medical and Surgical Journal" for publication.

Mr. President and Fellows:

The subject, as stated to you at our last meeting, upon which, at this time, I intend to make a few observations, is the therapeutical properties of the sulphate of quinine.

My fellows will excuse me if, in this essay, I studiously avoid to appear learned, and decline noticing all the dogmas and dicta of the different authors, who have, from time, written upon this highly interesting subject. My purpose, gentlemen, is to be practical—to deduce from my own experience, and the observations of others, the true medical properties of the sulphate of quinine. That this subject is of the utmost importance, you will all readily admit; and, to us of the South, where its use has become so general and common, and I might add extravagantly large, it surrounds itself with the most absorbing interest.

To prescribe any medicine, intelligently and usefully, it is indispensably necessary that the therapeutic properties of that medicine be definitely and accurately known.

To prescribe it, under the vague impression of its being inherently stimulant or refrigerant, tonic or sedative, must give instability and want of precision to our practice, and, at least, reduce our efforts in the healing art to the humiliating character of *legal empiricism*.

What, then, I would inquire are the real medical properties of the sulphate of quinine? Is it a tonic or sedative? Narcotic; anti-spasmodic, or anti-febrifugic?—if you will allow the latter classification. It is well known that physicians hold a contrariety of opinions upon this matter; and, as a necessary consequence, differ much in their practice. I believe, however, there is no difference of opinion, among the medical faculty, in regard to the tonic qualities of this agent in small doses. The experimental researches of physiologists, and the universal experience of physicians, attesting its virtues in anæmic conditions of the system, in which there is no doubt of the impairment of vitality and a state of positive debility, have long since placed this drug at the head of tonics in the *Materia Medica*. That it is also a powerful anti-febrifuge is a settled fact, and gathers confirmation from every day's experience of its use. But, how it operates to produce this result? whether, by giving *tone* to the system and *increasing innervation*? or, acting as a direct and powerful sedative? is a question yet mooted in the medical profession. I take the position that it is a *tonic*, and that its power in controlling and arresting fevers, subduing arterial excitement and nervous irritation, is in virtue of its *tonic action, conjoined*, as I believe, with *peculiar and strong anodyne qualities*.

In order to settle this question we must go back to first principles. Those principles we must hold as land-marks and the light to guide and direct us in our observation of the phenomena of nature, that we may properly arrange the facts which we observe, and deduce correct conclusions therefrom.

I maintain, gentlemen, that no medicinal or physical agent can, *per se* and inherently, possess antagonistic properties. It may have a diversity of operation, but the power by which it operates, or *modus operandi* of operation, cannot be inconsistent and antagonistic with itself. That which is stimulant, is *stimulant*; that which is tonic, is *tonic*; that which is sedative, is *sedative*: except as a difference in special pathology and the mode of appeal may make a seeming difference in operation.

I will illustrate, to endeavor to make my position clear and tangible. No one will deny the stimulating properties of cayenne or brandy. And, yet how many cases of conjunctivitis and laryngitis have we seen cured, and cured rapidly under the local application of these powerful stimulants? The experience of every housekeeper confirms this truth, and makes the practice common in all civilized countries. Now, here is the seeming absurdity of stimulants curing inflammations, and the apparent contradiction of stimulants acting as sedatives. But, if we bear in mind the important *fact* that inflammation may be connected with and actually dependent upon a *debilitated* and *dilated* condition of the capillaries of the affected part, those seeming contradictions immediately vanish, and the modus operandi of stimulating collyria in the cure of inflamed eyes, and of stimulating gargles in the cure of sore throat, is at once plain and rational to the intelligent pathologist, without resorting to the absurdity of asserting that the same remedy is both a stimulant and sedative.

No one questions the exciting property of electricity—that subtile fluid which pervades all nature and flies upon the airy breath of every atom of atmosphere. Our elasticity of frame and vivacity of intellect depend, no doubt, in a great measure upon its presence in a greater or less abundance in the atmosphere around us. More concentrated, but still controlled, as in the different electric and galvanic batteries, the most unquestioned evidences are given of its stimulant and tonic properties upon the human system.

But let the current be stronger still, as when the thunder cloud lets loose its pent up store and tracks the heavens by its zig-zag course, instead of stimulating, should the animal frame be placed within its track, a complete overwhelming of the energies or instant dissolution is the result. The unphilosophical beholder of these different results would say that electricity, in a small quantity, is stimulant and tonic—in larger doses, sedative. But, to the attentive and philosophic interrogator of nature's laws, another and a different conclusion would become manifest.

It is well known, and universally acknowledged by all physiologists, that every impression made and action had upon the living frame is through the excitability of the nervous system. It is well known, also, that any impression too long continued or too powerfully applied to this excitability, exhausts the principle itself and fails in its wonted

effects, either partially or completely, as the application has been more or less protracted or intense.

In the instance of stunning, or death by lightning, the appeal has been made so powerfully to the excitability of the nervous system as partially or completely to exhaust that principle, and with it, as a necessary consequence, the impairment or the extraction of every vital function. Again, no one will deny that venesection is constantly, powerfully, and from the beginning, sedative in its effects. And, yet, what physician has not seen its employment act as a stimulant when the vital and animal functions have been oppressed by an active plethora or congestion of the vital organs? But, because blood-letting has taken off this oppression of the vital organs, loosed their trammelled energies, permitted the free exercise of their functions, quickened the circulation, and, as a necessary consequence, given manifest and decided increase to all the operations of life, will any one maintain that this potent sedative agent is a stimulant or tonic?

These observations are made to show the necessity and importance of studying general and special pathology, in order to understand the application and difference of action of remedial agents, as constituting a truer, more reliable and rational basis for the practice of medicine, than the absurd notion of attaching to the same medicine opposite and antagonistic properties.

Now, in reference to the action of quinine upon the human system, much disputation has been had in consequence of the disputants losing sight of, or overlooking, that peculiar and special pathological condition of the system, upon which depends the different and various actions of this agent.

Those who regard it as a sedative, in large doses, have been led into that error simply from observing its apparent effects in cases of vascular excitement, without regarding or detecting the peculiar condition of the system in which that febrile excitement originated. They lose sight of the important and settled fact, that the highest arterial excitement and perturbation of the system, and even inflammatory action itself, may and often does, supervene upon a depraved and debilitated habit of the constitution. And, such, I regard, to be the real condition of things in all those cases of fever, with or without inflammatory complaints, in which quinine has been so successfully given. If we look to the localities, either in this or other countries, in the treatment

of whose diseases Quinine has been so eminently successful, we shall see they are such as are supposed to abound in malarious influences, or of such climatic and atmospheric peculiarities as to *debilitate* and derange the whole nervous system. I simply refer to the fact, that in every section of country, where this agent is extensively used, intermittent, remittent and continued fevers, are the prevailing and endemic diseases, to establish my position; for, I believe all authors of late agree that this whole family of diseases originates in some cause acting upon and *debilitating* the nervous system.

The highest and most unquestioned authority, which I can quote, to support my views is the language of Dr. Watson, page 451 of his unequalled work on the "Principles and Practice of Physic." I might adduce other authority, and much of it; but when I have told you what Dr. Watson says, you have the best medical authority known. He says, "When I have told you that *debility*, any how produced, constitutes a predisposition to intermittent fever, I need scarcely add that all the multiform causes of debility may also be regarded as predisposing causes of this same disease, as they are of so many others." His language is still stronger when speaking of continued fevers, bearing upon the same point, but too long to quote, and I refer those who are not familiar with his views to his work.

But we need not written authority to establish this; every physician who has practiced his profession in a marked malarious district, as I have done, has seen in the inelasticity of frame, laxity of fibre, sallow complexion, erratic pains, and general malaise, evidences of the most unquestioned operation of some debilitating agent or influence on the nervous centres.

Now, it is in these localities and latitudes, where *debilitating* causes are known to exist, that quinine exerts its most herculean powers. As you go to more elevated regions, where climate and local causes give buoyancy and a high degree of vitality to the animal system, and develop diseases of a more acute and highly inflammatory character, *quinine* ceases to be used, and those remedial agents resorted to which are acknowledged to be *ab initio* depletive and sedative in their operation.

How comes it, if quinine be a sedative in large doses, that its use is limited to those diseases connected with or dependent upon *debility*,

or restricted to the localities where *debilitating* causes are known to operate? Why is it not given in large doses in open, undisguised, high inflammatory affections, occurring in vigorous, athletic constitutions, if it be a sedative? Sedatives, proper, are not so dealt with. It avails nothing to say that quinine is given, either in large or small doses, in the same diseases and in connection with blood-letting and other depletants. It only shows the mixed and compound nature of diseases, and the imperative necessity resting on physicians to analyze every disease they are called to treat, and to understand its seat and nature, whether mixed or otherwise, before they undertake to prescribe. This may be irksome and seem foolish to the indolent and unobserving practitioner; but not so with the intelligent, practical physician; for he sees many cases in which it becomes necessary to combat local inflammations by depletants, general and local, and at the same time husband the resources and nourish and support the failing strength by the judicious administration of nutriment, stimulants and tonics.

If, then, I am correct in my position that in all those cases of high febrile excitement, with or without an inflammatory complication, in which quinine is successfully administered, the vital energies though lashed into excitement, are really laboring under the *oppression* and *debilitating* influence of some morbid agent, corrupting and vitiating the life-blood, or working some profound lesion of the *nervous centres*, the conclusion is inevitable that the "methodus medendi" of quinine is not sedative, but tonic and anodyne.

If we look to the elementary constituents of this potent alkaloid, as given by the learned Prof. Liebig, in his animal and vegetable chemistry, these views will be still further strengthened. I quote from the *New Orleans Medical Journal* of November, 1845, as reported by Dr. McCormick. He says, "However strange the idea may, at first sight appear, that the alkaloids of opium or of cinchona bark, the elements of codeine, morphia, quinine, &c., may be converted into constituents of *brain* and nervous matter, into organs of vital energy, from which the organic motions of the body derive their origin; that these substances form a constituent of that motion, by the removal of which the seat of intellectual life, of sensation, and of consciousness, is annihilated; it is nevertheless certain that all these forms of power and activity are most closely dependant, not only on the existence,

but also on a certain quality of the substance of the brain, spinal marrow, and nerves; insomuch, that all the manifestations of life or vital energy of these modifications of nervous matter, which are recognized as the phenomena of motion, sensation, or feeling, assume another form as soon as their composition is altered. We must not forget that in whatever light we may view the vital operations, the production of nervous matter from the blood presupposes a change in the composition and qualities of the constituents of the blood. That such a change occurs is as certain as that the existence of the nervous matter cannot be denied. In contradistinction to the chemical character we find that the substance of the brain exhibits the characters of an acid. It contains far more oxygen than the organic bases or alkaloids. We observe that quinine and cinchonine, morphia and codeine, strychnia and brucia, which are respectfully so nearly alike in composition, if they do not absolutely produce the same effect, yet resemble each other in their action, more than those which differ more widely in composition. We find that their energy of action diminishes as the amount of oxygen they contain increases, and that strictly speaking, no one of them can be entirely replaced by another. There cannot be a more decisive proof of the nature of their action than this last fact; it must stand in the closest relation to their composition. If these compounds, in point of fact, are capable of taking a share in the formation, or in the alteration of the qualities of the brain and nervous matter, their action on the healthy as well as the diseased organism, admits of a surprisingly simple explanation. It is singular that we find medicinal agencies, all dependant on certain matters, which differ in composition; and, if by the introduction of a substance, certain *abnormal* conditions are rendered normal, it will be impossible to reject the opinion that this phenomenon depends on a change in the composition of the diseased organism—a change in which the *elements* of the remedies take a share—a share similar to that which the vegetable elements of the food have taken in the formation of fat, membranes, of the saliva, of the seminal fluid, &c., &c. Then, in a chemical sense, there is no objection to the opinion that substances of a composition analagous to that of nervous and cerebral matter, may be employed instead of the substances produced from the blood, either to furnish the necessary *resistance* or to *restore the normal condition.*”

Adopting Dr. McCormick's language, I would say: "Now the cause of fever acting on the human organization may cause therein the waste of some element which is supplied in the quinine when given; and, as quinine and the cause of fever both act immediately upon the nervous system, it must be in that part of the organization we are to search for the explanation of fever. It would, therefore, seem that the proximate cause of fever consists in some modification of the nervous system by malaria, creating a change of structure or function and preternatural waste therein, as is seen to occur in other tissues during the progress of fever, and which are only remedied by the exhibition of articles containing the appropriate elements for the formation of such tissues; for instance—as gelatine is supposed to act in convalescence in restoring the cellular tissue, cartilage, &c., so *quinine* may be supposed to act as food, supplying waste or change in the organism produced by fever in the nervous tissue."

With these views, how is it possible to regard quinine as a sedative? If fever result from a depressed and debilitated condition of the nervous system, and quinine in its elementary composition is shown to be food for that system, how, in the name of common sense, can it be regarded as a sedative in the cure of fever? Or, if it be a sedative in large doses, how can it be reconciled with consistency and sound rational treatment to give an agent in such quantities as engenders that identical state of the organism which is believed to be the foundation of diseased action? Regarding quinine as a *tonic* possessed of *peculiar anodyne* properties, a position which seems to be sustained by every view we can take of it, all the effects resulting from its administration, at once become plain and rational. If high febrile excitement, resulting from and depending on a peculiar depressed condition of the nervous system, be relieved by the free administration of quinine, it is plain that quinine has accomplished it by controlling or removing the cause which kept up that excitement, by supplying the food or material needed by the nervous system to enable it to assume a normal condition.

The effects of general relaxation, tremors and prostration, which are always noticed when it is given in excess, (and which have given rise to the doctrine of its sedative action,) have resulted from the appeal having been too powerfully made upon the nervous centres, and in-

stead of responding to the appeal, a stunned, confused, nervous agitation is the result, and an impairment of the functions corresponding to the degree of stunning produced. Given in proper cases, and in doses to meet their exigencies, it is a medicine whose virtues cannot be too loudly extolled; but, because it is thus potent for good and applicable to such a numerous family of diseases, I would not have its reputation blasted by its indiscriminate use in all cases, under the erroneous impression that it is both a tonic and a sedative, according to the quantum given, or the volition of the prescriber.

All will see the certainty of medical prescription when this notion lies at the foundation of practice, and the blunders and serious mistakes that must grow thereout. General and special pathology, that nice distinction in the nature and seat of morbid action, which distinguishes diseases and determines their appropriate treatment, is overlooked and leaves the practitioner to prescribe at random, and fight symptoms or an unknown disease, as best he may, in the dark.

While, then, I object not to the free and liberal use of quinine, and admit the wide range of its application in the diseases of the Southern and Southwestern portions of our country, still I maintain that it is important to entertain correct opinions in regard to its medicinal qualities, and the peculiar morbid actions of the system requiring its administration. If we entertain the opinion that in large doses it is sedative, regardless of the seat and intimate nature of diseases we are called to treat, much serious injury may be done, as already has been. I know that physicians have written much in our medical journals in regard to the safety and utility of quinine in certain forms of inflammation, and the disposition on the part of some to class all agents as sedatives that are capable of reducing febrile excitement, lessening nervous irritation, and under some circumstances admissible and useful in inflammation.

Knowing the sources of imperfect and fallacious observations in the field of medicine, and the strength of that ambition which prompts men to desire the applause of their fellows, we should receive all reports of the kind with caution and scrutiny. As is the case with other *patent* medicines, so with *quinine*; the good it has done, or seemed to do, has been widely circulated; while the injury which has resulted from

its improper and excessive use is hid in the dark and undivulging bosom of oblivion.

Our own experience and careful observations, had in accordance with the settled and established principles of medical science and the laws of vital action, must be the criterion by which we are to judge of the truth or fallacy of the observations and deductions of others.

Let us then look back over the field of our experience and call to mind the nature of the diseases, the quantity and mode of prescribing *quinine* and the phenomena attending its administration, and see if the views here advanced are not fully sustained.

In all those cases of high febrile excitement in which it has lessened the action and force of the heart and arteries, subdued pain, released the skin and thrown out upon its surface a free and genial perspiration, will not the pathology of fever, as herein stated and sustained by the highest medical authority, and the *tonic* and *anodyne* properties of quinine, as deduced from long experience and corroborated by the learned Liebig, in its chemical composition, fully explain the phenomena observed? And will not the same views rationally explain its salutary effects in certain forms of inflammation, engrafted upon a debilitated constitution? I think so; fully and clearly.

In regard, however, to its use in inflammation of any of the viscera or vital organs, whether supervening upon a broken down constitution or occurring during the progress of fever, my experience teaches me to be cautious. I have always found local inflammations, in whatever organs occurring, either at the beginning or during the progress of fever, materially to complicate the treatment and interfere sensibly with the administration and salutary effects of quinine. And, I hazard the opinion that in *active congestion* or *inflammation* of any of the viscera, except when preceded by free *depletion* or existing in a *sub-acute form*, connected with *diminished innervation*, that its use in large or full doses will always be injurious and perilous.

P. S.—Since this essay was written, I have had the pleasure of forming the acquaintance of Dr. W. J. Hord, a young man of intelligence, and a graduate of the University of Pennsylvania. I learned from him that Dr. Wood, the distinguished professor of the Theory and Practice of Medicine in that celebrated institution, holds opinions

in reference to the medical properties of the sulphate of quinine which strengthen and confirm the views which I have here advanced to-day. Upon referring to the last edition of the U. S. Dispensary, page 1175, I find Dr. Wood expresses himself in the following language: "Given largely, in diseased states, it has been the obvious cause of fatal results, not so much, however, by its peculiar action as by coöperating with the disease in establishing *intense local irritation* or *inflammation*, especially in the brain. From its occasional effect in diminishing the frequency of the pulse and the general strength, it has been supposed to be essentially sedative in large doses. Such an opinion, unless well founded, might lead to *hazardous practice*. The probability is that the apparently sedative effect upon the circulation arises from an *overwhelming stimulant* influence upon *cerebral centres*, whereby the system is deprived of the support of these centres, and the heart's action is depressed with other organic functions. Similar effects may be obtained from excessive doses of most of the cerebral stimulants. Examination of the brain in the lower animals, after death from quinine, has shown *great congestion* of that organ and its membranes, and even *meningitis*. In the present state of our knowledge, therefore, it is safest to consider the sulphate of quinine as a direct and powerful *stimulant* to the brain."

ART. VII.—HYSTERIA—TÆNIA—HYDROPHOBIA.

BY ALLEN DEDRICK, M. D., A. M.,

OF NEW ORLEANS.

History of the Case.—Patient a female, aged 45 years, a native of Alsace, France, married to a third husband, the mother of several children, living in comfortable circumstances and resident here several years.

Temperament, bilious inclining to the nervous; stature under the medium height; form slender; previous health none of the best, being subject to rheumatic pains; never received any great injury of either head or spine.

Diet preferred and habitually used, farinaceous substances and

crude vegetables preserved in vinegar; most common beverage, coffee.

April 28th, 1854.—Found her suffering from an attack of subacute metritis with a muco-sanguinolent discharge from the womb. Symptoms of hysteria were also present—moreover she had passed, the previous morning, about thirty pieces of tape-worm. By the use of the aloes and myrrh pill with belladonna and the hip-bath, she was speedily relieved of her pains. During the operation of this medicine, she passed another piece of tænia, measuring fifteen and a half feet. Feeling now very well, the administration of a vermifuge was postponed until other symptoms should indicate it.

May, 8th.—Her husband stated that her right arm had become partially paralyzed. He was directed to use frictions with some stimulating liniment.

May, 10th.—Found her again ill; complaining of nothing but a slight pain in the right arm, which was probably due to the action of the remedy. Observed no fault in the use of the arm.

On examination, there appeared to be no disorder of any of the organs contained in the thorax or abdomen; but peculiar spasms, very much like those of hysteria, affected the muscles generally, more especially those of the throat. She complained of thirst and an inability to satisfy it,—nay the bare sight of any fluid threw her into these spasms. This was noticed first, this morning. The eye did not betoken hysteria nor was any pelvic pains complained of. Pulse, slightly accelerated; skin warm and dry; speech animated; defæcation normal. She could swallow only pieces of moistened bread, the juice of oranges or lemons and small pieces of ice. Spasmodic efforts accompanied the act of deglutition. Antispasmodics were given every hour, and frequent enemata administered. Under this treatment, an abatement of symptoms was observable.

On making inquiries, I learned that she had been bitten in the hand, by a strange dog, about six months before: moreover the man, who subsequently killed the dog, averred that he was rabid.

May, 11th.—Learned that she had passed a bad night—the medicine having had no further effect. Deprived of sleep; tormented by a craving for water; incessant nausea; retching and vomiting, at first of bilious matter, afterwards of the contents of the stomach;

profuse flow of saliva, which immediately excited spasms in the throat, unless spit out; unable to swallow anything but moistened solids, she exhibited a state of suffering which may be imagined but not easily described.

She was sitting in bed, and complained of fatigue and weakness. The vomiting became more frequent and painful and deglutition more difficult. Her eyes sparkled; hands cold and shrunken; pulse scarcely perceptible; speech coherent, except when speaking of drinks or of the dog, whom she seemed inclined to defend against any accusations of madness. Evacuation of urine and fæces as in health. She manifested no disposition to bite or to do any injury. About 1 o'clock, P. M., she became tranquil and asked for a drink of vinegar and water. She now drank apparently without difficulty. Her face was bathed with the same, when she expressed herself much relieved, feeling only a slight pain in the head, and desired to be left to sleep. She remained quiet, apparently sleeping; the warm perspiration starting forth; breathing regular. A few minutes after she was observed lying with her eyes turned upward, and was then bathed with the spirit-wash. Reviving, she expressed her hope of soon being well, and again desired to sleep. Shortly after this she breathed her last as gently as happens in death from any other cause.

This took place thirty-three hours after the appearance of the symptoms of hydrophobia.

Saw the body two hours after death. Countenance placid; cicatrix on the hand, scarcely perceptible; body spotted with ecchymoses, particularly in the course of the nervous trunks. No autopsy.

Observations.—The above is a succinct history of a case which rarely presents itself to the observation of the general practitioner. Viewed simply with regard to the various obscure symptoms therein revealed, it becomes interesting; but studied with an endeavor to discriminate between them and to refer each to its cause, it becomes exceedingly difficult and yields much food for thought. A cursory review of the case may not prove unprofitable. A female, after passing through a laborious and eventful career, arrives at that period, universally acknowledged to be the most critical era in woman's life, and finds herself, in addition to the other ills incident to that stage,

affected with one of the worst of internal parasites, and that too, while her system is under the silent influence of one of the most dreadful poisons known. For the previous three weeks, a change had been going on in her system; the womb was about ceasing from its function, and a white and red discharge was taking the place of this her last monthly evacuation. Symptoms of hysteria, then first manifested themselves; but yielded readily to simple treatment. The first appearance of *tænia* was noted on the evacuation of the pieces above mentioned. I could not learn that she had ever before been troubled by this kind of parasite. A few days before death she experienced a sensation, as if a worm was advancing up the throat; but a draught of water removed this feeling. Was this an hysterical symptom, or was it the prelude to the more frightful spasms which followed in the course of the next thirty hours? A pain, succeeded by numbness and partial paralysis, was felt in the wounded arm some three days before spasms manifested themselves, or an inability to swallow liquids was observed. Death ensued some thirty-three hours after, during a calm, when no signs of disease were noticeable, and only appearances of utter prostration, marked the close of the scene.

Now several questions, not devoid of interest, present themselves to the inquiring mind. A few of these I shall state, and venture thereon such comments as the case seems to bear out.

1st.—Was there anything in the constitution or habits of the individual, that would conduce to the generation of these parasites?

This query gives rise to several others, and until we know what relation there is between the existence of *tænia* and any of the hereditary or acquired diseases, it will be impossible to answer the first satisfactorily. It will, however, be borne in mind, that the patient was of the bilious temperament, was subject to rheumatic affections and preferred a crude diet. As to the second query, one would suppose, on first thought, that it could be readily answered in the affirmative. Yet, who can decide, whether her morbid tastes were the cause or the effects of the presence of tape-worm. This is a subject which will be more fully treated by the Editor of the Journal.

2d.—What caused the expulsion of the fragments of *tænia*, as above recorded?

The portions first voided, were in small pieces, and, nothing but as

common cathartic pills had been taken, it is presumable that, having become broken by the peristaltic action of the intestines, they descended into the rectum and were evacuated with the contents of that bowel.

Will the same explanation apply to the expulsion of the last piece, measuring fifteen and a half feet; or shall it be attributed to the action of the aloes, myrrh and belladonna pill, on the worm itself? Taking into consideration the tenacity with which this entozoon clings to the human entrails—resisting often every means employed against it—I am inclined to believe that its death is due to some other cause, and that its expulsion merely was effected by the cathartic operation of the medicine.

3d.—What relation had the tænia to the uterine disturbance?

Reasoning from known pathological conditions and therapeutical effects, one can affirm, that such an irritation in the bowels, could not be without some influence, tending to aggravate the disorders, incident to that period of life. Moreover those symptoms ceased, on the expulsion of the worm, much too soon to be wholly attributed to the action of the medicine on the system. Whether the entire worm was expelled or not, I am unable to say; yet enough had come away to materially lessen the intestinal troubles.

4th.—Had the tænia any influence in calling into action the poison of rabies, so long dormant in the system?

The known effect of intestinal irritation, in exciting and aggravating all nervous affections, (I do not like to say *reflex* actions of the nervous system) particularly those of a spasmodic nature, would lead one to answer in the affirmative. Yet the case is obscure and the relationship doubtful. That the presence of a worm of such a size, could aggravate the malady, can easily be conceived; but to aver, that rabies would not have manifested itself, had tænia been absent, is more than one can do with any show of reason.

5th.—Did the belladonna have any influence in postponing the attack of rabies?

The purely nervous symptoms were very much alike in the first and second attack. In the first they were removed by the medicine and by the expulsion of the worm, and did not again appear, until a day or more after the pain in the wounded arm. Now belladonna has been

extolled as a remedy in this affection; yet like too many others, it has failed. The only influence I can attribute to it in that regard, is due to its known effects in quieting excitation of the nerves.

6th.—What was the mode of death?

“Death is best defined to consist in a cessation of excitability.” This may be brought about, (1st) by anything that annihilates the sensibility of the nervous system; (2d) by anything that exhausts this excitability. If only a portion of the nervous system be affected, death may ensue, simply from the arrestation of some function indispensable to life. Exhaustion is the consequence of persisting super-excitation, which is necessarily caused by an excitant. This excitant may be of such a nature, as to cause normal manifestations; and it may also be of such a nature as to cause abnormal-spasmodic-manifestations. If the former, the patient dies simply exhausted; if the latter, death may be caused, but is not necessarily, by arrest of some vital function. The first may be relieved by procuring rest; the second by coördinating the manifestation and then rest. Consequent on, or coincident with exhaustion, is congestion of one or all the nervous centres, and thereby pressure and consequent cessation of function. (Apoplexy from excess.)

Again, if the excitant be of such a nature as not to deteriorate the blood, life may be saved even after congestion has taken place; but if it has already deteriorated the blood, life can be saved only by the elimination of the poison; since a congestion of normal blood is obviously less dangerous than a congestion of abnormal. For, remove the congestion of the former and a normal stimulus is still there; while in the latter case an irritant would remain. Certain poisons are of this class of excitants. Those that directly attack the nervous centres, produce an immediate effect and death is speedy; while those that affect the nervous system, only after deteriorating the blood produce a slower death. To this latter class belong the poisons called septic. The former may fail to cause death through want of intensity; the latter, from being neutralized, or by being speedily eliminated from the system. I believe that the poison of rabies belongs to the latter class, and hence that the tendency is to a death by exhaustion, manifested by signs of congestion of the nervous centres. (Paralysis, Apoplexy.) But this termination is not always reached, since, du-

ring the stage of super-excitation, spasmodic actions may take place, and apnœa ensue. English and French writers, insist on asphyxia as the mode of death; while the German assign also palsy and apoplexy as the termination.

My patient being a German, doubtless preferred dying after the mode in vogue in her fatherland. That she did thus die, I am convinced, by the sensation of pain and fulness in the head; by the up-turned eye-balls; and by the easy sinking into sleep—none of which symptoms were noticeable until half an hour before death.

7th.—What is the nature of the poison?

On this subject, I am forced to confess, that the above case throws no new light.

From experiments, made with common saliva, it would appear that the injection of this fluid into the veins or arteries will produce symptoms almost identical with rabies. Nay, it has been asserted that the saliva of a healthy dog, received into a wound, can cause in certain unknown conditions of the system, all the manifestations of hydrophobia. Hence, it would appear that there is a principle in healthy saliva, which, if taken in sufficient quantities into the circulation, will act as a poison.

Those curious on this subject may consult vol. 8 of Ranking's Abstract; South's Notes to Chelius, &c.

8th.—How does it enter the system?

Dr. Marshall Hall (*Diseases of the Nervous System*, Lond., 1851, p. 334,) says: "The poison is probably inserted into the substance of the fibrillæ of excitor nerves." That the nervous branches are wounded is highly probable, but that the rabies is due to such wounding, I deem it very improbable; for, if his supposition were true, there would be a speedy manifestation of the symptoms, or, at least as in tetanus, when cicatrization took place; and, moreover, the wound would exhibit some anomaly in healing; neither of which takes place—hardly as an exception to the contrary as a rule. The wound heals as kindly as any other bite, and pathognomonic symptoms do not manifest themselves often not till the lapse of months—nay, even years. Moreover, the experiments above alluded to, rather countenance the opposite view.

Hence, it is more probable to suppose that the poison enters the

system by means of the absorbents, remains either dormant, or most likely undergoes a zymotic change. That it does not pervade the whole system, at first, is probable, from the fact that on its breaking out, the neighborhood of the wound is the part first affected. Moreover, the prophylactic treatment is wholly based on this idea, and thus, also, therapeutics strengthens the view in opposition to the supposition of Marshall Hall.

The above case merely corroborates the fact long ago observed.

9th.—Is rabies a curable malady ?

If the view of its pathology, which I have but partially developed in the sixth section, be correct, we may confidently pronounce it curable. But, as from our ignorance of the nature of the poison, we are unable to select an antidote, the successful cases will be few. Many cases, like the one above described, present a stage of quiescence, as if the disease had exhausted itself, before destroying the patient. In this stage, suitable medication, it seems to me, would not fail to support the patient and hasten the elimination of the morbid matter. Patients have been saved in the collapse stage of cholera, why not in this stage, which so nearly resembles it ?

10th.—What treatment promises to be the best ?

Knowing no antidote, we should endeavor to prolong existence, in order that the patient may have as alternative the kind of death most remote, viz : exhaustion with congestion. To attain this, it is obvious that all means should be used to still the inordinate actions, as perfect quiet, absence of light, sedatives, baths, &c. ; and, if the remission takes place, stimulants.

I would recommend the following course, modified to suit the case. The patient to be kept, as above mentioned, under circumstances to ensure quiet. A complete evacuation of the bowels, either by pill or by an enema introduced by means of a flexible tube into the colon ; a bath, warm, cold or vapor, using that kind which is practicable in the particular case ; ice ad libitum as long as it can be swallowed ; chloroform, inhaled if possible, or introduced into the bowel, repeating it as often as judged necessary ; lastly, stimulants externally and internally. I prefer quinine and ammonia given in a beef tea enema, frequently repeated. If spasm of the glottis threaten to be fatal, tracheotomy should be resorted to. I would

give nothing by the mouth except pieces of ice to allay thirst, for obvious reasons. By such means, with close attention, I believe that some cases can be saved. From cases reported, (Ranking's Ab.) chloroform promises more than any medicine of its class.

It is usually given by inhalation, but this is often impossible from the spasms that are excited by it. I tried that method in the above case. It was my intention to give it by enema, but before I could procure my apparatus the patient expired.

Much more might be said on this subject; but as I did not contemplate writing an article on hydrophobia, and only wished to present several points in the above case, I shall leave the subject for future study.

ART. VIII.—ON THE REPUTED CAUSES OF YELLOW FEVER, AND
THE SO CALLED SANITARY MEASURES OF THE DAY.

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I propose, in the present communication, to offer a few reflections on the notions, ætiological and preventive, that to a great extent prevail here touching the subject of yellow fever. In proceeding to do so, I shall offer a few remarks appreciative of the filth theory, as deducible from the condition of things in and about Gormley's Basin and Canal, during the epidemic of 1853. I may say, once for all, at the outset, that I am no advocate of filth, nor for any government or individual who is. Filth can never be salutary, but the reverse, when it has any effect at all. It is not only clearly noxious to the *physique* of our species, but it offends our moral sense. It sustains the same relation to the body that we are assured sin does to the soul; and, as body and soul mutually re-act on each other, so do sin and filth intermingle, and, as it were, become confounded with each other. The wages of filth, as well as the wages of sin, should be by every possible means avoided by "a Reform Council." But, filth though bad, is neither a basilisk nor a Gorgon's head; nor is it the cause of yellow fever. Sanitarian philosophers in our midst appear to regard it as all of these. The physician who looks beyond the hold of a ship, or who raises his thoughts above the contents of the gutter, is regarded by many as in-

capable of discerning the *punctum saliens* of all epidemics. The shiphold and gutter-philosophers desire to claim sway; and in their crusade against filth, on the yellow fever basis, go for expending millions of the public money, and devouring the commerce of New Orleans. But *revenons à nos moutons*. All are familiar with that remarkable caldron, the Gormley Basin and Canal. The basin occupies the large square bounded by Felicity Road, Dryades, St. Andrew, and Hercules streets; and the canal, which is about eighty feet wide, leads from the basin towards the swamp, for a quarter of a mile, and there, at Ellen street, suddenly terminates in a ditch two feet wide. The canal and basin were originally about ten feet deep, but during the last eighteen years they have constantly been receiving the discharges from St. Andrew street and from Felicity Road, and from all the streets that enter into the latter at an acute angle. None of the deposits ever having been removed from the basin and canal, they have gradually filled up, till within the last three or four years the dregs of the whole area have begun to show themselves above the surface of the water. After a heavy rain the basin has about four inches of water in the centre, with just sufficient depth to cover the two deltas formed at the *embouchures* of the gutter of St. Andrew street and Felicity Road. This enormous area, from which no filth ever yet made its escape, has no other outlet than the narrow ditch in which the canal terminates, and at every point within the area a pole can be thrust down seven or eight feet, piercing, at the same time, thrice the number of kindred ingredients to those enumerated in the "Wierd Sisters'" catalogue. For all we know, indeed, it may contain some of the "grease that sweaten from the murderer's gibbet," for it was formerly the site of the gallows. The appearance of the hot sun, after a rain, speedily covers the caldron with a deep green mantle, which a few hours of solar action converts into an elevated black foam. As the evaporation goes on, nearly the whole space becomes uncovered, the basin yields up its dead, and the whole necropolis of departed animal and vegetable life lies naked to the rays of the sun. To crown all, the whole district is occupied by a series of soap-factories and tanneries, which no precaution can prevent from exhaling an offensive odor, as stale animal matter is here the material operated upon.

If there be a word of truth in the "filth theory," would not the

yellow demon have naturally erected his throne on the vapors of the Gormley canal, and after having smitten all in the immediate vicinity, have carried the war into other districts? Such, however, was not the case. The wayward disease did, it is true, early make its appearance at "the caldron," rapidly carrying off one Kehm and his wife, the former dying with the black vomit. There appears to be a discrepancy with regard to the dates of these cases. My own recollection fixes them early in May. The sanitary commission, who have "sent for persons and papers," have fixed the cases about the first of June. The discrepancy is, however, quite immaterial as affecting the conclusions which I have drawn from these cases, namely: *First*.—That they were amongst the earliest, if not the very first cases, that occurred. *Secondly*.—That they not only clearly occurred without any communication with the shipping, but that they made their appearance, and terminated fatally many days before it has been pretended that there was any case on board of any ship that came into the port of New Orleans. *Thirdly*.—That Kehm and wife could not have contracted the disease by personal contagion, as they had not been in contact with any person laboring under yellow fever. *Fourthly*.—That Kehm and wife did not communicate the disease to any person, though immediately surrounded by unacclimated persons; for no person in the neighborhood was attacked for six weeks after the time of Kehm's wife's death, following the date given by the sanitary commission; and after the expiration of that time no less than fifteen persons were attacked in the same square almost at the same time.

Now, we would ask the filth theorists, how came it to pass that the disease abandoned the shores of the Gormley Canal and Basin for six weeks, in which the hot month of July was included, and betook itself to the front of the Fourth District, attacking squares which were but sparsely populated, free from stagnant water, and comparatively free from filth? From the beginning to the end of the epidemic I was in the midst of it, and I can positively affirm that the disease showed no special predilection for localities having a concurrent excess of heat, moisture, and animal and vegetable putrefaction; the driest, most elevated squares, the cleanest streets, houses and yards, being alike, and simultaneously, invaded with places in which the reverse prevailed. The early appearance of the two yellow fever cases at the

canal and basin, and the disappearance of the disease from these places for six weeks, while it ravages the front of the Fourth District, shows conclusively that the "ingredients of our caldron" did not in the least affect the health of the neighborhood, but that the disease actually ran away from the most awful repository of filth in the corporate limits of New Orleans, and that it is probably a matter of supreme indifference, so far as yellow fever is concerned, whether the rear of the Fourth District be girt with the piney woods of Biloxi, the bluff of Natchez, the sand hills of Vera Cruz, the rocks of Havana, or the seething canals and basins that "boil and bubble" in the rear of the Crescent City. In fact, the filth theory is wholly untenable, not only from the entire history of yellow fever in this community, but from every analogy known to science. Every product, whether solid, fluid, or aëriform, or imponderable, whether resulting from the decomposition of animal, vegetable, or mineral matter, produces effects which are remarkably uniform. They do not discriminate between the acclimated and the unacclimated, nor are their noxious or innocuous properties at all modified by such circumstances. For instance, the deposits at Gormley's Basin give off amongst other substances, sulphuretted, carburetted, and phosphuretted hydrogen, and ammoniacal gases, substances which with ordinary ventilation merely give evidence of their existence by offending the nostrils, in bad ventilation become oppressive, and when highly concentrated quickly destroy life. The effects are uniform in all climates, and to assume that the mere inhaling of a mixed atmosphere, which one person breathes with perfect impunity, should give another the yellow fever, another the cholera, and a third the typhus fever, is simply a *reductio ad absurdum*. Our knowledge of these gases demonstrates that the reputed cause is insufficient to produce the effect, and there is not a shadow of proof to show that it does produce it. The exhalation from departed animal and vegetable life mingle with our every breath, and it has never been shown, nor ever can be shown that they act otherwise than uniformly. Touching the filth question, let us not be misunderstood. Let every alderman "search the scriptures." There is scarcely a chapter in that sacred book which does not admonish all to "wash and be clean." Common decency also speaks aloud. False and improbable theories, however, can do nothing else than lead to useless

and extravagant and corrupt legislation, and to lull the public into a false security.

The distinguished Professor Riddell, of the University of Louisiana, has adopted the atmospheric theory of yellow fever which he deduces mainly from the filth platform. His views are liable to all the objections which we have urged, besides others peculiar to themselves. He holds the following language :

“That the disease has been marked by characters of infection, and infectious communicability, the poisonous matter (doubtless some species of living organism) maturing its germs or spores on the surface of solids devoid of life, surrounded by impure air ; which germs become diffused in the impure atmosphere.

“Three peculiar conditions seem to favor the development of the infection. 1st.—The absence of ozone, the great chemical promoter of oxidation, which absence permits the undue development of obscure cryptogamic life. 2d —Abundant emanations from decomposing and disintegrating organized matters, complex products, gaseous, liquid and solid, the *placulum* or blastema of cryptogamic growths. 3d.—The presence of the specific organism, whose perfected spores constitute the material cause of yellow fever.”

The cryptogamic origin of yellow fever is untenable for two reasons : In the first place, it is denied that such cryptogamia are known to exist, although Professor Riddell appears to have no doubt on the subject ; and in the second place, whatever forms “of obscure cryptogamic life” may be discovered to exist, all analogy shows that they cannot be admitted as the cause of yellow fever no more than the gases of which we have spoken ; for all the poisonous fungi or cryptogamia must be admitted to act pretty uniformly and to injure all persons alike who are subjected to them. Acclimation could no more render a poisonous fungus innocuous than it could render a poisonous mineral so. Professor Riddell’s poisonous cryptogamia contravenes all toxicology which teaches us that all animal, vegetable, or mineral poisons not only affect those who never have before been affected by them, but that they will act equally a second, a third, or a twentieth time. We need not follow the learned professor in relation to his views of ozone. The condition of the atmosphere in regard to that shadowy agent, whether plus, or minus, does not appear to have any

effect in the production of the ordinary cryptogamia of the botanists, and why on "the obscure forms of cryptogamic life?" Did ozone exist in such quantity in the atmosphere as to prevent the development of *any* form of either vegetable or animal life, including "the obscure forms" of Professor Riddell, can it be otherwise than probable, nay certain, that the whole population would be thereby asphyxiated? Professor Schönbein who first described ozone, says that its effects on the lungs are similar to those of chlorine and bromine; that a mouse was killed by it in five minutes, and that he himself, was seriously affected by breathing an atmosphere charged with it. Neither Berzelius nor Schönbein were able to say positively what ozone is.

According to Professor Riddell's hypothesis there was an absence of ozone in the atmosphere of New Orleans, and of course at Gormley's Canal and Basin, and, also according to the same, of course in the latter locality an abundance of the "pabulum or blastema of cryptogamic growths," as there was no lack of "emanations from decomposing and disintegrating organized matters, complex products, gaseous, liquid, and solid,;" and yet, the disease showed no preference for the locality, first appearing and then leaving it for many weeks. Waiving all opposition to Professor Riddell's gratuitous and imaginary premises, and supposing he had actually discovered the non-ozonic state of the atmosphere, and that it was charged with myriads of *fungi venenati*, he would not have thereby made the first step towards elucidating the ætiology of yellow fever.

As was to be expected, the disease reappeared at the Gormley Canal, but it was not till about the first of August. And here it will naturally be asked whether or not all the non-ozonic, cryptogamic and filth agencies—the latter needing neither retort nor microscope to prove their existence—tended to increase the number of cases or render them more malignant when they occurred: or whether the patients would have fared any better in the hill-side towns and piney-woods where the disease prevailed? The number attacked by yellow fever in 1853, in New Orleans, could not have been less than 27,000, and the number of deaths were probably from the same cause not less than 9,000, making a mortality of thirty-three and one-third per cent., of those who suffered. Here is a picture truly frightful to contemplate, but it bears no comparison to the ratio of mortality that occurred in most of the country towns, villages and plantations, many of them

dry, elevated, and free from filth agencies. In many of these localities nearly the whole population was attacked, and death occurred at a ratio that would have carried off our whole unacclimated population in less than forty days! Comparatively speaking, New Orleans was but lightly scourged, but she lost one in three of those attacked as a general result. As might naturally be expected some portions of the city exhibited a higher and some a lower ratio of mortality, and according to my observation, which was very extensive, though the disease attacked at the Gormley Canal, as it did throughout the city nearly all of the unacclimated, it was far less malignant there than in any other portion of that suffering region, the Fourth District of New Orleans, excepting around the Saraparu Market. On examining my records, I find that I attended forty-seven patients immediately on the basin and canal, the most of them residing in soap factories, nine of them being children under 10 years of age, the other thirty-eight were of various ages; twenty-seven males and eleven females. The women and children all recovered; four of the males died, one a young man, aged about 24 years, who died in Ellen street, was apparently doing well till he was taken one night to the upper story of the soap factory, and I believe his death resulted from the heated air that arose from the furnaces, rather than the intensity of the disease; another was a young man of about the same age who was removed from Ellen street to Magazine street, where he died of black vomit; a third was aged 60, and was affected with tuberculosis of the lungs; and a fourth aged 35, relapsed after going out to work, and died with black vomit. The forty-three who recovered were mostly mild and manageable cases. In the square occupied by Mr. Ebinger's soap factory and residence, which is bounded on one side by the Gormley Canal, and on another side by the Gormley Basin, and which receives the whole venom of the caldron, there were fifteen cases; occurring, however, after the 3d day of August. I attended on twelve of these cases. All fifteen recovered. There was not a death on the square around which exists more filth—more of the “pabulum” of which Professor Riddell speaks, than can be found in any square in New Orleans, and where according to the filth theory, the maximum of mortality ought to have been found.

As for the treatment pursued, whatever may be its merits, one of

the most important elements of the success in these cases was the mild type of the fever; for, in pursuing the same general course in certain other quarters of the city, no such result could be obtained. I attended thirty-five cases in the squares around the Saraparu market without losing a case—one of these too being a case of black vomit—whilst in the neighborhood of Washington street, in one house, three were attacked and two died; in another, five were attacked and three died; in another, three were attacked and two died. To show that the disease was malignant without any discernible local cause, in the house in which the three out of five died, no less than six persons were attacked in the adjoining house, and every one recovered. I found but little difficulty with children. Some were attacked with convulsions, but rarely. In them the disease was so mild that in many cases I found the *vis medicatrix naturæ*, altogether trustworthy, yet all toxicology shows that children are fearfully sensitive to all poisons, including the cryptogamic and gaseous. "Doubtless," says Professor Riddell, "the poisonous matter" is "some form of living organism." I should say that doubtless the professor is mistaken, for if the disease has such origin it would not be so merciful to children. Immersed in a common atmosphere, in a common room, the stalwart father falls mortally poisoned, while the tender infant is affected with a mild fever, which in a few hours spontaneously goes off in a perspiration. Strange fungi! strange gases! strange poisons! A strange foundation have the advocates of these theories! A strange pretense for all the extravagant systems of disinfection, quarantines, and Quixotic schemes of drainage! Professor Riddell gravely proposes to restore the atmosphere to its wonted quantum of ozone!

Touching the great epidemic of 1853, Professor Riddell comes deliberately to the conclusions "that the towns and plantations of the Southwest have *this year* derived their yellow fevers from New Orleans;" and, further, "that although black vomit fever, or types of yellow fever, *may perhaps originate in this region*, yet, the *germs* of our epidemic of 1853, have probably been derived from countries further South." The "germs" alluded to are, according to the professor, the "specific organism, whose perfected spores constitute the material cause of yellow fever." If no further evil could result from the above than the mere enunciation of purely speculative ideas, on the

part of the able professor, we should have no demurrer to enter in the premises. But, when all the evils of expensive, ruinous, and oppressive legislation are sought to be saddled on the people, under no better pretence than the groundless dogmas here put forth, the case is different. The professor is a member of a learned and salaried commission, who are sitting in judgment on the causes and prevention of yellow fever; in the employment of the city government. The opinions and theories put forth should be worth paying for, and worthy of being a guide to legislation. The public will hold him emphatically to the proof of what he has set forth in his official report, published by authority of the State. For example, at what time, place, and under what circumstances, and before what witnesses were cryptogamic bodies discovered, "whose perfected spores" Professor Riddell categorically deduces to be the "material cause of yellow fever?" Were they (if they exist,) more numerous last summer, than the summer of 1852? What were the circumstances of time, place, witnesses, and re-agents, that led to the non-ozonic revelation?

If these views of the professor be correct, then why is not he consistent with himself, and instead of saying "that is proper and feasible for New Orleans to have *some kind of quarantine*;" why did he not recommend, in the name of common humanity, the most rigid and uncompromising system of restriction? "Some kind of quarantine," indeed! Alas! if it be true, as he suggests, that the great epidemic was freighted into New Orleans, what can excuse the luke-warmness here exhibited in relation to quarantine? Would any commercial or pecuniary sacrifice be too great to save 27,000 persons from the bed of suffering and 9,000 from the bed of death? And, if it be true, that the pestilence was freighted out of our city to ravage six States of the Union, what sacrifices ought we not be willing to make to prevent our city, if possible, from being the scourge of the South! According to Dr. Riddell's ætiology, we ought to stop at nothing, even to the absolute interdiction of the Mississippi, when the yellow fever makes its appearance in New Orleans. No ordinary quarantine could be trusted. Of what avail is it that yonder ship has passed the ordinary quarantine, and no cases of yellow fever have occurred, and she is pronounced clean? Who can say that she may not contain, at the same time, "perfected spores" of millions of the Riddellian cryptogamia?

Now, we deny that importation played any part in creating the epidemic of 1853; and we are prepared to prove that the causes of yellow fever, whatever they may be, had produced the terrible black vomit case of Kehm, and also the case of his wife, independently of anything "further South." It is easy, throughout the country, to designate scores of instances in which the disease appeared without any possibility of its having had its origin either directly or indirectly in New Orleans. We further maintain that clear and unequivocal proof of isolation and independence of origin, in a single case of yellow fever, is evidence of more value in determining the question of importation and exportation, than all the coincidences, successions of events, and consequences that Professor Riddell has elicited from his extensive correspondence throughout the country, ten times told.

In order to have carried the disease from New Orleans and to have rendered it epidemic throughout the great Southwest, and at the same time to reconcile the whole with his ætiology, an impossible prodigy must have been performed, and Professor Riddell's "three peculiar" [atmospherical] "conditions which seem to favor the development of the infection," must also have been shipped on board of the steamers and carried along with the fugitive patients! The non-ozonic, the miasmatic, and the cryptogamic conditions of the New Orleans atmosphere must have been freighted on the same boats with the flying people. No one can abandon this ludicrous position, and adhere to the professor's ætiology, without abandoning the whole allegation, unjustly charged against New Orleans, that she created an epidemic in six States of the Union. If the New Orleans atmosphere was not shipped into the country, the three conditions must have existed in the country at the time of, or before the shipping of the yellow fever patients, and consequently the allegation falls to the ground; and, if as Professor Riddell admits, and as all ought to know, "the disease has not been personally contagious," to what other conclusion can even he arrive, than that the great epidemic could have extended itself in no manner inconsistent with entire independence of its doings in New Orleans.

The disease might have been carried into New Orleans and carried out of it; but it could not have become epidemic from anything that could be carried in or out, even on Professor R's. ætiological conclusions. The disease of 1853 was in no respect different from the dis-

ease which has occurred here every summer, either in the epidemic or sporadic form, for the last twenty-five years ; and during the epidemics of '37, '39, '41, '43, '45, and '47, had it been possible for New Orleans to have originated a Southwestern epidemic, it would have made its appearance. The unknown conditions, however, which produce the disease were present in New Orleans and absent from the country, and, hence, all the power of steam in wafting away goods, patients, bed, and bedding, could not confer an epidemic on any plantation, town or city, any more than could absolute non-intercourse have prevented the general epidemic of 1853. The mere matter of fact is very true, that the disease was, during the great epidemic, carried into the country in the bodies of individuals ; but the epidemic was not. The whole was merely "carrying coals to Newcastle ;" and New Orleans, instead of being chargeable with contaminating and poisoning the mighty ocean of the atmosphere throughout six States of the Union, has been placed before the world by her chivalric sons simply in the light of an angel of mercy to the stricken Southwest. New Orleans was indeed the great position of safety against a common enemy, whose favorite field, and multitudinous birth-places were, in 1853, in a special manner in the wide spread country itself.

There can be no worse sample of bad faith exhibited, than to take any hand in deceiving the public in relation to our city on the health question. The truth should be frankly asserted, and published without either croaking or palaver. I am far from considering New Orleans an unhealthy city, so far as her resident and native population is concerned. The articles that have, from time to time, appeared in this journal, by my preceptor and brother, its present editor, and Dr. S. A. Cartwright, have held up in their true light the fallacies that have been promulgated by those who have attempted to write down the reputation of the city with regard to salubrity. But what will be thought by every physician, possessing the commonest capacity for observation, of this specimen of see-saw disbelief displayed by Dr. Riddell, when he says, "black vomit fevers, or types of yellow fever *may perhaps* originate in this region." Not "yellow fever" quoth he, but "black vomit fevers or types of yellow fever," and only "perhaps originate in this region," at that. Professor Riddell surely is more at home with his retorts, in the Laboratory of the University, than in pronouncing on the nativity

of yellow fever. This question can be put at rest by too many reliable witnesses who do not dogmatise, but who know. We ourselves are fortified with eighteen years of observation and experience here, and as an off-set to what our able professor of chemistry has said, we assert what we know, and have seen. Not a summer has passed, during the whole period, in which we have not seen *bona fide* cases of adynamic and hæmogastric yellow fever, with black vomit. The local origin of yellow fever is fully shown from the fact that it has appeared here annually, either epidemically or sporadically, for the last twenty-five years. The unmitigated form of yellow fever is not to be mistaken. Cholera itself does not imprint its ghastly character on the body with more fearful truth. A doubting professor of chemistry must throw off his Pyrrhonism. The disease has appeared in the Charity Hospital every summer for the last eighteen years, and the chances are, that for every single sporadic case in the Hospital there were twenty out of it, throughout the city. The disease has not only become native and local here, appearing either epidemically or sporadically every year, but so pervading an influence has the disease, that it imprints its character on numerous other diseases throughout the year. Hence, as we have occasionally witnessed, measles, scarlet fever, and dysentery, sometimes terminate in black vomit; also intermittent, remittent, and continued fevers are found occasionally to terminate in black vomit, jaundice, and hæmorrhages. Space will not permit us to enter further into proofs that the *bona fide* yellow fever makes its appearance here every year. On this point we stand fortified.

Such then being the facts, notwithstanding the doubts of a professor of chemistry, with what blighting and damning force do the doctrines of the quarantinists and importationists strike at the prosperity and prospects of our beloved city of New Orleans! If the disease can be imported, it can be exported. According to the above doctrines a sporadic case carried into the country, or into another city, might originate an epidemic. If the disease then appears here either epidemically or sporadically every year, what can the doctrines of quarantine do save to invite the whole world to lay our outward bound commerce under the most ruinous restrictions? All the expensive mummeries of fumigation and disinfection of goods must be endured, all the robberies, extortions, and vampyreism of officials. If a quarantine be use-

ful it is not useful to New Orleans. Its advantage could only accrue to those who bring it to bear against our commerce and our interests, and treat our city as a pest-house; and if a *scintilla* of proof cannot be adduced to show that the yellow fever ever did spread from importation, and if it can be shown (as it can be) that the disease has been repeatedly imported into this city and other cities without spreading—if it can be shown that it is imported without spreading, and spreads without importation, then are the quarantinists struck dumb as the sheep before the shearer. Their doctrines are not only untrue, but injurious to this community.

The leading argument, indeed, which has been made use of in New Orleans in favor of quarantine is, that it will create *confidence*! There is not, in the whole dictionary, a word suggestive of more fearful associations than this word *confidence*. Let the histories of rotten banks, broken hearts, dishonored virgins—nay, the history of the world bear witness. Nothing is, nor has been, more common than confidence unworthily bestowed. And, are we justifiable in invoking the confidence of the people and lulling them into security, on the strength of a measure which has been a universal failure? What, save the most absolute proof of utility, should challenge confidence in the premises? Is the *confidence* argument justifiable on any principle of good faith or fair dealing?

The history of sporadic yellow fever in this community, besides being of the greatest interest in showing, *first*—that it may appear, as such, for years without becoming epidemic; *secondly*—the local and indigenous origin of yellow fever; *thirdly*—the absolute inutility of quarantine; and, *fourthly*—the impossibility of spreading the disease into the country by exportation, presents also some other curious matters for consideration. “Commerce is king,” and it is no more permitted to any physician to report cases of yellow fever with impunity, in the absence of an epidemic, than to foretell and “encompass the king’s death.” This is well appreciated in New Orleans. The laws of the counting-house are inexorable; and the unfortunate wight who takes hold of a sporadic case of yellow fever, and reports it, gets hold of the “Bottle Imp.” He is soon given to understand that there is no such case in either the day-book or ledger; and, he

is appealed from to "our most eminent physicians, who have each ten times his practice, and have not yet seen a case." If possible the disease must be ignored; but it has been found quite impossible. In some seasons, since 1837, the sporadic cases have become so numerous that they have been reported under different names; and amongst others was the very general appellation of "*febris flava!*" on the same principle that gave the *craziness* of George the Third, the name of "*His Most Gracious Majesty's indisposition!*"

Well might our mortuary records of sporadic yellow fever be marked with a *cætera desunt*. For instance, the cases of Kehm and wife, already alluded to, occurred long before the epidemic was declared—early in the season. He died ejecting the black vomit on his sheets. Afterwards his wife died also with the disease. A reverend father of the church administered the extreme unction, and a dozen persons followed them to their graves, now visible; yet, neither certificates, records, nor any written proofs can be found in the archives of the cemetery to show that any such persons ever either lived or died! Yet, we are often told that we cannot go behind the record, which is a finality! When such *lapses* as these are allowed to occur, what becomes of our implicit reliance on the records?

What man of common information would place implicit reliance on statistics based on mortuary certificates rendered, to a great extent, on the diagnostics of the laity—certificates, numerous rendered by citizens, scavengers, deputy-coroners, barbers, and all the charlatans, and quacks, that batten on the credulity of the public?

There has been no end to the assigned causes of yellow fever in this community. There is no end to proposed legislation; but no guide to sound legislation. It is in vain, in the present state of our science, that we call on our law-makers to regulate their action by our electrometers, our hygrometers, our thermometers, our barometers, our rain-gauges, and our river-gauges, in expending the money of a tax-ridden people to legislate the yellow fever out of New Orleans. Legislation had better come to a stand, when the blind offer to lead the blind. The theories deduced from this meteorological battery are all liable to the same objections that may be urged against the filth theory. These instruments *ad hoc*, are dangerous; for their uses are not understood in making out the result. Let the use of things be learned before

used. Let us not share the fate of the nervous youth, who was so fearful of lightning that he procured himself a pocket lightning-rod to hold in his hands in a thunder-gust! We hope to see a flood of light thrown on the causes and prevention of yellow fever by the sanitary commission. They will do much if they batter down prevailing errors.

The truth is, the causes by which by far the greater portion of the mortality of a city or community is produced are either conjectural or wholly unknown; though it is in the investigation of these that speculative medicine has exhibited its boldest flights and its greatest self-sufficiency. As a matter of course, in all climates a constant train of mortality lies in wait on the path of all that lives. Death though certain at last, has for its causes in every community those which are avoidable, and those which are unavoidable. To draw as far as possible the distinction between these two classes of causes, and as far as possible to apply the remedy to the former is to do all that lies within the competency of sanitary legislation. No earthly consideration can be of greater importance to any community than such distinction and such action. It overtops almost all other considerations known to medicine—not only securing the maximum amount of salubrity, but solving at once the question of the real salubrity or non-salubrity of any given district, and, consequently serving as a guide to the embarking of capital, labor, and life in the same. The magnitude of these objects has attracted the attention and occupied the pens of the most eminent inquirers in Christendom with results which though useful and interesting, are little satisfactory to the public mind. The controversial has largely predominated over the positive. The ground here proposed is, it must be confessed, one beset with extraordinary difficulties, and what is most remarkable in too many of those who have occupied it is, that the mania to do and to discover that which is impossible has thrown into the shade the great measures of known possibility, and practicability. The spirit that now characterizes sanitary movements in our midst fully illustrates the truth of this position and promises a grainless harvest. Nothing is attempted but the impossible. Nothing at present appears to secure sanitarian attention save only the causation and prevention of epidemics—subjects probably wholly beyond the ken and control of man. The avoidable causes of daily death exist in almost every square—are seen unheeded and unremedied.

We have no right from anything that is known, or from anything that is likely to be discovered, to rank yellow fever as one of the *avoidable* causes of mortality. Its epidemic appearance must be contemplated, in the present resources of human knowledge, as merely an ultimate fact—an accumulative manifestation of the general scheme by which all that is vital shall throw off vitality, and as little capable of explanation as are first principles or self-evident truths. Why matter *constantly* gravitates towards the centre, and why the yellow fever *inconstantly* makes its appearance in certain cities, are, for all that has been determined, equally inexplicable. Epidemics have existed in all ages, and no human agency has ever stayed their march, limited their boundaries, or shortened their duration. None of their laws or habits have ever been changed or annulled by man. Epizootics have also ever existed, and the animals useful to man have been swept away; and the sea itself has been invaded, and submarine pestilence has thrown to the surface millions of the finny inhabitants, that a few days before gambolled in health beneath the waves. *Epibotanic*s (if such a word may be coined,) lay waste the domain of the agriculturalist. Beneath the surface of the soil the fatal law is executed, and the food of the Irish nation (the potato) becomes gangrenous. Who has descended into the sea, explored the soil, or scanned the mighty ocean of the atmosphere, and can pretend that, in all this, we have to do with an avoidable cause of mortality? Who can ward off the execution of this inexorable law of accumulative mortality? The people of New Orleans may exhaust their private fortunes for protection, the government may lavish its millions, sanitary laws may grow into codes, and sanitary officers into standing armies, but New Orleans, and all other cities, must be visited by epidemics. “The pestilence that walketh in darkness and that wasteth at noon-day,” will walk and waste. At the present writing an ordinance is proposed to be passed by the Common Council, which revives and reëmbodies all that has hitherto been found expensive, useless and impracticable. The whole twenty-two sections must soon prove a dead letter and fall into disuse. The undertakings set forth in this ordinance are almost too stupendous for belief, and involve an interference with private rights and an extent of jurisdiction that can expect no quarter and

will receive none. The fourth section, amongst other things, gives the proposed Health Department power over the *elements* themselves! It says: "It shall be the duty of this department to have *surveillance and control over everything* that may affect the salubrity of New Orleans, or have a tendency to impair the same." Sublime surveillance and control! which of course includes the agent that produces yellow fever!

How is the provision in article four to be carried out, regulating the number of cubic feet of space for lodgers—one thousand cubic feet for each adult—unless it is proposed to partially rebuild the city, and provide houses for the laboring class, who, to economise, crowd into small houses. If the poor cannot pay for the requisite number of cubic feet, will the Health Department undertake to furnish it. Or, will not the people, outraged by this inquisition on the internal economy of their household affairs, eject these agents from their premises as intruders on their constitutional rights.

For the present, we take leave of our subject. I propose, in a future number of this journal, should time permit, to enter into the consideration of some of the *avoidable* cause of mortality in New Orleans. They are multifarious; their name is legion.

ART. IX.—CHLOROFORM IN STRANGULATED HERNIA.

BY B. F. TAYLOR, M. D.,

OF LOUISIANA.

Of all the maladies to which the human body is subject, that of irreducible hernia is probably the most formidable and important, calling forth the greatest amount of surgical and anatomical knowledge, requiring the most prompt and decisive action, as well as the utmost skill and dexterity in the performance of an operation, when that is rendered necessary by the inefficiency of the means usually employed to accomplish its reduction. In most surgical diseases, the patient can have the advantage of consultations with the most talented and experienced of the faculty, but not unfrequently an hour's delay will prove prejudicial to the success of the surgeon and fatal to the prospects of the patient,

It is known to practical surgeons, that the best directed efforts of the taxis, the use of cathartics, opium, bleeding and tobacco injections, not unfrequently fail in relaxing the strictured parts. In view of these difficulties, it has often occurred to me—since the introduction of chloroform as an anæsthetic agent in surgical practice—that the full influence of this article would fulfil the indications above spoken of, and probably dispense with—in the majority of cases—this formidable operation.

A case in point occurred recently, in my practice, where I had an opportunity of testing the correctness of these views. The patient, a middle aged man, had been afflicted with a hernia on the left side for a considerable length of time. Upon one occasion, when employed in wood-chopping, the hernia became strangulated; when I was sent for, and reduced it readily by the taxis. More recently, this man being engaged in the field with his hoe, strangulation again occurred. Upon visiting him I found his suffering to be very intense; pain in the abdomen and stomach, vomiting, &c. The tumor was very large and tender to the touch. The stricture, as is most generally the case in inguinal hernias of long standing, was situated at the external abdominal ring. The taxis was resorted to, and persevered in, but without beneficial results. He was then put under the full effect of chloroform. Relaxation of the external oblique muscle, and the stricture at the ring, was evident to the touch. The taxis was again resorted to, when the gut readily returned with a distinct gurgling sound.

This case is reported for the sole purpose of calling the attention of surgeons to the practical value and use of this most potent of remedies. It is an isolated fact, it is true; but in all cases where relaxation of the voluntary muscles is the object to be obtained—such as luxations, hour-glass contractions of the uterus, &c.—we are strongly convinced that the practitioner's most sanguine hopes will be more than fully realized,

Part Second.

EXCERPTA.

Art. I.—*The Perchloride of Iron.*

The French Medical Press has been and still is occupied with the therapeutic virtues of the perchloride of iron. Some of its advocates give it as good a character as Shakspeare gave to certain medicaments: “I have seen a medicine,” quoth he, “that’s able to breathe life into a stone, quicken a rock,”—curing—“unpleasing blots and sightless stains, foul moles and eye-offending marks—rotten diseases of the South, imposthumes, lime kilns i’ the palm, and the rivelled fee simple of the letter take and take again such preposterous discoveries:

The sovreinst thing on earth
Was parmaceti for an inward bruise.”

M. Pravaz’ remedy falls little short of Will Shakspeare’s in its claims, being adapted to aneurisms, varicose veins, erectile tumors, *nævi materni*, *fistulæ*, ulcers, wounds, cancers, and so on.

Early in January, 1853, M. Lallemand read a paper in the Academy of Sciences upon the new method of curing aneurisms by the injection of a few drops of the concentrated preparation of this substance,—a method discovered by the late M. Pravaz, of Lyons, and tested by MM. Lallemand, Giraldès, Debout, and others.

The fundamental principle of curing aneurism by M. Pravaz’ method, namely, by the coagulating of the blood at the seat of the lesion or aneurismal sac, had been enunciated prior to its demonstration by M. Pravaz: “M. Leroy d’Etoilles published several Memoirs in 1832—5, and 1844, the purport of which was, that aneurism

might be successfully treated by the injection of coagulants." But he who proves, discovers.

It appears, however, that M. Leroy d'Etoilles indignantly disclaims all pretensions to this discovery which he calls "disastrous," in view of its failures, and consecutive effects as inflammations, mortifications and deaths.

M. Pravaz and his friends found upon dividing the large arteries as the carotids of sheep, horses, and other animals, that the injection of from three to ten drops of this solution within the ends of the divided vessels, produced a firm, immovable clot, completely arresting all hæmorrhage in four or five minutes. When the quantity was too large, the clot was dissolved! An excessive quantity caused excessive inflammation with gangrene and sometimes death. The animals used for experiments were killed at various periods reaching to two or three months. The arteries were found to be obliterated above and below the site of the operation.

Much importance is attached to the quantity and still more to the quality and strength of the solution. Three or four drops were found sufficient for an aneurism as large as a pigeon's egg; a larger one, having a larger quantity of blood to be coagulated, requires a greater number of drops, say ten or twelve to the ounce of blood. M. Pravaz used the solution in its most concentrated form, which he injected into the vessel with a small trocar, which is made to enter obliquely, while at the same time the flow of blood in the vessel must be arrested by pressure for four or five minutes until a firm plug is formed in the tube.

M. Leroy d'Etoilles, "thinks that the injections should not be attempted in the aneurismal sac, but in the tube of the artery above. The canula does not always penetrate the concentric clots so as to reach the fluid blood; consequently, the remedy does not act upon the circulating current. The inflammation in so large a part is often excessive; the amount of coagulating fluid must be more than would be required in the normal tube. He thinks that the clots formed may be driven from the sac by the force of the heart's action, and that they may plug up some of the smaller arteries, and hence cause gangrene of the limb; and he thinks this explanation applicable to the cases related by MM. Malgaigne, Alquié, and Jobert."

During the year 1853 quite a number of aneurismal cases were reported in the French journals as having been treated by this method. On the 9th of November, M. Malgaigne, (*Rev. Thér. Méd. Chir.*, No. 23,) one of its strongest opponents, read a paper to the Academy of Medicine on this subject, enumerating eleven cases of aneurism treated with the perchloride. Four died. Two recovered. The residue, most of whom suffered much from the injection, remained uncured. Even the successful cases he regarded as deplorable, because they would lead to further, and, as he viewed the matter, disastrous results.

The veteran Roux—since dead—took a part in these discussions. He looked upon the new method with distrust, and as being full of hazard; he preferred the ligature, which he had applied eighty-four times—having seen gangrene following in only three cases, and secondary hæmorrhage in only five.

More recently, however, the perchloride of iron has gradually gained ground. It has been used more freely, and in a more concentrated form, than had been advised originally by Pravaz. In a case of false-aneurism of the brachial artery, thus treated successfully, the patient subsequently died of another disease; post mortem examination showed an obliteration of the brachial artery, with a dense clot occupying the aneurismal sac.

The formation of a clot in the sac, merely, how firm soever that may be, is probably not the essential point after all. The editor of this journal might, from his own experience in the ligation of arteries for aneurism, show that in case of traumatic aneurism, where the original wound had been completely healed for weeks, the clot proved firm, resembling, under the microscope and to the naked eye, dense, semi-organized membranes, mingled with firm parenchymatous and fibrous masses, not dissolving in water, but susceptible of being unravelled or separated.

M. Roux, though an advocate for the Hunterian method, and opposed to the perchloride injection, thought favorably of the local application of cold, wherewith to produce a clot and a cure. M. Valpeau observed that he had, by a freezing mixture of ice and salt, produced clots in the aneurismal sac, but these dissolved apace and the disease re-appeared. Pravaz' method probably produces not

only a clot in the sac, but one also in the proximal end of the artery; or failing in this, the cure fails. This is doubtless the fundamental, but overlooked point.

M. Giraldès, in his own name and in that of M. Goubaux, read a paper (April 18th, 1854,) in the Imperial Academy of Medicine, giving an account of some experiments performed at Alfort, with the perchloride of iron, from which he concludes that the solution having the strength of 45° to 50° (Baumé's areometer) ought not to be employed in treating aneurism and erectile tumors, on account of the casualties [accidents] that may ensue. He, therefore, recommends 30°, and still better 20°, as the preparation adapted for these cases; 30° to 45° may be employed in aneurismal sacs, (*Kystes hématiques*) and as a modifier of suppurating wounds.

Prof. Judkins, in a communication in the Ohio Medical and Surgical Journal, dated at Paris, Nov. 5th, 1853, says:

The treatment of aneurism, varix, &c., by injections of the perchloride of iron, is receiving a great deal of attention. I have seen its efficacy tested in an aneurism of the bronchial artery, under the care of M. Valpeau, at La Charité: it failed, and he was compelled to ligate the artery: it has failed also, in some of the other hospitals. Before the American Medical Society of Paris, last week, Professor Brainard, of Chicago, stated, that the failure in these cases, was owing to the fact that the canula for conveying the solution into the sac, had never been carried far enough, so that the injection was thrown between the layers of the old coagula, and could have no other effect than that of causing irritation and inflammation. He mentioned the steps of M. Valpeau's operations, and called attention to the fact, that true arterial blood did not follow the introduction of the canula. Prof. B. stated that he had used a solution of the lactate of iron, as injection in vessels, which caused their obliteration—this he reported in 1850. The quantity of the article used at each injection, varied from one to three grains. Recently, in London, he produced a complete cure of a large aneurismal tumor of the orbit by the same remedy. This case is reported in the London Lancet of October last.

In another letter Prof. Judkins says:

The new method of treating aneurisms by injecting into the sac a solution of the perchloride of iron, has, for several months past, engaged the attention of the whole profession in France, and I doubt not, but that a corresponding interest has been felt in other parts of the medical world.

Prof. J. A. Murphy, of Cincinnati, in a letter dated in December,

1853, at Paris, thus writes concerning the proceedings in the Academy of Medicine. M. Le Blanc :

All discoveries have had their dark days, and I do not believe it just to give an unfavorable opinion on this one, after the small number of facts which exist in science. He then gave the history of the question. He brought up the experiments of M. Pravaz upon animals, and the results which followed them; the manner in which this question was brought before the Institute by a communication made by M. Rayer, in the name of M. Lallemand. Then after a just tribute to the paper of M. Malgaigne, he passed to the relation of the experiments which he had undertaken, in connection with M. Debout, on animals, and some of which had been seen by M. Professor Denon Villiers. These experiments had for their principal aim the establishment of the innocuity of perchloride of iron. Now, this innocuity has appeared almost complete, and, excepting a little fever and anorexia, no accident has been noted on the horses experimented on. The dose of the perchloride was injected sufficiently large without producing any unhappy results. M. Le Blanc insisted on the necessity of maintaining for a sufficient length of time, the ligature or the compression above and below the point where the injection is made, if we wish to obtain a firm and adherent clot. Without this precaution, the clot detaches itself and is carried into the circulation. He explains in this manner the rapid disappearance of the clot noted in some experiments, and which have been falsely attributed, he believed, to absorption. M. Le Blanc desired with all his heart, that it was possible to realize the desire expressed by his honorable colleagues, MM. Gerdy and Moreau, to see the perchloride of iron tried, upon animals, in cases of aneurisms. Unhappily that is not possible, since domestic animals are not subject to this disease. Besides, it is difficult to cause artificial aneurisms on beasts. However, he proposed to make some experiments on this point of trying to produce, on animals, aneurisms in the regions that this disease occupies by preference in man. He will try also the action of perchloride of iron in cases of varices, a disease which we meet with sometimes in domestic animals. As to the irritant and caustic properties attributed to the perchloride, we are, in this respect, in a complete error. A solution of the salt of iron may very well become irritant and caustic, but it is when it is old, for then there is formed a certain quantity of perchloric acid. For the rest, it is easy to neutralize this action by adding to the solution, before using it, a little hydrate of peroxide of iron, which destroys the acid.

M. Velpeau's support of this new method of Pravaz has been too equivocal and wavering to please either its friends or its enemies.—
[EDITOR.]

M. Velpeau: we were not ready to judge definitely this question of perchloride of iron. I do not believe that the accidents are inherent to

the method even. The gangrene of the fore-arm which Mr. Malgaigne reported does not seem to me to be the consequence of the injection of the perchloride; there is an unknown cause. There was some inflammatory accidents, in truth, we could fear them a great deal. Pravaz had announced the perchloride as something very irritating. I was forced to employ it in one case. I introduced into the bottom of a vast wound, some boulettes of charpie saturated with perchloride of iron, without any inflammatory accident.

Behold a remedy which coagulates the blood in the vessels; in experimenting, this liquid does not act in an irritating manner, then there is nothing to fear from inflammatory accidents. This liquid coagulates, that is certain. If in coagulating the blood in the vessels, we can arrest the course of the blood, there is reason to experiment with the perchloride of iron on animals, but not on men.

There is a point on which I wish to give an opinion to our young colleagues. It is that the clot has a tendency to adhere. I have seen this in the large veins of horses, in which M. Le Blanc had made some injections of perchloride of iron; there is reason to believe that this clot may become organized in remaining adherent to the vein or can be reabsorbed, if it adheres, a canal more or less large, can be formed below it. But here is a difficulty, whether we inject the perchloride or some other liquid. It is not the same thing to inject an artery, or a healthy vein, or more an aneurismal pouch. Here you have a pouch which is not more clearly organized, or you have some concrete layers not organized. Another difficulty is that the orifice of the artery at the bottom of the aneurism is not always open.—*West. Lancet.*

For the arrestation of secondary hæmorrhages, following amputations, a preparation is recommended having the strength of 45° to 49°.—*Rev. Thér. Méd. Chir., May, 1854.*

It may be proper to state that the friends and enemies of this hæmostatic, in France, have indulged in a spirit of wrangling as to the properties of this article, some contending that the failures of the article to cure are owing to its bad preparation, (*mauvais; très-mal préparé; détestable;*) differing essentially from that of Pravaz; and that M. Dubuisson alone prepares it properly!

M. Malgaigne, however, maintained before the Academy that M. Dubuisson is wholly ignorant of the method of preparing the perchloride of iron: he adds—"M. Dubuisson has said, that the experiments made in Paris were not with his perchloride. It is certain that when in Paris, he placed in the hands of several surgeons the perchloride, which he had of M. Rousseau. Concerning the above, I entertained some doubts, and I enquired of our honorable colleague M. Loubeyran, who informed me that M. Dubuisson understood nothing of the

preparation of the perchloride of iron. M. Gobley, who has been for several years, under the direction of M. Loubeiran, occupied in its preparation, has also given me the same statement. M. Dubuisson has asserted that his concentrated perchloride of iron contains hydrochloric acid, I believe it truly, for it is very badly prepared. It is easy to make a concentrated solution of it, that will contain none of the hydrochloric acid. When well made there is no deposit in it; M. Dubuisson has sent some to the Surgical Society, which forms a deposit, therefore, it is not a good preparation, and yet it was with this bad specimen of the article, that they have operated in Lyons."

Should the remedy come into general use in New Orleans, it is hoped that the apothecaries will see to its purity, density, &c.*

The preparation of the perchloride of iron :

The preparation of the solution has been made public by M. Burin Dubuisson, pharmacien of Lyons, and is described as follows : Take of sulphate of iron of commerce (emerald color,) two pounds ; water, six pounds ; pure iron-filings, the fifth part of a pound ; sulphuric acid, half an ounce : put the whole into an enamelled cast-iron vessel and leave the latter upon the sand-bath until gas is no longer given off ; filter, add to the fluid half a pound of liquid hydro-sulphuric acid, and allow to rest for twelve hours. Put, after that time, the solution upon the fire, boil for half an hour, and filter. To the filtered liquid add six ounces and a half of pure concentrated sulphuric acid ; and place the mixture into an enamelled cast-iron vessel, which should be but half filled. Boil, and add in small quantities, pure nitric acid, until it causes no longer the escape of red fumes. Remove the vessel from the fire, add to the fluid twenty-five or thirty times its weight of cold water, and the whole of the iron will be precipitated in the state of peroxide by the addition of a slight excess of ammonia. Wash the precipitate by decantation with pure water a great many times, and dry it in the air by spreading it in thin layers upon linen.

This dry and pulverized oxide is then calcined to redness in a large and shallow iron vessel, so that the temperature may not rise too high. This is the martial saffron of the shops, which is in fact the peroxide of iron, when prepared as above.

The perchloride of iron is then obtained in the following manner : Take of the peroxide of iron, resulting from the process just described, six ounces and a half ; pure and white hydro-chloric acid, two pounds ; mix and allow the action to go on without fire for five or six

*Dr. Turpin, of New Orleans, recently mentioned to the editor of this journal, that he had been, and still was, using this preparation externally for a congenital erectile tumor, and with some apparent benefit in arresting its ratio of growth.

hours; then put the vessel on a water-bath, and boil until the almost complete solution of the oxide is obtained; this should be done in a porcelain capsule, weighed beforehand. The liquid is decanted to separate the undissolved oxide, and the former is carefully evaporated upon the water-bath, constantly stirring, to the consistency of thick syrup, which is then weighed. Half of this weight of distilled water is then added, the heat is kept up for a few moments, and the whole is thrown on the filter. The capsule and the filter should now be washed with a quantity of water, equal to that used in the last place, and to the first fluid obtained, as much of the second is added to get a mixture of the density of from 43.5 to 44°.

By proceeding in this manner a very limpid fluid is obtained, with a slight acid reaction, but perfectly pure, having reached the maximum of saturation, and always identical. It may be kept without any of the salt being thrown down, provided the bottle be well stopped; the color is dark-brown when the liquid is looked at in full, and of a greenish-gold color when held to the light, or seen in a thin stratum. Five or six drops of this fluid, mixed with the white of an egg diluted with six drachms of water, are sufficient to coagulate the whole into a mass in the space of fifteen seconds. This mass firmly adheres to the bottom of the glass when the latter is turned up, and takes a pretty long time before it slowly drops, when the watery parts begin to run off, as the serum separates from coagulated blood.

At the meeting of the Surgical Society of Paris, held May 4, 1853, M. Debout presented the two carotids of a horse, into which perchloride of iron had been injected. In one of these vessels, for the distance of two inches, the artery being held by the fingers above and below, only six drops of the perchloride had been thrown. The clot formed had, however, been re-dissolved by the current of blood, and carried into the torrent of the circulation. The lining membrane of this vessel was healthy, except a small spot where an abscess was about to form. In the other carotid fifteen drops of the perchloride had been injected for two inches and a half of its length, the upper and lower part of this length of artery being also compressed by the fingers. This vessel remained plugged all the time the animal lived. On a post-mortem examination, the whole length of the clot was found adherent to the parietes of the artery, the lining membrane being the seat of suppurative inflammation. The vessel was quite obliterated both above and below in consequence of adhesive inflammation.

In a subsequent meeting, M. Debout gave a minute description both of the experiment and of the post-mortem appearance, and concluded that M. Pravaz, the discoverer of this property of the perchloride, was right in advising injections of very small doses. M. Debout very properly added, that compression of the artery above the sac (as proposed and carried out by Dr. Bellingham, of Dublin,) should be resorted to as an adjuvant of the injection, as there is a tendency in the blood remaining free in the artery to break down the clot formed by the perchloride.—*Ranking's Abs. Jan. 1854.*

Messrs. Abel and Bloxam, in their excellent Hand-book of Chemistry, (1854,) give the following formula :

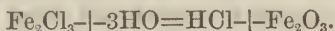
Sesquichloride, or Perchloride of Iron, Fe₂Cl₃.

To obtain this salt in the anhydrous state, coils of iron wire may be introduced into a tube of hard glass, and moderately heated in a pretty abundant supply of chlorine. It then sublimes in brown, crystalline scales, which are deliquescent, and very soluble in water, alcohol, and ether, yielding red-brown solutions.

Hydrated sesquichloride of iron is prepared by dissolving iron in a mixture of hydrochloric and nitric acids, or by treating the sesquioxide with hydrochloric acid. On evaporating the solution, yellow scaly crystals may be obtained, of the formula Fe₂Cl₃-|-5Aq. When these are heated, they evolve hydrochloric acid, and a compound of sesquichloride with sesquioxide of iron remains. A similar compound is deposited as a brown precipitate, when a solution of sesquichloride of iron suffers spontaneous decomposition, or when it is boiled for some time.

The alcoholic and ethereal solutions of this salt are decolorized by exposure to light, (proto-) chloride being formed, together with products of oxidation of alcohol or ether.

Sesquichloride of iron is decomposed by vapor of water at a red-heat, hydrochloric acid and crystallized sesquioxide of iron being produced :—



M. Moissenet (Hôpital de la Salpêtrière) uses the perchloride of iron as a hæmostatic in cancerous affections of the uterus and breast, attended with hæmorrhages. He uses a wash (thirty grammes of the perchloride to two hundred and fifty of water,) for repressing fungous growths, and as a wash or injection in cancerous affections.

Aneurismal cases treated by injections of the perchlorine of iron.

1.—*M. Niepce's Case.*—This case, which was one of popliteal aneurism, was brought before the Académie des Sciences on the 18th of April, ultimo. Five minutes after the injection the pulsation had ceased, and the tumor solidified, and on withdrawing the canula no blood followed. The next day, and the days following, acute inflammation set up in the sac and the surrounding tissue ; and on the 11th day there was distinct fluctuation on its inner side. A small puncture in this place gave exit to about 10 grammes of purulent serosity, with immediate relief to the inflammatory symptoms. Twenty days after the operation, all that remained of the aneurism, was a solid tumor about the size of a nut, and the patient was virtually well.

2.—*M. Lallemand's Case.*—This was a case of varicose aneurism, at the bend of the elbow. It was brought before the Académie des Sciences on the 9th of May. In this case the beatings ceased, and

the tumor solidified immediately after the injection. Acute local inflammation followed, ending in the effusion of a small quantity of sero-purulent fluid, which had to be evacuated by puncture. Eventually a considerable portion of the tumor sloughed away, but there was no hæmorrhage; and the parts healed with rapidity, and the patient got thoroughly well.

3.—*M. Dufour's Case.*—The patient in this case was a tolerably strong and healthy man, with a large aneurism of the right carotid, extending from the chin to the clavicle, and causing great pain and difficulty of breathing. The skin over the tumor was greatly distended and congested, and upon the point of bursting in several places. The solution of perchloride was first injected on the 2d of June, 1853, the sac having been first punctured by a needle. Blood followed the puncture, but this stayed immediately on making the injection. The operation gave rise to great local pains, but the throbbing and fluidity continued a week later; the inflammation in the sac continued, in spite of the cold applications used to repress it, and a dark, sanious fluid oozed from the opening through which the injection had been made. Under these circumstances two grammes of the solution were injected; violent pain and heat followed, as before, but on this occasion there was some evident coagulation in the sac. The inflammatory symptoms, however, progressed in spite of everything that could be done to repress them, and next day a gangrenous patch had formed on the superior and upper part of the tumor. This continued to extend, and three days afterwards the aneurism burst by a large rent, and the patient died instantly, a coagulum of the size of a hen's egg, and apparently the result of the second injection, being extruded at the time. M. Dufour is disposed to think that the operation might have been successful, notwithstanding the unfavorable state of the patient, if the solution had been a little more concentrated, and if it had been injected more liberally.

4.—*Mr. Wm. Adams' Case.*—In this case, which was that of an infant, aged three months, the posterior tibial artery was wounded in the operation of dividing the posterior tibial tendon for club-foot, and a pulsating tumor discovered ten days afterwards. Direct pressure by a graduated compress and bandage was applied, and the pulsation diminished, but a very small slough formed from the pressure not having been relieved for four days, and a copious arterial hæmorrhage took place. Pressure, a little above the aneurism, appeared to command the bleeding, and this was therefore tried; but it was discontinued on the second day, in consequence of the extension of inflammatory swelling to this part of the leg. A second arterial jet followed the removal of pressure.

On the 12th of May, 1852, a small quantity, (from five to ten drops) of the concentrated solution of perchloride of iron (prepared by Mr. Taylor, of Vere street) was injected into the aneurismal sac, which was about an inch in diameter. At the time of the injection,

loosely clotted blood plugged the small cutaneous ulcer, and through the clot the extremity of a long and finely-pointed glass syringe, containing the styptic, was introduced, and carried to the bottom of the sac, as nearly as possible to the situation of the wounded vessel. The femoral artery was compressed before the injection, and for five minutes afterwards, so as to insure the blood acted upon being as nearly as possible in a stagnant condition; a most essential point. The first effect observed was, that the exposed surface of the clot became firmer, and that straw-colored serum oozed from its substance. About twenty minims of serum oozed out in this way, affording conclusive physiological evidence of the firm coagulation of the blood, which was also indicated by a general feeling of hardness over the sac. A piece of lint and bandage were applied.

May 13th.—Ulcer concentrated and filled with firm clots. The surrounding skin, which presented a tense, shining, swollen, and slightly reddened appearance previous to the operation, was now pale, and less tumefied. It seemed that a process of shrinking and contraction had commenced, and that no inflammatory results need be apprehended. Progressive improvement took place; the ulcer healed in a week; shrinking and contraction advance.

May 25th.—A deep puckered cicatrix, and a little deep-seated induration, alone indicated the seat of the aneurism. The treatment of the deformity was proceeded with; the tendo achillis was divided on the 1st of June, and the restoration of the foot was in a few weeks as complete as in other cases.—*Ranking's Abs. Dec.* 1853.

In the New York Journal of Pharmacy, for May, 1854, it is stated that M. J. Ruspini, "a distinguished Italian chemist, has substituted with success the acetate of sesquioxide of iron in place of the perchloride of this metal, in the treatment of hæmorrhages. The experiments of Dr. Pravaz upon the employment of chloride of iron for the cure of aneurism are well known. It appears after several trials of a similar kind by Ruspini, that the acetate of the sesquioxide is far superior to the perchloride as a hæmostatic, and the former salt has the additional advantage of containing an organic acid whose action on the animal economy never produces any unpleasant consequences."

Art. II.—*Chloride of Zinc.—Aneurism Treated by the External Application of Chloride of Zinc:* BY M. BONNET, of Lyons.

M. Bonnet announces the complete cure of a patient suffering from subclavian aneurism, seen by M. Lallemand upon the fourteenth day of the treatment of the disease, by successive applications of the paste

of chloride of zinc. The chloride of zinc, penetrating by imbibition into the deeper parts of the tumor, produced complete coagulation of the blood, which was detached from the walls of the aneurismal cavity without any hæmorrhage. The patient quitted the hospital three months and a half from the commencement of the treatment. He could at that time take a walk for half an hour at a time, and presented, as the only trace of his disease, a superficial ulceration, which was nearly healed. The clot, which came away with the eschar, was forwarded to M. Lallemand by M. Bonnet.

The patient, æt. 25, had received in the left supra-clavicular region a blow with a knife, which, dividing the whole brachial plexus, and touching either the subclavian artery or one of its branches, was followed by profuse hæmorrhage, and subsequently by the formation of an enormous aneurism. The rapid enlargement of the tumor, and the frequent discharge of blood from the wound, threatened the life of the patient, and rendered immediate assistance necessary. The application of a ligature between the inner border of the scaleni muscles and the aorta was rejected, both by M. Bonnet and his colleagues, as too dangerous and too difficult. The galvano-puncture was also negatived, because it was impossible to suspend even for a moment, the pulsations and the *bruit de souffle* in the aneurismal pouch. The injections of the perchloride of iron were not then known (January 2); and, possibly, the enormous size of the aneurism, its proximity to the heart, and the impossibility of compressing the artery which conveyed the blood to it, would have counter-indicated its employment. In this difficult position, M. Bonnet determined upon cauterization by the paste of chloride of zinc.

The first eschar was produced in the centre of the tumor. Every two or three days, Bonnet removed with a bistoury, the superficial layers of the slough, and for five weeks he continued a series of applications of the paste, which penetrated daily both in depth and extent. Upon the fourteenth day, the pulsations and the *bruit de souffle* ceased; at the end of the second month, the eschar began to detach itself without any hæmorrhage. The clot came away with the eschar. Bonnet affirms, that modern authors are wrong in attributing this method of treatment to Ambrose Paré, Guttani, or Marcus Aurelius Severinus; in none of the original works is it mentioned. The ancients knew nothing of the chloride of zinc, which was introduced into practice by M. Canquoin.

Bonnet first thought of employing this escharotic in the treatment of aneurism, by observing, for the last ten years, its power in coagulating blood and arresting hæmorrhage. He found, 1837, that it got rid of varicose veins without the occurrence of the bleeding which frequently ensued after the application of potassa fusa. Both he and his colleagues—MM. Berrier, Bouchacourt, Desgranges and Vallet—have often taken a firm coagulum from vessels exposed to its influence. He has seen it entirely remove hæmorrhoids, varicoceles, and erectile tumors, by penetrating into the vessels by imbibition, coagulating the

blood, and rendering the whole tumor firm. That it arrests arterial as well as venous hæmorrhage was shown by its action upon goitre, as stated by M. Philipeaux, in a memoir founded upon some of Bonnet's clinical lectures.

The eschar separates about eight days after the application of the paste; it can, therefore, be easily retained by successive applications for a month or even longer.

MM. Bonnet and Gensoul have lately proved that the chloride of zinc may cure the disease known as aneurism by anastomosis. A tumor occupying the summit of the head, an inch in thickness and of rounded surface, about half a foot diameter (15 centimetres), consisted of an immense number of anastomosing arteries, pulsating with a loud *bruit de souffle*, and nourished by seven large trunks coming from the frontal, the temporal and the occipital regions, each about the size of the brachial artery. Injection was insufficient and impossible; pressure continued for a year, failed, and the surgeons were talking of tying the carotid arteries. The cauterization by the paste of the chloride of zinc effected the separation of the tumor, without any hæmorrhage whatever from each of the nutritious arteries. The result of the case not yet complete, is to be communicated.—*Ranking's Abs.*, Dec., 1853.

The following formulæ for preparing the paste of the chloride of zinc are given from Prof. Dunglison's New Remedies, 601, Fifth Edition.

Chloride of zinc may be applied as a caustic, by means of a moistened hair pencil, either alone or mixed with an equal portion of oxide of zinc, or sulphate of lime, or according to the following forms:

Pasta zinci chloridi.

Paste of chloride of zinc.

Pâte de Canquoin.

Canquoin's caustic paste.

	A.	B.	C.
R. Zinci chlorid.	p. i.	i.	i.
Farinæ tritici	p. iv.	ij.	ij.
Aquæ fontan.*	q. s.	ut fiat	pasta.
R. Zinci chlorid.	p. j.		
Farinæ tritici	p. iss.		
Antimonii chloridi.	p. ss.		
Aq. font.	q. s.	ut fiat	pasta.

* To each ounce of the chloride of zinc twenty-four to thirty drops of water may be added.

Art. III.—Acetate of Iron, for Aneurism.—Radical Cure of an Aneurism of the External Maxillary Artery, by the Injection of the Acetate of Iron: By DR. FILIPPO LUSSANA, of Milan.

Antecedent to the experiment of Pravaz, a celebrated Italian surgeon, Monteggia, had suggested in the treatment of aneurisms "the injection of astringents into the sac, after entering it with a trocar, in order to obtain a prompt and durable coagulation of the blood, the artery being compressed above the tumor." (See *Istituzioni Chirurgiche*, vol. ii, 2d ed., Milan.) The idea of this operator was never carried out, however, and it remained for the skilful and persevering Pravaz to introduce this great improvement into the practice of surgery. Soon after the researches of Pravaz were made known, and the use of the perchloride of iron in aneurisms had been followed by some disasters, Dr. Ruspini commenced a series of experiments with a view of substituting some less deleterious agent for the perchloride of iron. He finally employed the acetate of the sesquioxide of iron, a salt which contains no principle injurious to the economy, and which possesses great hæmostatic power. This agent had only been employed in cases of slight hæmorrhage and in experiments on animals, when Dr. Lussana determined to employ it in an operation on an aneurism. His experiment resulted most successfully.

M. Gelmi, a girl of twenty-two, presented an aneurismal tumor of the size of a large walnut, between the corner of the mouth and angle of the lower jaw. This tumor was punctured with a small lancet; arterial blood immediately issued in jets. A fine syringe was introduced, and ten drops of the ferruginous solution was injected. The hæmorrhage and pulsations were immediately arrested. The operation caused but little pain and no inflammation succeeded it. In ten days the tumor had sensibly contracted. An exploratory puncture was made in it without causing any hæmorrhage.—*Va. Med. and Surg. Jour.—From Gaz. Méd. de Paris, of Feb. 25th, 1854.*

Art. IV.—General Bloodletting in Insanity.

The American Journal of Insanity, edited by T. Romeyn Beck, M. D., for April, 1854, contains an elaborate article of 118 pages upon "Bloodletting in Mental Disorders," by Pliny Earle, M. D., of New York city, in which the latter analyzes with much ability, one hundred and thirty-six authorities, illustrative of this proposition, namely, "to what extent, in regard to both frequency and quantity, is the abstrac-

tion of blood required in the treatment of insanity ;"—from all of which, he draws the conclusions which follow :

A reply to the proposition at the commencement may now be attempted. It is evident, however, from the very nature of the case, that no positive, definite answer, couched in terms as fixed as the figures representing numbers, can be given. It must be merely approximative. I shall endeavor to convey it in a series of facts, truths or inferences, which I hope are fairly deduced from the substance of the foregoing pages.

1.—Insanity, in any form, is not, of itself, an indication for blood-letting.

2.—On the contrary, its existence is, of itself, a contra-indication. Hence, the person who is insane should, other things being equal, be bled less than one who is not insane.

3.—The *usual* condition of the brain, in mania, is not that of active inflammation, but of a species of excitement, irritability, or irritation, perhaps more frequently resulting from or accompanied by anæmia, debility, or abnormal preponderance of the nervous over the circulatory functions, than in connexion with plethora and enduring vital power.

4.—The excitement, both mental and physical, produced by this irritation, can, in most cases, be permanently subdued, and its radical source removed by other means, more readily than by bleeding.

5.—Yet insanity may be co-existent with conditions,—such as positive plethora, a tendency to apoplexy or paralysis, and sometimes sthenic congestion or inflammation, which call for the abstraction of blood. Therefore,

6.—Venesection in mental disorders should not be absolutely abandoned, although the cases requiring it are very rare.

7.—As a general rule, *topical* is preferable to *general* bleeding.

8.—In many cases where the indication for direct depletion is not urgent, but where bloodletting, particularly if local, might be practiced without injury, it is safer and better to treat by other means, equalizing the circulation and promoting the secretions and excretions.

9.—The physical conditions requiring bloodletting more frequently exist in mania than in any other of the ordinary forms of mental alienation.

10.—Insanity following parturition, other things being equal, is to be treated by bleeding less frequently than that which has its origin in other causes.

11.—If the mental disorder be the direct result of injury to the head, the treatment must be directed to the wound, or its physical effects, not specially to the psychic condition.

12.—In many cases where insanity is accompanied by typhous symptoms, and in some where the aspect is that of acute phrenitis, active stimulants alone can save the patient, and direct depletion from the circulation is almost certainly fatal.

The following extract from the last number of Ranking's Abstract, exhibits the state of opinion on the continent of Europe with respect to venesection as a remedy for insanity, and corroborates the conclusions of Dr. Earle :

Prejudicial Effects of General Bleeding.

Dr. Webster in his report on French Asylums, says:

Although it was not originally intended in the present remarks to discuss the medical treatment usually pursued in French Asylums, one point seems, however, of so much importance that it deserves some notice in these pages; particularly, as great unanimity of opinion prevails amongst the physicians of departmental institutions, with whom I had an opportunity of conversing upon the question. I now refer to the employment of bloodletting as a remedy in cases of insanity. Without an exception, every practitioner was decidedly opposed to the *general* abstraction of blood in maniacal patients, as they consider it not only unnecessary, but often injurious. In many cases venesection produced so much depression, that attacks of mania, which otherwise might have been of short duration, under a different but more judicious mode of treatment, were thereby prolonged, and even ended in fatuity.

Exceptions Requiring Bloodletting.

Of course particular cases of insanity presented themselves where inflammatory symptoms appeared so decided, or in which apoplectic congestion existed to such an extent, that local or general abstraction of blood was then absolutely necessary; nevertheless, these examples were exceptional, and only confirmed still the observations made by the most experienced officers, medical officers of French asylums, respecting the baneful consequences of bloodletting in most cases of mental disease, which came under their cognizance. Indeed, one gentleman remarked "the delirium of insane patients was never modified by frequent and copious bleedings, but often the reverse."

Being supported in these practical conclusions by the opinions of many English physicians, it cannot be too strongly impressed upon the minds of young practitioners, or of those who may not have had much experience in treating cases of insanity, to be always exceedingly chary of using the lancet.

Art. V.—*Intermittent Pneumonia*: By M. CONSTANT, M. D.

Dr. Constant, practising in one of the marshy districts of the department of the Lot, draws attention to the signs which distinguish what he terms intermittent pneumonia, as when they are overlooked the disease proves rapidly fatal.

1. The initial shivering is usually more intense and prolonged than in ordinary pneumonia. 2. The pleuritic pain is felt early and always in front of the chest, although the pulmonary congestion is almost always localized posteriorly. It is much more amenable to blisters than to leeches. 3. Violent cephalalgia is one of the earliest symptoms, being either frontal or sincipital, and it is often accompanied by severe lumbar pain, which observes the same stages of increase and decrease as itself. 4. The shivering is followed by intense heat, which after several hours gives place to abundant sweating. 5. The pulse, during the paroxysm, in place of being full, strong, and vibrating, as in ordinary pneumonia, is rapid, soft, undulant, and compressible. 6. There is never any purulent expectoration, these pneumonias never proceeding beyond the second stage—*i. e.* red hepatization, the pulmonary engorgement being rather a sanguineous congestion than inflammation. 7. Auscultation and percussion are of the highest value, often revealing the disease when unsuspected. A distinctive feature is the rapid passage from the first to the second stage of the disease, so that eight or twelve hours after auscultation had revealed only a slight circumscribed *râle*, a whole side will be found hepatized. Under the influence of large doses of quinia this rapidly disappears, giving way to returning subcrepitant *râle* during the remission of the fever, but returning again during the paroxysm, if this have not been cut short.* 8. The crepitant *râle* of the first stage is almost always moist, the parchment-cracking *râle* only having been heard for a short period, two or three times in more than sixty cases. It invades large surfaces rapidly, being heard posteriorly, sometimes laterally, but never in front. 9. This form of pneumonia especially affects the posterior part of the lower lobes. 10. It especially appears in summer and autumn, while ordinary pneumonia prevails in spring and winter. 11. It attacks all ages indiscriminately, except early infancy. 12. The blood which flows from a vein is often below the normal temperature, very black, and deficient in plasticity. After rest, its surface acquires a bluish color, especially if the patient is taking quinia. The clot is slow in formation, and soft. The buffy coat is absent, or very thin, and inclines to a bluish color. This condition of the blood, conjoined with the soft pulse and rapid hepatization, constitutes the chief distinctive sign of the affection.

In this district, during winter, purely inflammatory pneumonia is met with; but in proportion to the high temperature and the production of malarial emanations, this inflammatory element is replaced by the paludal one. There are, indeed, three forms met with. 1. Simple pneumonia; 2. Spring inflammatory pneumonia, complicated with the in-

* The editor of the N. O. Medical and Surgical Journal recollects a case of congestive intermittent from the Coast, (the written history of which might somewhere be found in his XXII vols. MSS.,) in which the dullness of hepatization was observed before death; but the post mortem examination about two days after percussion, revealed no lesion of the lungs—owing it is supposed to one of two causes: either the re-actionary fever before death, or the persistence of capillary and calorific action after death, had removed the pulmonary congestion.

The judicious reader will appreciate the significant facts re-produced in this article from Drs. C. and A.

termittent paroxysm. 3. Summer and autumn intermittent pneumonia. The first requires bleeding and antimony; the second antiphlogistic treatment with quinia, given either simultaneously or subsequently; and the third, quinia in combination with external revulsives. These forms may still undergo further admixture, according as the inflammatory or paludal element prevails, requiring appropriate modification in the treatment.—*Ranking's Abs.*, Dec. 1853.

Periodicity of Pneumonia.

L. H. Anderson, M. D., in his report to the last annual meeting of the Medical Association of Alabama, upon the diseases of Sumterville and its vicinity, arrives at the following conclusions concerning Pneumonia:

1.—The pneumonia of South Alabama is probably less severe than the same disease in more northern latitudes; and the intermittent or remittent fever accompanying it is more dangerous than the pulmonary inflammation.

2.—The treatment should be directed chiefly against the malarious element of the disease. Quinine should be freely given for its anti-periodic effect, and mercury for its influence on the secretions, particularly that of the liver.

3.—An exception to this pathology and treatment is to be made when the attending fever is of a typhoid character. Here the dothi-enterite is the more important disease, and the pneumonia should be treated with strict reference to it.

* * * I became convinced, from the decided, though often obscure periodicity that takes place in the disease, as it appears with us, (and perhaps in all miasmatic districts,) *that the remittent fever which accompanies it is the true source of danger, and that the fever being relieved, the pneumonia will spontaneously amend.* This conviction gradually growing upon me, I diminished, in each successive case, the quantity of tartar emetic and mercury given, and increased that of the quinine, till at last I came to make it the chief remedy, and used the others merely as adjuvants.

* * * There is usually no chill after the first one, or if any it is so slight as not to be noticed, and the fever is commonly not of a very high grade. The patient getting no better, however, a physician is sent for about the fourth or fifth day. He then finds the disease fully formed, the pulse frequent, small and tense, fixed pain in the thorax a troublesome cough, with a viscid expectoration colored with blood, tongue foul, and red at the edges, perhaps dry, the stomach nauseated, and the bowels either constipated, or, if purgatives have been given, discharging watery and brownish evacuations. The percussion will be dull or flat on one side of the chest, or a part of it, and the respiration crepitant, absent, or mucous, according to the stage of the inflammation. We have here to contend with a remittent fever, with gastro-intestinal disturbance, complicated with inflammation of the lung, more or less

extensive. We cannot cure such a case by a *coup de main*, but finding a number of organs implicated, must treat the affection of each, with reference to all others. Quinine alone is inadequate to the cure, but it should be liberally given during any period of remission that may be observed. At the same time, moderate mercurials, combined with laxatives or opiates, as the condition of the bowels may demand, so as to rectify their secretions, should be given. Cups, fomentations, or blisters should be applied to the chest, and syrup of squills, alkaline drinks and tartar emetic in dilute solution, prescribed as the judgment of the practitioner may indicate.

Dr. Anderson deviates from "the anti-malarious treatment" of pneumonia whenever the accompanying fever is of a typhoid character,

Dr. A. gives the following case as an illustration of his treatment of Southern pneumonia :

A servant of my own, aged 50, of good constitution, and having generally the best health, was taken 15th April, 1852, about 8 A. M., with a chill of an hour's duration, succeeded by smart fever, pain in the head and side, cough, and tenacious rust-colored expectoration. He had had an ordinary cold and cough for several days, which did not prevent him from attending to his usual duties, and I had seen him early in the morning of the day he took the chill, and given him some directions as to what I wanted done, I then rode away from home, and returning about midday, found him confined to bed, and complaining as above mentioned. The pulse being tense, and the pain in the head and side considerable, I bled him about eight ounces, and cupped him on the affected side, with some feeling of general relief. I then gave him 15 grs. quinine, with a laxative. Four hours after I gave him eight grains quinine additional, and at night, finding the cough abated, the expectoration less tinged with blood, and the pulse slower I repeated the dose. He passed a tolerable night, and in the morning there was scarcely a vestige of the pneumonia, save the cough, remaining. He got eight grains more of quinine, had no further chill or fever, and in a few days was up again. The case seemed to me at first a very threatening one, but seeing it so soon after the seizure, I determined to give the quinine very liberally, and to rely upon it almost wholly for the cure.

Art. VI.—*Small Pox—Vaccination—Re-vaccination.*

In a pamphlet by Joseph C. Hutchison, M. D., physician to the Brooklyn City Dispensary of New York, reprinted from the New York Journal of Medicine, upon "vaccination, and the causes of the prevalence of small pox in New York, 1853—4," it is shown that in

this city during five years 2,396 have died of the above named maldy—681 of whom perished in 1853: Dr. H. thus sums up the results of his researches:

From the materials presented in the preceding pages, and from other sources, the following conclusions may be legitimately deduced:

1.—That the recent extensive prevalence of small-pox in New-York may be, to a great extent, attributed to the *neglect of vaccination*.

2.—*To imperfect vaccination*, which may occur: (1.) From not observing the regular progress of the vaccine disease. (2.) From a want of proper regard to the state of the recipient. (3.) From injuring the vesicle. (4.) Performing the operation at too early an age. (5.) Using lymph taken at an improper stage of the disease. (6.) Peculiarity of the temperament of the recipient. (7.) Vaccinating with lymph deteriorated by age, or any of the above causes.

3.—To epidemic influence.

4.—To a neglect of re-vaccination.

On Small-pox and Vaccination; an Analytical Examination of all the Cases admitted, during Sixteen Years, at the Small-pox and Vaccine Hospital, London, with a view to illustrate the Protective Influence of Vaccination: BY F. MARSON, Resident Surgeon at the Small-pox Hospital.

The author concludes as follows:

1.—That natural small-pox destroyed about one-third of all whom it attacked.

2.—That small-pox after small-pox was of comparatively rare occurrence; that a second attack of natural small-pox was rare, but not often fatal, and that protection seemed to be the law. That after inoculated small-pox an attack of small-pox had more frequently led to fatal results; but there is reason to presume that the virus used for inoculation, like a great deal of the lymph used at the present day for vaccination, was often taken at too advanced a period of the disease, and thus did not afford the full measure of protection it was capable of affording if taken at a proper time.

3.—That vaccination performed in infancy afforded almost complete protection against the fatality of small-pox to the period of puberty; that a variety of circumstances conspired to make it almost impossible to ascertain exactly in what proportion to the vaccinated cases of small-pox subsequently occurred, or might occur, if all persons lived to an advanced age.

4.—That, as a matter of safety, it would be well for all persons who were vaccinated in infancy to be re-vaccinated at puberty; this measure being more especially requisite for those who were either indifferently or doubtfully vaccinated in infancy, and still more necessary for those who, though vaccinated, had no cicatrix remaining. Finally, as a matter of precaution, it would be desirable that all persons

should be re-vaccinated on small-pox existing in the house where they were residing—a precaution, however, that will cease to be necessary to advise when all persons have the benefit of proper and efficient vaccination.—*Ranking's Abst.*

The Results of Re-vaccination, as observed in two hundred and fifty-seven Cases: BY W. B. HERAPATH, M. D., Surgeon to St. Peter's Hospital, Bristol.

Dr. Herapath arrived at the following results upon the occasion of an extensive re-vaccination amongst the children of two large public schools in Bristol—the Red Maid's School and Queen Elizabeth's Hospital. All the children had had small-pox or cow-pox before admission.

1.—That three cases re-vaccinated within seven years were not again susceptible to vaccine.

2.—That vaccine, after the interval of from eight to seventeen years, does not prevent the reception of vaccine again, except in 22-174 per cent.

3.—That the distinctness or imperfection of the vaccine cicatrix does not materially alter these results.

4.—That variola does not prevent the formation of the vaccine vesicle, except in about 23-53 per cent.

5.—That the occurrence of small-pox subsequently to vaccination does not destroy the susceptibility of the human system to again receive the vaccine poison except in about ten per cent.

6.—That in all the previous cases, whenever the secondary vaccine vesicle assumed its perfect form, its subsequent history was the same as if the system had not previously labored under vaccine variola or varioloid.

7.—It is probable that the protective influence of vaccination has diminished in consequence of repeated transmission of the vaccine matter through the human body.

8.—It is desirable that re-vaccination should be extensively followed, as one means of giving additional protection to the masses.

9.—That when possible, the stock of vaccine should be renewed by going back to the original source.—*Ranking's Abst.*

Art. VII.—*Quinine in Cholera.*

The Editor of this Journal, has, for years used quinine in connection with morphia, laudanum, brandy, &c., by both the mouth and rectum with satisfactory results in the treatment of cholera. It may be given as an injection and retained in many cases, if but a small quantity of liquid be used.

From Ranking's Abstract, (Jan. 1854,) the following notice of the treatment of cholera by quinine, &c., is taken :

The Editor of the Medical Times and Gazette observes, that the lesson we may learn from the experience of the past, is not to persevere in modes of treatment which have proved useless. He remarks: Of all the remedies used during the previous epidemics, with the exception perhaps of opium as a palliative in certain stages, quinine probably is most deserving of a further trial. Modern experience in India is leading to increasing confidence in its powers. Towards the close of the epidemic of 1849, it was used in London. Mr. Spencer Wells injected a solution into the veins in four cases. Dr. Parkes did so in two others. He also injected the solution of salicine without any marked effect, besides alcohol, both in simple warm water and in saline solutions. The experiments were only made in desperate cases, and no recoveries followed, but the power and duration of the re-action which took place were very remarkable, and, as phlebitis only followed in one case, encouragement is afforded for a repetition of the trials in cases not so absolutely hopeless as those in which they were formerly made.—*Med. Times and Gaz.*, Oct. 8, 1853.

Art. VIII.—M. VELPEAU'S *Opinions of the Value of the Microscope in Cancerous Tumours of the Breast.*

In the work lately published "On the Diseases of the Breast," M. Velpeau expresses himself touching the value of the microscope for the diagnosis of cancerous tumours of the breast as follows :

If by means of the microscope it were possible to arrive at a knowledge of the intimate nature of pathological products, the instrument would be of great practical value. The diagnosis of cancer would acquire much certainty, and lose its vague and unsatisfactory nature, as nothing is more easy than to obtain small fragments of cancerous tumours by means of the grooved needle, &c. Some microscopists of the present time do not hesitate to believe in these wondrous results, but my remarks on the cancerous cell, in another part of the work, will prevent my readers from crediting such things. Microscopical examinations may determine the nature of cancerous tumours, when the latter are removed from the body ; but, in a clinical point of view, these examinations lead, in the present state of knowledge, to dangerous errors, if any importance were attached to them. When, by the bed-side of the patient, the microscopist declares that the cell submitted to his examination is of a cancerous kind, can the surgeon take such a declaration as a rule of conduct ? Will any one form a decided opinion upon so uncertain a testimony ? But this is not all : suppose even the cancerous cell were the fundamental element, the *sine qua non* of a cancerous tumour,

who would venture to say that that cell is not to be found in a tumour just examined, merely because it was not discovered in the fragment placed under the field of the microscope? Is it not possible that the grooved needle, though thrust with much care into the suspected texture, may only bring to light non-malignant particles, though the tumours may in reality contain many cancerous cells; a cancerous tumour is, after all, composed of different elements—viz., cellular tissue, fat, vessels, and sometimes hardly altered mammary texture. The most skillful microscopists agree that the whole of a tumour should be examined and that its different layers and lobules should be carefully studied, before a positive opinion can be given. Thus, to be quite sure that a tumour does not contain cancerous cells, must not the former be broken up *ad infinitum*, and every particle be placed under the field of the microscope? I may then say of the microscope what I said of pain, “it throws no light upon pathology, and gives rise to much doubt and uncertainty, especially where a solution is most needed—viz., in the first stage of cancerous tumours. In fact, I do not think that it is possible to diagnose tumours of the breast better with the microscope than by the ordinary symptoms and clinical observations.”—*London Lancet*. May, 1854.

Art. IX.—*Yellow Fever of Mobile, in 1853; its Causes, Cure, &c.*

Wm. H. Anderson, M. D., presented to the Medical Association of Alabama, which held its seventh annual session (Jan. 10th, 1854,) at Montgomery, a report on the yellow fever of Mobile, of 1853; in which he asserts that this disease, in that city, “was as contagious as the small pox.” This postulate he attempts to sustain in the following statement, which is given in extenso: “Within our own experience we could enumerate more than a dozen families who were isolated from the disease, who escaped till late in the season, and who took it within a few days after some transient person came into the family, either sick with the fever or on the eve of being down with it. If such cases occurred only in one or two families, we might pause and doubt, but they happened so often, and under such circumstances, that we cannot attribute them to anything else but absolute contagion. We have heard the argument of others, whose capacity for observation we have the highest respect for, but they have failed to convince us that the fever did not spread in many families by contagion. The history of the disease at Spring Hill, at Fulton, at Citronville, and up

in the pine woods, eight, twelve and twenty miles from the city, is to us conclusive evidence. A family living eight miles from Mobile, in a healthy pine country, has no communication with the city, and remains in perfect health until the middle of October. About this time a stranger arrives, just from the city, he is seized with the fever, and in a week from that time four or five of the family are taken down, and half the number dead with black vomit. Would it not require a logic more subtle than any yet practised, to convince us that that fever was not communicated by contagion. The limits of this report will not permit us to dwell longer on this subject. We have, throughout, studiously avoided theorizing, and attempted to state facts as briefly as possible. Were we writing an essay on yellow fever, we would go over much ground that we have not touched upon, but the object of this paper is to be as short and practical as possible."

Nevertheless, Dr. Anderson holds the following language with regard to the cause of the epidemic at Mobile:

With regard to its *cause*—we must candidly state that we are at an utter loss to know to what cause to attribute the past epidemic among us. It made its advent when the air was clear and serene, when the breezes were soft and pleasant, when the temperature was agreeable—and, indeed, when everything gave promise of a delightful summer and autumn. It raged with equal violence in the sunshine and the shower; and left, only, when there was no longer any food for it to ravage on.

We have purposely avoided entering into any dissertation with regard to the particular system on which the poison spent its violence. Volumes have been written upon this subject, and yet we are completely in the dark as to whether the nervous or sanguiferous system becomes the receptacle and bears the brunt of the poisonous influence. In malignant cases, *both* are profoundly altered, but which is the *first* in the category, we know with no more certainty than we do whether cryptogamia, or animalculæ, or electric influences, are the destructive agents which cause the disease. We cheerfully consign all such dissertations to those who are fond of mystery and speculation.

During the months of April, May, June, and July, Mobile was quite healthy, much more so than it generally is at that season. The month of August, at its commencement, offered nothing unusual in the way of disease, of temperature or of atmospheric phenomena of any kind. There were a few cases of bilious remittent fever, mild in their type, and yielding readily to the usual treatment. So far as we could ascertain, there was no appearance of malignant symptoms in any case, and it was a matter of general remark that the fevers yielded very promptly to remedial measures.

Early in this month, however, the pestilence which had ravaged New-Orleans and almost decimated its population, came upon us. Before its advent, we had expected it, and knowing the constant intercourse between the two cities, had been much surprised that we so long escaped. The daily reports from our sister city had raised the public mind to the highest pitch of excitement, and the actual appearance of a dozen cases among us caused the panic-stricken inhabitants to fly in every direction. The first eight or ten cases that occurred in Mobile were among persons who came from New-Orleans, and so far as your reporter could ascertain, the majority of them proved fatal. In about ten days after these cases occurred, others appeared simultaneously in different parts of the city, and some of them wore from the commencement a very malignant aspect. In this particular, as in many others, this epidemic differed from those of previous years. As a general rule, the yellow fever in Mobile, commenced in one portion of the city and extended itself slowly towards other parts, so that there was ample time for the inhabitants of distant districts to fly unharmed from the plague. Within three days, however, from the date of the first case *originating* here this season, there were perhaps fifteen or twenty cases of genuine yellow fever under treatment, and many of these were remote from each other, as if the poison had located itself in some eight or ten different centres, from which to radiate until every neighborhood was completely infected. The tables of mortality will show that this radiation went on with fearful rapidity. How many persons fled it is impossible to ascertain, but it is supposed that in three weeks, not more than ten or twelve thousand were left in the city, and of this number it is probable that four or five thousand were attacked in less than a month after the disease appeared.

The large majority, perhaps nine-tenths of the first cases, were among the poor and destitute—those who lived in crowded and badly-ventilated apartments, and who were necessarily exposed to the sun and the night-air. But in the course of a fortnight or three weeks all classes were seized with the infection, and many of the most malignant cases occurred among the better classes of the population, who, unable to leave, had taken all precautionary measures that prudence could dictate.

Having never been professionally engaged in an epidemic of yellow fever before the past summer, your reporter can draw no parallel from experience between the late and previous epidemics, in many important phases of the disease. With regard to the frequency of hæmorrhage for instance, it would be interesting to know what relation the late epidemic bore to previous ones. Some of our most experienced practitioners are of opinion, that the hæmorrhages during the past summer were less frequent than in former years. Be this as it may, it is quite certain that a large number of cases suffered from hæmorrhage, either from the nose, the gums, the bladder, or the bowels. Most generally the oozing took place from the gums, and although this symptom was always indicative of a malignant attack, yet still it

was not regarded as highly unfavorable; on the contrary, when the oozing from the gums supervened on the fourth or fifth day, it seemed often to take the place of the black vomit, and to inspire some hope of the recovery of the patient, provided it was not very profuse. Hæmorrhages from the bowels were more unfavorable than those from the gums—at least they were so regarded by many practitioners. Little has been said about the appearance of the tongue in this report, for it was so variable, and of so little importance as a diagnostic sign, that it is scarcely worth dwelling upon. In many cases this organ was clean, or nearly so throughout the attack; in others it was furred, white, brown, dry or moist, without any of these states being particularly indicative of the state of the stomach and alimentary canal.

Dr. Anderson, while "giving his testimony in favor of the quinine treatment," seems to ignore the other very powerful agents always given along with the quinine. Thus, when twenty or twenty-five grains of quinine were given, from fifteen to twenty of calomel bore it company.

With regard to the treatment of yellow fever, it was in Mobile very variable, and it is difficult to say which plan was the most successful. In the most malignant cases, it is our belief that no treatment was available, and if such cases recovered at all, it is doubtful whether medicine had anything to do with hastening the happy event. A case, for instance, which, if let alone, would have proved fatal in forty-eight or sixty hours, was beyond the reach of medical aid. The patient seemed doomed from the commencement of the attack. If, however, the case would have lasted four or five days, there was more time for medicine to act, and such cases were often brought under the control of medicine on the third day, and cured on the fifth or sixth.

Every practitioner, of course, lauded his own peculiar mode of treatment, and thought it most successful. With due deference to the opinion of several highly respectable physicians, who opposed what is called the quinine treatment, we must give our testimony as decidedly in its favor. This treatment was much used in this city. Your reporter and his associate in practice being much prejudiced in its favor, from the experience of former years, used it from the very commencement of the epidemic to its close. They treated upwards of eleven hundred cases, and without claiming any superiority in success, they feel sure that their tables of mortality will compare favorably with those of any other physicians in the city. They used quinine in almost every case, regardless of age, sex, idiosyncrasy, or any other circumstance. They have every reason to be pleased with their manner of treatment, and with their present experience, would not exchange it for any other that they have heard of. The marked and almost magic effect of a large dose of quinine at the outset was so apparent, that they would have considered it little short of trifling with human life to have adopted any other treatment. They will not deny that

there were cases in which it did no good; in fact, in those cases where there was at the commencement decided congestion of the brain, it may sometimes have done harm, but such cases were very few, and could hardly have been aggravated by any medicine that could be given.

What was the *modus operandi* of the quinine is not so satisfactory. It may have been a powerful sedative, undoubtedly it was in many instances. It may also have had the effect of annihilating the poisonous influence that was so fast hurrying on the blood of the victim to a state of decomposition, and depressing the vital power. It may, like mercury in syphilis, have substituted one poison for another. We cannot tell *how* it acted, but we are sure, from experience, that its action was favorable in the highest degree. Many patients can be cited who could not use quinine in ordinary intermittents without great trouble in the nervous system, but who, in yellow fever, could take with impunity five-and-twenty grains without experiencing any other effect except a temporary deafness of a few hours.

It must not be inferred that quinine was the only powerful remedy given. We never trusted to it alone, but always gave it combined with calomel or blue mass. The usual prescription was as follows:—*R.*: Quinæ sulph. grs. xx. or xxv.; sub. muriat. hydr. grs. xv. or xx.: *M.* ft. pulv. and give at one dose, in a little syrup.

If this was rejected, repeat it immediately. In the course of twelve hours this dose was repeated, minus five grs. of the quinine and five or ten of the calomel. If the second dose produced decidedly bilious evacuations, the following prescription was given on the second day:—*R.*: Quinæ sulph. grs. xxv.; sub. muriat. hydr. grs. x.; mass. hyd. grs. xv. *M.*: make into pills No. x.: give three every three hours.

Rhubarb and magnesia either singly or combined, were administered, if the calomel did not act of itself. A large blister, six by eight inches, was applied at the onset to the epigastrium. This was considered as highly important, both to allay the nausea and vomiting, and to excite the liver to action. If this organ could not be brought into action by the third day, the plan pursued was to salivate the patient as speedily as possible by small doses of calomel every hour or two. To meet the symptoms of nausea, many prescriptions were resorted to, and among them the phosphate of lime (one drachm dissolved in six ounces of water,) a few drops of lemon juice in a tablespoonful of pure sweet oil, and sometimes a little morphine, deserve particular notice. We must confess, however, that the morphine was not given if it was possible to do without it. While on this subject, it may be remarked, that the quinine itself had a powerful influence in allaying nausea and vomiting. Very often, on visiting a patient the first time, he would be found with distressing sick stomach, which was immediately relieved by a large dose of quinine and calomel. In some inveterate cases, however, the symptom could not be allayed, and it was quieted only by death itself. The tendency to hæmorrhage from the gums was often checked by solutions of tannin and creosote, alum,

and other astringents used as a mouth wash ; and sometimes tannin and creosote were administered internally, but we think with doubtful effect.

In a large proportion of yellow fever cases, there was a period in the disease when stimulants were indicated, and very highly serviceable. This period came on generally about the close of the third day, and it was accompanied with a slow gaseous pulse, a feeling of anxiety, a disposition to wakefulness, a tendency to sigh and to take deep inspirations, and a feeling of general sinking of the system. All kinds of stimulants were resorted to, but those of a gaseous nature seemed to fulfill the indication best, and to be most grateful to the patient. Ale and porter were freely used, and champagne often became necessary when the former were too heavy, and disagreed with the stomach. Iced champagne was highly grateful to the patient, &c.

Art. X.—*Yellow Fever of Selma, Alabama, in 1853.*

J. C. Marks, M. D., in his report on the diseases of Selma, during the year 1853, gives an account of the prevalence of the yellow fever in that town.—*From Trans. Ala. Med. Association.*

July and August were proverbial for a freedom from almost all diseases. The weather continued hot and dry until near the end of August, when a change took place, and we had abundant rains for three or four weeks. In July there were a few intermittents, and one or two cases of bilious fever. The last month in the third quarter, September, was noted for the appearance of yellow fever in our city ; and we have now to turn from the pleasant contemplation of almost uninterrupted health, afforded us by the first two months, and record the destructive career of that fell disease—note its debut upon our stage, its tragic performance of death and desolation, which for weeks hung around us the habiliments of grief and mourning.

Selma having been heretofore exempt from an epidemic of yellow fever, although the disease had been twice introduced here on former occasions, its approach in the present instance was little expected and little feared. Its ravages, though uncompromising in other places around us, brought no misgivings to the minds of our citizens, who, lulled to sleep in fancied security, dreamed on of the continued reign of hygeia and exemption from the dread destroyer.

The first case of unmistakable yellow fever occurred about the 17th of that month. The subject of it resided near the river, but had not been to Mobile, or any place where the disease prevailed, and at that time no case had been brought here by the boats. Its course was rapid and fatal. No other well marked case was witnessed until the 7th of October.

On the 8th, other cases of yellow fever occurred, and on the 9th still others. On the 10th, the first and only case brought here by the boats was landed, and died in a few hours. On the 14th there were two deaths from the disease, and a number of new cases, presenting the characteristic symptoms.

It had now evidently assumed the form of an epidemic, and many of our citizens becoming alarmed, quickly fled the place, leaving us, in two or three days, with only some thousand or twelve hundred persons, out of a population of three or four thousand. The disease continued to rage with fearful violence for about two weeks, attacking nearly all classes of our inhabitants, irrespective of condition, age or sex: the negro, however, seemed most exempt, not more than two or three cases occurring among that variety of our population.

On the 26th of the month we were favored with cool and frosty weather, and the frightful career of the epidemic was immediately arrested. It is true we had several cases afterwards, among those who had been exposed previously to the morbid influence, yet there was a marked decline in the number of cases from the above date, and on the 13th of November the disease entirely disappeared.

So extensive was its prevalence here, that few families indeed, who remained in town, escaped wholly a seizure among their members, and in a few instances two or three in a family were attacked; still, when we regard the number of victims, we discover the mortality to be less than we had supposed, and, if we have been correctly informed, will not compare with that witnessed in many other places this year. It has been estimated with considerable precision, that about one hundred and twenty cases occurred in the city during the epidemic, of which some thirty-two terminated in death, which makes the per centum of mortality only about one in four.

During the whole course of the disease there were manifested two forms, or types—one violent, and to some extent unmanageable, the other mild; the former predominated in the beginning, the latter in the decline of the epidemic.

There was nothing in the history of the epidemic that could, by the most favored construction, be made to support the opinion of the contagionists, but, on the contrary, everything was presented that could be desired to confute and overthrow it.

Treatment.—The remedies employed by us in the management of this fearful malady were such as seemed best calculated to overcome the state of congestion upon which it was conceived to depend. We had, indeed, but one general indication presented to us; but, inasmuch as there were different types of the fever, that is, different degrees of this congestion, it became necessary that there should be a corresponding difference of degree in the activity of the treatment resorted to; and we may here observe that we believe the great discrepancy of opinion which has long existed amongst medical men, with respect to the most judicious means of combatting the disease, has been owing to the want of settled notions of its pathology, and especially the

failure to recognize the presence and importance of this invariable condition of congestion. The name of the disease has, as a consequence, been mainly treated, and as the type of the fever varied, that is, degrees of the pathological state, congestion were presented at different places, and at the same place at different seasons or years, and even in the same epidemic, there must, of necessity, from the fluctuations attending the success of every proposed measure, have been the most conflicting and variously empirical treatment employed; while, in reality, these alternately praised and condemned plans of management are equally certain and reliable—equally to be depended upon, when the condition of the disease is recognized, and they are addressed to the degrees or prevailing type of that condition to which each is suited.

We have said that there were two forms of the fever presented to our notice—one violent, the other mild. In the first we sought the removal of the extensive and dangerous congestion by the following measures:

Cathartics.—These were employed usually in the beginning of the attack, and were carried only so far as served for the free and complete actions of the bowels, the evacuation of their acrid matters, and a judicious depletion of the congested vessels. We commonly give a full dose of calomel and rhubarb, or colocynth, to accomplish this purpose.

The Warm Bath.—This in the outset of the disease, provided the febrile action was not already so high as to be above the secreting point, when of course it would but act deleteriously, we regarded of the first moment. It was prescribed both locally and generally, with the object of inducing free diaphoresis, and a salutary determination to the surface, in which we frequently succeeded. Its use, however, was not persisted in after the first thirty-six hours, because its relaxing and debilitating effects then rendered it hazardous.

Bloodletting.—Venesection exerted the happiest effects in every case, whether employed by myself or others, in which its use was restricted to the form of the disease under consideration, and the robust constitution. Indeed, we know of no remedy whose immediate and necessary influence over the essential pathological state better fits it for such cases, especially in the early stages; and we confess ourselves at a loss to conceive how such a weapon of successful conflict with so terrible a disease could have fallen into disrepute, save only from an indiscriminate resort to it, in mild or low forms, and weak and enfeebled constitutions, or the failure to follow up and perpetuate its good effects over the congestion, by the prompt and judicious exhibition of the *great remedy*, whose action in turn it so much favors, and of which we shall now speak.

Mercury.—In some form mercurials, of all the means used, were deemed the most reliable, and they were prescribed in every case of violence.

We generally commenced the mercurial treatment, for its specific

effects soon after the operation of our purgative, or the employment of other preparatory measures, and by the exhibition of calomel in small and divided doses. The quantity prescribed was usually about two grains, combined with from two to three grains of quinia and one-eighth grain of morphia, which was repeated every two or three hours, as circumstances seemed to require.

Art. XI.—*Typhoid Fever.*—*Typhoid Fever of Alabama.*—(From Dr. L. H. ANDERSON'S Report, Trans. Ala. Med. Ass.)

The average age of the subjects was about 20, and the average duration of the attacks about thirty-five days; the shortest about twenty, the longest about fifty days. About two-thirds of the cases were blacks, and three-fourths females. The whole number of cases in the vicinity was about sixty-five, of whom twelve died. Two of these cases were not seen by your reporter at all—three, in consultation: of the other seven, three only were under his entire management. One of these died, after apparent convalescence had set in, of intussusception; autopsy revealing but two small ulcers near the caput coli, nearly healed. Another was worn out by suppurative discharge from the uterus, twenty days after the typhoid symptoms had disappeared; the third would probably have recovered, if she could have been kept free from the excitement of company.

Post Mortem Appearances.—I have made but three autopsies, the notes of which I beg leave to present.

Case 1.—* * * aged 12, black, slave of Mr. Asa Amason, died 21st June, 1853, of typhoid fever, as was supposed. I had not seen the girl during the illness, but being called upon by her owner to take charge of a number of others who were ill with the same fever, I insisted on making an autopsy as a preliminary. She had been ill about four weeks, had taken several doses of calomel, and a good deal of quinine; bowels had been alternately constipated and relaxed, and purgatives and opium had been prescribed accordingly; no hæmorrhage except free bleeding from the nose ten days before death.

Examined twelve hours after death. Emaciation—abdomen distended. A diagonal incision from the left hypochondrium to the right ileum, sufficiently exposed the parts I wished to examine.

Stomach healthy; lower part of duodenum and jejunum slightly injected.

Ileum intensely injected on its outer surface, and exhibiting many dark and purple patches, some almost black. Numerous ulcerations of Peyer's patches and the solitary glands, some almost perforating the intestines; more abundant near the valve.

Colon intensely red without and within, especially near the caput,

several ulcerations in it. It becomes almost healthy near the rectum. Contains very bilious looking fæces.

Peritoneum highly inflamed, about three pints of yellowish serum in its cavity; the inflammation seemed greatest under the parts covered by the blister, which had been applied twenty-four hours before death. Pelvis half full of pus, apparently from softening of the mesenteric glands.

Spleen four times the natural size, not appreciably softened.

Liver healthy, except peritoneal coat: juicy with bile; gall bladder full of clear, healthy looking bile: bile in duodenum.

Note.—In this case there is little doubt that the ulcerated patches were much irritated by the acrid bile so abundantly drawn from the liver by the calomel. They were no doubt aggravated by the blister also.

Case 2.—Hannah, black, slave of same. Aged 20, stout, and of fine constitution. Had been ill thirty-six days. Fever grave at the first, but bowels not deranged. Improved so much in fifteen or eighteen days as to be able to go about the house, though still perhaps feverish. It was supposed she got to an apple tree in the yard, and ate some of the unripe fruit. Was taken down again with fever; bowels distended and painful; some headache. Cups and fomentations were applied to the abdomen, followed by slight pustulation with tartar emetic; under which treatment the pain and tumefaction subsided. The pulse was generally seventy-six. In a few days the bowels became loose, and the discharges yellow and watery. They were restrained by acetate of lead injections, and sulphate zinc given internally, after which the bowels became costive, and were opened daily by enema, the fæces having nearly a healthy appearance. She got the saline solution four times a day, with twenty-five drops oil turpentine morning, noon and night, with buttermilk and water for diet. The fever appearing to be periodically remittent about the twenty-fourth day, quinine and salicine were given freely to break up the periodicity. No permanent benefit resulted from it. About the thirtieth day, she seemed better, tongue moist and clean, some appetite, and pulse less frequent. In a day or two, however, she began to suffer with great distress in the abdomen; retention of urine, requiring the catheter; and she died on the thirty-sixth day.

Autopsy three hours after death. Great emaciation; abdomen flat and shrunken; large ulcer on sacrum. *Head* not examined. *Larynx, trachea, bronchia* and *œsophagus* pale and bloodless.

Lungs not examined. No doubt healthy, as there were no pectoral symptoms before death.

Stomach full of amber colored fluid, and the broth, wine, &c., she had taken for several days before death. Mucous membrane pale and healthy; duodenum pale; jejunum also pale: *it has five intussusceptions; one, eight inches long; one five; one, three; two, one and a half*

inches in length. No inflammatory appearances about the invaginations.

Ileum, injected throughout; two small ulcerations of the solitary glands near the caput coli. *Colon*, ascending and transverse portions injected; becomes very pale near the rectum; moderately filled with healthy looking fæces.

Liver healthy, gall bladder moderately full of deep amber colored bile; very white externally. *Spleen* healthy, not enlarged. *Peritoneum* very dry; no traces of irritation.

Note.—This woman would probably have recovered but for the invaginations. The ulcers in the ileum were very small and seemed to be in a healing condition.

Case 3.—Nelly, aged 22, black, slave of Mr. W. T. Simms, taken about 1st October, 1853, with chill, followed by continued fever; got blue mass and quinine—the latter freely for several days without abatement in the fever. I saw her a week after she was taken. She was sitting up, and could hardly be persuaded to go to bed: some appetite; tongue red and clean, bowels open freely: pulse one hundred and twenty-eight; pain in the neck and back of the head, which was relieved by applying chloroform and aqua ammoniæ. Made her lie down, and restricted her from eating. To take the saline solution three times a day. She improved so much in a few days, that she was thought to be getting well; but her bowels became deranged, and after they were checked, she suffered with retention of urine, requiring the catheter, and died on the nineteenth day.

Autopsy eight hours after death. Emaciation not great, abdomen not too full, (nor was it much painful or distended during her illness.) Opened the abdomen from the umbilicus to the iliac fossa, so as to expose the caput coli and the lower part of the ileum. This part of the bowels was of the deepest purple, and the lower part of the ileum and the upper colon, are one mass of ulceration, nearly black, and have a gangrenous odor; the bowels being three-fourths of an inch thick. Higher up the ileum, and lower in the colon, the ulcerations were not so aggravated. The transverse and descending colon was comparatively healthy.

Note.—No ulceration could scarcely be conceived more exaggerated than this; and it is remarkable that such frightful disease could exist with so little tumefaction and pain, and so little disturbance of the action of the bowels during life.

Treatment.—In all cases that have been managed by your reporter during the last year, a general expectant plan has been pursued, and every day's experience adds to the conviction of its being, in the majority of instances, the safest course. I have seen many cases which had been actively treated by venesection, purging, quinine and other anti-febrile remedies, but have always been satisfied that they were materially injured by such measures. The weight of medical

testimony on the subject, seems decidedly in favor of allowing the disease to run its course, so long as no urgent local symptoms, requiring interference, are observed.

One of the principal difficulties I have found in the treatment of this fever, has been that of keeping the patient quiet. It has been impossible to prevent them from being too much nursed, and too much visited by their friends; and though, as the community generally are becoming more familiar with the affection, and the importance of repose both of mind and body, the evil diminishes, yet it still exists to some extent. In my experience with the disease, negroes have got through with it more safely than whites, for the reason that there was not so much anxiety to be constantly doing something for them, and that they were not troubled with company. They have the advantage, too, of being less susceptible of excitement than the white race.

The following, in brief, is the course of management I have commonly pursued. If I saw the patient in the first few days, and found him constipated, and with a foul tongue, bad taste in the mouth, &c. I gave a laxative generally, of blue mass and comp. ext. colocynth, to unload the alimentary canal freely. After this, the bowels usually remain sufficiently open, and further purging is unnecessary, unless constipation should again occur; when simple injections, or a few grains of comp. ext. colocynth, given at night, are sufficient to keep them in a soluble condition. I then put the patient upon the saline draught or solution mentioned in a previous part of this paper, composed of carb. (or bi carb.) potassa, nitrate potassa, bi carb. soda and muriate of soda, each one drachm, water a pint. Give a tablespoonful four, six, or eight times a day. This solution allays thirst, and is generally agreeable to the patient. Some become fond of it, and desire to take it oftener than directed. I adopted this combination as a substitute for the chlorate of potassa, so highly recommended by Dr. Stevens, as being a less expensive, equally as efficient and more accessible every where. A good many cases of some severity have been treated during the year successfully without my seeing them, with no other medicine than this solution, and every day's experience gives me more reason to be satisfied with the prescription. I have never known it to affect the bowels, and rarely to irritate the stomach, and it seems to me to have no other action than a refrigerant one. The diarrhœa of typhoid fever is for the most part easily held in check by opiates and astringents; and the prescription I have found most successful is solution of acetate of lead 2 grs. to the ounce, given every two or three hours, with or without a small quantity of acetate morphine. Tannin, or decoction of oak bark, with burnt brandy, is also a good prescription, when the lead offends the stomach.

To excite diaphoresis, the effervescing draught, or spiritus mindereri, are good preparations, though the former often irritates the bowels. In the fever of the past year, however, I have not found diaphoresis to be as important as it formerly seemed.

The heat of the surface should be allayed by frequent sponging

with vinegar and water, and when the head is hot and painful, pouring cold water upon it, occasionally, is highly beneficial.

For pains and tumefaction of the bowels, I have used dry cupping and fomentations, and very rarely blisters: sometimes croton oil or an antimonial counter-irritant. After the first week or two, especially if the tongue be dry, the oil of turpentine in doses of ten or twenty drops, three or six times a-day, has seemed to me to act very favorably. It no doubt prevents or exerts a curative influence upon the intestinal ulcerations.

Rigid abstinence I consider indispensable. It is often very difficult, however, to secure proper forbearance on this point in the friends of the patient. Toast or barley water, rice water, thin gruel, slippery-elm mucilage, or, what is perhaps best of all, ice water (though we cannot always get it in the country,) is all the patient should be allowed, as long as the force of the fever is kept up. In the latter stages, when the system needs a more nourishing diet, broths, beef tea, Borden's meat biscuit, and articles of that sort, are necessary to support the patient, and build up his strength: the return to solid food should be by the most cautious gradations, always remembering that there is no disease in which relapse is more frequent or more dangerous than this.

The cases of congestive fever and of jaundice which I saw, were mostly out of the immediate neighborhood of Sumterville; and only one, in which both diseases were combined, need be noticed. This happened in a young gentleman of good constitution and very regular habits, who had spent the summer in Texas, and had had frequent attacks of intermittent fever, so slight, as, with the assistance of a little blue mass and quinine, not to prevent him from attending to his ordinary business. He was taken with a chill one morning, while hunting, and in a few hours began to pass urine mixed with about one-third of its quantity of blood. During the night, as the fever went off, the urine became pale and natural, and though he took quinine freely, he was not sufficiently careful of exposure to the air, and had another chill about the same hour as on the previous day, so slight as to be scarcely recognized, but followed by light fever, in which bloody urine was again copiously passed. Quinine was repeated in still larger doses, and the patient kept covered up till some hours after the time for the chill to return, and an immediate cure took place.

The progress of this case, and the means by which it was cured, left no doubt on my mind of its being a pernicious intermittent, in which the congestion attacked the kidneys, instead of the brain, the liver, or the stomach and bowels, the parts usually invaded, and the kidneys being organs of small size, and unable to support the load forced upon them, allowed the blood to strain through their texture along with the urine. I had met with several cases of this character in previous years, but they were all in an advance stage, and the renal hæmorrhage had become a permanent symptom. They were all consequent upon neglected chill and fever, and I have no doubt that

the hæmorrhage was intermittent at first in this instance, and might have been cured by the timely use of quinine. The strictly congestive nature of the disease, however, never occurred to me until I met with the case just detailed, and I have no doubt that it would have ended like the others, but for its intermittent character being ascertained so early in the attack, and the antiperiodic treatment at once adopted. I will not dilate further upon it, but respectfully submit it with the rest of this hastily written report, to the consideration of the Association.

Typhoid fever of Alabama.—(Report of M. G. Merriwether, M. D., Montgomery county.)

During the summer months there were cases of typhoid fever, and on one plantation it was very severe; and, what was remarkable, of nine infants under one year of age, every one had the disease or some disease so similar that neither my father nor myself could detect the difference. Until I saw those cases, I had believed that children under two or three years of age were not subject to the disease. Of the nine cases, five died; each having an attack of pneumonia supervening on the typhoid fever, which is a fatal symptom so far as my experience goes, in adults as well as children. The rose-colored spots, laid down in the books as distinctive of the disease, are not observable in half the cases; in fact, vibices are more frequently seen. I saw one case during the season, of vibices in the negro of unmixed African descent. Treatment of typhoid fever: the expectant or French plan of treatment, except the frequent administration of the oil of turpentine in combination with laudanum, in thick mucilage of gum-arabic and water. The oil of turpentine appears to have a special curative effect on diseases in which the blood is changed from its normal constituents, and should be used in all diseases which have a hæmorrhagic tendency. The abortive, or Dundas plan of treatment, in typhoid fever I have never seen tried exactly as it is recommended—that is, in the administration of quinine in doses of from 20 to 40 grains—but I have seen a case in which the patient took 500 grains during his illness, which was about three weeks—the case terminated fatally. Since that time, I have been afraid to give the great febrifuge in the doses recommended by Drs. Dundas, of Scotland, and Fenner, of New Orleans.

Typhoid fever of Alabama.—(Report of J. W. Crawford, M. D., of Centreville.)

On two farms, both belonging to the same individual, there were thirty-five cases of this fever, all well marked; out of this number, there were ten deaths, which, it must be admitted, was a frightful mortality. It is due to myself, however, as I attended all these cases, to say that only two cases proved fatal, to which I was called at the commencement or during the *first week of the attack*. In two

of the fatal cases, the attack had lasted two weeks; in two others, three weeks; and in four, eight days before I was called; and a number of those who recovered, had no regular medical attendance for a week or more after taking their bed. Quinine was given to two of the fatal cases in the commencement, by their master; and to others by myself, in large and repeated doses, but without the fever being in the least checked. Post mortem examinations were made in two cases, by my friend Dr. Moren and myself; an account of which, as requested by the Association, I attach to this report. One other case, the son of the owner, aged about fifteen years, originating on this farm, and treated by Dr. Moren and myself, commenced as an intermittent, but after the second paroxysm, assumed a congestive character, and terminated fatally at the end of the second week, with typhoid symptoms. The day after the first chill, he was going about as usual, and was persuaded to go into the river to bathe, where he remained some time. He was treated with calomel and morphia, in small doses; *the cold dash, wet sheets, mustard and dry cups to the spine and the whole abdomen, to relieve the congestion; and quinine, in ten grain doses.* The quinine, it was thought, did not act favorably, but increased the depression. At the end of the first week, vesicles containing at first transparent serum, afterwards becoming opaque and degenerating, when not rubbed into a scab, were discovered coming out on the back, and spreading to other parts of the body and limbs, until the larger portion of the cuticular covering was destroyed. After the typhoid symptoms set in, the treatment was expectorant, with stimulants and astringents, to check the bowels.

Case 1.—Mary, aged 38, the mother of two children, one 17 years old, the other 1 year: has had for the last four years an abdominal tumor, which was supposed to be ovarian, from which she suffered very much during her last pregnancy. Has been sick eight days. Has had from her master an emetic of ipecac., followed by blue mass and quinine. Found her with a furred tongue, pointed, fiery red at tip and edges; tremulous when protruded; no pain or thirst; skin hot and dry; abdomen somewhat tender, and distended; no diarrhœa, but stools liquid, yellowish and offensive; wakefulness, but no delirium; pulse one hundred and twenty to the minute. She was given, blue mass one grain, ipecac. half a grain, and opium one-fourth of a grain, every two hours; *verat. viride* eight drops every four hours, to be increased one drop at each dose until ten drops are taken,

unless nausea is produced; mustard to abdomen, to be followed by mush poultices. On next day, only one operation from bowels for past twenty-four hours; skin not so hot; pulse reduced to one hundred; in other respects about as before. To continue medicine. Next morning Mary was found in collapse, sweating, cold extremities, &c., and continued to sink in spite of stimulants, until ten o'clock when she died. She had not taken the medicine during the night.

Post-mortem four hours after death by Dr. Moren. As she had had no important head or chest symptoms, these cavities were not examined. Liver and gall bladder healthy; the latter moderately distended, with green bile, spleen enlarged considerably and *very much* softened. Kidneys and urinary bladder healthy, and the latter containing some straw-colored urine. Stomach and intestinal tube without any marks of disease until the lower portion of the ileum was reached, when we found one of the patches of Peyer's glands, more than an inch in diameter, thickened, indurated, and raised two lines or more above the mucous membrane: still nearer the end was an irregular, ragged ulceration, an inch wide, with thickened edges; and nearer still, in fact partly on the ileo-cecal valve, was another ulceration, smaller, but similar to the last described. Several of the mesenteric glands were very much enlarged. Ovaries healthy. The uterus was found to be a nodulated mass, larger than a child's head: there was situated on it, and partially buried in its walls, five tumors, from the size of a small orange down to one inch in diameter. They were all of the same character; not fibrous, but slightly vascular, and had much the appearance of cerebrel matter, only they were more yellowish. In laying open the cavity a small one was found presenting on the inside, which did not show on the outside. How could such an organ carry to its full term a child and expel it? yet, that this one did, I know, as I was present at its birth. The labor was regular and easy.

Case 2.—Alex, a large muscular boy, a brother to Mary: has been sick over two weeks, but not considered much so; on Saturday rode on horseback, getting up and down, and catching the horse himself, three hundred yards to another cabin, saying he was tired staying at his own. He was considered convalescent by his owner. I was called to see him on Sunday night, and found him in a profuse cold sweat; extremities cold; intellect clear; no pain; abdomen *very tender*, but not distended; the discharges from his bowels offensive, and being passed without his control; pulse one hundred and thirty, small, thready and scarcely perceptible. Ordered mustard to abdomen; friction, with dry mustard to extremities, and warmth; and internally brandy, opium in large doses, camphor and acetate of lead. He had rallied some on the next day, but afterwards sunk gradually, and died on Thursday night, having had but one discharge from his bowels, which was just before his death: it was said to be principally blood. His mind was clear all the while.

Post-mortem by Dr. Moren, six hours after death. On opening

the cavity of the abdomen a pint or rather more of serum, mixed with pus and flakes of lymph, was found; and on further examination two perforations were found in the ileum, one eight inches from its termination, and the other three or four more; one was perhaps two lines in diameter, and the other rather larger; around each, the peritoneal coat was much thickened by lymph, but not attached to any other portion. On opening the intestine, two other ulcerations were found, one involving the ileo-cecal valve, and the other higher up the ileum than the perforations. Colon filled with dissolved blood. No other disease in small intestines. Mucous membrane of the stomach, in two places of the great curvature, thickened, and so soft as to be removed with the slightest touch, and, throughout its whole extent, very much injected. Spleen very considerably enlarged, and so much softened that the finger can be passed into it at any point with scarcely an effort. All the other abdominal organs, with those of the chest, healthy. Head not examined.

Art. XII.—*Goitre in Alabama.*

W. Taylor, M. D., of Talladega, concludes his report to the Ala. Med. Association, (1854,) with the following summary:

1.—That goitre exists in some sections of the State, to the extent of about one per cent. of the population, and will in time, unless proper measures are adopted to prevent it, assume an endemic form.

2.—The disease being confined principally to the older geological formations of the State, it will, in all probability, never prevail to any considerable extent in the more recent systems.

3.—That the cause of goitre is pretty certainly traced to the character of the water used in the infected districts.

4.—There is strong reason to believe, from the observations of M. Chatin, and the scarcity of iodine in goitrous districts, that the disease is not produced by any positive agent, but has its origin from the want of a due amount of iodine in the waters of said districts.

5.—That goitre is not found in regions where the waters are impregnated with an appreciable quantity of iodine; hence it will never exist, to any considerable degree, in the tertiary and cretaceous systems, nor in the coal meadows of our State; since iodine is abundantly diffused throughout these formations.

6.—Iodine is the remedy, in the treatment of goitre, and this fact, under the circumstances, is strong corroborative evidence that the disease originates from the want of that agent; and if the absence of iodine is the cause of bronchocele, it is but reasonable to expect that great good may be obtained by impregnating the water used as drink, as a prophylactic measure in goitrous districts. May not the remedy

in time become as certain a preventive of goitre, as vaccination of small-pox?

7.—Iodine does not seem to form any chemical combination with the blood, nor with any of its elements, but seems to impart something to the system—the lymphatic system especially—necessary to the maintenance of its healthy functions.

8.—Surgery has done but little for the relief of the goitrous patient. Extirpation has been attempted, with unfavorable results; it is now totally abandoned by the profession. The seton has been resorted to with limited success. The formation of an abscess in the tumor has, in some instances, resulted in a spontaneous cure; tying the thyroidal arteries has also been attended with partial success.

9.—Certain gum resins are applied externally in South America, and also in some parts of the republic of Central America, with reputed efficacy.

Art. XIII.—*Functions of the Spinal Cord:* By T. LOCKHART CLARK, Esq.

The principal results of Mr. Clark's investigations, made on the ox, calf, cat, rat, mouse, and frog, are drawn as follows:—1. That the posterior roots of the spinal nerves consist of three kinds; two of them entering the posterior gray substance at right angles; the third kind with different degrees of obliquity, tending upwards, a small proportion only of the latter taking a longitudinal course, and becoming lost in the posterior white columns. 2. That in no instance were any fibres of the anterior roots seen to ascend with the anterior white columns, before they had entered the gray substance. 3. That besides the transverse bundles forming the anterior roots, a continuous system of exceedingly fine transverse fibres issue from the anterior gray substance, and become lost as they proceed towards the surface of the cord. 4. That from the preceding facts, it may be inferred that nearly all if not the whole of the fibres composing the roots of the spinal nerves, proceed at once to the gray substance of the cord; and that if any of them ascend directly to the brain, it must be *those only* of the *posterior* roots which run longitudinally in the posterior white columns. 5. That the communication between the sensorium and the spinal nerves is not established by the posterior white columns, but by the antero-lateral columns, especially the lateral. 6. That many of the fibres belonging respectively to the anterior and posterior roots in different regions of the cord, terminate there by forming with each other a series of loops of various sizes and lengths; and that it is not improbable that some of them may reach even as far as the brain. It is not perfectly denied by the author that a portion of the roots may be connected with the vesicles of the cords, but he considers the evidence of any such connexion as very

unsatisfactory. 7. The fine longitudinal fibres described by Stilling have not been found by the author. He is inclined to believe that the gray substance of the cord does not transmit impressions to and from the brain. 8. That there is great correspondence in the fibrous arrangement between the gray substance of the cord and the chiasma of the optic nerves. The author further remarks that the circumstance of the nerve-roots diverging upwards in the cord and intricately intermingling with each other, may explain why impressions made at one particular spot are communicated to distant parts of the cord, so as to excite simultaneous and sympathetic actions in classes of muscles which otherwise would appear unconnected.—*Br. and For. Med. Chir. Rev. April, 1854.*

Art. XIV.—*The Effects of the Recumbent Position Physiologically Considered*: By MR. RICHARDSON.—(*London Lancet, June, 1854.*)

The author commenced by stating that though the fact that the horizontal posture often affords marked relief in syncope is generally admitted, no very distinct attempt had hitherto been made to explain the principles on which it acted. One view, however, had fixed itself in the professional mind, and required to be carefully refuted. This view is—that the horizontal posture relieves syncope by allowing the blood to reach more freely the brain and medulla; so that these centres, gaining energy by this process, re-act on the heart and supply it with new vigor. This theory has been supported by Dr. Pulteney Alison, Dr. Ash, Sir G. Lefevre, and many other authors. Sir G. Lefevre has related a case in which syncope occurred on the patient assuming the erect position. The swooning was found to be connected with the presence of varicose veins in the leg, and was prevented by the application of bandages. But Mr. Richardson showed that the diminution of blood in the brain in this case was secondary to the fact that the propelling power of the heart was to a great extent lost through the mechanical impediment in the circulation,—an impediment which the bandages relieved. It was obvious that the blood detained in the lower extremities could not reach the brain without first passing through the heart. Any renewed force which the heart might receive from the nervous centres would be useless, unless it contained blood in which to expend its force. When we transfuse blood, we do so to fill the heart with its natural stimulus—not for the immediate purpose of exciting the nervous centres. The recovery of consciousness, on laying a person in the supine position, is no proof of the correctness of the hypothesis above mentioned, for when consciousness ceases through syncope, it ceases as a consequence of the failure of the circulation, and returns in proportion as the circulation is re-established. Mr. Richardson had observed that the first symptom of recovery from syncope

was invariably the return of the heart's heat; and that volition, consciousness, and animal heat followed. In some instances the action of the heart fails, while the functions of the nervous system generally remain perfect; and, on the other hand, the manifestations of the nervous system may be suspended by narcotic poisons, while the heart continues to act with power. There may also be extensive disease in the cerebro-spinal axis and yet the heart's action remain unaffected. Again, in the animal kingdom, the size of the heart and the activity of the circulation bear no relation to the development of the nervous system, while in the formation of the vertebrate-embryo, the heart pulsates before it is in any way connected with the nervous centres. Mr. Richardson next proceeded to offer his own theory of manner in which the horizontal position produces its good effects. The explanation appealed to mechanical laws alone, and was very simple. It must be remembered that the arterial blood, sent from the heart, first ascends; and that the venous blood descends from the upper and ascends from the lower parts. When blood is withdrawn from the upper part of the erect body, the heart loses in the end its power to drive the blood over the aortic arch; hence the blood, losing its *vis a tergo*, stagnates in the veins of the lower half of the body. At the same time, the heart, not having sufficient power to force its blood to the brain and other parts, consciousness necessarily ceases, together with muscular motion and the production of animal heat. Death would now soon occur from the heart ceasing to pulsate, and from the blood coagulating in the veins. But at this moment the body falls, or is laid down, and the blood contained in the lower half of the body is poured by simple gravitation into the heart, and again excites it to contraction. Thus the whole circulation is restored, and the brain and every part of the body receiving a fresh supply of blood, resume their proper functions; but to no one of these parts is due the least credit for having restored the heart. When blood is withdrawn from the lower part of the erect body, the chances of recovery are much lessened, for what was in the former case a reservoir now becomes a running cistern. The recumbent position is, however, equally valuable, since it leads to a free distribution of blood through the vessels above the heart. It might be advantageous in these cases to put the head slightly lower than the trunk, until the cause of the hæmorrhage was removed. But as a general rule, the simple horizontal position is all that is required. In cases of syncope, dependant on an overburthened state of the heart, or on debility of the cardiac walls, the horizontal posture relieves by allowing the blood to flow more easily through the pulmonary artery and aorta, and by rendering the venous current more equable.

Several experiments on narcotized animals were here related by the author, in which, after having laid bare the heart, he had placed the body in various positions, and abstracted blood. The experiments all confirmed the mechanical view that had been described. The paper was concluded with the remarks on the physiological and practical bearings of the views adduced.

Part Third.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Rev. I.—*Pneumonia: Its Supposed Connexion, Pathological and Etiological with Autumnal Fevers; including an Inquiry into the Existence and Morbid Agency of Malaria*: By R. LA ROCHE, Member of the American Philosophical Society, of the American Medical Association; Fellow of the College of Physicians of Philadelphia; Corresponding Member of the Imperial Academy of Medicine of Paris, &c. &c. Philadelphia: Blanchard & Lea, 1854.—8vo. pp. 502.

“A great book” is said to be “a great evil.” This, we have been painfully taught ever since the medical press of the country has been teeming with *foreign tours* either “with or without notes,” which have overlaid our native literature and compelled us to look abroad for what we can manufacture *much better at home*; better, because much more adapted to our condition and more applicable to the real wants of the country. To counteract all these foreign, and to a certain extent, injurious influences, the purity as well as the usefulness of our literature, demands from the constituted authorities, the protection of a copy-right law: with the prospect then of fair remuneration, and of being put upon an equal footing, we should no longer hear the exclamation of “who reads an American book—what does the world yet owe to American physicians and surgeons?” If in spite of all this competition, where the copy-right costs nothing, two such works—as that at the head of this article, and Drake’s great work on the Mississippi Valley—can be brought forward, surely an international copy-right is all that is wanting to furnish the comparatively slumbering talent of the profession the opportunity of development. They are, each, a worthy tribute to the genius of the country—alike remarkable for extensive research and profound erudition.

We are, we confess, proud of *this* volume. There are evidences throughout its five hundred pages, that the author has been careful not to give advice, which he has not taken himself; and that is, "in thoroughly making himself acquainted with the literature of the subject on which he writes." And so far from any accusation of ostentation being brought against him, from the necessarily large multiplication of references, there is little difficulty in detecting the difference between a vain and meretricious display of authorities, and when it is used only for corroboration or correction. In every page we observe the untiring scholar, the skillful analyst, the accomplished medical philosopher. We rejoice to welcome back to the ætiological department of the profession, one of the most distinguished of that corps of writers that, more than twenty years ago, elucidated, with their brilliant pens, the medical annals of our country.

Probably, in no department of the profession is this so much wanting, or in which so much good could be expected to be derived from it by the community at large, as in the important, and we are disposed to believe, little studied and less understood subject of ætiology. Confining themselves to pathological specialities, the sweeping causes that overwhelm *communities* in their desolating track, are, by the mass of our writers, left consigned to "occult causes," and the term "constitution of the atmosphere," is a complete barrier to all further investigation. This should not be so; and, we trust, the time is not far distant, when these "epidemic meteorations" will be so fully analyzed that all the physical qualities of the atmosphere will be thoroughly understood; it is alone, thus, that we can think of and apply a remedy.

The great object of the work of Dr. La Roche is to show that there are not sufficient grounds for the "idea of a close connection, as regards both causation and nature, between thoracic inflammation and malarial fevers of various grades and types," not "only on the other side of the Atlantic but in various parts of our own country, and especially in the Southwestern States."

In our own humble opinion, the sentiment thus expressed, as pervading the professional mind, is believed to be derived by them, more from *authority* than *observation*; and an importance has been given to it, to which it is not justly entitled. Be that as it may, it is now set-

tled beyond controversy ; no man will be, hereafter, bold enough to defend an hypothesis so demonstrably shown to be untenable. In the extended, welcome and approbatory extracts which have been made from this work by the principal journals of the country, numerous or lengthy extracts will not be expected of us: we shall endeavor to condense his principal propositions and proofs in as narrow a space as possible.

Our author has proved, to our entire satisfaction at least, “that pneumonia is of common occurrence where fevers are seldom or never seen; that pneumonia is not necessarily prevalent where fevers prevail; that they occur at different seasons; that they appear under the influence of opposite winds; that while pneumonia is a common endemic of a locality, the other is not; that while the one is more liable to occur in high, airy and exposed situations, the other occurs in valleys, low grounds, and on the banks of streams; that fevers are influenced by the nature of the soil, which is not the case with pneumonia; that these are arrested by the very thermometric conditions, which give rise to pneumonia.” To which the Dr. might have added with great propriety, that the “drying power” which is so fatal to periodic fevers, is exactly that condition which gives rise to pneumonia, more than any other whatever.

The liability of the different races to these two diseases is shown to be in a primary inverse ratio: “that the negro is very slightly subject to periodic fevers, and remarkably so to pneumonia: that the exemptive power of acclimatizing does not apply to pneumonia, although it does to malarial fevers.”

The extent to which the protective power of acclimatization furnishes defence against the recurrence of fever, it seems to us, is much overrated everywhere—further than an observant experience will warrant. Nobody pretends to doubt, that during the time a new country is passing through its “transition state,” from forest growth to entire cultivation, that no human being exposed to it, enjoys the alledged immunity; *nor does the actual occurrence of the fever itself entirely exempt one.* It is a matter of common observation, that an equable and still life—the ordinary routine life of one’s home, the habituation to things around one, is much less apt to be followed by disease—whether epidemic, endemic, or even an occasional visitor (as cholera,

&c.)—than the stranger, the traveller, the new comer, the immigrant; those who have, by this very fact, broken into their own routine of habits of living, eating, sleeping, exercises &c., and have made themselves much more liable to the numerous new influences around them. Now beyond these and the meteorological condition, whose action and influence can be easily explained physiologically, we very much doubt whether there is any farther acclimatization, with *the single exception of yellow fever* and that is against itself alone, while the individual continues in a hot climate. That there is none to filth, and all the nameless exhalations from the soil, is conclusively shown from the large mortality the natives of Egypt are subject to from the plague, the indemic fever of that country; and the constantly injurious and often fatal consequences known to result from over-crowding human creatures together in the filthy localities of all large cities.

That pneumonia and malarial fevers are not *convertible diseases*, is also a proof mentioned, by our author, why they are not the same; that they may both exist in the same individual, at the same time, may be considered another; nor, is it any answer to this to say that typhoid pneumonia prevailed some years ago as a wide spread epidemic; that there should be causes giving rise to pneumonia in an epidemic degree, and that it should be accompanied with a fever of a typhoid character, detracts not at all from the truth of the observation.

Different periods of life are also adverted to, as furnishing different susceptibilities. The *two extremes of life* are very subject to pneumonia, and *middle life* to autumnal fevers, while in the opposite condition they are comparatively exempt.

That each should have periodical tendencies is no proof of identity. The law of periodicity is impressed upon the whole system of our living, and applies to nearly, if not every, pathological as well as to every physiological condition.

We wish we could give as hearty a concurrence to what our learned author has said of malaria as to that which has preceded. We can certainly say this much, that no defender or expounder of its many occult principles and attributes, has exercised a power of analysis—a critical acumen or force of argument, comparable with the present, and that if he has not carried conviction wherever his luminous pen

has touched the subject, it must arise from the fact that there *can be no such specific agent*. So far as the term expresses a morbid state or influence of the atmosphere of certain localities—indeed, in whatever impairs the purity of the air—no matter what may be its precise cause or nature, we have no objection to, satisfied as we have long been that something else than mere physical qualities of the atmosphere and the physiological susceptibilities of the individual, is required to produce, at least, a large amount of our maladies. Nevertheless, it can hardly be denied that a very considerable proportion of the diseases to which our race is subject, can be most satisfactorily accounted for by extremes and transitions of temperature and moisture, and dependant conditions alone, and pneumonia is eminently one of them. Imperfect observations are the result of defective experiments. Our senses alone are not sufficiently acute and well-trained to ascertain these changes, nor do we take the trouble to record the most obvious, much less those which alone can be discovered by instrumental observations: they soon pass off from memory into the sea of oblivion, and are often, after a while, denied.

Of the various atmospherical agents, inimical to our health, an accurate knowledge of the hygrometrical condition is the most recent, if not the most important point gained, to aid the practical physician in his investigations in ætiology. The late applications testify to its great importance. When it shall have been more extensively used, it will aid in furnishing a key to many parts of practical medicine of inestimable value. The discovery of any valuable truth in medicine is of great importance—beyond the abstract truth itself. It fixes the fact, it corrects the exuberance of the imagination, curtails the “false facts,” so long the bane of the science and the stumbling blocks of progress, and substitutes the nakedness of truth for ingenious explanations. So let it be with hygrometry. Let us study it with care and apply it with exactitude. Of itself, it is but one of the several conditions to produce that complicated result (termed “fever”) which is the great avenue to death of unnumbered millions of our race, it being calculated that one-sixth of the entire annual mortality fall victims to it.

Our author has been eminently successful in showing that pneumonic and malarious fevers arise from different conditions externally; they do so likewise internally; the structures, respectively affected,

are different also. While the first affects the parenchymatous, the second attacks the mucous structures. And, here we cannot avoid the remark that our *treatment* often produces a symptom to which undue importance is attached, and which is the cause of much obscurity here. *Bilious* pneumonia is often an artificial disease, (if not a misnomer) produced by medication, and is the result of a revulsive action on the liver, whose active depletion is invited to equalize the lost balance in the system—it is a perturbation and often hazardous practice. In delicate states it is the game of the gambler—"all or none"—and its chief danger consists in the *uncertainty* of our ever being able to *force secretion*, and the *certian injury* we commit if we fail.

The French pathologists, by their different and more soothing treatment, have not so many "*bilious diseases*" to treat; they act less upon the theory of antagonizing forces; they seldom revulse upon one organ to relieve the suffering condition of another. Their main means of relief are either those of "expectancy," or depletion direct and especially local, and with at least equal success, the constitution sustains less injury, and the patient runs less risk.

With these few hasty remarks, we take leave of our talented countryman; we have read his work with as much pride as pleasure; it is a fountain of instruction, where all may imbibe valuable knowledge. It will long remain a standing monument to his fame—may it ever stand, to teach the aspiring student that the true road to usefulness and reputation, is by industry and indefatigable research.

E. H. B.

Rev. II.—*Types of Mankind, or Ethnological Researches, based upon the Ancient Monuments, Paintings, Sculptures, and Crania of Races, and upon their Natural, Geographical, Philological, and Biblical History*; illustrated by selections from the incedited papers of Samuel George Morton, M. D., (late President of the Academy of Natural Sciences, at Philadelphia,) and by additional contributions from Prof. L. Agassiz, LL. D.,; W. Usher, M. D.; and Prof. H. S. Patterson, M. D.: By J. C. NORT, M. D., Mobile, and GEO. R. GLIDDEN, formerly U. S. Consul at Cairo. Royal 8vo. pp. 738. Philadelphia: Lippincott, Grambo, & Co. 1854.

Article First.—To the pages of Professor Agassiz, in this learned work, the present notice will be restricted, leaving the residue of this large book, so far as it pertains to the Medical sciences, for future examination.

The rôle of Professor Agassiz is quite limited, though his subject

is of vast expansion and of high import, requiring the most profound research, abundant illustration, together with a wide survey of the analogical evidences wherewith to fill up many existing hiatuses in direct experiment and positive demonstration. All who have heard or read Professor Agassiz' lectures, and who are acquainted with his geological, zoölogical, physiological, palæozoic, and embryological researches, will admit his ability to give a far more satisfactory "tableau of the natural provinces of the animal world, and their relations to the different types of man," than his eighteen meagre pages of this work afford.

Prof. Agassiz' prefatory note, indicates his purpose in this paper, namely: to give "some general remarks upon the natural relations of the human family and the organic world surrounding it; in the hope that it may call the attention of naturalists to the close connection there is between the geographical distribution of animals and the natural boundaries of the different races of men—a fact that must be explained by any theory of the origin of life which claims to cover the whole of this difficult problem. I do not pretend to present such a theory now, but would simply illustrate the facts as they are, to lay the foundation of a more extensive work, to be published at some future time."

It is evident that a foundation of this kind should not proceed upon abstract propositions in advance of the proof. Whatever evidence can be adduced in favor of Professor Agassiz' weighty postulates—weighty if proved—ought to form the foundation. All the known facts of the palæozoic series constituting the fossiliferous strata, from the oldest to the most recent; the identity, difference, succession, and gradation of organic forms, including their ætiology and zoölogical relations and changes, should have formed the "high argument" of his abstract theory. The succession and variations in the organic world—not its original creation—may be illustrated at every step by the books of stone which the earth's crust presents. And Prof. Agassiz is the philosopher preëminently qualified to open the book of stone and read this concrete Word of God correctly, if any one can: what is, is—its *origin* is beyond the ken of philosophy—let him read what is. The palæozoic ages furnish many editions of the concrete word, the physical *λόγος*, as well as many volumes: thus, the lower and upper silurian formations consist of nearly twenty volumes, in which the

fishes predominate. The second grand palæozoic series of the great stone book—the carboniferous—in three volumes; the saliferous oölitic and the chalk formations, treating of reptilians; while the tertiary, the eocene, the miocene and the pleiocene, describe the mammalian series, the more immediate precursors of the book of humanity, the first lines of which open thus: “In the beginning God created the heaven and the earth. So God created man.”

Professor Agassiz advances the following hypothesis touching the origin and unity of the human race:

“1st.—Either mankind originated from a common stock, and all the different races with their peculiarities, in their present distribution, are to be ascribed to subsequent changes—an assumption for which there is no evidence whatever, and which leads at once to the admission that the diversity among animals is not an original one, nor their distribution determined by a general plan, established in the beginning of the creation;—or,

2d.—We must acknowledge that the diversity among animals is a fact determined by the will of the Creator, and their geographical distribution part of the general plan which unites all organized beings into one great organic conception: whence it follows that what are called human races, down to their specialization as nations, are distinct primordial forms of the type of man.

The laws which regulate the diversity of animals, and their distribution upon the earth, apply equally to man, *within the same limits and in the same degree*; and that all our liberty and moral responsibility, however spontaneous, are yet instinctively directed by the All-wise and Omnipotent, to fulfil the great harmonies in Nature.”—xxv. xxvi.

In the following extract the reader will find a summary of Prof. Agassiz' notions of species, varieties, unity, diversity, &c.

In order not to enter upon debateable ground in answering the first of these questions, let us begin by considering it with reference to the animal kingdom; and, without alluding to any controverted point, limit ourselves to animals well known among us. We would thus remember that, with universal consent, the horse and ass are considered as two distinct species of the same genus, to which belong several other distinct species known to naturalists under the names of zebra, quagga, dauw, &c. The buffalo and the bull are also distinct species of another genus, embracing several other foreign species. The black bear, the white bear, the grizzly bear, give another example of three different species of the same genus, &c., &c. We might select many other examples from among our common quadrupeds, or among birds, reptiles, fishes, &c., but these will be sufficient for our purpose. In the genus horse we have two domesticated species, the common horse

and the donkey; in the genus bull, one domesticated species and the wild buffalo; the three species of bear mentioned are only found in the wild state. The ground upon which these animals are considered as distinct species is simply the fact, that since they have been known to man, they have always preserved the same characteristics. To make specific difference or identity depend upon genetic succession, is begging the principle and taking for granted what in reality is under discussion. It is true that animals of the same species are fertile among themselves, and that their fecundity is an easy test of this natural relation; but this character is not exclusive, since we know that the horse and the ass, the buffalo and our cattle, like many other animals, may be crossed; we are, therefore, not justified, in doubtful cases, in considering the fertility of two animals as decisive of their specific identity. Moreover, generation is not the only way in which certain animals may multiply as there are entire classes in which the larger number of individuals do not originate from eggs. Any definition of species, in which the question of generation is introduced is, therefore, objectionable. The assumption, that the fertility of cross-breeds is necessarily limited to one or two generations, does not alter the case; since, in many instances, it is not proved beyond dispute. It is, however, *beyond all question*, that individuals of *distinct* species may, in certain cases, be productive with one another, as well as with their own kind. It is equally certain that their offspring is a half-breed; that is to say, a being partaking of the peculiarities of the two parents and not identical with either. The only definition of species meeting all these difficulties is that of Dr. Morton, who characterizes them as *primordial organic forms*. Species are thus distinct forms of organic life, the origin of which is lost in the primitive establishment of the state of things now existing, and varieties are such modifications of the species as may return to the typical form under temporary influences. Accepting this definition with the qualifications just mentioned respecting hybridity, I am prepared to show that the difference existing between the races of men are of the same kind as the differences observed between the different families, genera, and species of monkeys or other animals; and that these different species of animals differ in the same degree, one from the other as the races of men—nay, the difference between distinct races are often greater than those distinguishing species of animals one from the other. The chimpanzee and gorilla do not differ more one from the other than the Mandingo and the Guinea negro: they together do not differ more from the orang than the Malay or white man differs from the negro. In proof of this assertion, I need only refer the reader to the description of the anthropoid monkeys, published by Prof. Owen and by Dr. J. Wyman, and to such descriptions of the races of men as notice more important peculiarities than the mere differences in the color of the skin. It is, however, but fair to exonerate these authors from the responsibility of any deduction I would draw from a renewed examination of the same facts, differing from theirs; for I maintain distinctly that the differences observed

among the races of men are of the same kind or even greater than those upon which the anthropoid monkeys are considered as distinct species.

Again, nobody can deny that the offspring of different races is always a half-breed, as between animals of different species, and not a child like either its mother or its father. These conclusions in no way conflict with the idea of the unity of mankind, which is as close as that of the members of any well-marked type of animals; and whosoever will consult history must remain satisfied that the moral question of brotherhood among men is not any more affected by these views than the direct obligations between immediate blood relations. Unity is determinative by a typical structure, and by the similarity of natural abilities and propensities; and, unless we deny the typical relations of the cat tribe, for instance, we must admit that unity is not only compatible with diversity of origin, but that it is the universal law of nature.

This coincidence, between the circumscription of the races of man and the natural limits of different zoölogical provinces characterized by peculiar distinct species of animals, is one of the most important and unexpected features in the natural history of mankind, which the study of the geographical distribution of all the organized beings now existing upon the earth, has disclosed to us. It is a fact which cannot fail to throw light, at some future time, upon the very origin of the differences existing among men, since it shows that man's physical nature is modified by the same laws as that of animals, and that any general results obtained from the animal kingdom regarding the organic differences of its various types must also apply to man.

In the above passage Prof. Agassiz asserts "that there are entire classes of animals in which the larger number of individuals do not originate from eggs," and yet, in his *Principles of Zoölogy* published in 1851, he holds the following language:

"§ 275.—That all animals are produced from eggs (*omne vivum ex ovo*) is an old adage in Zoölogy, which modern researches have fully confirmed. In tracing back the phases of animal life, we invariably arrive at an epoch when the incipient animal is inclosed within an egg." 132.

In the same work, however, one is surprised to read thus:

"§ 328.—We have shown in the preceding chapter, that ovulation, and the development of embryos from eggs, is common to all classes of animals and must be considered as the great process for the re-production of the species. Two other modes of propagation, applying, however, to only a limited number of animals, remain to be mentioned, namely, *gemmaiparous* re-production, or multiplication by means of buds, and *fissiparous* re-production, or propagation by division; and also some still more extraordinary modifications yet involved in much obscurity." 156.

These discrepancies are not alluded to with a view of underrating the great and well merited reputation of Prof. Agassiz as a profound naturalist and physiologist, but for the purpose of keeping questions not fully settled, still open for further research, that Nature may be interrogated rather than the authority of men how learned so ever they may be.

In vain may philosophy call to its aid, physiology, comparative anatomy, geology, palæontology, ethnology, archæology, monuments and manuscripts in order to account for man's origin. He who will not accept the Mosaic history, abandons the last plank, and must sink in hopeless uncertainty. If the miraculous origin of man be admitted, so from the same source, it is reasonable to suppose came his anatomical diversities as to color, hair, crania, and the like; or, if these changes resulted from former climatic laws which ceased after the effectuation of their finalities, more miracles must be admitted, which certain philosophers reject altogether, while others seek as far as possible to explain the origin and primary types and variations of man by laws now in operation, without rejecting miraculous interposition where philosophy fails. "In the beginning God created the heaven and the earth."—"Here will I hold."

The visionary speculations of Lamarck, from whom the author of "The Vestiges of Creation" has borrowed much inconclusive ratiocination and downright rant, have not, in a single instance, explained the origin of an animal, much less man. The massive intellect of Prof. Agassiz is not biased by these fictions concerning the assumed omnipotence of the abstract law of progressive development stimulated by inexorable want, or *besoin*, as Lamarck calls it, which is inaugurated as a positive entity in place of the *Infinite Ens*, and which instead of explaining the creative acts, throws an impenetrable veil of darkness over the past. Will the remote past ever reveal to science the origin of the universe? The vibrating pendulum of eternity seems to answer—*Ever? Never! Ever? Never!* [EDITOR.]

Rev. III.—*Handbook of Chemistry: Theoretical, Practical and Technical*: By F. A. ABEL, Professor of Chemistry at the Royal Military Academy, Woolwich; and Assistant-teacher of Chemistry at St. Bartholomew's Hospital; and C. L. BLOXAM, formerly First Assistant to the Royal College of Chemistry. With a Preface by Dr. HOFMANN: and numerous Illustrations on Wood. Philadelphia: Blanchard & Lea, 1854—8vo. pp. 681.

Chemists, apothecaries and doctors, both actual and intending, might be deceived by the title of this work, called "A Handbook;" that is, they might suppose that it is like many other handbooks—a mere compilation, pirated from the living and the dead, and worked up in the closet, without skill, without analyses, without method, and without experimental research. Such is not the case with this able work. Its scope—theoretical, practical, technical and analytical, without being exhaustive, is nevertheless very comprehensive and complete—running through the whole gamut of the science.

The wisdom of the ancients, in whatever pertains to scientific chemistry, compares most unfavorably with that of the present generation; affording, indeed, a subject in which contrast rather than parallelism predominates.

Chemistry—organic and inorganic, theoretical and practical—is virtually a modern science; yet, notwithstanding the secure basis on which its general principles rest, the progress of discovery, the details of new analyses, both qualitative and quantitative, all conspire to establish the presumption that, other things being equal, the newest chemical books are the best; at least, the work above mentioned appears to belong to this category, since it contains in a high degree the postulates and the practice, the science and the art of chemistry, methodically arranged and extensively illustrated by wood cuts, intercalated in the text, so as to occupy a small space.

The following extract is given,* not as a specimen of the researches of Messrs. Abel and Bloxam, but as the latest summary of information concerning an agent little known, namely, *Ozone*:

Ozone.—This remarkable body was first discovered by Schönbein. He detected it in the atmosphere (by means of tests to be presently described,) and found it to be formed in almost every instance of electric discharge into the air; also, when water is electrolyzed, and when phosphorus is allowed to act upon moist air at ordinary temperatures.

Preparation.—Ozone is best obtained by placing a piece of recently

* See the Article on the Perchloride of Iron, in the present number of this Journal.

scraped phosphorus, about half an inch in length, into a clean bottle (of about two quarts capacity), in the bottom of which is as much water as will half cover the phosphorus; the mouth should then be closed slightly (to prevent any mischief ensuing if inflammation of the phosphorus should take place), and the bottle set aside. Ozone is almost immediately produced, its formation being indicated by the ascent of a column of vapor from the piece of phosphorus, and the luminosity of the latter in the dark. Ozone may be detected in the bottle within a minute after the introduction of the phosphorus; if allowed to stand for six or eight hours, the air in the bottle will be abundantly charged with it. The phosphorus should then be removed, and the air freed from phosphorus acid by agitating some water in the bottle.

Properties.—The ozone thus obtained (in admixture with air) has the following properties: it is a colorless gas, possessing a very peculiar odor, which, when concentrated, much resembles that of chlorine, but when diluted is precisely the odor observed when an electric machine is in action. When air has been powerfully charged with ozone, it can be inspired with difficulty; it acts powerfully on the mucous membrane, producing very disagreeable sensations; small animals immersed in it soon cease to exist. Pure ozone must therefore be highly poisonous.

Ozone is insoluble in water; it possesses powerful bleaching properties, and also acts as an energetic oxidizing agent, transforming phosphorus into phosphoric acid, and powerfully oxidizing many metals, converting them and their lower oxides into the highest oxides they are capable of forming. Thus, lead and silver are converted into oxides, antimony and arsenic into arsenic acid and antimonious acid; the salts of the protoxides of manganese, cobalt, nickel, are decomposed by it, the acids being evolved and the binoxides formed. It also decomposes many hydrogen acids (*e.g.* hydrosulphuric acid), and oxidizes organic compounds. It combines with chlorine, bromine and iodine, and is in many respects analogous in its action to the binoxide of hydrogen.

Two views are entertained respecting the constitution of this body; the one, that it is oxygen in an allotropic condition—the other, that it is a compound of oxygen similar to binoxide of hydrogen. The former is the view which possesses the greater number of supporters, particularly since it has been proved that, on passing dry ozonized air through a redhot tube, the destruction of ozone by the heat (it being only capable of forming at ordinary temperatures) is unaccompanied by the production of any water.* Many organic compounds, such as ether and turpentine, when exposed to the action of air and light un-

* By very recent researches, Baumert believes that he has shown the ozone obtained in the electrolysis of water to consist of a tetroxide of hydrogen. He passed the perfectly dry ozone, first through a tube containing anhydrous phosphoric acid, which was unaffected by it, then through a tube heated to redness, and lastly, through a second tube, containing phosphoric acid, which indicated the presence of moisture produced in the decomposition of the ozone. The proportion of oxygen was determined by passing the ozone into a standard solution of iodide of potassium

dergo peculiar changes, and acquire very powerful bleaching and oxidizing properties, apparently by association with ozone.

Tests for Ozone.—The most delicate test for the presence of ozone is prepared in the following manner: one part of pure iodide of potassium and ten parts of starch are boiled together, for a few moments, with two hundred parts of water, and white filtering paper is saturated with the liquor thus obtained. Such paper is immediately turned blue when introduced moist into ozonized air. If introduced dry it will remain colorless, but becomes blue immediately upon being moistened.

Paper prepared with a solution of sulphate of manganese is also a good test for ozone, becoming rapidly brown from formation of binoxide when introduced into ozonized air. [EDITOR.]

Rev. IV.—*The Science and Art of Surgery: being a Treatise on Surgical Injuries Diseases, and Operations:* By JOHN ERICHSEN, Professor of Surgery in the University College, and Surgeon to University College Hospital. Edited by JOHN H. BRINTON, M. D. Illustrated by 311 engravings on wood: Philadelphia: Blanchard & Lea, 1854.—pp. 908, 8vo.

The material processes and obvious eventualities of surgery address themselves directly to the senses, and may, to a very great degree, be appreciated by all classes of persons, as in the reduction of a dislocation, the amputation of a limb, the tying of an artery, the removal of a tumor, &c. On the other hand the physician may save a hundred lives were the surgeon saves one, and yet in the former case, neither the patients cured, nor the observers, can determine in a satisfactory manner what the therapeutic value of his services may have been. Hence, a physician may not be an operating surgeon, but a surgeon must be a physician—aye, and a physiologist too—otherwise, he must sink to the character of a mere mechanical operator, who is seldom to be trusted out of the sight of the physician.

Hence arises the questionable expediency of studying and practicing specialities in medicine. The oculist, the aurist, and even the mere operative surgeon, generally succeeds well or ill, just in proportion to their comprehension and appreciation of anatomy, physiology, pathology, and therapeutics. The enlightened and conscientious surgeon holds, as a fundamental article of faith, that of all the means of cure, surgical operation must be kept in abeyance, or be avoided altogether, as a reproach on the healing art and as a proof of its imperfection, unless it shall afford the patient a reasonable certainty, or at least a probability of a more favorable result than that founded on the

best devised plan of medical treatment. The dangers resulting from an operation are often great, and require the utmost skill of the physician to obviate them and complete the cure. The grandest generalizations of the surgeon are those of the physician, as in the doctrines of inflammation—"first, last, midst, and without end"—ranging from concussion to hyperæmia—from effusion to gangrene—from snake-bite to frost-bite—from contusion to ulceration—from carbuncle to syphilis—from scrofula to cancer—from gonorrhœa to phlebitis—from rickets to necrosis—constituting a duality in unity, and a unity in duality.

But, it is time to pass from these platitudes to the work named above; a slight examination of which warrants the conclusion that, notwithstanding the avalanches of surgical works recently hurled forth upon the medical world, all possessing much merit, Prof. Erichsen's work, for its size, has not been surpassed; his 908 pages, profusely illustrated, are rich in physiological, pathological and operative suggestions, doctrines, details, and processes, and will prove a reliable resource for information, both to physician and surgeon, in the hour of peril.—EDITOR.

Rev. V.—*On Rheumatism, Rheumatic Gout and Sciatica: their Pathology, Symptoms and Treatment*; By HENRY WILLIAM FULLER, M.D., Cantab., Fellow of the Royal College of Physicians, London; Assistant Physician to St. George's Hospital, &c. &c. New York: Samuel S. & William Wood, 1854—pp. 322, 8vo.

This is a valuable, well timed, and very complete monograph, upon the causes, symptoms, various forms, nature, seats, and pathological anatomy of rheumatism; including the general and special treatment adapted to its stages and diversities—comprehending the various complications of this malady, particularly that most formidable one, heart-disease, as carditis endo-carditis, peri-carditis, valvular alterations, fibrinous vegetations, serous effusions and infiltrations; also, articular, pulmonary, pleural, cerebral, sciatic and neuralgic affections. The author investigates the rheumatic diathesis—its parallelism with gout, alludes to mal-assimilation, the morbidity of the blood, the predominance of lactic acid, and other phenomenal manifestations of this malady.

Dr. Fuller has examined into the therapeutic value of the remedial

agents usually employed in the treatment of rheumatism—purgatives, baths, mercury, antimony, cinchona, colchicum, guaiacum, nitrate of potash, lemon juice, alkalies, and their salts, and, above all, opium. Of the latter, he says :

I can testify most strongly to the value of the sedative in full and repeated doses: in doses far exceeding in amount the quantity usually administered. In the early and most painful stage of the disease, occurring in adults, it may often be given with the greatest advantage, to the extent of six or eight grains in the course of twenty-four hours; and to children, without the slightest fear, in half grain doses every three or four hours. * * * I have never seen it check secretions, or produce the slightest cerebral disturbance. I am satisfied that in many instances it has materially hastened the period of convalescence, and has lessened the frequency of inflammation of the heart. Therefore, whilst I join issue with those who would treat acute rheumatism by opium alone, I admit most fully the advantage of its employment in conjunction with other remedies, and in quantity sufficient to allay or subdue pain.—p. 83.

The editor of this Journal, can from experience recommend large doses of opium, morphia, and Dover's powders, in the earliest stages of the most acute form of rheumatism; articular infiltration, tumefaction, and pain and fever, usually subside gradually, sometimes suddenly, as soon as the system is brought under the influence of opium. In this manner he treated, not long since, the President of the Board of Assistant Aldermen of New Orleans, Dr. Dalton, himself an accomplished physician, who suffered a sudden and violent attack. As soon as the opium subdued the pain, though without narcotization, the fever, infiltration, and articular engorgement and tumefaction, began steadily to decline; the ferrocyanate of quinine and aperients completed the cure in a week. Opium is not only a subduer of irritation, pain and restlessness, but of inflammation itself.—EDITOR.

Rev. VI.—*Women: Her Diseases and Remedies: A Series of Letters to his Class:* By CHARLES D. MEIGS, M. D., Professor of Midwifery, and the Diseases of Women and Children, in Jefferson Medical College at Philadelphia; Member of the American Medical Association; of the American Philosophical Society, and of the Council; Vice-President of the College of Physicians of Philadelphia; late, one of the Physicians of the Lying-in Department of the Pennsylvania Hospital, &c. Third edition, revised and enlarged. Philadelphia: Blanchard & Lea, 1854.—8vo. pp. 672.

Napoleon, the prisoner at St. Helena, was told by one of his exiled companions that which would never have been uttered to Napoleon,

the Emperor, at the Tuileries, namely—that he had no style! Yet his bulletins affected the style of Ossian. Dr. Meigs has been much—if not wisely—criticised on account of his remarkable style, even by those who have accorded to him the highest qualifications as a thinker, teacher, practitioner and instructive writer.

It was but recently that periodicals, pamphlets and octavos, put the question—“Have we a Bourbon amongst us?” In the same vein it may be asked—Have we a style amongst us? Has Dr. Meigs a style? Ask not—Is his style faultless; but, is it original, characteristic of any school, or proper to himself, something by which he may, as a writer, be identified. It is not intended on this occasion to give an affirmative or negative answer to this question, further than to say that if Dr. Meigs has a style of his own, he must be, independent his preëminent scientific knowledge, an extraordinary genius. But, without going into the natural history and the composite character, and neological tendencies of Dr. Meigs’ composition, a few remarks may be allowed illustrative of the question—Have we a style among us?

Let it not be supposed that the writer of this article claims for his own style any excellence whatever. A just conception, by no means, implies the ability to execute. Some one stated that he who undertook to criticise poetry must himself be a poet; to which, Johnson replied that, by this rule, “he who kills fat oxen must himself be fat.” Johnson was himself a great critic in poetry—not a great poet.

The writers of the preceding generation dedicated their works to some powerful patron or to posterity, and barely escaped starvation. The authors of the present day dedicate their books to the multitude, caring little for posterity, and grow rich. Milton sold out at £15; a modern novelist gets as many thousands, and lives to see a score of editions. Yet the works of the former class shine with an increasing lustre; while those of the present generation, with few exceptions, will pass away, meteor-like. The reigning passions, the temporary interests, and utilitarian views of the moment are reflected upon the multitude; which, being delighted with a panorama of itself, leaves antiquity and posterity to take care of themselves, as no better than old fogies and humbugs—to use the cant phrases of the day.

Among an host of living writers, some are correct and elegant, not a

few are eloquent, pathetic and natural, enlightening the understanding, swaying the passions and directing the destinies of humanity, showing that "the pen is mightier than the sword;" yet, it will be difficult to designate among the whole, one that is recognizable by originality—one who has a well defined style, proper to himself, belonging to a distinctive school, except that which may be called the composite, in which there is but little individuality or unity of type. It is from the title page, and not from the style that the reader, with rare exceptions, learns who the author is, and how to distinguish him from all others.

Individuality of style by no means implies personal individuality. In that "Mighty orb of song—the divine Milton"—his personality, his great thoughts and his sightless eye-balls, seem to go together. The same is true of Young, Burns, Byron and many others. But when the reader launches on the vast stream of Shakspeare's mind, humanity, not Shakspeare is individualized. Each person of the drama thinks, feels, wills, acts and speaks, without losing the unity of a common type. The colorless diamond reflects the greatest variety of lights.

The exuberance of medical observations, experiments, and ratiocinations and inductions, now extant would be far more attractive, more read, and consequently, far more useful, were they embodied in a suitable style.

It will be found, as a general rule, that a feeble, redundant and inaccurate style in medical composition, is characterized by corresponding defects in mental ability, both as regards accuracy in observing and reasoning.

The student of language must fall back upon the last century and the earlier part of the present for models of pure English composition; as nearly every great master of that era had a characteristic style proper to himself, equally good with that of his neighbors', though different in kind. Among these various masters and different styles one may be preferred to the other, without incurring the charge of bad taste—whether Bunyan, Blackstone or Junius, Swift or Addison, Gibbon or Goldsmith, Hume, Robertson or Johnson—be chosen. These writers are not known solely by an exuberance of exclamations, dashes, colloquial phrases, antitheses, neologies; nor by

exaggerated bad spellings and solecisms of language carefully copied from among the vulgar, mixed up with studied periods, both long and short, ornate diction and pathetic climaxes—throughout thin serials and thick octavos.

Among the popular authors of the day it would be difficult to find one whose style is so original and withal accurate, as to admit of a striking and easily recognized parody or comic imitation. Without reliance on this test, which is among the weakest, it is possible to travesty Johnson, Milton, and burlesque Homer; while a burlesque or imitation of many of the present race of writers could only be found out by means of a title page and preface, just as in the case of a certain innkeeper's sign, whereon a horse and a bear had been painted, under each of which, it was found necessary to put the name, that the public might distinguish the one from the other.

Critics lay down certain postulates, often no better than platitudes, for the formation of style—the style of science they think should be simple. True. But this simplicity, properly understood and applied, is of all acquirements the most difficult and the last attained. He who is master of this, must have mastered all the other qualities of style. A dry, didactic, by no means implies a simple style, much less an interesting one. Medical facts reduced to numerical tables, are, for example, less read than the most indifferent composition in which the same truths may be detailed. The *Principia* of Newton is didactic, technical, mathematical, and in Latin; but, if its great truths can for the most part be taught without mathematical formulæ, let no one grieve thereat. If the cause of science can be advanced by ornaments, tropes and figures—by Carlyslisms, Meigsisms, and neologisms—grieve not; although Jonathan Swift hated foreign words and idioms, as much as Johnson loved Latinisms and magnificent periods.

In writing on medical subjects, perspicuity is the fundamental quality; but if the writer can, by any other properties of composition not incompatible with the first, throw attractions around his topics so as to interest his reader the more thereby, he is justified in doing so. Here, at least, the end justifies the means.

Obscurity in writing and in thinking, dislocated sentences and arguments, a lack of syntax and a lack of sense, are, with rare exceptions, connected as warp and woof. In the office of this Journal are

rejected essays, in which there is not a single stop ; in which the syllables are disconnected ; in which capitals form the middle of words—some being sprinkled about like sand, as if by accident ; papers in which the spelling is right only by chance, clearly showing that the medical colleges have granted the degree of M. D., to men who cannot spell the word p-h-y-s-i-c-i-a-n except as follows—phisisian ; patenst ; elusteration ; resent [recent] ; phelm [phlegm] ; greate interprize ; haveing ; takeeng ; asure ; stile ; malady ; facalty ; tecnhical [technical] ; modren ; nesenary ; delereum ; once [ounce] ; wether [weather] ; brused ; penus, &c. Now, these papers are, as such usually are, as defective in matter as in manner ; as incoherent in thought as in grammar.

Suppose, what rarely if ever happens, that a writer whose style is inaccurate, his words ill-chosen, his sentences badly arranged and obscure in import, possessed, nevertheless, profound knowledge ; suppose that he should write the history, including the pathology and therapeutics of yellow fever for the last three centuries, as deducible from thousands of volumes, pamphlets and reports, now passing rapidly to oblivion ; and suppose, furthermore, that the same subject should be selected by another writer possessing the same degree of scientific knowledge, and the additional qualification of an accurate and elegant style—like that of Sir William Blackstone, Dean Swift, Addison, Johnson, or, still better, good John Bunyan,* Hume, Gibbon or Robertson—-which of these histories would be preferred ? which would be the most useful ? which would stand the scrutiny of talents and time, and serve as a light-house to the future voyager upon the dangerous, dark and uncertain coast to which humanity shall be often borne, when the perilous storm of pestilence shall rage ?

It is not fine writing, as it is improperly called, that should be aimed at by medical authors ; but that more difficult style, the elements of which are simplicity, clearness, and strength ; in which nothing is redundant, nothing deficient—consisting of proper words in proper places ; which convey the greatest amount of knowledge in the smallest space ; which suggest more than they express ; which give

* Although Bunyan's personages are allegorical, his narratives rival those of ancient and modern times ; being admired alike by the learned and the unlearned—and the more, as time rolls onward.

to the longest essay, brevity; and, which make an indelible impression on the memory.

Look into the narrative passages in the historians already mentioned, and compare them with the reports of medical cases—which latter, also, are essentially narrative. Now, which of these classes will contain the greatest number of facts and ideas? which will be arranged best? which will be expressed in the most systematic manner? Which will be the easiest to understand, will make the most enduring impression?

Dr. Meigs censures medical writers for their disgusting tediousness, dullness and jargon. He says of his own style :

“I will write in the same language I should address to any one of you, whom I might be instructing in my library here, at home. Whether such a mode of writing might prove agreeable to the brethren, so as to meet their approbation, remained to be seen. If I should fail in this attempt, I may still hope that some one else will invent a new and happier method than mine, to get rid of our medical dullness and time-honored clergyableness.”—viii.

It may be allowable to give, in this place, a few of Dr. Meigs' “pet” words and phrases, which are for the most part so often repeated, that, had Falstaff been one of the young gentlemen to whom the Letters are addressed, he might have sworn, as on a former occasion: “O, thou hast damnable iteration!”

“Pet vaginal; scrobicle; maladive; delimitation; delimitary membrane; ovi-posit; intestinal tractus; life-status; status of texture; penultimate of life; wild heterologue; saccadée; ballooning of the belly; atmospheric mixt; privities; granular retinacula; cellular tela; panoptical; perpend; neurosity; vector of the ovulum; fecundative conflict; the uterus dispensing an aura; is it depôt? depôt unchanged; understandable; uncognoscible; clergyable pride; clerkly; clergyableness; macilent; intempestive; hypogaster; outsider classes; *quo modo* disparitions; auscult; ausculted; gestasio-ventosa; strange vitality; powers of the stroma; endangium; endangial malady; endangitis; endangial membrane; lila-porule; arterioles; venueles; prava ingesta; somatic innervation; heterologue organ; a good index expurgatorius.

The blood is confined within and bounded by the endangium, which is the delimitary membrane and its manufacturing apparatus. The endangium, as solid, contains the cause of the hæmatisis, in the fœtus, and *a fortiori*, in man.

The arterial system, by injecting oxygen, develops the brain and the whole nervous system, the flash of life: where its injecting force is greater, the life is greater; where it is less, the life is feebler.

Dr. M. thus speaks of a tympanitic lady :

You are not pregnant ; you are resonant upon the whole abdomen, down to the very hypogastrium, and there is no click to the foetal heart. The motions you have felt and the distention you have suffered were motions of borborygmi and the inflation by gasses, &c.

The stroma of ovaries is the true sexual tissue of females ; in fact, that stroma is sex for them ; after a long tractus its strange vitality and economic connection.

It is clearly a case of physical sin, and absolute rebellion against the specific authority and laws of the economy.

The brain will, generate and radiate its nerve force, its biotic force to the organisms : the aphrodisiac power to a status of texture.

The whole spinal cord vibrates under the tension of the vital forces. The motor cords excite the muscular tissues, the sensitive cords feel the pressure. This is an after pain. *An after pain.* therefore, is a good thing and a natural.

The uterus *grows* smaller and smaller.

The surgical cervix [of the womb] projects half an inch.

Burdach is a German Kanteio-Schelling-ian-Okenian physiologist ; cure your hæmatomic membrane ; your hæmatomic tissue is not in health ; your endangium is out of order, weak and pale ; the endangium perfect ; no, siree ; tilly-vally ; exquisite citadine ; artistical imagination with perfectionings ; no neurosity developed ; immiscence ; literary martinets.

Suppose that Dr. Meigs, in asking Miss Helen Blaque to make him a cup of tea, were to use the following language : “ add to *quant. suff.* of *thea chinensis*, the oxyd of hydrogen saturated with caloric.”

The witty Helen would quote Shakspeare, thus :

“ An honest tale speeds best, being plainly told.”

Tautology.—I have read the balance, of the rest, of the residue, of the remainder of Dr. Meigs’ first letter.

“ Damnable iteration,” says Falstaff.

“ In front, the vagina is attached to the bladder and urethra ; behind, it touches the rectum, and is soldered to that gut.” There can be no need in the world to tell young gentlemen that a soldered thing touches—the greater implying the less. “ The womb contracted by the tonic action of its muscular fibres ; (equal to water turned into ice by the abstraction of free caloric,) “ hyperæmic status ;” (the status is implied or rather constitutes the hyperæmia,) “ functional forces ;” is in the same category : so are the phrases “ injuriously deranged organs ; chronical, long continued maladies ; uterine globe ; modifications of altered organs ; very enormous ; recumbent rest ; I am opposed to

ovariotomy on grounds of objection; an epidemic cause is uncognoscible, recondite, and beyond the scope of the human understanding; direful effects of life-force; the life-force excited with preternatural energy; the tension, and pressure, and intrusion, gave her much pain; quasi organization, and losing the power of increase from atrophy of the most distal and attenuated branches; vile quackery and charlatanism; a great moiety of cases; stringent necessity," &c.

Dr. Meigs, in one part of his book, utters the most violent maledictions against medical statistics, while nearly every letter is full of statistical tables, reasonings and conclusions, concerning the duration of pregnancy, therapeutics, post mortem appearances, convulsions, change of life, &c.

Dr. Meigs urges with great pertinacity the advantages that must result from the popularization of medicine, than which few errors can be more mischievous. With these views, it is remarkable that Dr. Meigs should use a style which is a polyglot of Greek, Latin, French, German, Italian, and other tongues unknown to the people. Dr. Meigs recommends the making of anatomical drawings and diagrams at the bedside, illustrative of the organs that may be affected by disease, as the vagina, bladder, rectum, and represents that these have convinced the understanding and won the confidence of his patients. Hunter dissuaded medical students from all these pictorial illustrations of anatomy, before having seen the dissected subject, as the impressions made by the former method must be erroneous, and must be effaced or unlearned, before the true anatomy could be fixed in the mind.

That Dr. Meigs may have met with female patients who were polite enough to compensate him for his pains, by assuring him that they comprehended his anatomical and physiological illustrations, is not wonderful, since this would be mutually complimentary to the teacher and his pupils. It is, however, probable that out of the female medical college of Philadelphia, there is not a lady in that city who understands the anatomy and physiology of her womb, excepting perhaps one midwife in five hundred. All such labors at the bedside, and in the school-rooms, must fail in conveying that exact information required to form a sound judgment. Indeed, books on domestic medicine are, for this reason injurious, because they cannot be compre-

hended by the pupil, while they shed an uncertain light, which confuses rather than guides in the hour of peril.

Dr. Meigs' book on Woman is rich in suggestions, in physiology, pathology and therapeutics. In the physiology of the nervous system he marches with the multitude of excito-motor mystics. He appeals to "neurosis" in all his difficulties; says much of the sensory and motory cords, as if they really existed as anatomical facts; he tells you how to treat the motor, and how the sensiferous cords, as if there could be no sin committed in dynamical neurology, nor in anatomical neither, though he admits of "*physical sin.*"

Nevertheless, Dr. Meigs' books sell—they ought to sell—being among the very few which make readers think, reason, feel, and act in the right direction.

The moral teachings of Dr. Meigs are pure, elevated and apostolic. It is here that his earnestness gets the advantage over his hard words and exuberant neologies; while passing from "grave to gay, from lively to severe," he touches the chords of the human heart, which make "the whole world akin." Here, too, the English idioms abound. The waywardness, the weakness, and sickness of woman are depicted with whatever dignity these subjects allow. In relating the occurrences in the sick room, which are sometimes comic as well as tragic, his playfulness and quaint humor never sink into vulgar witticism, or sensual hilarity. The tear and the smile are twins without difference.

In his bedside pictures, where danger is imminent, his directions are like his style on these occasions, clear, graphic, and simple. Dr. Meigs' personality often appears in his book, but without intrusion, and without incurring the charge of egotism. The reader wishes to know the inmost thoughts, the private opinions, and the individual experiences of such a gifted man; and, if like Bossuet, he were to apostrophize his own gray hairs, no one would complain.—[EDITOR.

Part Fourth.

MEDICAL INTELLIGENCE.

Art. I.—*Professor Meigs' Protocol* to Critics.*

PHILADELPHIA, May 6, 1854.

Dear Sir:—As it is common to all men to prefer commendation rather than reproof, I could not well avoid a feeling of regret not un-mixed with surprise on receiving your note of the 1st inst., which was marked “confidential.” I regret in the first place, that your Journal should be the medium by which I am to be assailed, and I was surprised to find that you should use it against a work of mine with which you are apparently not dissatisfied. At least I gather from the tenor of your letter that you do not disapprove of the tract in question.

I am much obliged to you, sir, for the favorable expressions and kind wishes contained in the closing paragraph of your letter, and beg to assure you you are quite correct in supposing that I “make a better use of ‘my’ time than those who read reviews of themselves after having written the best books extant.” I believe there are not a few reviews of my publications that I have not read, and while it is true that I should thankfully receive and strive to improve every truly obvious suggestions in the way of emendation, I confess I have but very little concern in the opinions of angry and unreasonable or incompetent writers of criticism, some, of which, I have found to be beneath contempt for knowledge or temper exhibited by them.

I hope that I have not by any one been charged with the indecency of praising my own writings which have often been the subjects of very sharp comment. I know and admit that my writings have many faults, but I claim that even were I a good writer, I have been too

* The Editor is responsible for the caption of Dr. Meigs' letter. The Editor's note to Dr. M. was unnecessarily marked confidential. It communicated the isolated fact that the review was asked, not because of the reviewer's dissent, but because of the reviewer's ability. Those who turn to page 837 of the May number of the Journal will see the propriety of the note mentioned.

Dr. Meigs had not seen the review, nor did he know who the reviewer was, nor could he have been apprized of any special ground of dissent, at the time he penned his letter, as will appear in the sequel.

busy a man to write with care or with very special regard to the manner of expressing my thoughts. If I should have waited for time to write, I should never have made public a line on medical topics, and yet, as you know, I have written a good deal: perhaps I should have been a wiser man if I had never published a paragraph on medicine! and were I governed only by the opinions of these young gentlemen of our brotherhood who *do* most of the American medical reviews, I should long ago have resolved never thereafter to open my mouth in their presence, but holding my peace, leave them alone in their self-sufficiency.

It is a difficult thing for a man to judge on a question of this kind. Here now are young people in New York and Virginia, and elsewhere, who review not my books only, but me, even when my books are not in the caption; and who inform the public that I cannot write English, and that what I do say is wholly unintelligible, and worse and worse, that what I have written is "unworthy of his (my) eminent position."

I have not claimed to be in an eminent position, saving and excepting only, that I shall ever deem it a fortunate and creditable circumstance that I am sustained by my colleagues of the College, conjointly with whom I have labored as a public instructor of students of medicine, in perfect harmony and concord for a great many years. This I presume, I may without vanity, be allowed to regard as an enviable position, seeing that our medical brethren in the States do send to us a great number of their pupils, which is a certain mark of their confidence and respect.

I know not then what these young gentlemen mean by "his eminent position," unless they be pleased to refer to my writings, which nevertheless they do reprobate, and I might say, trueulently condemn and destroy—if they be indeed, destroyed by these public spirited and most learned guardians of our sacred fane!

What would you have me to do Dr. Dowler? Shall a man lay his hand on his mouth and his mouth in the dust, because a * * * writer of squibs shall deem him unworthy of his "eminent station."

I do think that Heaven knows I never wrote for my own sake, but for the sake of my brethren, to whom, I owe an unpayable debt of thanks and grateful respect for their goodness, by me scarcely deserved. I say that I am deeply in debt to my medical countrymen for the some thousands of their students whom they have permitted to hear my public lectures, and for their approbation of my writings, most clearly expressed in the fact that they have taken 15 or 20,000 volumes of them from my booksellers, and are now asking me for others that I am preparing to send them. In fact I have just finished for the binders a new and enlarged edition of my *Letters on Woman*, which I hope may be found emended as well as augmented, for it was much abused, with the rest.

I repeat then, what ought I to do? Am I to believe the young gentlemen, the sophomore *scollards* [?] who assail me, or may I not

venture rather to rely on the seniors, my brethren, who buy 20,000 volumes of my medical tracts, and ask me for others that are forthcoming. I have too good an opinion of American doctors to think they would purchase so large a library that has in it neither English nor common sense.

As to the particular tract which you tell me is to be reviewed in your forthcoming number, I will be so weak as to confess, I should be sorry to find it a failure, not on account of the personal mortification merely, but because I have good reasons to believe it contains much sound and wholesome instruction, well fitted to aid the young and inexperienced brethren in a difficult department of clinics; wherein many, nay the majority of us, commit the most scandalous blunders and do the most blameable mal-practice.

I hope I have not the least desire to rescue the volume, however much I confide in the principles and methods which I have inculcated, from a condign condemnation. Yet I confess it is hard for me to understand how it should be, that, while supposing myself to be very intimately acquainted with the history and bibliography of that particular subject, I should make the grave mistake of regarding the book as not only a useful but an original and novel exposition of these matters, if it should in the end prove to be not worth a rush. Assuredly considering the place I have long occupied as a practitioner and teacher, the duty I owed to my brethren of being a man of studious habits as well as a careful observer of diseases and results of treatment, I ought by this time to have learned something worthy of being told to others. Still, your reviewer may be a person far more variously and accurately informed than I, and so, prove himself quite able to show that I have learned nothing in cases that have attracted much of my attention for many years. Let him in that case, cut my book into shreds if he will; I shall endeavor to think no evil of him on account of his evil intent towards me, or my book rather. If he rails at us, much happiness and self-gratulation may he find in his railing. I shall endeavor to find contentment, nevertheless, and to that end perhaps I might do well to read in the Bible:—In the 2d Chapter of II Book of Kings, I shall find a story concerning the prophet Elisha; he was old and well stricken in years, and so am I; he had a bald head, and so have I; he went on his way in the world, and so do I; he met angry and naughty boys, so have I; they scorned his gray hairs and hooted at his bald crown; probably they thought him unfit for his “*eminent station,*” and they cried out upon him, “*go up thou bald head, go up thou bald head.*” The prophet turned and “*cursed them,*” so do not I; and the Lord sent two she bears out of the mountain and “*they tare forty-and-two of those children that day.*” I am very sorry for the poor dear little Jew boys that were torn, and I hope my reviewers may keep clear of all such, and other vermin. And I even go so far in humanity as to trust, humbly, they will not feel themselves hurt by the reflection

that their brethren and mine have bought some 20,000 volumes of medical works from a writer whom they so greatly disapprove.

I heartily reciprocate your kind wishes for my welfare, and while I regret you should use your Journal to do me hurt and damage, I am not the less an admirer of your talents and industry, and I rest with respectful consideration,

Your serv't,

CH. D. MEIGS.

Dr. B. Dowler, New Orleans.

P. S.—Were it not that you have marked your note to me “confidential,” I would invite you to use your pleasure as to the insertion of this into a number of your Journal—not that I am desirous to defend my book against criticism, but only in the view of saying what I believe to be quite true—that I have reason to look upon my writings with less doubt as to their usefulness, on account of the undeniable fact that they have met with considerable favor at the hands of the medical public in our country. Yet after all, perhaps your reviewer may have been pleased to say nothing that I should not be willing to agree to. In that case I should have no answer to make.

C. D. M.

Art. II.—*Experiments showing the Contagious Properties of the Splenic Blood of Sheep affected with Charbon.* Translated for this Journal, from the *Revue de Thérapeutique Médico-Chirurgicale*, of May 15th, 1854. By C. R. NUTT, M. D., of New Orleans.

The following experiments, reported in the Academy of Medicine and in the Institute, have produced a great sensation, not only in the agricultural world but in the highest circles of the medical corps. M. Godeau says: About one year since I made known to the Society the labors of certain physicians and veterinary surgeons in the Department of Chartres, in relation to the disease known as charbon, among our domestic animals. A commission was appointed, of which I was chairman, for the purpose of laying before the public the result of their joint labors upon the investigation of the causes of “charbon,” and to ascertain if it belonged to the class of septic poisons.

The cause of the scourge which decimated the flocks of Beauce, Brie, Picardie, and a large portion of the wool-growing districts, was considered by Prof. Delajond “to be a sanguine plethora, produced by too nutritious grasses, resulting in the too great a proportion of globules, fibrin and albumen of the blood.” According to this view of the disease, it was not considered contagious or communicable to

man or animals. From its rapid progress, frightful ravages, simultaneous existence in the same localities in which the malignant pustule of man was found to occur with individuals tending the flocks, or who had handled those diseased, or inoculated from the blood or pus taken from the spleen, the physicians and veterinary surgeons of Beauce were at once led to suspect its contagious nature.

The experiments instituted for this purpose have shown that this disease of sheep, the charbon of the horse, and of the cattle, is caused by a septic poison, and is communicable to both man and animals. With a view of ascertaining the identity of the malady of Chartres with that of Beauce, a commission, of which M. Godeau was chairman, was ordered to report, and also upon the improved hygienic treatment.

Experiment 1.—On the 29th of July, at 11 o'clock, A. M., a sheep and two rabbits were inoculated with the blood taken from the spleen of a sheep, which, with seven others, had died the day before on the estate of Mme. Pocheron. Seventeen hours after death the inoculation was made on the inside of the thighs, by two or more punctures. On the 1st of August, the sheep died, at 4 o'clock, A. M., sixty-five hours after inoculation; and the rabbits on the 31st of July, at 9, P. M., fifty-eight hours after inoculation. Autopsy of the sheep:—Petechiæ on the surface of conjunctivæ; spleen exhibited diffused softening throughout its structure, yielding like pap on pressure; mesentric glands softened; throat highly injected; serous fluid of a reddish cast found in the abdomen, &c. The autopsy of the rabbits showed no appreciable lesion except in the blood, which was thick and turbid.

Experiment 2.—On the 1st of August, immediately after the autopsy of the sheep and rabbits, at 10 o'clock, A. M., a sheep and two other rabbits were inoculated as before, with the blood of those which had died from inoculation. The sheep died on the 3d of August, at 5, A. M., forty-three hours after inoculation. The rabbits died the same day, two hours later than the sheep. Autopsy showed the same lesions as in the former cases. A number of similar experiments were tried upon sheep and rabbits with slightly varying results. On the 3d of August, the blood from the spleen of a sheep was used to inoculate a young and vigorous mare. The punctures were made on the inner side of the thigh. The mare died on the 7th of August.

Autopsy of the mare :—Ædematous infiltration diffused throughout the sub-cutaneous cellular tissue, with effusion in the abdomen of a small quantity of sero-sanguineous fluid ; softening of the mesentric glands and spleen.

Experiment 6.—A she ass was inoculated with pus taken from the spleen of a sheep which had died at Mme. Mapé's. The punctures were made on the inner side of the thigh, and on the left side of the chest, near the medium line. The ass was inoculated on the 18th, and died on the 22d of September. It presented an enormous abscess on the left side, having all the characteristics of charbon. The autopsy was the same in appearance as that of the mare. From an incision made in the tumor, which presented a yellow infiltration, an infectious odor escaped, produced by the disengagement of confined gas.

Experiment 7.—A sheep and two rabbits were inoculated with the blood of the ass, on the 23d of September, at 6 o'clock, P. M. The sheep died on the 25th of September, at 11 o'clock, P. M., fifty-three hour after inoculation ; and the two rabbits on the 26th of September, at 5 o'clock, A. M., six hours later.

Experiment 10.—On the 7th of August, a cow died on the estate of Bijou, whither, some days previous the flocks of Mme. Pocheron, decimated, as we here stated, were brought. One sheep and two rabbits were inoculated with the blood of this cow. On the 8th of August at 7 P. M., the inoculation was performed. On the 10th, the rabbits died at 5 o'clock, A. M., thirty-three hours after inoculation. The sheep died the same day, at 3 o'clock, P. M., forty-four hours after inoculation. It is a fact worthy of remark—the simultaneous death of the rabbits in each experiment, two minutes scarcely intervening.

A summary of the experiments shows that thirty-five inoculations with the blood of the spleen, in the first and second degree, were practiced, of which nine were upon sheep, twenty upon rabbits, one upon a mare, another upon ass, and four upon chickens. All these animals perished with the exception of two chickens inoculated from the rabbits. The experiments are not as numerous as we desired, in consequence of the small number of flocks, compared with years past, attacked this summer, though the disease was unusually fatal. In the prosecution of these experiments all means were used to procure animals isolated from the disease, and those brought from abroad were

the subjects of inoculation. The following facts, concerning the disease, as it prevailed at Mme. Pocheron's, show very clearly its communicability to man. On the 28th of February, at the request of Mme. Pocheron, I visited her farm, where, upon examination, I found that eight of her sheep had died of charbon. On the same day, a woman on the farm had introduced her finger into the rectum of many of the sheep, for the purpose, as she stated, of disengaging the confined air, which was killing them. I advised Mme. Pocheron to have her flocks moved to Bijou, to low marshy grounds to pasture, which she accordingly did the following morning. The Juncaceæ and Equeselaceæ (rushes and scouring rushes) abounded in these grounds, and other aqueous plants, very different from the valley of Subdray, where the soil was dry and calcareous, affording a rich aromatic and highly nutritious pasturage. This was done in accordance with the views expressed by M. Delajond of the cause of the disease. No benefit, however, was experienced by the change, and as the mortality continued to decimate the flock, Mme. Pocheron, frightened by its ravages, called another veterinary surgeon to my aid, who treated the whole flock without mitigating, in the slightest degree, the violence of the disease. The per diem mortality was fifteen to twenty, when the flock, on the 2d of August, was driven back to Subdray. In despite of medical treatment, and the removal of the flock, which consisted of four hundred, the disease continued till the entire flock perished. On the 2d of August, the woman who had applied her finger up the rectum of several sheep, as before stated, for the purpose of relieving them, fell sick and complained of great prostration, (said she was foundered,) continued to grow worse in spite of medical treatment—the charbon fever manifested itself, and only terminated with her death, on the night of the 3d and 4th of August.

An infant, aged three years, which had amused itself with steeping its hands in the blood of the sheep, which had been skinned, was attacked on the same day (2d August) with malignant pustule on its upper lip. Grave symptoms supervened, but it was relieved by repeated applications of the actual cautery. The following Saturday, August 6th, a shepherdess who skinned many of the sheep was soon after attacked with a malignant pustule on the back of the left hand, accompanied with grave and alarming symptoms, when she too was

relieved by the use of the cautery and the most active treatment, by my colleague, M. Gerin.

On the 11th of August, another shepherdess was attacked with malignant pustule on the chin, produced by the spattering of blood, two days previous, whilst aiding me in opening the sheep. This was likewise relieved by repeated application of the cautery, by M. Gerin. Whilst at the Subdray the contagious nature of charbon, from animals to man, was thus strikingly illustrated, at Bijou, whither the flocks had been removed by my advice, the disease appeared in a horse and a cow, both of which died, the one on the 11th, and the other on the 7th of August. The blood of this cow afforded experiment No. 10, as above related. In the history of this malady, it appears that for many years past, its ravages here have not been confined to sheep, but have extended to cattle and horses, with terrible mortality.

In 1846, M. Adolphe Mignon lost one-fourth out of eight hundred sheep; seven horses out of twenty. In 1850, M. du Tronçay, at Prioul, lost three hundred and twenty out of three hundred and fifty, five horses out of seven, and one bullock.

These facts, resting upon the most satisfactory testimony, go to prove in the most conclusive manner, that the physicians and veterinary surgeons of Beauce have shown that the malady among the sheep is identical with the charbon of the horse, malignant pustule of man, and the malady of cattle. That each and all these are the result of a septic poison, capable of being communicated from animals to man, and from man to animals. Every variety of treatment was exhausted in combatting this disease, without results sufficiently satisfactory to offer any mode of cure to the public. We trust, however, that further investigations into the nature of this disease will enable us to mitigate, if not to cure this terrible malady.

GODEAU.

Art. III.—*Obituary Notice—Prof. H. S. Patterson:* By H. J. RICHARDS, M. D.

With feelings of deep regret we announce to the Southern medical fraternity the death of Dr. H. S. Patterson, of Philadelphia; late Emeritus, Professor of *Materia Medica* and *Therapeutics*, in the

medical department of Pennsylvania College; Fellow of the College of Physicians; and Recording Secretary of the Medical Society of the State of Pennsylvania, &c.

Dr. Patterson's health had been in a declining state for the past three years. A voyage to Europe in the summer of 1852, so greatly restored his powers as to permit himself and his friends to hope, for a brief period, that the progress of his malady had been stayed; but the resumption of his arduous professional and literary labors overtasked his physical strength, and he was compelled to abandon the active exercise of his profession. In July, of last year, he resigned the chair of *Materia Medica* in the institution with which he had been connected, in the conviction that he was taking a final leave of the scenes and pursuits of his daily life. Nevertheless, in his resignation of its hopes and prospects; amid the distraction of pain, and his anxiety for a young and cherished family soon to be deprived of their natural protector, his ceaselessly active and disciplined mind was ever projecting and executing. The biographical memoir of the late Dr. S. G. Morton, prefixed to the late work of Gliddon and Nott, was undertaken in the scarcely formed hope of his ability to complete his "labor of love." The revision of a large portion of the proof-sheets from the stereotyper, as stated in Mr. Gliddon's preface to that work, also occupied and beguiled a portion of the weary and monotonous hours of his sick chamber. In a letter to the writer, dated in February, of this year, he stated that he was occupying himself in the intervals of his paroxysms of acute suffering in collecting and arranging materials for a work on the *Cabala*. The fresh page that every day of life presented was never left blank; until upon the last leaf of his life's record, is inscribed the melancholy "FINIS"—it is ended.

Dr. Patterson's position as a practitioner and teacher of medicine was prominent in that city which, probably, presents the finest array of medical talent, and may be regarded as the medical metropolis of America. As a practitioner he evinced those practical qualities which are to be attributed to long and careful clinical observation, combined with a strictly scientific course of study. As a teacher he was the reliable exponent of current medical literature and opinions: and the steady opponent of empiricism in all its phases. His lectures

were frequently revised, and enriched from the French and German schools of pharmacy, whose literature he regularly perused.

Scarcely a month has elapsed since the subject of our brief notice made his last living sign to the world. From a bed of sickness and suffering he uttered his testimony—since hallowed by his death—to the worth of a departed contemporary. The friend and biographer of MORTON, is now entitled to the same friendly tribute, which he so cheerfully and eloquently recorded to the memory of his fellow and co-laborer.

We cannot doubt, that from among the many friends to whom he was in life endeared—by his goodness and amiability of disposition, no less than by the worth and more shining qualities to which we have briefly adverted—a willing and competent hand will undertake the discharge of this grateful duty.

To the above graceful tribute to departed worth, the editor subjoins the following memoranda, abridged from the June number of the New Jersey Medical Reporter :

Dr. Patterson was born in Philadelphia, Aug. 15th, 1815; received the degree of M. D. in the University of Pennsylvania, in 1839, and the honorary degree of A. M. from Newark College, in 1843; in the same year he became professor of *Materia Medica*, and acting professor of chemistry in the Pennsylvania College of Medicine; in 1846, physician-in-chief of the Blockley Alms-house. Owing to indisposition he resigned his professorship, traveled through Europe in 1852, in pursuit of health and scientific information. The next winter he spent in Florida, with little improvement in his health; whence he returned to the bosom of his family, and sunk into his bed where “he wrote the inimitable biography of the late Samuel George Morton, and two critical notices of the ‘Types of Mankind.’”

One of these was published in the last number of this Journal. All his previous bibliographical notices for the “Reporter,” were written in his chamber. We will view him at this point for a moment, where we now are—in his chamber. We saw him, wasted by disease, and yet firm in mental power. He spoke to us of his sufferings, as a victor speaks of conquered enemies: they may have been strong, but the mind had soared above them. His words were few but they *spoke*. Some words sound only; *his*, uttered meaning. As he entered the stream that separates the noise of time from the stillness of eternity, and felt its waters to chill his blood, and its cold waves to approach

the centre of life, the spirit still seemed to hold its gaze upon an unending existence, and with a firm and steady, yet quiet advance, he passed away, and left these words of the psalmist to answer the question—How did he die? “Thy rod and Thy staff, they comfort me.” Thy rod for discipline, Thy staff for support; they both comfort me.

A word about his acquirements. In the profession of his choice, Dr. P. was thoroughly versed. Its history was familiar to him; and in discussing the doctrines of those who have given to our profession its *caste*, in times, ancient and modern, in all countries, he seemed quite at ease, both in social intercourse and in the lecture-room. His knowledge of the Latin and Greek languages enabled him to cull from the past, much that was embellished by his own rich style, in the various literary and scientific productions which have emanated from his study. The Hebrew he knew so familiarly as to be able to read with a critical eye, its translations; and to dwell upon the imagery of this virgin tongue with delight and profit. In the modern languages he was quite a proficient: the French, German and Italian, he read and spoke with ease, and had so acquainted himself with the classical poetry of the latter, as to be always ready to render quotations off-hand, but without ostentatious effort at display. In Egyptian philology he found much to interest his taste for studying oriental symbolisms, and had his life been prolonged, he would probably have published his notes on the subject. In the practice of his profession he was gentle, kind and skillful.

His Disease.—When a prominent member of our profession passes from earth, it is expected that his disease should be known to his professional friends, and that the post-mortem revelations made by the scalpel, should be the common property of the profession. With a few exceptions, we believe this is usually the case, and we admire the devotion to science, manifested by domestic and personal associates who cheerfully allow the profession to use these means for advancing the common interests of our race. In presenting the following statement, we do not write from any examination into the diseases of Dr. Patterson, made by ourself, but from information obtained from his physician, friend and colleague, Prof. Darrach, of this city. In April of last year, (1853,) Dr. Darrach made a critical examination of his case, and gave to his family and friends the following diagnosis, which was repeated before the college class, in an introductory lecture, delivered in October last, as a reason for Dr. P.'s resignation from the faculty. He declared his retirement to be owing to “pleuritic and cardial sequelæ of rheumatismal influenza, which, however much it embarrasses the mechanism of the arterial circulation, and emaciates and weakens, has not lessened the pulmonary function, nor in the least abated his emotions, passions and intellect.” Dr. P., was exposed during a stormy night in 1844, and “contracted an influenza which established a liability to attacks of neuralgic rheumatism.” There followed upon this an “insidious, painless, chronic pleurisy, which plastered down the right lung; and, subsequently, a similar persistent

transmutation, was made upon the valves of the heart, to embarrass their action." These sequelæ, according to Dr. D., "occasioned an atrophy of the right, and a vicarious enlargement of the left lung, which displaced the heart to the middle sternal region of the right side of the chest.

The Autopsy.—Thirty hours after death, the emaciated frame was exposed for dissection, and the following appearances are reported. (The cranium was not examined): "*Thorax*—heart, moderately enlarged, displaced towards the right side, extending one and a half inches beyond the articulation of the right ribs, with the cartilages—pericardium healthy, about two ounces of fluid in the cavity—right cavities; auricle, filled with a very dark colored heart clot; endocardium, normal; walls thinner than normal—ventricle, walls thinner than normal—tricuspid valves not affected—clot clinging to chordæ tendinæ—semilunar valves of pulmonary artery red, thicker than normal, end fleshy to feel—long clot in the pulmonary artery—left cavities; auricle empty—endocardium normal, except mitral valves, which were thickened with deposits; the deposits being in lumps or beads along the edges, and causing the valves to curl upon themselves. Ventricle, nothing peculiar. Valves of aorta, red, thickened and fleshy to feel; the redness of these valves, together with those of the pulmonary artery, could not be washed off by water, and they presented a strong contrast with the color of the rest of the lining membrane of their respective arteries.* This condition was more morbid in the valves of the aorta than in those of the pulmonary artery.

Lungs—right firmly bound down by adhesions; being almost one-fourth the natural size. The lateral diameter greatly diminished: the vertical slightly. At the apex, the air vesicles were very much dilated, and few in number; the great portion of lining tissue being absorbed. No evidence or sign of tubercle. Lung did not crepitate, the lower portion being tough and carnified. It contained air sufficiently to float it in water. At the postero-inferior portion, was a patch of lardaceous membrane, of about the sixth of an inch in thickness. Left lung adherent, but by no means as firmly as was the right; hypertrophied and crepitant throughout—much congested and œdematous. No evidence of tubercle in any part of the organ. The adhesions of both lungs were so firm, that much of the costal pleura was torn out in the attempt to liberate them. Bronchial tubes—fine injection—fibrous appearance internally, with hypertrophy of follicles.

April 28th, 1854.

* How true soever it may be, in this particular case, that the color mentioned was altogether morbid, it should be constantly borne in mind, that very soon after death, as well as at later periods, the membranes of the heart and its appendages, and, indeed, almost all the tissues and organs, may form causes purely physical—as endosmosis, exosmosis, and gravitation, according to position—acquire an intense color, and become literally dyed like a piece of cloth. Washing will not remove this color, in many instances. The bowels become black or red throughout their extent from effusions of black vomit or blood—in the same way the bile often discolors or dyes surrounding tissues. The same causes which tend to efface morbid redness, especially where the post-mortem examination has been delayed, as in the above case. Another cause, which often tends to obliterate vascularity, turgidity and redness, is that of vital capillary action, which persists after ordinary death.—[*Editor N. O. Med. Jour.*

Art. IV.—Prof. SANFORD.—*Lithotomy.*—*The First Case of Lithotomy performed on the Male, in the State of Iowa.*

Joseph Harding, aged thirteen, who had been treated five or six years for stricture of the urethra, having been sounded, was found to have a calculus, which Professor J. F. Sanford, M. D., of Keokuk, an accomplished surgeon and scholar, removed, by the lateral method, while the patient was under the influence of chloroform. The stone weighed half an ounce. The patient soon recovered. The case is reported in the March number of the Western Medico-Chirurgical Journal.

This case has an interest and is worthy of record in two respects: first, there were some symptoms which are almost invariably present when vesical calculi exist, which were absent here; and, second, it is the *first operation for stone upon the male*, ever performed in the State of Iowa. This latter consideration will give it an interest in the surgical annals of our commonwealth in the minds of some who may not see, in the recital of the symptoms and surgery of the case, any thing new or out of the range of former observation.

Art. V.—*The Mortality of the City of Memphis, Tenn.*

Prof. Quintard's able report on the health of Memphis for 1853, published by order of the City Council, affords the following notice of

Yellow Fever.—It will be perceived that three cases of this disease, are recorded on the books. The first two cases had been exposed to the causes of the disease in New Orleans, and the third case, that which occurred in September, was returned by the sexton as bilious fever—as we were informed by the secretary of the board of health—but it was a case so like yellow fever that the secretary in his weekly publication of the deaths, reported it as such. There was a good deal of discussion at the time, as to the exact character of the disease. The occupation of the individual was such that he was frequently exposed to contact with persons from the affected districts. No quarantine regulations were adopted by our municipal authorities, although boats were landing the sick and dying at our wharf nearly every day, from New Orleans and the towns below us. Sixty-two cases were carried from the landing to the hospital, many of them in a dying condition. The first case admitted by Dr. Smith, hospital physician, was on the 24th of July. The last case recorded on the hospital books, was on the 28th November. Of the sixty-two cases

at the hospital, from boats, thirty-six proved fatal; and of this number, twenty died within twenty-four hours after admission.

Prof. Quintard in dissenting from one reputed cause of yellow fever at Natchez—namely, the grading operations, long pursued in that city—says:

Natchez has time and again been visited by epidemic yellow fever, and notwithstanding a rigid system of quarantine, it, with nearly all of the towns below us, was scourged by the pestilence during the last summer. There have been and are causes for this prevalence of the disease in Natchez, which do not exist in Memphis. Nor can this be traced to the grading. In 1851, the city of Memphis paid, for street-grading, \$27,806 99; in 1852, \$40,519 25; in 1853, 40,931 92. In 1851, the mortality of the city was 717; in 1852, 705; in 1853, 412. It is apparent, therefore, that notwithstanding the work of grading was vigorously carried on during 1853, the mortality greatly diminished.

Prof. Q. estimates the mortality of the city of Memphis in 1853, at less than two per cent., in a population of more than 15,000: the deaths having been 412.—*Report &c.*, 1854.

Art. VI.—*The American Medical Association.*

The American Medical Association, which met on the 2d of May, 1854, at St. Louis, adjourned after a session of three days. From the Iowa Medical Journal, the following items have been gleaned concerning the proceedings of that body:

Dr. Pope, chairman of the Committee on Prize Essays and Volunteer Communications, now made the following report:

Mr. President:—The Committee on Prize Essays and Volunteer Communications, respectfully report that the essays submitted to their consideration were nine in number, of which one was presented as a volunteer communication. The committee have carefully examined the whole of these essays, and bestowed upon them the attention which a sense of the importance of the duty assigned them imposed. They feel free to say that some of these essays possess undoubted merit, both in matter and style, and they admit in them evidence of high scientific attainment as well as a familiarity with the graces of composition. But whilst cheerfully according these claims to their authors, the committee have preferred to be governed in the choice, by considerations of originality and practical import, rather than of mere theoretic speculation, however finely portrayed. The committee have consequently concluded to award but a single prize. The essay select-

ed is entitled "An Essay on a New Method of Treating Ununited Fractures and certain Deformities of the Osseous System." It bears a motto in French.

If it pleases the Association, I will now break the seal of the packet superscribed by the same motto, and declare the name of the successful competitor.

Dr. Pope then broke the seal, and announced the name of Professor Brainard, of Chicago.

Dr. McPheeters moved that Prof. Daniel Brainard take the stand, and give the Association an abstract of his new mode of treating fractures, &c.; which motion was carried, and Prof. Brainard accordingly came forward, and in an able manner gave the requisite information.

Dr. Mussey, of Cincinnati, made a motion to suspend the order of regular business, to allow Dr. Linton of St. Louis, to express his views with regard to the pathology of the yellow fever. A suspension of business was made for this purpose.

He expressed his views very clearly and at some length on this subject. He advocated the idea that vegetable decomposition was not necessary to the production of the autumnal diseases of this country. He considered yellow fever nothing more than an aggravated type of bilious fever, caused by the retention of hydro-carbonaceous substances in the blood. In other words, the agencies producing yellow fever were Northern blood subject to the heat of Southern latitudes.

A motion was made and carried, that Dr. Linton be requested to draw up the substance of his remarks, to be presented to the committee on publication.

The following officers were elected for the ensuing year:

For President—Charles A. Pope, of Missouri. *Vice-Presidents*—E. D. Fenner, of Louisiana; N. S. Davis, of Illinois; William Wragg, of South Carolina; John Green, of Massachusetts. *Secretaries*—Edwin S. Lemoine, of Missouri; Francis West, of Pennsylvania. *Treasurer*—D. Francis Condie, of Pennsylvania.

Art. VII.—*Health of Philadelphia.*

At a meeting of the College of Physicians of Philadelphia, March 1, 1854, Dr. Ruschenberger presented the Annual Report on Meteorology and Epidemics, for the year 1853.

During the year 1853, the thermometer fluctuated between 90° and 94° of Fahrenheit's scale: the range being 85°. For the year 1851, the range was 87°; and, for the year 1852, it was 96°.

The mean temperature of the year was 54.56° F., which is 2.52° F. above the average mean of sixty years.

The quantity of rain and snow which fell during the year was 40.657 inches, which is 3.103 inches less than the yearly average.

The mean temperature of the year 1853 was 54.56° F., which is 2.52° F., above the standard mean.

The total number of deaths in the city and liberties of Philadelphia, according to the reports of the Board of Health, for the year 1853, is 9,711, which is 444 less than the aggregate mortality for the preceding year. Deducting the number of stillborn (571), the total mortality is 9,140; or, 515 less than for the year 1852.

Including the stillborn, 2,722 died within the first year, 968 between one and two years, and 1,018 between two and five years. Excluding the stillborn, 4,237, or, 46.33 per cent. of the mortality was of children under five years of age.

Between the 19th of July and 7th of October, there were 170 cases of malignant or yellow fever; of which, according to the valuable reports of Dr. Jewell, 128, or, 75 per cent. died.

The total number of deaths from every species of fever named in the reports of the Board of Health for the year 1853, was 1,005, or, at the rate of 10.99 per cent. of the total mortality.

The number of deaths from scarlet fever was 384, or, at the rate of 4.20 per cent. The number of deaths from this disease during the past four years has been at an average rate of 4.37 per cent. of the whole mortality.

Art. VIII.—*Human Petrifications.*

Dr. Porcher, one of the editors of the Charleston Medical Journal, late a traveller on the continent of Europe, in a communication to that Journal, dated at Florence, Italy, says:

The celebrated petrifications of human flesh, by Segato, are deposited here. The art died with him. I saw a table made with the different portions of the human body in mosaic—muscle, heart, liver, kidney, &c., quite easily distinguished. The corners are garnished with hardened brain, and a bit of kidney or liver decorates the centre. Another preparation was the entire head and bust of a young girl, with the skin rather whiter than natural, but its structure perfectly preserved.

Art. IX.—*Excito-Motory System of Dr. M. HALL.*

The following conclusions of Dr. Davey, "On the Excito-Motory System of Marshall Hall," are copied from the June number of the Virginia Medical and Surgical Journal:

That the excito-motory phenomena, as explained by Dr. Marshall

Hall, rests on a basis which even *he*, up to this time, has failed to recognize; that the reflex actions, properly so called, are nothing more than an amplification of the ordinary spinal functions. He denies the correctness of Dr. Hall's experiments; that the more complicated anatomy of the spinal cord, suggested by Dr. Hall, and sought for by Mr. Grainger, has not yet been realized: says that there is no necessity for any distinct division of the nervous (spinal) system, for the production of the excito-motory phenomena: that the exercise of the functions of the spinal cord is due to the peculiar influence of the ganglionic nervous system, exerted on its organism, &c., &c.

Art. X.—*Cholera.*

As the last sheet of this Journal was in the hands of the printer—June 28th—letters and journals both foreign and domestic, received at the office of the editor, seem to warrant the conclusion that there exist cholera zones to the North and Northeast, and to the South, and Southwest of New Orleans. The cases of cholera in the civil hospitals of Paris for twenty days, ending on the 3d of May last, were, for each successive day, as follows: 4, 7, 9, 11, 11, 17, 16, 21, 19, 24, 41, 76, 64, 45, 48, 26, 21, 23, 28, 18, 25; the number treated in the civil hospitals from Nov. 1, 1853, to May 3, 1854, amounted to 1,782: discharged, 661—died, 897—remaining, under treatment, 224. The 10th and the 11th arrondissements, which have suffered most from cholera, according to the latest accounts, are reckoned among the richest and most salubrious places, especially in the neighborhood of the government offices, (le quartier des ministères,) where several distinguished personages (hauts personnages) have died, and where the disease has been most prevalent. The oscillations of the disease in the military hospitals were analogous with those already enumerated in the statistics of the civil hospitals.

In several of the principal towns in Scotland, cholera has recently appeared. It seems, that in numerous places, as Boston, New York, Nashville, and the cities of Mexico and Kingston, this malady is assuming an epidemic character. From dispatches received in New Orleans, it appears that for forty-eight hours, ending on June 23d, eighteen deaths from cholera had occurred in Nashville, Tennessee. Prof. Quintard, of the Memphis Medical College, in a letter dated June 3d, at the Poplar Springs in Randolph county, Tennessee, says: "I have had about thirty cases of cholera on the plantation during the last three weeks. It is prevailing all through this section of country."

Although the sanitary condition of New Orleans has been favorable

since the close of the epidemic of 1853, and the more so as the summer advances, yet, in the winter, isolated cases of cholera occurred; they still occasionally happen, while the general mortality is rather declining than augmenting.

From the published reports of interments in the city of New Orleans for the four weeks ending with the 25th of June, the mortality has declined as follows: the week ending June 4th, 194; 11th, 184; 18th, 189; 25th, 170.

Forewarned, forearmed! Observe cleanliness! Unhealthy articles of diet, unripe fruit, exposure to the weather, and immoderate labor should be avoided at all times, but more particularly when cholera prevails. Take no nostrums—take a doctor. Medicines, miscalled cholera *preventives!* are cholera *incentives!*

Art. XI.—*A Voice from the Office of the New Orleans Medical Journal.*

Contributors will oblige the Editor by sending their communications to this office two months in advance of the time of publication, in order that they may appear in the first part of the Journal,—the printing of which begins immediately after the last issue. The Editor contrary to his wishes has been compelled to furnish the first articles in the present number, owing to the late period at which the original communications were received.

The labors of the Editor are sufficiently onerous without writing articles for the Journal, which is intended to be a medium for diffusing the researches of other and abler laborers in the field of science—a field comparatively new, varied, peculiar, and of vast expansions. In all the medical regions of the civilized world, from the Seine to the Ganges—from the more Northern States of the Union to the Scandinavian Peninsula, the medical mind has been, and still is, active—thousands of the richest contributions have been made to the common treasury of science. Is it so in this great region washed by the Gulf of Mexico? Are the medical men of this region willing to trail eternally in the rear, skulking from their duty? Aye, their duty! They have reaped the benefits of the labors of others. Are they not bound to make some return? If Southern physicians will neither write books, nor essays for Medical Journals, what proof will there be that the Southern medical mind is superior to or even equal with that of the North? Will a slavish dependance on the great masters of science in other latitudes win their respect? Never! The physicians of the South must be their *equals*, not their *apes*. Let each do his utmost to advance medical science, and our brethren of other States and climes will not withhold their respect and gratitude.—EDITOR.

ADVERTISEMENTS.

MEDICAL COLLEGE OF VIRGINIA.

At a meeting of the Board of Visitors, held at the College on the 16th day of March, 1854, the following resolutions were adopted :

Resolved, That the Board will, on the 25th day of May, proceed to fill the Chair of Materia Medica and Therapeutics, and that of the Institutes of Medicine and Medical Jurisprudence; and that notice thereof be given through such Medical Journals and Newspapers, and in such other mode as the President of the Board may deem proper.

Resolved, further, That all applications and testimonials, in reference to the proposed appointments, be directed to "the Secretary of the Board of Visitors."

THEO. P. MAYO, M. D., Sec'y Board of Visitors.

Richmond, Va., March, 1854.

ORTHOPÆDIC INSTITUTION,

FOR THE CURE OF DEFORMITIES,

Nos. 457, 459 and 461 Pacific Street, South Brooklyn,

Afford to in-door patients, afflicted exclusively with bodily deformities, domestic accommodation, Orthopædic Apparatus, and a superior and attentive medical treatment hardly procurable at a private home.

Office hours, before 11 o'clock, A. M., for private patients only. The poor attended gratuitously on Tuesdays and Fridays, between 3 and 6 o'clock P. M., when clinical instruction will be given to medical students, who may favor the institution with their presence.

The profession at large is most respectfully invited to honor the institution with their attention and visits. Physicians of the Institution:

L. BAUER, M. D, Physician and Surgeon, (Berlin,) formerly Surgeon of the Royal Orthopædic Institution in Manchester, Great Britain; Member of the Royal College of Surgeons, of England; Fellow of the Medical Society of London, etc.

R. BARTHELMESS, M. D, Physician and Surgeon, (Wurzburg, Bavaria,) formerly Assistant Physician of the Lying-In Hospital of the University of Wurzburg, and of the City Hospital, Nuremberg, etc.

UTERINE COMPLAINTS, AND DISEASES OF FEMALES.

From fifteen years experience specially devoted to the study and treatment of Uterine Diseases in all their various forms, and complaints peculiar to Females, the undersigned has been induced to establish an institution for the purpose of combining every possible advantage and means requisite for successful treatment and cure of this intricate class of disorders. Patients from abroad can be accommodated with board, and every facility desired, to suit their various cases.

Lying-in patients also received, and offered the best of professional treatment and nursing.

If medical gentlemen at a distance should recommend patients, they will confer a favor by sending a statement of the disease and treatment.

A. STONE, M. D.

Brooklyn, New York State, No. 343 Gold street.

THE PRACTICE OF MEDICINE. WHAT IS IT?

The regular Practitioner of Medicine has almost insurmountable difficulties to contend with, in the fact, that his prescriptions are necessarily little better than experiments, more particularly the Physician of the South and West from the fact that the purity and strength of Medicines vary so very materially.

It is well known that Laudanum is usually made from the most inferior and unsaleable pieces of Opium, which seldom if ever contain half the proper quantity of morphine.

Rhubarb is powdered from roots varying in price, from 20 cents to \$1 00 per pound; the lower prices, of course, more or less decayed and worm-eaten; and thus, being unsaleable, are powdered and colored to sell 'cheap.'

Instead of Jalap, large quantities of the "Spurious Jalap," and also of a variety known as "Jalap Tops," are sold at about one-quarter the price of the true Jalap.

For Peruvian Bark, at least a thousand pounds of the worthless, inert Carthagena and Maracaibo Bark, are ground and sold for every pound of the true Peruvian Calasaya Bark.

Now, if these are facts—and they certainly are well-known facts, and very serious facts too—how is it possible to prescribe with any certainty? Is prescribing with such Medicines anything else than experimenting.

And that they are facts, is abundantly proven by the Report of the Custom House Inspector of Drugs and Medicines, and also by the Report of the Special Commissioner to the Secretary of the Treasury, on Adulterated and Spurious Drugs; which Reports, shocking and humiliating as they are, do not show a tithe of the facts in regard to the wholesale adulteration of Medicines.

The Report of the U. S. Examiner says:

"Such sir, are the fruits, thus far, at this port, of the wise and eminently beneficial sanitary measures, so heartily approved of by every friend of humanity; that measure, too, which met from its inception, the open, determined and unremitting hostility of a God-forsaken portion of our trading community. From the moment the question was first agitated, and during the progress of the bill through Congress, intense excitement and ill-feeling was manifested among certain importers and speculators who had long made the murderous traffic not only a source of profit but of wealth, and no means were left untried by them calculated to defeat the measure.

"Most persons, we admit, can judge very correctly, by sight, of the quality of most articles of food and clothing; but where is the man who can, by simply looking at the almost countless number of medicinal preparations, chemical and otherwise, say whether they are pure or adulterated? or by looking at the various preparations of morphine, say whether they do or do not contain five, ten, or twenty per cent. of *amygdaline*? or can detect by sight, *corrosive sublimate*, *prepared chalk*, *gypsum*, and other impurities in calomel? or can by sight say whether blue-pill mass contains its full equivalent of mercury, or only one-fourth or less of the requisite quantity? or can say whether hydriodate or iodide of potash is pure, or is adulterated by the admixture of *sal acetosella*, *sup. tartrate* and *sulphate of potash*? or can in the same way detect *salicine*, *mannite*, *sulphate of barytes*, and *oxide of zinc* in sulphate of quinine? or can say whether Croton oil is, or is not, adulterated by the admixture of inert *fats* or whether it is, not, in fact, an entirely *fictional article*? or by looking at the powdered cinchona bark, say whether it is genuine powder of that species which affords the largest quantity of quinine and some cinchonine, or whether it contains thirty or fifty per cent. of the powdered *Maracaibo* or *Carthagena* bark, which affords but a trace of either of these important alkaloids, and is consequently worse than worthless for medicinal purposes; or whether it is not, in fact, composed entirely of the latter worthless variety? or can say, by looking at powdered rhubarb, whether it is of that prime quality which

affords from sixty to seventy per cent. of soluble matter, and some twelve per cent. of pure resin, or whether it is an article produced from the decayed and worthless root, (the color and smell having been heightened by artificial means.) which affords not to exceed fifteen per cent. of soluble matter and no resin at all?

"The several barks before alluded to, although differing in physical appearance, are those generally known in the trade as the red and yellow Maracaibo and Cartagena barks; and as they resemble the true officinal bark in color, they have long been used in a powdered state for the purpose of adulterating those barks, or sold to the unsuspecting as the genuine article. This fact shows very clearly why it has long been almost impossible to find on sale in the country, or even in our minor drug and apothecary establishments in town, one pound of the red or yellow cinchona bark, of the requisite strength and purity; or, in other words, that will afford, on analysis, a per centage of alkaloids corresponding with that produced by the genuine barks. Some samples that have been obtained afforded neither quinine nor cinchonine in any perceptible quantity! others less than one-fourth part of the alkaloids found in the true and pure barks; and, so upward, according to the extent of the adulteration. From the quality of samples that have been forwarded to me from a distance, I am satisfied that the country is filled with such base mixtures and worthless trash.

"The question now very naturally and properly comes up, will prime crude drugs, after having been powdered and prepared, be found on sale in town and country in as pure a condition as when imported; or, in other words, be found free from adulteration? I fear not, unless a strict watch is kept over the operations of the unprincipled portion of those among us whose mission it is 'to buy, sell, and get gain,' honestly if they can; if not, get it.

"It has heretofore been too frequently found that drugs become astonishingly reduced in strength and purity during their transition state from root, bark, gum, &c., &c., to powder. Prime fresh drugs are no doubt (as well as worthless) sent to the drug-mill; but somehow or other, 'by falling into bad company,' I suppose, they are apt, during their stay, to lose their virtue; and as a matter of course are returned to their owner, and sent out into the market, with a character decidedly tarnished—an article fair to look upon, but whose touch is death. Badinage apart—the business of drug-grinding or powdering requires a searching and thorough reform.

"I have already alluded to the mysteries and trickery of the laboratory when in skillful but dishonest hands; but be assured, sir, its conjurations and diablerie if I may so express myself, in the preparation of adulterated chemical medicinal compounds, hardly exceed in ingenuity, deception, and iniquity, the frauds committed under the roof of the drug-mill.

"I have in my possession the voluntary confession of a drug-grinder, who has retired after amassing a fortune in the business; but I will not swell this report by entering at this time into an extended detail.

"This is a very important subject; and one, too, which the profession throughout the country, as well as the medical staff of the army and navy, whether on duty at a distance or at home in hospital practice, should lose no time in investigating; for how is it possible for the physician to do justice either to his patient or himself, however judicious and correct his prescriptions may be, as long as there is so much uncertainty as to the strength and purity of the curative agents he may recommend? I cannot but believe that many, very many valuable lives have been lost, owing to this lamentable condition of things."

Ought not the whole Profession to feel that their reputation, their success, and the lives of their patients should rest on a surer foundation than "guess work" or experiments?

Are not Pure Medicines far safer for the reputation of a Physician, and far more economical, taking success in view, than the cheap Medicines, which are entirely unreliable, even when they have any virtue?

THEREFORE, your earnest attention is requested to a branch of business intimately connected with success in the treatment of disease.

It is well known among dealers, and yet not generally known by the profession and the public, that pure and genuine medicines, particularly pure powdered drugs, from the first quality of gums and roots are scarcely procurable in this

country, and therefore physicians often prescribe medicines to meet certain indications in the disease of the patient, without obtaining the desired and expected beneficial result. To enumerate the articles of adulterated medicines that are daily sold in market would be to name almost the entire list of the materia medica. From the finer and more important chemicals and pharmaceutical preparations, such as Morphine, Quinine, Hydriodate Potass, Calomel, Blue Pill, &c. &c., down to the most common, and those of daily use, such as Cream Tartar, Rhubarb, Ipecac, &c., the adulterations are so adroitly made, that (without analyzation) even the closest inspection will fail to detect them. Quinine is often found largely adulterated (in some instances more than half) with manuite and other substances. Blue Mass and Calomel have been found much more than half adulteration. A gentleman at one time connected with an extensive manufacturing establishment, informed us, that just before he left England, the factory turned out more than four thousand pounds of Blue Pill, containing Barytes, instead of Mercury; and it was all designed for the American market.

Knowing this matter to be worthy the first and earnest consideration of the practitioner, we would respectfully ask attention to the accompanying

CIRCULAR.

We wish to call particular attention to our Extra Powders, which are pulverized from selected roots and gums of the very best quality; and when necessary, every piece is broken and examined under our own immediate supervision, and consequently possesses a purity hitherto unknown in this country, and a uniformity of action upon which the physician may rely with perfect confidence.

Our powdered Ipecac, extra, also will be found much superior to the usual article of commerce, being made from the true Brazilian Ipecacuanha, and consisting solely of the active outer coating of the root, carefully separated from the ligneous parts, and from all other inert matters. We pulverize only the true Mexican Jalap. In pulverizing Colocynth, extra, we retain only the active pulp of the apple, rejecting the seeds, which latter constitute the principal part of the weight of the fruit and are nearly inert. Powdered Rhubarb, extra, we prepare from the best East India Rhubarb, which is culled over with great care, every root being broken to detect any unsoundness. The Powdered Resin Guaiac, extra, is the pure Resin collected in tiers entirely from the dross and dirt usual to the ordinary Guaiac of commerce.

The Scammony also is powdered from an article differing in appearance and very much superior to what is usually sold for Aleppo Scammony. Blue Pill, bearing our label, will always contain one-third part of mercury, and our Hydrosublimed Calomel will be found to be of superior and regular quality.

Many of the roots from which the Extra Powders are prepared, are sifted and washed, and so many extraneous roots &c. are culled out, that the loss is often from one-quarter to one-third of the original weight, making, consequently, a very different article from the ordinary powders of commerce.

PARTICULAR NOTICE.

Having repeatedly heard that it is asserted that the superior quality of our "Extra Powders," and the unusual care in preparing them is all pretence, and that no one would take so much trouble and time, we merely state that we have on hand to show to any one doubting the facts, the stems culled from cubebs, the seeds taken from the pulp of the Colocynth, the woody, inert parts of the Ipecac, the extraneous roots culled from Pink, Senega, Serpentina &c., and various other tangible proofs of the difference between our "Extra Powders" and the ordinary powders of the trade.

Although many Druggists denounce the Extra Medicines as all humbug, yet they have imitated our style of bottles and put in them the ordinary inferior powder of trade.

Powders can be imitated so easily by coloring, those wishing the pure would do well to compare them; such for instance as Rhubarb, Gum Arabic.

Care is taken to have these "Extra Medicines" not only pure, but of the best quality procurable.

When required, any of these articles can be obtained of us in their original state as some may desire a superior article to use unpowdered.

The life of the patient as well as the success and reputation of the physician and apothecary, depend so much upon the prompt action of the medicines used in sickness, that we feel every confidence in any effort to furnish them with pure and superior drugs will be fully appreciated.

To preserve the preparations from being injured by the air and moisture, they are generally put up in bottles and jars containing one pound each; and also in five and ten pound canisters. They should be kept as much as possible from the light.

It will be observed that the prices of these superior articles are necessarily higher than those of the ordinary kind; and physicians and merchants at a distance, when they wish this quality sent them, are particularly requested to write for the 'Extra Medicines' of Philip Schiffelin, Haines & Co., Chemists and Druggists, New York.

EXTRA POWDERS.

Aromatic Powder, U S P	Pulv. Blood Root	Pulv. Lobelia
Pulv. African Pepper	Bitter Root	“ Seeds
Alum	Black Root	Mandrake
Allspice	Borax	Marsh Rosemary
Aloes, Cape	Buchu	Mace
“ Socot	Caraway Seeds	Mezereon
Assafetida	Cantharides	Nux Vomica
Antum Ref. Blk. Sulph	Banella Alba	Nitrate Potass, pure
Agustura Bark	Cardamon (Seeds)	Nutmegs
Ariseeds	Cicuta	Nutgalls
Elecampane	Cranesbill	Pleurisy Root
Digitalis	Cloves	Rhubarb, Turkey
Ergot	Cinnamon, ordinary	“ East India
“ 1 oz. phials	“ Ceylon	Rhatany Root
Extract Colocynth C'd	Cohosh, Black	Sarsaparilla, Para
“ Licorice, Calab	Columbo	Sage
Fœnugrek Seed	Colchicum Root	Summer Savory
Gentilan Root	“ Seed	Sweet Majoram
Ginger, Jamaica	Colocynth Pulp	Sabina
Golden Seal	Cubebs	Senua, Alex.
Opium, Turkey	Guaiaic Resin f. Tears	Seneka Root
Orange Peel	Gum Arabic, Turkey	Scammony, Alp. No. 1
Orric Root	“ Gamboge	“ Virgin
Pepper, Cayenne, A.	“ Mastich	“ “
Pepper, Black	“ Myrrh	“ “ 1 oz.v.
Peruvian Bark, Loxa	“ Tragacanth	Snakeroot, Virginia
“ “ Red	Hellebore, White	Squills
“ “ Calasaya	Hyoscyamus	Sulphate Copper
Prickly Ash Bark	Ipecac	“ Iron
Pink Root	Ipecac and Opium	“ Potass
Bayberry Bark	Jalap	Uva Ursi
Belladonna	Kino, True	Valerian, English

BOTTLES AND CANS AT COST,

When put up in Quarter and Half-Pound Bottles additional ten cents per pound.

As many of the Gums, &c., are of unusual purity, for instance Guaiac, Aloes, Assafetida, &c. they are very liable to run together and become solid. Even the ordinary common Gums of commerce are so apt to run together that Drug-Ginders usually grind with them some woody substance, such as Licorice root, Gentian root, &c.

Some roots that are rich in resin, such as Rhubarb, Jalap, &c., are also apt to agglomerate. The Extra Powders being perfectly pure and free from all foreign substances, are therefore more liable to become solid than the common qualities, but where they do so, we will replace them with others that are freshly powdered.

PURE CHEMICALS PREPARED AT OUR LABORATORY.

Ammonia, Aqua	Mercury, Bin Iodide	Spirits Nitri Dule
“ “ Liquor Fort	“ “ “ crystals	Syrup Iodide Iron
Ammoniated Alcohol	“ “ Protoiodide	Sulphuret Potassa
Argent Nitras	Morphine Sulphate	Vallets Ferruginous Mass
“ “ Crystals	“ Acetate	Zinci Acetas
Lunar Caustic, Nos. 1, 2, 3	Granvilles' Lotion	Zinci Sulphas
Ferri Carb, precipitated	Gallic Acid	Zinci Chloride
“ “ Sulphas, pure	Oil Capaiva	Chloride Soda, Labarraque
“ “ “ Exsiccate	Oil Cubebs	Confection Rosas
Iodide Arsenic	Precipitated Chalk	Confection Senna
“ Lead	Prussic Acid	Blue Pill Mass
“ “ Crystals	Potasse Nitras, pure	Ung. Mercurial
“ Iron	Strychnine	

Many of these chemicals differ decidedly in appearance from the chemicals of commerce as well as in their valuable properties, and bear externally the style of our manufacture, being in crystals and having the peculiar crystalline characteristic of each article; while those usually sold are in the powdered state, in which form it is difficult to judge of purity.

The Crystals of the Iodines of Lead and Mercury, and the Sulphate of Morphine more particularly, are much esteemed by all who have tried them, and our Soluble Precipitate Carb. Iron, Nitrate of Silver, Extra Blue Pill Mass, hold the first rank among choice chemicals.

Our Hydro Alcoholic Extracts are prepared by steam process, in a patent Vacuum Apparatus, and at a low temperature of nearly an hundred degrees below the boiling point. so that the valuable properties of the plant are preserved uninjured, and at the same time a consistence, color and taste are obtained, which are sufficient evidence of their superiority. Among them are

Extract of Belladonna	Extract of Digitalis	Extract of Quassia
Butternut	Gentian, opt.	Rhubarb
Buchu	“ Ordinary	Sarsaparilla Simplex
Bloodroot	Hyoscyamns	“ Compound
Bonset	Hops	Para Sars. Alcoholic
Conium	Jalap	Hon. Sars. Alcoholic
Chamomile	Lupuline	Stramonium
Colocynth, Ordinary	Mandrake	“ Sceds
Colocynth, Opt.	Nux Vomica	English Valcrican
Dandelion, Alcoholic	Opium	Dutch “
“ Inspissated	Pinkroot	

Observe that it is a well established fact that many plants which in their native conditions are possessed of very active medicinal properties, lose by cultivation their peculiar characteristics and become nearly inert. Some vegetable which belong to poisonous families of plants, by cultivation, are made innocuous, and are freely eaten as food, as the potatoe, parsnip, cellery, &c., &c.

To insure to our extracts, such as Hyoscyamus, Belladonna, Conium, &c., all the active therapeutical effects which they should possess, we take pains to procure the herbs, from which they are prepared, from places where they are indigenous to the soil, viz: from Germany, France, England, India, &c., &c., and they are consequently much superior to extracts made from the cultivated plants of American growth.

The difference is very apparant between these extracts and those of any other make, not only in appearance but also far more in flavor and in the peculiar aroma of the herbs; so much so, that even without the labels they are easily distinguishable by the taste and smell. These are facts of so much importance, that they should be remembered by all who have any occasion to use medicinal vegetable extracts.

These Extracts together with our pure Chemicals and Extra Powders have received the unqualified approbation of various Medical Associations, and of Phy-

sicians and Apothecaries, and have elicited many complimentary letters on their good qualities. The following token of approbation is from the Ohio Medical Convention:

"Resolved, That the thanks of the Medical Profession are due to the house of Philip Schieffelin & Co., of New York, for their efforts to furnish the community with pure Drugs; and we recommend their Extra Medicines to the confidence of Dealers and Practitioners."

And also from the following eminent practitioners:

PHILIP SCHIEFFELIN, Esq.:

Dear Sir—It is with pleasure I add my commendation to that of other physicians as regards the superior quality of the Extracts and Chemicals, prepared and sold by your house. The extraordinary care and assiduity shown by you in obtaining and putting up Drugs free from adulterations, and Chemicals prepared perfectly pure, deserve the highest commendation, not only from the physicians, but also from the public, whose safety is eminently concerned in the employment of articles of the *Materia Medica*, free from inert or injurious additions. Although the public generally has been warned by the publications of "Inspectors of Drugs," and by the medical press, that such adulterations have been made by dishonest vendors and speculators, still the imposition continues to be exercised, and, in many cases, almost without reserve. The Cod Liver Oil, manufactured for your house, I find far superior to any other, being, I believe, perfectly pure, and yielding to the tests the absence of the oils generally employed in the adulteration of this really valuable article. My patients also find it far less disagreeable, and more readily digestible than the kinds I have formerly prescribed.

Wishing you the success you so fully deserve. I remain yours, very truly,
H. P. DEWEES.

SEAMANS' RETREAT HOSPITAL, STATEN ISLAND, June 21st, 1850.

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Trusting that the public, as well as the profession, will appreciate your endeavors to furnish them with pure articles in medicine. I remain, yours respectfully,

JAS. R. BOARDMAN, M. D.,
Resident Physician and Surgeon.

Their superior efficacy, in all prescriptions, will be at once apparent to every one who reflects upon the difficulty oftentimes experienced in the administration of the common Drugs of commerce, and the *loss of life* and of reputation, consequent upon the use of inert remedies.

COD LIVER OIL.—The great and increasing demand for Cod Liver Oil, and the difficulty of procuring the oil in its pure state, and such as we can guarantee to our customers, have induced us to send our agent to the fisheries for the purpose of having the best article that can be offered in the market. This article will also bear our label when put up in bottles, and be warranted pure, when ordered in bulk.

TO DRUGGISTS.—In addition to the Extra Medicines, we also keep a large and well assorted stock of the ordinary Drugs and Medicines of commerce, carefully selected, and the best that can be procured. Our Essential Oil, and other Liquids, we obtain from the most reliable sources, and are submitted to every known test for impurities; and we avoid purchasing any kind of Drugs in the powdered state.

Our arrangements and facilities are such, that we can offer inducements to dealers, which must influence all, who, not only like to have a fair equivalent for their money, but at the same time to have goods that are what they purport to be, and such as will bear the strictest examination and analysis.

PURE CHLOROFORM.—Much of the Chloroform of commerce being very impure, and its use having in some cases been attended with unpleasant consequences, we have been repeatedly urged to make some at our Laboratory, of a quality superior to that generally for sale in this market. We would, therefore, inform the Medical Profession that we have prepared an article, the purity of which can be implicitly relied on.

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MORPHINE.—Our Morphine having acquired a reputation superior to any

other, those who have occasion to use the article will be satisfied of its excellence by giving it a trial.

☞ We also prepare the SYRUP OF IODIDE OF IRON U. S. P., now so highly esteemed as a remedy in Scrofulous Complaints, and also Dupasquier's Syr. Iod. Iron, which is a much milder preparation, and better adapted for Ladies and Children. These articles [which it is the greatest consequence to physicians to have of reliable quality,] are, with our other preparations, offered to the notice of those desiring pure Drugs and Chemicals.

☞ N. B.—Letters directed to "Schieffelin & Co.," intended for us, have frequently gone to other houses, there being several firms of that name; therefore please be careful to write our names in full.

PHILIP SCHIEFFELIN, HAINES & CO.,

September, 1853-ly.

107 Water Street, New York.

THE
NEW ORLEANS
MEDICAL AND SURGICAL
JOURNAL.

VOL. XI. SEPTEMBER, 1854. No. 2.

EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia; Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresponding Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c. &c.

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1854.



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KENTUCKY SCHOOL OF MEDICINE.

THE Fifth Session of this Institute will commence on Tuesday, October 31st, and continue until the end of February, with the following

FACULTY:

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- HENRY M. BULLITT, M. D.,
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Professor of Chemistry.
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Material for Practical Anatomy abundant. The Dissecting-rooms will be opened on the 1st October.

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R. J. BRECKINRIDGE, Dean of the Faculty.

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Session of 1854-'55.

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- ALFRED STELLE, M. D.,
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- JOHN NEILL, M. D.,
Principles and Practice of Surgery.
- J. M. ALLEN, M. D.,
Anatomy.
- JOHN M. REESE, M. D.,
Medical Chemistry and Pharmacy.
- FRANCIS G. SMITH, M. D.,
Institutes of Medicine.
- JOHN B. BIDDLE, M. D.,
Therapeutics and Materia Medica.

The Lectures will commence on Monday, October 9th, and continue till the 1st of March ensuing. A Medical and Surgical Clinic is held regularly twice a week at the College, and at the Pennsylvania Hospital. Second Course Students receive tickets to the latter Institution, free of expense.

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For each Professor's ticket,	- - - - -	15 00
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Practical Anatomy,	- - - - -	10 00

The Dissecting Rooms will be open in the month of September.

For further information, apply to

JOHN J. REESE, M. D. Registrar,

No. 122, Ninthstreet, Pbiladelphia.

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THE Fourth Annual Course of Lectures in this Institution will commence on Monday, the 30th of October next, and continue till the first of the ensuing March.

- ROBERT M. PORTER, M. D.,
General and Special Anatomy.
- J. BERRIEN LINDSLEY, M. D.,
Chemistry and Pharmacy.
- C. K. WINSTON, M. D.,
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- A. H. BUCHANAN, M. D.,
Surgical and Pathological Anatomy.
- THOMAS R. JENNINGS, M. D.,
Institutes of Medicines and Clinical Medicine.
- W. K. BOWLING, M. D.,
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- JOHN M. WATSON, M. D.,
Obstetrics and the Diseases of Women and Children.
- PAUL F. EVE, M. D.,
Principles and Practice of Surgery.
- WILLIAM T. BRIGGS, M. D.,
Demonstrator of Anatomy.

The Anatomical rooms will be opened for Students, on the first Monday of October.

A full Preliminary Course of Lectures, free to all Students, will be given by the Professors, commencing also on the first Monday of October.

A Clinique has been established, in connection with the University, at which operations are performed and cases prescribed for and lectured upon, in presence of the Class.

Arrangements have been made to accommodate all patients requiring Surgical Operations.

Amount of Fees, for Lectures in the University, is \$105. Matriculation Fee, (paid once only,) \$5; Practical Anatomy, \$10; Graduating Fee, \$25.

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J. B. LINDSLEY, M. D., Dean of the Faculty,

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Editor's Office—Notices.

SEPTEMBER 1, 1854.

BOOKS AND PAMPHLETS RECEIVED.

- I.—*Indiana Medical Journal*: (new) Edited by Professors W. H. BYFORD, M. D., and HUGH RONALDS, M. D.; quarterly; Evansville, Ia.
- II.—*Annual Report of the City Inspector of the City of New York*, for the year 1853. Pp. 264.
- III.—*Clinical Lectures on Pulmonary Consumption*: By THEOPHILUS THOMPSON, M. D., F. R. S.; Fellow of the Royal College of Physicians, London; Physician to the Hospital for Consumption and Diseases of the Chest; author of *Annals of Influenza*, prepared for the Sydenham Society, &c. Philadelphia: Lindsay & Blakiston, 1854. Two plates; 8vo., pp. 259. From Mr. T. L. White, Bookseller, 105 Canal street.
- IV.—*A Clinical Introduction to the Practice of Auscultation, and other Modes of Physical Diagnosis in Diseases of the Lungs and Heart*: By H. M. HUGHES, M. D.; Fellow of the Royal College of Physicians; Assistant Physician to Guy's Hospital, &c. Second American, from the second and revised English edition. Philadelphia: Blanchard & Lea, 1854. Pp. 304. From Mr. J. C. Morgan, Bookseller, Exchange Place.
- V.—*A Universal Formulary, containing the Methods of Preparing and Administering Officinal and other Medicines. The whole adapted to Physicians and Pharmaceutists*: By R. E. GRIFFITH, M. D.: with illustrations. Philadelphia: Blanchard & Lea, 1854. 8vo. pp. 651. From Mr. T. L. White, Bookseller, 105 Canal street.
- VI.—*The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases, frequently mistaken for, or associated with Phthisis*: By J. HUGHES BENNETT, M. D., F. R. S. E.; Professor of Clinical Medicine in the University of Edinburgh, &c. Phila-

delphia: Blanchard & Lea, 1854. Svo. pp. 130. From Mr. J. B. Steel, Bookseller, 60 Camp street.

- VII.—*The Modern Treatment of Syphilitic Diseases, both Primary and Secondary*; comprising the Treatment of Constitutional and Confirmed Syphilis, by a safe and successful method; with numerous Cases, Formulæ, and Clinical Observations; from the third and entirely re-written London edition: By LANGSTON PARKER, Surgeon to the Queen's Hospital, Birmingham. Philadelphia: Blanchard & Lea, 1854. Svo. pp. 311. From Mr. J. C. Morgan, Bookseller, Exchange Place.
- VIII.—*The Half-Yearly Abstract of the Medical Sciences*: Edited by Drs. Ranking and Radcliffe; from January to June, 1854. Philadelphia: Lindsay & Blakiston. Svo. pp. 318.
- IX.—*Observations on Exhaustion from the Effects of Heat, (coup de soleil;)* By H. S. SWIFT, M. D., Resident Physician of the New York Hospital.—Pp. 15.
- X.—*An Analysis and Description of Alum Waters from Rockbridge and Bath Springs, in Virginia; their Remedial Effects and Curative Properties, with Certificates of their Efficacy in Disease*: Frazier, Randolph & Frazier, Proprietors.—Pp. 36.
- XI.—*Braithwaite's Retrospect of Practical Medicine and Surgery*: to July, 1854. New York: Stringer & Townsend. Svo. pp. 336.
- XII.—*Napoleon Bonaparte and Sir Hudson Lowe, at St. Helena*: From Prof. Henry Dickson, M. D., Charleston.—Pp. 25.
- XIII.—*An Address Delivered before the Literary Societies of Centenary College, at Jackson, Louisiana, on the 25th July, 1854*: By Prof. CHRISTIAN ROSELIUS, LL. D., of New Orleans.—Pp. 14.
- XIV.—*Transactions of the Fifth Annual Meeting of the Medical Society of the State of North Carolina, held at Raleigh, N. C., May, 1854.*—Pp. 97.
- XV.—*Remarks on Throat Diseases and Consumption*: By IRA WARREN, M. D. Boston: 1854.—Pp. 48.

COMMUNICATIONS RECEIVED.

A Communication has been received from N. WALKLY, M. D., of Mobile.

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ORIGINAL COMMUNICATIONS.

ART. I.—REMARKS ON DYSENTERY AMONG NEGROES.

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The following remarks, on dysentery among negroes, have been elicited by a letter recently received from the Hon. John Bragg, a planter of Lowndes county, Alabama, asking my opinion in regard to the character and treatment of a very fatal form of typhoid dysentery, at present prevailing on a number of plantations in South Alabama, where, for many years it has occurred at irregular intervals, killing on some plantations all it has attacked. He informed me that "the populous planting counties of South Alabama, are those which have been chiefly afflicted by this terrible scourge, called by physicians typhoid dysentery, and by the country people bloody-flux." He says: "Like the cholera, it frequently appears without any apparent cause on one plantation, while the next neighboring plantation is entirely exempt from its ravages. In the year 1851, General Fitzpatrick lost some twenty-five negroes with the disease, his nearest neighbor remaining unaffected. Wm. Robertson, Esq., a wealthy planter of Lowndes county, suffered in the same year to a still greater extent, and in the year 1852, in the months of June, July, and August, Mrs. Bonnell, of Lowndes county, two overseers

and some fifteen or twenty negroes fell victims to the disease, which seemed not to extend beyond the immediate premises, leaving unmolested the families of her neighbors, several of whom lived not more than half a mile from her residence. In her case, and I believe in others, every individual attacked with the disease perished. It seemed to defy utterly all medical skill, and that particular section of the country was not destitute of experienced and skillful physicians. The disease has recently appeared on many of the large plantations, and, as usual, is committing great havoc." "It invariably presents itself by a discharge of mucus, at first slightly tinged or streaked with blood: as it progresses, the discharges become apparently pure blood; and, at last, and for a day or two before dissolution, they present the appearance of claret-colored water. Generally, it is not rapidly fatal. The patient seems to linger more or less, according to the strength or weakness of his constitution, sometimes living for two or three weeks from the first development of the disease. The pulse is high, almost from the beginning, showing from one hundred and thirty to one hundred and forty pulsations per minute. The patient seems to suffer but little pain, and almost in *articulo mortis*, will tell you he feels greatly better, and expects soon to be up. So much for the symptoms. The treatment, with some occasional variations, has consisted in exhibitions of blue mass and opium, and sometimes astringent injections."

The letter concluded with an urgent request for my opinion of the character of the disease, and what ought to be its treatment. After I had dispatched my reply, under date of July the 3d, Dr. Atchison, an old friend and acquaintance, called on me, who has had much experience in the treatment of the diseases of negroes, as he has long practiced in Adams county, Mississippi, in a rich planting district, where the negro population is greatly in excess of the white. I showed him the Hon. J. Bragg's letter, and he assured me that the year before last, precisely such a typhoid dysentery as the one described, prevailed on a number of plantations in Jefferson county, and proved very fatal under the practice recommended in the text-books. He remembers hearing me say, what proved to be no jest, that Gil Blas was a better and safer book to practice by, among

negroes, than Watson, Bell and Stokes, and other books of the kind, written by authors who know nothing of the negro, or the peculiarity of his diseases, but suppose that he is like the white man in every respect except the color of his skin, and erroneously assume that the same remedies would do equally well for both. The Sangrado practice recommended in Gil Blas, (if the bleeding were omitted or kept in bounds,) is very much the same as the French p^tisan expectant treatment, and would be much safer for negroes, indiscriminately used for their diseases, than the white man's medicines, recommended in the European text-books, written by authors who regard the negro "*as a black white man,*" as Dr. Van Evrie would say.

Dr. Atchison resides in a neighborhood, amidst the very planters who had presented me with a piece of plate, in the shape of a vase, worth a thousand dollars, for saving their negroes from a terrible epidemic cholera, which appeared among them and scourged the neighborhood and the surrounding country. When called, therefore, out of his immediate neighborhood to visit the plantations in Jefferson county, afflicted with typhoid dysentery, he immediately recommended the same plan, which twenty-two years ago had saved the negroes in his own neighborhood, in all cases where it was adopted, from the ravages of the cholera. The plan consisted in making an impression upon the mind as well as the body, by breaking the chain of those superstitious influences which render epidemics so fatal among negroes, and at the same time to get out of the infectious atmosphere causing any unusual sickness among them. This is effectually accomplished by sending them back to an imitation of African barbarism in the neighboring fields, woods and wilds, to lead a savage life, exposed to the open air and weather, and unprotected by houses. The sick and the well alike to be moved. For the former, however, temporary coverings or open sheds to keep off the rain are permitted.

When Dr. Atchison arrived at M. Rowan's plantation, in Jefferson, he found forty sick with typhoid dysentery. Two or three had already died, and many of the neighbors had lost from ten to fifteen. He ordered the sick and the well out into the woods, and permitted

some sheds to be built over the sick. The whole forty got well, and there were very few new cases, and those of very mild grade.

A little elixir vitriol in water, and occasionally a tea-spoonful of sulphate of soda with mucilaginous drinks, as slippery-elm water, or *prickly pear tea*, was all the medicine that was used after the removal. On the neighboring plantations the latest and most approved European methods of treating the white man's *colitis* and *ileo-colitis*, and all the most approved methods for the prevention and cure of follicular inflammation and dysentery in the xanthous race, continued to be put in practice upon the Ethiopian, but without any salutary effect. The poor negroes, treated like white men, continued to get sick and die. At length some of the planters boldly ventured to do as Dr. Atchison advised M. Rowan to do—to carry their negroes, sick and well, back to an imitation of savage life, in the woods or open fields, and all, who did so, found the measure to put an instantaneous stop to sickness and death among their people.

The system of removing negroes, when cholera, dysentery, or any other malignant scourge appears among them, has gained ground very slowly since I first advised it, nearly a quarter of a century ago. The reason is, that although the planter well knows, from dearly bought experience, that it will not do to treat negroes like white men when they are in health, unless he would ruin them forever by making them sulky, dissatisfied and ambitious to be above the master that would put himself on an equality with them, yet some how or other most of them seem to practice upon the equally false doctrine that negroes ought to be treated exactly like white men when sick. Such persons, when they remove their negroes, build houses for them before the removal is made, and thus defeat the half, if not the whole, of the good effects for which it is made.

On a large sugar plantation on the coast, belonging to Capt. Wm. J. Minor, where the cholera appeared about two years ago, a removal of the negroes from one set of houses to another set, (one of which had been built for the purpose,) was twice tried without arresting the progress of the disease at all. At length, after forty had died, being called on to visit the plantation, and invested with full power to do as I pleased, I took about three hundred negroes, sick and well,

a mile or two back into a dry, open place in the swamp, where there was no house to be seen, or any preparation begun for building any. The overseer refused to obey until I recorded this fact in the plantation book, with an additional statement over my own signature, that a cloud was coming up, and in all probability there would be rain. Having made the record, I headed the line of march, and, sure enough, the rain came up and all got wet. They encamped in the open air and built fires, although the weather was warm, and some booths were directed to be made over the sick to protect them from the sun and the rain. The ashy-colored, dry skin conjurers, or prophets, who had alarmed their fellow-servants with the prophecies that the cholera was to kill them all, and who had gained, by various tricks and artifices, much influence over their superstitious minds, were by my orders, at twilight, called up, stripped, and greased with fat bacon, in presence of the whole camp—a camp without tents or covering of any kind, except some bushes and boards over the sick from the carts that conveyed them to the camp. After being greased, the grease was well slapped in with broad leather straps, marking time with the *tam tam*, a wild African dance that was going on in the centre of the camp among all those, who had the physical strength to participate in it. This procedure drove the cholera out of the heads of all who had been conjured into the belief that they were to die with that disease; because it broke the charm of the conjurers by converting them, under the greasing and slapping process, into subjects for ridicule and laughter, instead of fear and veneration. The next morning, by times, all who had been able to join in the dance the over night, were ordered into the cane-field to work. There were no more cases of cholera, or deaths from that disease after the removal, except one man who had strayed away from the camp, and except also among some half dozen who had been left to take care of the houses, about half of whom died. The removal included not only the negroes, but the horses, mules and dogs, that there might be no excuse for revisiting the houses and haunts of civilized life. They remained in the camp at night, and labored in the fields by day for some six weeks before they were brought back to the houses, and during all that time they enjoyed good health.

For about a month before the removal, the cholera had prevailed among them, proving very fatal, in defiance of the best directed treatment of some four or five well-read and excellent physicians, some of whom remained on the plantation all the time, and had the assistance of experienced nurses from the Charity Hospital. Those who recovered had not, up to the time of the removal, regained their strength, and had been liable to relapses, often proving fatal. The error lay in treating the convalescent like white men, guarding them against exposure to the sun and open air, and keeping them on a light diet, as teas and soups, instead of treating them like negroes, by giving them a good meal of fat pork, and sending them out in the sun to work to promote their digestion, and thereby improve their strength. There is no cruelty in such a procedure. The cruelty lies in treating convalescent negroes like white convalescents are treated. It keeps them sick and weak, and brings on them an oppressive dejection of the mind; whereas, by letting them obey the instincts of their nature, eat a hearty meal of animal food, and get in the sunlight to work, they feel stronger and better as its digestion progresses. If they do not, it is a sign that the disease is not removed, and should be directed to quit work if they do not feel better under it.

A very learned and thoroughly educated physician from Philadelphia, who had had the advantages of foreign travel, married a wealthy young lady, the owner of a plantation in the vicinity of Natchez. Two of the negro men fell sick with a violent fever, which their new master, the doctor, who had just married their beautiful young mistress, treated until the fever went off; after which they asked him for a dish of fat pork and turnip-greens, or, if he pleased, some bacon and cabbage and corn bread. He refused to give such strong diet to them, fearing that it would kill them, or bring back the fever. As they became clamorous for it, he mistook the cravings of nature for a morbid condition of the stomach, called bulimia, placed a close guard over them, to watch day and night by their bedside, to prevent them from getting any strong food clandestinely, but at the same time supplied them bountifully with barley water, soup-maigre, and the nice, light preparations he had seen served to convalescents, from fever, by the European and Philadelphia physicians of the most eminence in their profession. Some two weeks

passed away, and the poor negroes, under this treatment, had become greatly debilitated and emaciated. At length my advice was called for. On asking the negroes "what was the matter," they said "nothing at all, only young master don't know us people, and keeps us on white people's soup, that makes us weak." I advised the fat pork, corn bread, and turnip-greens to be immediately given, after which, as they were too weak to walk, they were lifted out of their beds and set in the hot sunshine, almost hot enough to roast an egg, to promote the digestion of the strong food. Some light work to exercise their arms, as they could not use their legs, was also advised. In a few days they were out in the cotton-field working with the rest of their fellow-servants.

Far be it from me to advise negroes to be treated like brutes or with inhumanity. I advise their nature to be diligently studied, and then to receive the full measure of that treatment most conformable to their nature, which is always the best for their own comfort, health, contentment and happiness. So long as any portion of our Southern physicians continue to be guided in their practice on Southern plantations by the rules and methods of treatment in Northern and European medical works, to the utter neglect of the experience and observation of Southern practitioners, as contained in our Medical Journals, (as if they were not worth looking at or the paltry price of subscription and postage,) just so long will we continue to hear of frightful mortality among negroes from dysentery, cholera, pneumonia, measles, whooping-cough, fevers, &c., in defiance of the best directed medical skill; meaning, thereby, that skill that would treat an owl like an eagle, by bringing him into the light to make him see better; the skill that would keep ducks out of water lest they drown like chickens. Owls and eagles, ducks and chickens, are not more different in their nature than the xanthous and melanic

..... melanic
 races of mankind. The fallacious theory that they are identical, and what helps or hurts one, will equally help or hurt the other, has done mischief enough already by disturbing the harmony of the people of the various States of this confederacy. It is time for it to be discarded from the science of medicine, because it is untrue and leads to a pernicious practice,

The true doctrine is to treat the negro like a negro and the white man like a white man. Whether they both came from Adam or not is a speculative question, which neither comes within the proper province of the practical physician or the true Christian. Suffice is it for the former to know, that whatever may have been the origin, there is a radical physical difference now, as great as any found among the accipitre order of birds—which may have had a single pair of accipitres as their common progenitors—yet notwithstanding the accipitre called the owl, differs radically from those other accipitres called the hawk and eagle—particularly in regard to the effects of light and darkness upon them—like liberty and slavery in the white and black races. And as to the Christian, it is sufficient for him to know, that the negro is the black man Ham and Canaan of the Bible, and that that Book would have been incomplete if it had not have named him—and not only named him, but given a key to his whole character in the name it gave him, which literally translated, taking the two names together, means “*the hot and black knee bender.*” If our Southern physicians would look closely into the Hebrew of the Bible, and then compare every thing there recorded in relation to Ham and his descendants, with the actions and well known nature and character of the negroes on our cane and cotton fields, they would find in those actions, nature and character, a beautiful and harmonious counterpart of the Hebrew, and they would see in it the very *bud itself* in which the characteristics of the melanic race, they have been studying, are delineated and shut up in miniature, true to the life. With that knowledge to direct them what to observe, and how to observe, united with a general knowledge of the principles of the healing art, improved and corrected by the experience and observation of those around them, in and out of the profession, on the same subject, they would soon become so wonderfully useful in preventing and curing the diseases of negroes, and in correcting the vices both of body and mind so prevalent among them, that no planter, who values his own interest, would forego the services of a regular physician on his plantation or think of trusting the lives of his people to an ignorant quack or overseer. Nothing but surgical cases and the good sense of those country physicians, who throw away Watson, the famous text book of the profession in England and America—treating diseases among negroes, not by

that or any other European book, but according to the lights gathered from the experience of those around them, have prevented the planting interest from abandoning the profession entirely. If it had been left alone to those who carry nothing but Watson, Bell and Stokes, and such books into the negro quarter, to draw from these the rules of their treatment, the services of the profession in plantation practice would long since have been dispensed with entirely. I would by no means depreciate or underrate Northern and European works on Medical Science. I know their value, when kept in their proper place, but I protest against applying the rules of practice and the plans of treatment, which a few great men at the head of European hospitals, filled with wretched, half-starved white paupers, have found to be the best and most expedient under the circumstances for those unfortunate beings under their care in that hyperborean region, to the strong, stout, happy and well fed negroes of our sunny South.

The success of Dr. Atchison in preventing and curing an epidemic typhoid dysentery, which had been very fatal in Jefferson county, Miss., having been reported to me by the doctor himself, immediately after I had dispatched my reply to my Alabama correspondent, recommending, in its concluding portion, the very method that the doctor found so eminently successful, has induced me to offer the same to the medical public through the pages of this Journal. In my reply to the questions asked me, I took occasion to say in the beginning of the letter, that notwithstanding the want of success in the treatment of the typhoid dysentery of South Alabama heretofore pursued, that I considered it safer to trust to the good sense of a qualified medical adviser on the spot, than to any specified plans of treatment to be carried out by non-professional persons, inhibited, from want of qualification, from discretionary action. I gave as a reason, when a physician finds out that the treatment laid down in the text books of Europe will not cure the disease in question, in a different race of people, in a different climate, he will feel released from the authority that misled him, and be nearer seeing into the true character of the disease than he was before, and will be apt to hit on the right remedies by his previous medical training. In this he differs from the empiric, who, when led into error by *his* book, has no other book to get

him out; being unacquainted with the principles of the healing art, he has no other chart or compass to guide him.

The great Sydenham treated a very severe epidemic dysentery very successfully, by bleeding and an opiate the first day—a purgative the next morning, followed by an opiate—two opiates the day afterwards—another purgative and opiate the day after that. But in a subsequent epidemic, he found that such treatment “*availed not at all.*” Instead of it he used “large quantities of diluting drinks, preferring whey to chicken water”—“to be drunk cold, and injections of warm whey to be thrown into the bowels;” and “always found the bloody stools to cease after the fourth glyster.” He wisely says: “If this method be certain and speedy, no judicious person will reject it because it does not come recommended with a pompous multiplicity of remedies.”

Others have found very threatening forms of dysentery to yield readily under the Sangrado practice of free potations of warm water. Cold drinks generally increase the gripings and the frequency of the dejections, unless warm glysters be used at the same time.

In some adynamic dysenteries a combination of quinine, camphor and capsicum, with laudanum where there is much pain, has proved to be a valuable remedy. In other severe bloody fluxes a table spoonful of vinegar, saturated with common salt, repeated a few times at regular intervals, and assisted by mint tea, will cure the complaint quicker than any thing else.

Such contrariety of treatment, for what passes for the same disease, often leads to skepticism and want of confidence in medicine as a science. The more, however, we look into nature, the greater variety do we see in her operations, and can better understand the necessity for contrariety of treatment in a disease produced by so many different causes. We can well conceive, that when dysentery arises from some morbid cause in the blood, that warm water or the diluting Sangrado plan may be the best means of throwing it off. Mere evacuants would not do, because the offending matter is not in the alimentary canal, but in the circulating fluids. When the vital energies are not too much depressed, bloodletting tends to prevent a fatal localization on the bowels; but if the circulation, instead of being maddened, be weak and flagging, stimulants and tonics are indicated. When

the dysentery consists, as it generally does, of "a fever thrown inwards on the bowels," by any causes suddenly arresting the cutaneous discharge, a cure can be speedily effected by revulsing to the surface and keeping up a perspiration for a sufficient time. When from congestion of the liver, calomel; when from malaria, quinine; when from offensive substances in the digestive passages, evacnants; when from acrimony, charcoal and demulcents, are the best remedies. The treatment used in the dysentery of South Alabama would no doubt have been successful, if the disease had originated from common causes, void of malignity and the mind had not been involved. In that case, the blue mass to act on the liver—opium to allay pain, and astringent injections to arrest the discharges and save the strength, would no doubt have led to the cure of a white man in a cold climate. But as fifteen negroes all died on one plantation and none got well, and as twenty-five died on another estate and very few recoveries took place any where, it is very evident, that such treatment did not reach the cause of the malady. The cause of this frightful mortality was, no doubt, more in the mind than in the body. There was nothing in the treatment addressed to the mind. The writers of the text books from which it was drawn, were writing for white men and not for negroes, or if they had intended their treatment of dysentery to apply to the Ethiopian race, they are evidently ignorant of the nature and character of that race; because there is nothing in the treatment to guard the most vulnerable and dangerous avenue to death in severe or malignant forms of disease occurring among that people. The African mind is so constituted that superstition and panic immediately begin to dig the negro's grave, on the occurrence of a few fatal cases, from any unusual or malignant complaint among them, and will speedily kill him, unless counteracted—kill him in defiance of any medicine in the *Materia Medica*, and in defiance of the best directed efforts of the most skillful physician, unacquainted with the important fact that his patient is not a *white man with a black skin*. Superstition and panic, after dysentery, cholera, or any other severe disease once begins on a plantation, raise their hideous heads and become executioners, outstripping by far the disease itself, unless banished from the premises by a stronger power, deriving its strength and magic influence from Natural History—the *Philosophia Prima* of Lord Bacon—teaching the knowledge of the negro's peculiar nature.

That superstition and panic, more than the dysentery itself, were the causes of the extraordinary mortality on the plantations in South Alabama, is inferred from the fact that the patients almost in *articulo mortis* would say they "felt a great deal better, and expected soon to be up." This is exactly what they say, when dying from that mental affection which might be termed a *cholera of the mind*. They have some trifling bodily symptoms of the disease, perhaps a few loose stools, by no means copious or alarming; they say, when questioned, that they are weak, but "suffer no pain, feel better and expect soon to be up." Yet, notwithstanding, they die when their mind has a splinter of superstition run into it from the appearance of cholera, or some frightful disease among their comrades, with as much certainty as tetanus from a splinter in the foot. This peculiar psychological condition, so fatal among negroes, might properly be termed a tetanus of the mind; a dream, a prophecy, or any idle thing, when cholera or any malignant disease is about, acting the part of a splinter to excite it. To call it fear or panic, conveys a wrong idea of it. So far from there being any manifestations of excitement and trembling, inquisitive anxiety, or any of those perturbations of the mental faculties, witnessed among white people when in danger, the negro acts exactly the contrary way when his mind becomes seriously affected. He seems to have taken an opiate, which has quieted all his previous fears, if he had any, removed all his anxiety about the affairs of this world or the next, cured all his pains, and transformed him into a true stoic. In this condition of mind he dies from the veriest pretence of a disease, and sometimes without any apparent bodily infirmity at all. Some frightful or fatal malady among his fellow-servants is the remote cause, and some insignificant circumstance, connected with some superstitious notion, is the exciting cause of this singular mental affection, which, when it occurs, makes all medical treatment, for any corporeal disease associated with it, utterly abortive. It was surely this psychological condition, more than the dysentery itself, which caused such an excessive mortality among the negroes on certain plantations in South Alabama. That disease, in all probability, originated from some poisonous air breathed, or morbid substance swallowed in the food or drinks, and carried into the circulation—the dysenteric symptoms being a mere effort to throw it off. I have generally found such bad forms of dysentery to arise from unclean quarters,

drinking bad water, eating sour pork, bread made from weevil-eaten corn, musty meal from new corn, or some such thing. I have long been in the habit of using in such cases, and for all others, where dysentery partakes of the nature of a fever thrown inwardly by a sudden check of the perspiration, a remedy which has proved very effectual. Its prompt effect in giving relief, and speedily curing the disease, prevents any bad effect its appearance on a plantation may produce on the minds of the other laborers. The method was published, some years ago, in the New Orleans Medical and Surgical Journal. But, who reads a Southern Medical Journal? Surely, no one who believes in the mischievous and erroneous doctrine set forth, directly or indirectly, in almost every book and publication, which for the last thirty years has emanated from the British and Northern press, that the *negro is a black white man*, and in morals, politics and medicine, what is good for the one is equally good for the other. Who, with such a belief ground into him by that powerful lever, the printing press, although contradicted by his every day experience, would take the trouble to rummage among dusty old magazines and papers, to look up a dilapidated journal to inquire into the causes, prevention, and treatment of dysentery, or any other disease among negroes, if he had a fine library at his command, filled to overflowing with observations diligently made, ably and correctly reported, with copious indexes for ready reference to all that is known in regard to the disease for a thousand years or more, as it occurred among a people exactly like his patients in every respect, *except that of having white skins?*

The method, published in the New Orleans Medical and Surgical Journal, was called the "*one dose cure*," because one dose was generally found to be sufficient to effect a cure, when timely given and the proper method adopted to assist its action—as that of disposing the patient for sleep and perspiration, and giving the medicine in pills or in some vehicle to make it not disagreeable to the palate. It consisted of from thirty to forty grains of ipecac and forty or fifty drops of laudanum, combined for one dose—followed by warm aromatic and grateful drinks, to keep up a perspiration for twenty-four hours. All mucous and bloody dejections immediately cease, under a warm, wholesome, generally diffused perspiration. This sweating method is only particularly applicable to the first stage of the disease. After

it becomes firmly localized in the lower bowel, to which it has a tendency to descend, so much heat and dryness of the skin attends it, that perspiration cannot be easily excited and sustained, and it would be injudicious to resort to any extraordinary means to force it, as steam doctors do.

The letter to Hon. J. Bragg, more directly in response to his questions, goes on to say—There is a radical difference in the dysentery he describes and the one in which the ipecac and laudanum proved so successful. Pain was a prominent symptom in the latter, whereas in the dysentery of South Alabama, it was mostly absent or not distressing. Whether this absence of pain be due to a bodily or mental cause, is not now under consideration, although it is an important question. But in either case, opium is not indicated. Opium, in some form or other, is Nature's balm for pain and spasm. When that remedy has strong, intermitting spasmodic pains to allay, (as it had in the dysentery in which I used it combined with ipecac,) its force is spent in removing pain and spasm—great evils—out of the way, without substituting any other in their place. Where, however, there is little or no pain, opium is apt, except in some cases of excessive hæmorrhage or sudden exhaustion, to affect the system injuriously in the first stage of acute diseases, by locking up the secretory organs, the very avenues through which the body frees itself of morbid causes. Typhoid dysentery does not tolerate purgatives. The best method of treating it, that I have ever seen used, is by giving the ipecac in one effectual dose without laudanum or opiates in any form. My formula consists of 30 grains ipecac, 20 grains mustard flour, 2 scruples of cream tartar, and a tea spoonful of charcoal—that made from burnt cork is preferable. This combination generally vomits once or twice and causes perspiration, which requires to be kept up for a day or two, by warm, grateful and nourishing drinks. Mint, calamus or capsicum tea in the first instance, and chicken water afterwards. At the time of giving the emetic the patient should be rubbed all over with hot oil, to soften the skin and to favor perspiration. A piece of fat bacon, held to the fire till it is well heated, is an effective and convenient substance for making the inunction. The friction should be continued for half an hour or more until perspiration is produced. The good effect of inunction, with warm oil in the plague, has been repeatedly verified. I have long

been in the habit of using it in the treatment of various diseases of negroes, as the pages of the New Orleans Medical and Surgical Journal will show. Negroes are a people in whom that method of medication is particularly useful. Scarcely any thing else is necessary in their scarlet fevers; united with lime juice, the external application of oil greatly assists internal remedies in removing those scorbutic conditions of the blood, which render negroes vicious and unmanageable.

Cloths wrung out of hot water, (as hot as the hands can bear to wring them,) and applied half a dozen folds thick around the whole head and covered with a dry cloth to retain the heat, and also hot wet cloths over the stomach and bowels, will greatly assist the emetic and inunction with hot oil, in making a thorough revulsion of the disease from the bowels to the skin. Dr. Taylor, of Livingston Parish, La., too wise to be deterred from a good remedy because quacks abuse it or make it a hobby, revulses to the skin by placing the patient in a chair with an open bottom, covering him with blankets, and then setting fire to some whiskey, in a shallow vessel, placed under the chair. The cheap kind of whiskey, that which will barely burn, is the only kind that should be used; because with alcohol or good proof spirits, the flame would reach too high and burn the patient, and scorch the blankets. A tin tube, some six or eight feet long, one end fixed in an opening made in the lid of a tea-kettle, containing boiling water, and the other end placed under a chair, in which the patient is sitting, covered with blankets, is another method of revulsing to the skin by steaming, not without some objection, however, on account of danger in fat or plethoric patients, from determination of blood to the brain.

The late Dr. J. A. McPheeters, of Natchez, a more plethoric and larger man than Prof. Warren Stone, of New Orleans, and like him, a greater giant in mind than in body, fell into a fit of apoplexy immediately on rising from a chair in which he had been taking, for some slight indisposition, a hot-water vapor bath, and died in less than an hour. In a plethoric lady of the same city, who seldom or ever perspired, this kind of vapor bath was used to relax the surface, but it brought on convulsions while she was in the chair. Happening to be near at hand, I succeeded in relieving her. In the case of another

plethoric woman, the steaming process caused an alarming vertigo, flushed face, thirst and dryness of the mouth, hot skin and anhelation. Called in great haste to her, I was at first at a loss to know what was the matter, or what to do. But the mystery was soon explained on ascertaining that a steam doctor had just left the patient's room as I came in.

From these and other cases that might be named, and from the inconveniences often attending the application of the vapor bath, as a means of revulsing to the surface and inducing perspiration, I prefer cloths, many folds thick, wrung out of hot water and applied all around the head and over the stomach and bowels, as more convenient, safe, and effectual than the vapor bath, or the hot bricks of the Thomsonians. After perspiration is induced, it should be kept up by drinking warm teas of a kind most agreeable to the patient, assisted, if necessary, or if the stomach does not retain them, with full injections of warm whey, or chicken-water, retained for a time in the bowels by external pressure. In the late terrible epidemic yellow fever of New Orleans, which was of a decided typhoid type, a similar plan of treatment, to that just mentioned for typhoid dysentery, was, I am quite sure, more successful than any other. The cloths wrung out of hot water, applied around the head, over the stomach, and to the back, and a full dose of the ipecac, mustard, cream tartar, and charcoal powder, above mentioned, almost immediately relieved the pain of the head and back and brought out a general perspiration, which had only to be kept up for twenty-four hours, and then the patient to be cooled off by cold water, and the bowels moved by a purgative to effect a cure.

The sweating process, if properly conducted, adds to the strength, instead of reducing it, if more fluids are absorbed than excreted. Neither dysenteric nor choleraic stools can co-exist with it, and it is the surest means of eliminating from the system those subtle substances which attract and feed malignant maladies. I have now under my charge a patient—a hard drinker—in whom the whiskey in his blood is being washed out by keeping up a perspiration and supporting the same with copious draughts of cold water. The sheets and blankets, enveloping him, are from time to time changed. Their strong odor of whiskey, after every sweat, proves that the work is

going bravely on. He will, no doubt, be left a free-agent when all is out.*

The appearance of any malignant disease on a plantation, while other estates in the same neighborhood are healthy, affords presumptive evidence that something is wrong—that some cause, natural or artificial, lies hid—requiring the light of medical science to find and remove it.

The lowest duty of an overseer and the least valuable part of his services consist in watching the negroes to see that they perform the required labor, in any kind of work they are accustomed to and know how to do. When healthy, contented, properly provided for and under good discipline, they will work of their own accord as much as their own health and comfort and their master's interest require they should, without watching or driving. Hence the overseer can spend his time much better in watching and guarding against those causes, which breed epidemics or discontentment among them. When they have not sufficient vegetables and ripe wholesome fruits, the adults, as well as the children, are apt to have worms and a morbid condition of the blood, showing itself in spongy gums receding from the teeth, an ashy color of the skin or a morbid state of the glandular system. Worms, glandular enlargements, a scorbutic state of the gums or an unhealthy state of the skin are so frequently found to be associated with cholera and dysentery as to appear as causes of those maladies. They are effects, not causes of that state of the system, which predisposes to fatal attacks from these and the like ailments. Negroes thus affected are less apt to recover than those whose constitutions have not been impaired or a predisposition to disease generated by improper management, as exposure to the weather, deficient bed-clothing, want of fuel or a laxity of that wholesome discipline, which alone can restrain them from running about at night and other vicious practices injurious to their health, morals and comfort.

* Since the above was written, the patient alluded to, has been made a free agent. His health and strength have greatly improved. He has no desire at all for spirituous liquor. His nervous tremblings have ceased, and he feels like a new man. The mode of washing out the spirituous liquor in the circulating fluids, was the same as that pursued in the cases of Bulwer and Prentice. The negroes call it "*steaming with cold water.*" He recovered as soon as the sheets and blankets wrapped around him, during the sweating process, ceased to have a whiskey odor. He has gone, or is able to go to his business in the editorial sanctum, which he has long been absent from.

It is not to be expected that the overseer or master can guard against all the causes of disease and untimely deaths among negroes, particularly among children, without knowing what those causes are. The medical adviser of the plantation should make it his business to point them out. Unfortunately our system of medical education is very defective. It is acquired in schools where negroes are almost entirely unknown and their diseases unrepresented, or in branches of such schools located in the South, where chairs are established to teach every thing else relating to the healing art, except the anatomy, physiology and diseases of the black population; about equal in numbers in the Southern States, to the whites, and whose labor produces three-fourths of the exports of the whole United States. Our system of medical education is defective, because it is almost exclusively directed to the causes, prevention and cure of the maladies of the white race in cold climates; thus leaving the profession almost entirely in the dark in regard to those agencies and influences, which, acting unseen, unknown and uncared for, produce more than half the mortality on Southern plantations.

In the letter to my Alabama correspondent, I reminded him of some matters, which I have already touched on in these hasty remarks. For instance, that so strong is superstition among negroes and so enervating is panic, that they often die without being sick; at least without any physical cause sufficient to account for their death. I stated that when the air, in or about the hospital or quarters, is suspected of being corrupt, and cholera, dysentery or any malignant or unmanageable disease makes its appearance, an immediate removal to the open fields or woods, of both the sick and the well and the erection of temporary booths or sheds over the sick *after* the removal is made, is the only sure and safe means left the planter to save his people from being decimated—perhaps half destroyed—by the combined action of disease and panic. Under such circumstances the best system of medication cannot be depended on, because the negroes themselves, when panic struck, cannot be depended upon to report or let their indisposition be known, until they are nearly half dead and can no longer conceal it. They will often hold the most nauseous medicine in their throat, for hours together, without swallowing it, waiting for an opportunity to spit it out. Hence the necessity of an

entire change of air and scene—not from one set of houses to another, but to abandon houses altogether. When houses are altogether abandoned and negroes are carried back to an imitation of barbarism in the woods or fields, a revolution takes place in their ideas and they almost immediately recover from that mental stolidity, so apt to fall on a greater or less number on every plantation, from the occurrence of a few fatal cases of some new or strange disease. Under such circumstances they very commonly have some undefined or indefinite notion that they are conjured, bewitched, or from some cause or other, inevitably doomed to die. When such a notion becomes the ruling idea in their shallow intellect, it greatly tends to counteract the best directed medical treatment for any bodily ailment they may happen to be afflicted with. They see in the remedies, given to cure them, the agents of the sure and certain destruction that awaits them. This state of mind does not affect all the negroes on the plantation, but only a few at a time. Those afflicted with it are very artful in concealing it, believing that the very medicine that cures others will kill them. This is different from that morbid state of mind, connected with drapetomania and dysæsthesia that I have heretofore wrote of in this Journal—diseases witnessed on almost every plantation under bad management—and which the medical faculty seem to disbelieve in, because they are not treated of in the text books. That mental distemper, above alluded to, which makes cholera, dysentery, &c., so fatal on plantations is cured by carrying them back to an imitation of barbarism in the open fields or woods, with as much certainty as the Swiss are cured of nostalgia by re-visiting their native Alps.

Although, a removal to the field or woods is the best means known, it is not the only one. A few steps into that unexplored field—*the psychology of the Ethiopian*—will show that it is rich in antidotes for those poisons of the mind, that not unfrequently make the negro's moral nature blacker than his skin, and which re-act with such telling force upon his physical system as to baffle the physician who does not take cognizance of them, not only in the treatment of such diseases as typhoid dysentery, but negro consumption, and many other maladies now so fatal among them.

ART. II.—OSTEO-SARCOMA OF THE CLAVICLE—OPERATION.

BY J. A. OWENS, M. D.,

I beg leave to report, through the *New Orleans Medical and Surgical Journal*, an account of an operation performed by me on the 3d of this month, which consisted in the removal of about one-half of the left clavicle from its sternal extremity, for an osteo-sarcomatous affection.

The patient, a negress, aged about 35 years, the property of Mr. Gaster, has been suffering for about twelve months with severe pain in the region of the left clavicle, extending to the shoulder and along the arm of the same side, with considerable enlargement of the bone near its sternal articulation. The disease continued gradually to increase without any alleviation from the remedies that were used up to the first of this month, at which time the tumor was about the size of a hen's egg, and the patient had entirely lost the use of her left arm: I came to the conclusion, under the circumstances, that an operation by removing the bone afforded her the only chance of relief. I invited Drs. Hankins and Gillespie to see the case with me. They fully concurred with my opinion, and kindly consented to assist me in the operation. Saturday morning being the time appointed, we met and proceeded to the

Operation.—The patient being under the influence of chloroform, I made an incision down to the bone, from the sterno-clavicular articulation to the middle of the clavicle; at the termination of this incision, I made another at right angle with it; I then dissected the soft parts from around the bone sufficiently to enable me to pass a straight steel elevator beneath it, when, by using a carpenter's tenon-saw, (in the absence of a more scientific instrument) the division of the bone was effected. The dissection was then carefully continued from the point of division towards the sternum. Dr. Hankins, by elevating the bone and keeping the parts on the stretch, enabled me to dissect beneath it with less difficulty and danger of wounding important blood-vessels, which lie almost in contact with it. The dissections being carried to the articulation, the knife was

passed around the capsular ligament and the disarticulation effected. The subclavian artery and vein, were laid bare for an inch and half in their course. During the operation but two vessels were divided, neither of which required a ligature, and not more than three ounces of blood were lost. The wound was brought together with stitches and adhesive straps; a compress and bandage were applied, and the woman put to bed. This is the tenth day since the operation. Her recovery has been rapid; union is complete throughout the extent of the incisions: the shoulder remains in its proper position, and she has very good use of the arm. Small and superficial as the clavicle is known to be, no bone in the body has more important surgical relations; below it, and nearly in contact with it, pass the subclavian artery and vein, and brachial plexus of nerves; and, just above it, is found the termination of the internal and external jugular veins. Many arterial branches are found on its upper and lower surface, which are often increased in size when the clavicle has become so enlarged by disease as to make pressure on the subclavian vessels. The surgeon, therefore, should have a precise knowledge of the position of these parts, and especially of those connected with its internal extremity, which cannot be disarticulated without more or less difficulty and danger; the least slip of the knife being liable to open a vein, so as to allow the entry of air into the circulation, or give rise to hæmorrhage that it would be difficult or impossible to arrest.

MONTICELLO, Ark., June 13, 1854.

Dr. Owens in a letter accompanying the above interesting case, remarks that the operation for the removal of the clavicle "has been performed but a few times in the United States." He might have included all Christendom in this proposition. Indeed, this is to a very great extent an United States operation, having been, as some writers contend, first performed by Dr. Mott, of New York, who, in 1828, removed nearly the whole clavicle for an osteo-sarcomatous tumor as large as a man's double-fists, which was so vascular as to require more than forty ligatures. The patient recovered, and was a few years ago in excellent health, and enjoyed the almost complete use of his arm. Dr. Warren, of Boston, four years later performed a simi-

lar operation, which, however, was less formidable in regard to hæmorrhage, though the patient lived only a month after.

M. Velpeau, in the first edition of his operative surgery, republished in this country in 1835, calls this operation a daring attempt, never carried into effect except in a few cases of operations upon the shoulder. "Yet," says he, "there are circumstances sufficiently numerous which require this operation, if we wish not to abandon the sufferers to certain death." M. Velpeau, himself, in a case of necrosis of this bone, removed its sternal third. Other examples of its partial and even entire extirpation might be enumerated, among which none is more complete than that of Meyer, "who, in 1823, removed the whole clavicle for caries, and in seven weeks the wound healed. The periosteum, which was in this case detached from the bone, and allowed to remain, formed a new ossific deposit; and, at the time of the patient's death, which occurred five years afterwards, a thin bone was found to have been formed, sufficient to support the movements of the arm, three inches and three-quarters long, united to a fibro-cartilagenous ligament, extending from the sternum to the acromion." (Pancoast's Oper. Surg. 118.)

M. Lisfranc's valuable work on operative surgery (*Précis de Médecine Opératoire*,* Tomes III, Paris, 1845-6-7,) under the head—*extirpation of the clavicle*, mentions two authorities only, that is, Mott and Warren; ii. 596; though in two preceding pages in treating of operations upon the scapular and sternal portions of the clavicle several authors (without any dates) are alluded to. M. Lisfranc regards the removal of the clavicle when unaffected by tumors and enlargements as devoid of any great difficulty.

Hence it is very evident that the extirpation of this bone in simple necrosis is much easier, and less dangerous than in osteo-sarcoma—a compound of osseous and fleshy degeneration verging closely on the

* It is remarkable that Lisfranc's *Précis* has not been translated, and republished in the United States, seeing that it is a most able work and displays an acquaintance with the progress of surgery beyond the confines of France. The two first volumes which he lived to complete, amount to 1861 pages; the third volume printed after his death from his unfinished MS. consists of 332 pages, making 2193 pages—a rich treasury of facts, personal experiences, enlightened criticism, and literature—a grand monument to his fame that his exaggerated accounts of successful amputations of the neck of the uterus, as given by M. Pauly and others, cannot destroy. (Sec, *Maladies de l'Utérus d'après les Leçons Cliniques de M. Lisfranc; par II. Pauly.* Paris, 1836, pp. 536.)

characteristics of cancer—growing often to an enormous size, forming extensive connections with surrounding organs, and being occasionally very vascular, even like anastomosing aneurism.

Professor Pancoast, in his account of the resection of the clavicle for necrosis, remarks,—“An operation somewhat analogous may be required in cancer of this bone, but in which case, if the tumor is large and irregular, the difficulty of resection will be singularly increased.”—(*Oper. Surg.* 117.)

Professor A. J. Wedderburn, M. D., of the University of Louisiana, on the 21st of January, 1852, removed the entire clavicle (which was carious) from a young man, who on the following day was able to sit up. The clavicle was so decayed that it broke during the operation.—(*N. O. Month. Reg.* Oct. 1852.)

The clavicle is of high import in zoölogy, serving as a distinctive feature in the classification of certain vertebrated animals having an anterior thoracic member or arm which may be forcibly brought towards the breast, as in man, apes, birds, bats, and the like, while in animals whose limbs move not laterally, but longitudinally as the horse, and other hoofed animals, it is wanting. Many writers on human anatomy, in their deductions concerning the physiological (more properly the teleological) anatomy of the shoulder-joint, have fallen into an error as to the essentiality of the clavicles for the separation of the shoulders and for the use of the arm; or, else, the clavicle must soon after its removal, be replaced by a new one, seeing that in nearly all the reported cases of its extirpation whether entire or in part, the use of the arm has been speedily restored.—[EDITOR.]

ART. III.—TRISMUS NASCENTIUM. ✓

BY ROBERT H. CHINN, M. D.,

OF BRAZORIA, TEXAS.

This disease ranges more extensively in the low, marshy, than in the uplands of the South. Along the Gulf coast, where the land is flat and dotted over with salt-marshes, even to the highlands of the interior, it is very common, except where the pine ridges jut up near or against the coast. It prevails in the Antilles and other tropical

countries. It is almost universally fatal, and peculiar to a certain period of life when the system is least able to bear a disease.

Causes.—The improper tying and management of the umbilical cord, which cause abrasion, irritation, congestion, and inflammation of the cord, which may extend to the umbilicus, the abdomen, and its viscera; the compression of the abdomen by a very tight belly-band, that will not permit of the action of the abdominal muscles, nor give room for the expansion of the viscera; improper handling of the child, by which compression or congestions of the various organs are caused; cold and improper impressions made on the skin, which are conveyed to the cerebro-spinal axis—the same that increase and prolong tetanus in the adult. I have never observed an inherent cause; no doubt such may exist either in the mother or in the locality. The compression of the occipital bone cannot be caused by the birth, because our women are as well formed as any in the world, and it is too general a disease to arise from exostosis of the pelvis, or any pelvic prominence, and because a woman may have a dozen of children, and numbers indiscriminately die from this disease. It is thought that some localities produce it more than others; but, cannot that supposed cause arise from the fact that certain habits are acquired and held, too, for life by some people? In reference to this matter the following facts are submitted: On a plantation every one may die from the disease; on another, a few yards off, presenting naturally and artificially all the same features, and the children live; both have their own nurses—commence planting at the same time—both lost at the first by the same disease: at first they tried to stop its progress; one persevered and eventually succeeded, and the other always regarded it as a peculiarity of his place. The predisposition to this disease is general, and extends over all that section of country which presents the same geological appearances. It requires preventives, slight in their appearances, but of the most vital importance.

Mr. P. D. MacM. opened a plantation, sometime before 1828, on Gulf Prairie, and has not saved more than twenty children, though his force was always large. The following cases occurred on his place:

Case 1.—September 15th, 1851: a negress was delivered; her child was treated according to directions herewith laid down; child is now alive.

Case 2.—October 5th, 1851: a negress was delivered; the child treated in same way; is now alive.

There are several large plantations in that neighborhood, as the location is extremely desirable, all presenting the same general features, and raising the same produce. The plantations have existed for a great many years; most of the owners are connected by family ties; the original settlers having been three families, and indiscriminately on some of them there are the greatest number of children, ranging from birth to adult; on others hardly a child.

Symptoms.—The state of the mother, before the birth, would probably give the surest indications for such attention as ought to be paid to premonitory symptoms. Mothers, who by nature, by education, by habits or manner of living have acquired the nervous temperament, or who are accustomed to spinal irritations, nervous and hysterical symptoms, sleeplessness, or who are weakened and debilitated by some drain that renders the system prone to any anormal nervous impression, will very probably transmit that impressibility to their offspring, and it becomes more apparent in the first few days of the child's existence. The premonitory state, applicable to the child from an excess of liquor amnii, or disease of the fœtus, or morbid condition of the placenta, or some constitutional disturbance of the mother, may have caused the child to be emaciated at birth. For the first day or two of its existence the infant nurses and sleeps well; its excrements pass naturally away; the mother will nurse it, lay it down, it will fall asleep, but the muscles of the brow will contract; it will suddenly start, awake, and cry in tones somewhat unnaturally harsh: again, it will become quiet, fall asleep, start, awake, and cry: the intervening time between these cries become longer and longer, until they cease. The mother will again nurse it, and after nursing it a minute or two it will stop, and cry in more harsh tones; a repetition of this will occur several times, then the child ceases to nurse, takes a nap, and the disease has fully set in. The tones of the cries assume, more and more, a harsh feebleness, until it becomes almost imperceptible; emaciation increases, and at last the little sufferer dies from inanition. This is the mildest form of the disease. When the contraction extends to the muscles of the face there is a strange expression of the countenance; the mouth may be partially open and the lips drawn forcibly in, presenting in appearance a small circular aperture, or the lips may be firmly drawn

out showing a portion of the upper and lower gums, or the jaws may be firmly locked against each other, the lips expanded, showing the gums and their contact, or the lips may be contracted and puckered, the levator labii superioris alæque nasi muscle of one or both sides may be contracted, drawing up one or both corners of the mouth; the nostrils may be elevated, or expanded, or drawn in and contracted. The contraction seems to be generally around the mouth, but the rest of the facial muscles may contract, entirely changing the usual placid expression of the infant. The posterior part of the body is more liable to these contractions than either the anterior, right, or left; the head then will be drawn far back, the spine curved inwards, the abdomen and chest prominent, the thighs straight, and even the legs and feet drawn up, when on the anterior region the opposite state exists, and both interfere to a considerable extent with respiration. But the contractions of the right or left side very rarely occur. The muscles in this state do not relax only for a short period. Death may occur both from inanition and asphyxia, caused by spasm of the respiratory muscles. In this asphyxia the lips and face become purple, the arteries and veins of the neck prominent, the motions of the chest considerably abated, if not checked, and the air passes in and out of the nostrils and sometimes the mouth, with extreme rapidity; pulse at the wrist imperceptible, in the carotids excessively small and quick. In the inanition of this complaint the little sufferer lays contorted in a curious way, and sleeps its soul into eternity.

Case 3.—July 10th, 1850: Mrs. L. had been delivered; three days afterwards called; examined the child; it could not nurse, nothing else anormal; under extreme quiet and rest it recovered.

Case 4.—November 20th, 1852: Mrs. W. was delivered; two days afterwards the child could not nurse; it was not suffered to be touched for twelve hours; then attended to, it nursed a little, and the same course, with shorter intervals, was pursued until it recovered.

Case 5.—June 15th: Servant of the Hon. J. G. MacN. was delivered; three days afterwards called; jaws firmly set, mouth open, lips drawn in, brows contracted, paroxysms lasting about fifteen minutes; the period between them grew shorter and shorter. ℞ Calomelanos, iv. grs.; Pulv. Doveri, viij. grs.; Sulp. Morphia, grj., in pulv. No. xvj.; one three times a day. Child died on the fourth day from that time.

Where the umbilicus and subjacent parts are violently irritated, convulsions are very apt to attend the disease, and from compression of the occipital bone we naturally turn to coma, and besides the other terminations in death, we may have the tongue coated, dry, and harsh; lips parched, skin hot, pulse one hundred and eighty or two hundred, bowels constipated, and no urine.

Case 6.—November 28th, 1853: Mrs. W. was delivered; in two days visited her child; it could not swallow, violent convulsions every ten minutes, not able to count the pulse, tongue dry and harsh, skin dry, umbilicus abraded; applied poultices. Child died in four hours.

The diagnosis is easy. The most prominent symptom is the inability to swallow. But the unnatural curves of the neck or body, the forcible contraction of the facial or other muscles, and the paroxysms becoming gradually longer, will enable any one to distinguish this disease.

The Prognosis.—Recovery from the mere inability to swallow may occur, but the other symptoms mark almost certain death.

Pathology.—The cord may be natural, dry, or moist, and flaccid, empty, and closed at its umbilical orifice, or filled with a clot of blood or pus, and open or not at the same place; the skin covering the umbilicus natural or abraded, the umbilicus normal, or presenting every stage, from irritation to inflammation; then the parietes of the abdomen, urachii, peritoneum, liver, and capsule of glisson may bear the marks of progressive inflammation, and the umbilical vein (afterwards the round ligament of the liver,) may be filled with pus or blood, somewhat clotted. The occipital bone may be driven in bending or lacerating the lambdoidal or posterior fontanel, but the membranes and the brain seldom present any abnormal conditions. Some slight red lines are at times traced on the gastro-mucous membranes. The little sufferer may die, and a post mortem examination reveal nothing.

At all stages of life the weakness of the nervous system increases its impressibility, and liability to assume a morbid action. In the adult, as in the new born infant, poor blood causes a deficient nutrition, and its action upon the nervous system of both is similar; the difference between the two may consist in one thing—the entire system of the adult is matured but in the infant it is only sufficiently so for extra uterine life. Hence also the maturity of a tissue enhances

its power of resistance to any morbid action. The umbilical cord is a vital organ to the foetus—but an unnatural appendage to the infant. During intra-uterine life the capillaries of this cord receive its circulation from the blood passing through the cord at birth; this current of blood is stopped; the parietes of the cord having no blood-vessels, no nerves, no lymphatics, that communicate directly with the child it is effectually dried, taking on a state somewhat peculiar to itself, and resembling gangrena seniles, until, finally, the cicatrization of the umbilicus destroys its connection with the child. Whatever disturbs this process conveys a morbid action to the umbilicus by continuity alone, after which, the constitution fully sympathises, and all predispositions are brought into play. The softness of the fontanel very clearly proves that a degree of compression can be without injury exercised upon the bones of the skull, but their increased firmness shows that the danger of that compression would increase. There is no organic derangement of the cerebrum peculiarly marked by spasms, while such occurrences in the membranes are attended with convulsions; in these cases it may be functional, as the post mortem examinations show nothing.

Treatment Preventive.—Select a place on the cord, from four to six inches from the abdomen, wrap it well and tightly with a tape about an inch wide, for one or one and a half inches; in the centre of such a bandage put your ligature. The wrapping ought to be sufficient to stop the hæmorrhage, the ligature serving to maintain the bandage in its position; between the bandage and placenta, cut the cord. In ligaturing the vessels of an adult, their inner coats are purposely bruised, adhesive inflammation completes the stoppage of the hæmorrhage; it is followed by ulceration, and the ligature is expelled as a foreign body; but, here, the first object is to stop the hæmorrhage, the foreign body being the vessels themselves. A morbid process, by continuity, is that in which the action passes to, or rather grows on the tissues in contact with each other. The same occurs in the drying up of the cord; the first, unnatural; the second, healthy, and as both cannot exist at the same time, they present opposite characteristics: the second affording a relief from an incubus, while the first may be

determined to a diseased action of the system. Let the bandage for the abdomen, and the clothes in contact with the skin, be composed of the softest materials, so as to avoid abrasion. All bandages and articles of dress about the child should be extremely loose. The compression of any or all parts, causes serious lesions of the abdomen, it stops the necessary action of the stomach and bowels, renders the liver torpid and liable to congestions, the action of the abdominal muscles difficult, puts the diaphragm on the stretch, causes the abdominal viscera to encroach upon the thoracic cavity, and prevents the expansion of the body. Place the infant upon a soft, thick pillow, and that pillow upon the bed, and do not remove the child from the pillow for twelve days. The disease commences, or its predisposition is acquired, within the ten days, but there ought to be an extra allowance. Another consideration calls for the pillow, the different positions in which the child is held, not only deranges the locality of the various viscera, and stretches the connexion between them, but also causes temporary congestion in those parts to which gravitation may call the blood.

Case 7.—February 10th, 1853; Mrs. B.; treatment as already detailed; child now living. Case 8.—June 5th, 1849; Capt. S.'s negress; treatment same; child now alive. Case 9.—June 8th, 1852; Capt. S.'s negress; same treatment; child now alive. Case 10.—April 4th, 1852; Hon. J. G. MacN.'s servant; treated as the rest; child now alive.

I cannot call to mind, nor does my case-book furnish me with a single instance of the occurrence of this disease where this simple treatment was faithfully carried out.

Remedies.—I know none. For a hot skin, dry, parched, or coated tongue, diminished urinary, or alvine discharges, from the sixteenth to an eighth of a grain of calomel repeated p. r. n.; for an enlarged, swollen, hard, or soft and tender abdomen, anodyne poultices. For a swollen, congested, abraded, or inflamed umbilicus, the simplest cerates and emollient poultices, with or without anodynes. For convulsions, hyosciamus.

ART. IV.—TWO CASES OF OSSIFICATION OF THE PLACENTA.

BY JOHN HENRY JOHNSON, M. D.,

OF KARNES COUNTY, TEXAS.

Case 1.—On the 20th July, 1852, I was called to visit Donna Flora P. del Gava, aged 26, a Spanish lady, a resident of Bexar county, in which county I at that time resided. She had been delivered of a fine boy the day previous; but the placenta was yet retained. I learnt on inquiry, that considerable hæmorrhage had taken place immediately after the birth of the child; in fact, I scarcely needed information of this, to convince me that such had been the case, for her countenance and general condition was alone sufficient to assure me of this. This was her fourth child. I proceeded at once to make an examination. I found the placenta adhering firmly to the uterus, and commenced at once a dissection with my finger-nails, and in about fifteen minutes succeeded in detaching the placenta from the uterus, and in a few moments a contraction of the uterus expelled the placenta. No hæmorrhage of consequence followed. It was some two months before the patient was in perfect health, which I have no doubt was caused by the great hæmorrhage on the birth of the child. To use your ideas—"the bony matter intimately connected with the placenta, consisted chiefly of plates with fine cancelli and reticulations, resisting pressure like the egg-shells of small birds."

Case 2.—January 26th, 1854: Bridget, aged 32, the property of A. C. Davis, of Karnes county, was delivered of a fine girl, after a painful and prolonged labor of thirty-eight hours; considerable hæmorrhage; waited one hour; made an examination, found the placenta firmly and rigidly adhering to the uterus; hour-glass contraction; succeeded, after considerable difficulty, in detaching the placenta, which by a strong contraction of the uterus was expelled; no hæmorrhage of consequence. The recovery was as favorable as could be wished for. This was her seventh child. In neither instance had any difficulty been experienced at any previous birth, nor had the attention of a physician been required. The appearances of the placenta resembled each other so much, in both instances, that it is

useless to give a description of the last one. Both individuals reside on the San Antonio river, the water of which is of the strongest limestone.

I have given you only the facts, in my own crude manner, leaving it to older and wiser heads to enlighten us young physicians with the physiological and pathological causes of those ossifications of the placenta.

KARNES COUNTY, Texas, June 18th, 1854.

ART. V.—AN ESSAY ON YELLOW FEVER.

Devoted to the discussion of the two following important and interesting questions: First.—Is yellow fever identical with bilious fever? Second.—Is yellow fever a contagious disease?

BY W. J. TUCK, M. D.,

OF MEMPHIS, TENN.

The subject of fever, is, perhaps, the most interesting and important that can engage the mind of the physician. It is estimated by a learned and eminent medical author, Dr. McIntosh, that four-fifths of the deaths that have occurred in the world have been occasioned by fever under its various forms and modifications.

That form of fever, usually denominated yellow fever, from the characteristic color of the skin will engage our attention in the present essay.

The discussion of this subject, we think, may be regarded as particularly appropriate under the present circumstances. The most fatal and malignant form of this remarkable disease has recently swept, with its deadly wing, over the fairest, the most fertile and populous portions of our Southern country. Perhaps, no epidemic known in the annals of medical history has spread so much death and desolation in its fell career. The type has not only been the most malignant previously known, but is regarded as having presented some new phases, not hitherto noticed, and now undergoing the investigation of some of our most able and scientific Southern physicians.

It is true, that those cities situated in a more Northern region of

our great valley have entirely escaped its ravages, but they cannot tell how long they may thus be blessed. Its broad and lengthened death-shade, extended, during the past autumn, over a wider scope of territory than has ever been known before, and for ought we know, another fell incursion of this ruthless enemy of human life, may break over its previous confines and carry with it a similar train of death and desolation.

It is well, then, that this subject should claim our most earnest and careful attention, and that we should endeavor, as far as human capability will permit, to investigate and understand its *nature*, its *cause*, its *laws and its modes of diffusion*, its *pathology*, and the *best modes of treatment*. We have endeavored, amid various interruptions, to arrange and methodize our thoughts and views in relation to several of these points. The limits we have assigned ourselves will not admit a discussion of them all. Having had occasion, at various times, to examine the productions of many able and learned authors who have carefully investigated the subject, and having also enjoyed the opportunity of personal observation and experience, while residing in New Orleans during the prevalence of an epidemic, we shall endeavor to give the results of our investigations and observations in as plain, clear and concise a manner as it is in our power to do. The object of all medical investigation is *truth*—the only thing indeed worth attaining—and by this principle we shall, at least, attempt to be governed in the present discussion.

Yellow fever has been styled by some authors an American disease. It is, indeed, a very singular and unaccountable fact, that it prevails much more frequently, and to a much greater extent, on the American than on the Eastern Continent, even where the climate, the density of population, and all other circumstances in the latter would seem especially to favor the production of the disease. It is well known to be a very frequent epidemic in most of the Southern cities of the United States, and in the cities and sea-port towns between the tropics in America, it scarcely ever ceases to prevail to a greater or less extent; whereas, it is almost unknown in Asia, Eastern Africa, and the Southeastern portions of Europe. It is said never to have been found in some of the most densely populated cities in Southern latitudes, such as Canton, Calcutta, Alexandria, Smyrna, and Constanti-

nop'le, while it would seem that all the favorable circumstances which appear to produce or develop the disease, would render these cities much more liable to its invasion than Philadelphia and New York, where it has at different times prevailed with great fatality. No writer, we believe, has been able to give any rational explanation of this remarkable peculiarity. This peculiar feature of the disease will be again alluded to as bearing upon some of the points of the questions under discussion.

We come now to the discussion of the question—whether yellow fever and bilious fever are identical? Dr. Rush, and some of the eminent medical writers of his day, as well as of the present period, have contended for the identity of these two diseases, and regard them as originating from the same cause, and differing, not in kind, but only in degree. We have endeavored to examine carefully the arguments of those who have had the best opportunities of investigating the nature of yellow fever; and, as a result of this examination, as well as from our own personal observations, we are compelled to dissent from this opinion, and have been unavoidably conducted to the conclusion that yellow fever is a peculiar and specific disease, originating from a specific cause, and differing essentially in its nature from the bilious remittent fevers of our country. In this opinion we are sustained by a very large majority of the best medical authorities, and of the most observing and experienced practitioners who have resided and practiced in Southern latitudes.

The first argument we present in support of our views, is that which is very naturally and logically derived from a statement of fact, formerly made, namely—that yellow fever has never been known to exist in the most populous cities of Europe, Asia and Africa; while it is a well ascertained and admitted fact, that bilious remittent fevers have been observed to occur very frequently, and sometimes in the most malignant and fatal forms, in those very cities and localities where not a case of yellow fever has ever been found to exist.

It is useless, we think, for the advocates of the identity of these two diseases, to argue that if the causes which produce bilious remittent fevers were more concentrated or powerful, that yellow fever would be the result. Is it not a well known fact to every practitioner

in our Southern States that thousands have been swept away by violent and malignant attacks of remittent fevers, unaccompanied, both in their progress and termination, by any of those well known symptoms which characterize yellow fever? Old and experienced Southern practitioners have informed me that, although they had been practicing medicine for twenty or thirty years, during which time they had been constantly attending cases of bilious fever, yet, not one case had ever come under their treatment or observation, similar in symptoms to those causes of yellow fever which they had witnessed in cities where the epidemic prevailed. We recollect to have heard a statement made by Dr. Fearn, an old and eminent practitioner of Huntsville, Alabama, that while he was practicing in that city, a most malignant and fatal form of remittent fever made its appearance, and that every case terminated fatally, spreading a panic throughout the whole country, and yet we could not learn that any of these cases terminated with symptoms of yellow fever. Here was a cause, we might suppose, sufficiently concentrated and violent for any purpose, yet we do not find the effect, which gentlemen contend for, who advocate the identity of bilious and yellow fever. It does really seem reasonable to suppose, that if these disease be identical, we should certainly have, at least, *some cases* presenting the usual characteristics of yellow fever, in those numerous districts of our Southern and Western country, where bilious remittent fevers prevail so frequently, so extensively, and often so fatally, and, yet, where the oldest practitioners will tell you they never saw a case of yellow fever.

In the Eastern part of Virginia, in the Roanoke Valley, the inhabitants are subject to severe and sometimes fatal attacks of bilious remittent fever. We remember, while residing many years since in this region of the State, that the whole community was startled by the sudden death of one of our merchants, who had recently returned from a visit to New York, where yellow fever was prevailing. A few days after reaching home, apparently in fine health, he was seized with the most violent symptoms and died in a few days; his physicians declaring they had never witnessed a similar disease, but pronounced it yellow fever, to the best of their knowledge, as the attack was accompanied by all those peculiar characteristics attributed to that disease.

Now, all these facts and statements cannot be gainsaid by gentlemen who hold opposite views on this subject, and in arriving at a correct conclusion on the question under discussion, we hope that these facts and arguments will command their due consideration and importance.

Another circumstance may be here briefly noticed, which, to some extent, distinguishes yellow fever from bilious fever. It would seem, from our own observation and from the statement of others, that one attack of bilious fever does not tend to act as a preventive of a second one, but appears in some instances to predispose to a second attack of the same disease; and, we were once acquainted with a gentleman who was in the habit of having an attack of this disease every autumn, and regularly expected it. The writer has suffered from repeated attacks of bilious remittent fever during his residence in the Southwest. On the other hand, although physicians differ with regard to the degree or extent of the protection, yet, we believe, it is a generally admitted fact that one attack of yellow fever, is to some extent a protection against a second one, and that a second attack is comparatively of rare occurrence with those who have not lost their acclimation by residence in a more Northern latitude. As an argument in favor of the identity of these two diseases, it is contended by some that they are known to prevail at the same time, and that yellow fever is often preceded by bilious remittent fevers, which latter assume the type of the former as soon as the cause becomes more concentrated. This circumstance, it is true, sometimes occurs—at other times it is not the case. Dr. Dickson states that yellow fever raged violently in Charleston, while the whole country around, as well as the city, was singularly exempt from bilious fever. In his work on the practice of medicine, the following statement is made by this gentleman, who has had ample opportunities of observation. He says: “In looking over our bills of mortality, the yellow fever years are not distinguished by any increase in the deaths from malarious fever, but very strikingly the reverse. They have prevailed but twice together in the last quarter of a century, viz., in 1827 and 1835. On the contrary, the summer of 1824 was one of terrible pestilence with us in the city, but there were no bilious fevers, and the surrounding country was healthier than usual. The same thing is true of 1828, when dengue preceded yellow

fever. In 1837, '38, '39, '40, we had yellow fever, but little or no bilious fever. Here, then, the line of distinction is broadly drawn, for here we have always subjects in abundance for the two forms of fever, their causes concurring, as in 1827 and 1835. Elsewhere, the apparent contrast might be ascribed to an exclusive or paramount agency of a more intense or malignant vehemence of the generative cause, which, it is clearly evident, is not even supposable here."

These facts would seem to indicate that in Charleston, at least, yellow and bilious fevers are so slightly cognate or related to each other, that they are scarcely ever known to occur together during the same season.

It is, however, true that in New Orleans and other cities, yellow fever is often preceded and accompanied by bilious remittent fevers, and it is a common statement, as well as a true one, that when yellow fever makes its appearance, all other diseases which may exist at the time, seem to be merged into the prevailing epidemic; and it may, indeed, be possible that a condition of atmosphere which gives rise to a prevalence of bilious remittent fevers, may predispose the systems of those who are subjected to this influence, to an attack of yellow fever as soon as there is a development of the specific poison which produces this disease; but this does not, by any means, demonstrate that the cause and nature of these diseases are identical. A condition of the atmosphere, or any cause whatever, might predispose the system to an attack of diarrhæ, would certainly aid in the development of an attack of Asiatic cholera, in those regions subject to this disease; yet, there are few perhaps who would not admit there must be superadded to this predisposing condition, some peculiar specific cause, in order to give rise to that terrible and inscrutable scourge which has spread such devastation throughout so many portions of our country.

Yellow fever has been styled by our standard medical authorities as a fever of one paroxysm, continuing without abatement for two or three days, and then ceasing entirely, either to terminate fatally or in recovery. In this respect, also, yellow fever differs from bilious fever. On the other hand, the advocates for the identity of these diseases contend that no such difference is found to exist, and that a regular remission takes place in one as well as the other. In reply to this, it is only necessary to state that while in bilious remittent fevers the re-

mission usually occurs at regular intervals of twenty-four hours, in yellow fever the paroxysm continues without abatement from thirty-six to forty hours, and sometimes to sixty or seventy. And, again, while in bilious fever there is only a partial abatement of the fever, to recur at a stated period, in yellow fever, when the paroxysm is over, there is a complete intermission—"a stadium," as Lining terms it, without any febrile excitement whatever. This period of repose—for it cannot be properly called remission—continues from twelve to twenty-fours, (the length of time depending upon the violence of the attack, and the character of the first stage,) after which time the patient sinks into hopeless collapse and death, or reaction and convalescence commence. Dr. McArthur, an eminent and experienced physician and writer, and who, for a number of years, had superintendence of a public hospital at Barbadoes, in the West Indies, had an opportunity of seeing much of yellow fever, and has written a concise and very able article on the subject, giving a history of it as occurring under his immediate observation. He makes the following statement: "I have never noticed a remission during the whole course of the fever. Several cases of remittent fever came under my care, terminating in the endemic." Other authors also speak of having seen many instances of bilious remittent fever terminating in yellow fever, and this probably has been observed by every practitioner of our Southern cities; and, thus it is very easy to explain how a careless observer may confound these two diseases, or class them as identical, overlooking the fact that all diseases which may exist at the time are prone to assume the form of the prevailing and overruling epidemic.

Again, it is the opinion of many who have had an opportunity of being much with yellow fever, that there is a peculiar expression of the countenance, in those affected with the disease, by which it is readily distinguished from all other forms of fever. In the first case of the disease which was brought to this city last summer, a friend of ours, Dr. Harris, was called to prescribe, and requested me to visit the patient in consultation, remarking, at the time, that there was something peculiar in the appearance of the patient's countenance and in the symptoms, which he had never seen before, although very familiar with bilious fever as occurring in his practice in Alabama and in this city. The patient, when I saw him, was in the midst of the severe febrile paroxysm, from which he had been suffering for more than

twenty-four hours—hot, dry skin, a red, muddy, yellowish complexion of the conjunctiva, and great gastric uneasiness were presented. Next morning, a “stadium,” or an entire intermission was observed, and to any one who had never learned the delusive character of this condition of entire repose, the symptoms would have indicated as favorable prospect of recovery as could have been desired. In a few hours, however, the skin assumed a deep yellow tinge, black vomit supervened, and to the great astonishment of several physicians who witnessed the case, the patient died before night; and, we are confident that none who witnessed the history of this case could ever be convinced that it was, in any respect, identical with bilious remittent fever, such as so often had occurred under their observation.

We remember distinctly the first case of yellow fever which came under our notice in New Orleans, in the summer of 1841. It was the first case which made its appearance in the city, and was taken to the Charity Hospital. Being anxious to become acquainted with the disease, I repaired immediately to the hospital, and the physician in charge directed me to his ward, requested me to examine the patient and give my opinion of his condition. When I saw him, the febrile paroxysm had entirely subsided—it was the third day from the attack—his pulse was calm and natural—complained of no pain—was entirely rational—informed me when he was taken ill—said he felt very well, and the impression produced on my mind was that he was free of disease, and would soon be well; and when the attending physician inquired how I found his patient, I at once responded, that he seemed to be doing finely and would soon be up. His brief reply was, that he would be dead by six o'clock that evening—it was then morning. I would not believe this statement, until it was verified by another visit to the Hospital, next morning, when I found that the patient had died about the time predicted, and we proceeded to make a post mortem examination. Such are some of the remarkable phenomena of this disease as have come under the writer's observation. We may here also appropriately allude to a remark made by Dr. Geo. B. Wood in his excellent work on the Practice of Medicine. Dr. W. states that “though perfectly familiar with bilious fever, when he first saw a case of yellow fever, he was at once struck with the latter as something he had never seen before.”

Again, the yellow color of the skin distinguishes this disease from

bilious remittent fever. Although this color occurs to some extent, occasionally, in bilious fever, yet it is comparatively rare, and may be said to form an exception to the general rule; whereas, this symptom so uniformly accompanies yellow fever, that it has given rise to the name by which it is almost universally denominated; and the absence of it is so rare that it constitutes an unexpected exception to an almost universal rule. And, moreover, it has been remarked by some close observers, that there is a peculiar dusky orange color in the latter which is not observed in the former.

The black vomit is another well known characteristic of yellow fever, and has been demonstrated by the best chemists as being entirely different in its nature and combinations from the dark discharges occurring occasionally in bilious remittent fevers. The black matter discharged from the stomach in the latter period of this disease, is regarded by the best authorities as "minute flakes of coagulated blood, suspended in gastric mucous, produced by sanguineous exhalation from the abraded surface of the mucous membrane of the stomach." It has been proven that the dark discharges in some of the "high grades of bilious and typhus fevers differ essentially from the black vomit of yellow fever. The former will dissolve in water and communicate a deep bilious tinge to it; whereas, the black matter which forms the *black vomit* of yellow fever, consists of small insoluble flakes which are held suspended in a viscid fluid, and will not communicate a yellowish or greenish tinge to water when agitated with it." Dr. Bancroft says: "In taste, also, they differ. The black matter which occurs in common bilious fevers is always intensely bitter; but that which is thrown up in yellow fever is either insipid or acid."

Dr. Rhees, of Philadelphia, remarks that on instituting a series of observations with the solar microscope upon black vomit, he found it to contain innumerable animalculæ. I remember Prof. Gibson, of Philadelphia, to have stated that Dr. Physic and himself made similar examinations, and these animalculæ were discovered. It is stated that "comparative examinations were made of the discharges of the stomachs of patients ill with autumnal bilious and remittent fevers, and no similar appearances were discovered."

We might further extend our observations, and allude to the different pathological phenomena of these two diseases, as revealed by dissection; but the limits we have proposed to ourselves do not admit

of a more extended discussion of this branch of our subject; and besides, we fear, we have already trespassed too much upon the patience of the reader. From the facts and arguments already presented, if duly considered, we are satisfied that the candid investigator of truth will be inevitably led to the conclusion, that yellow fever and bilious remittent fever are not identical diseases, and that they are *essentially different in their nature, and originate from distinct and different causes.*

We shall now proceed to enter upon a brief discussion of the question of the contagiousness of yellow fever:

It is hardly necessary here to allude to the great importance of forming correct views on this question. This is obvious, upon a moment's reflection. A correct decision of it is not merely a matter of curious interest to the mind of the speculative philosophers, but it involves questions of great practical importance to the world; such, for instance, as whether the substitution of sanitary or hygienic measures for quarantine restrictions would afford a more certain and effectual protection against the disease; a question evidently, and intimately connected with the great interests of the commercial world, as well as with the preservation and defence of the health and lives of millions of the human family.

The medical world, as is well known, have been long divided upon the question of the contagiousness of yellow fever; and volumes have been written upon the subject; many of the writers evincing feelings by no means creditable to themselves or the profession, and regardless of that calm, candid and dispassionate temper which should always characterize the medical philosopher in search of truth. In the opening of this discussion, the inquiry presents itself; what do we mean by a contagious disease? Let us endeavor to acquire as clear a conception as possible of the proper meaning of the terms employed in the question, and, thus, we may be able to remove difficulties which may otherwise thrust themselves in the way of progress towards the attainment of truth.

It is said that the celebrated metaphysician John Locke was led to the conception and performance of his great work on the conduct of the human understanding, from hearing two persons engaged in a long, warm and useless discussion, while he discovered that the greatest ground of their difference resulted from their not having a

clear understanding of the meaning of the terms employed to express the question under debate.

We attribute to the term contagion that meaning or sense in which it is employed by our best standard authors, and as established by common consent and general usage. This is the true mode of arriving at a correct meaning of words.

A disease is termed contagious which is capable of producing the same disease in another person, and thus capable of propagating itself through a number of unprotected persons who may be exposed to it. We believe, it is also a generally admitted fact that a contagious disease may be communicated in any climate, and in the purest state of atmosphere, where the contiguity is sufficiently close. Such is the case with small-pox, measles, scarlatina, &c., diseases which, by general consent, are regarded as contagious by all writers.

Bearing, then, in mind the above definition of the term contagion, as defined and acknowledged by the best authorities, and common usage, we believe that the position may be clearly and fully maintained that yellow fever is not a contagious disease.

We proceed now to examine some of the arguments of the advocates of the doctrine of contagion. It is contended by them that the disease is transmitted, by contagious influence, from one city to another, and as an evidence of this, that it first makes its appearance about the wharves and localities contiguous thereto, where ships from an infected port land and discharge their freight and passengers. Let us look for a moment to the facts in relation to this statement. Let us take, for instance the city of New Orleans. We were residing in that city, and were connected with the Charity Hospital, during the summer of 1841, when the epidemic made its appearance, about the 1st of August. It was a well understood fact, at the time, among the prominent medical men of the city, that the disease originated from local causes, and that it could not be traced, in any way, to foreign importation, by means of ships or otherwise. Dr. Fenner, an able and experienced physician of New Orleans, has devoted especial attention to this subject during the last twelve years, and has written a history of all the epidemics that have appeared in that city during his residence there since 1841, and he, as well as other prominent medical men of that place, has given his decided opinion, supported by a number of facts amounting to demonstration, that yellow fever has never

been imported into the city, and that it has generally been found to commence its starting point at some locality remote from the wharves and ships, and between which there could not be traced any communication.

Let us recur to the facts in connexion with the late epidemic in New Orleans, which prevailed with so much malignity during the past autumn. Dr. Cartwright, an eminent resident physician of that city, states, in a recent number of the *New Orleans Medical and Surgical Journal*, that the first person who was attacked with the disease, which was on the 27th of May, was an Irishman direct from Liverpool. Three days after, Bremen in Germany, contributed a case. Seven days elapsed, when Ireland contributed another; and next, New England, not to be out-done, brought in a sacrifice under the black vomit flag. He remarks that some have accused Jamaica and Rio, of having introduced the disease, but if this be so, then Ireland, England, and Germany, where there did not exist a single case of the disease should all be held guilty, as they are all ahead in the mischief. We think that these facts are sufficient to demonstrate that yellow fever is not imported into New Orleans from other ports such as Havana, Vera Cruz, &c., but that there are local causes in that city which give rise to the production of the disease, as well as in the other more tropical cities of America. And, besides, if the disease be imported from other cities, why does not the epidemic occur more frequently? Why is it that, sometimes, for five or six years, there is no epidemic at all, when the intercourse is just as constant between New Orleans, and those other cities where the epidemic prevails more or less every year? Ships come into the port of New Orleans almost daily from highly infected cities—landing their freight, and passengers dying with yellow fever, who are taken to the hospital or to the hotels of the city—why is not the disease communicated, in these cases, by contagion, if it be a contagious affection? It is useless to argue, as some have done, that a certain corrupt or tainted condition of the atmosphere is necessary to exist before the disease can spread or diffuse itself; for this is contrary to the well known laws of those diseases admitted by all to be contagious, which may be communicated in any atmosphere where the contiguity is sufficiently close; and moreover, this argument looks like begging the question, for how do they know

but that this corrupt or tainted condition of the atmosphere which they consider necessary to exist for the dissemination of the disease, may be the very condition itself which is sufficient to produce an epidemic, without resorting to any contagious influences, or importation from abroad. Again, it is a well ascertained fact, that when a number of patients affected with yellow fever are removed from the* infected region, into a healthy atmosphere, the physicians and nurses have always escaped the disease. Such is well known to have been the case in New York and other cities where the epidemic was prevailing. A number of the sick were removed to a healthy position in the neighborhood, and the disease did not communicate itself in a single instance to those who were exposed by nursing or visiting the sick.

The same circumstances occurred, in a very notable manner, in this city last autumn, when there was every imaginable opportunity for the communication of yellow fever to our inhabitants. Boats arrived here every day from the infected region—from New Orleans, Natchez and Vicksburg, at which places the disease was prevailing with unprecedented malignity. These boats were freighted with merchandise of every description, which had been exposed to the atmosphere of the infected city—a number of the sick and dying were almost daily discharged from these boats, and carried through the streets of the city to the hospital, or taken care of at hotels or private houses. We adopted no quarantine regulations. No efforts were made by the citizens, in any way, to prevent the communication of the disease. I have seen whole families as well as friends crowded around the sick bed of the dying patient. Now, in all reason and common sense, was there not here the most ample opportunity for a contagious disease to propagate itself? Can it be for a moment supposed that if a large number of cases of small-pox, measles, or any of the diseases ordinarily termed contagious, could have been thrown thus upon a dense population of unprotected persons, without, to some extent at least, disseminating the disease?

And, yet, notwithstanding all these favorable circumstances for the

* We employ here the participle "infected" as a brief and comprehensive term to express any distempered condition of the atmosphere. It is used in this sense by the best writers. The noun infection or adjective "infectious" is used in a different sense, and is applied to diseases acknowledged to be contagious, and indicating the mode of contagion.

production or importation of yellow fever, we entirely escaped its ravages, and the city was never known to be in a more healthy condition. These facts, we think, are sufficient to demonstrate clearly that yellow fever is not contagious and cannot be communicated in a healthy atmosphere, and when the advocates of contagion are compelled to fall back upon the position that it can only be communicated in a corrupt or tainted atmosphere, we reply as was stated formerly, that this is contrary to the acknowledged laws of contagious diseases, and the argument appears to us very much like begging the question, or a mere subterfuge to avoid the difficulties or objections which are unanswerable.

We shall here briefly allude to another fact which came under our own observation during the prevalence of the late epidemic. The large steamer *H. D. Bacon*, on her way from New Orleans to St. Louis, landed at our wharf and discharged some eight or ten passengers, sick with yellow fever; several cases remained on the boat, and my professional services were retained to attend them to their point of destination—St. Louis. It would seem, that on this boat, there were the most favorable circumstances for the communication of the disease to any persons taking passage on her on or after her departure from New Orleans. The boat, although a large one, contained a very small, close cabin, being designed as a freight boat, and nearly every state-room, at the time she arrived here, had been occupied by some one who was sick or had died from the epidemic. It may reasonably be supposed that if the atmosphere of any locality could be thoroughly charged with morbid elements, such should have been the case with this boat, coming as it had done, directly from a highly infected port, with a large number of the sick and dying on board, heavily freighted with merchandise and thus well adapted to the conveyance of fomites. Yet what are the facts as developed? Not a single passenger, out of quite a number, who took passage at some point above the infected region, suffered from any symptom of the disease. The writer escaped every symptom and enjoyed better health than usual, although constantly exposed, by being with the sick and sleeping near their state-rooms. Another circumstance may be briefly alluded to which came under our notice during this trip to St. Louis. We were detained several hours at a sort of quarantine establishment some few miles

below the city. During our detention we visited the hospital erected for the sick, situated a short distance from the river, and witnessed some cases of yellow fever in the last stages of the disease, and the odor from which was intolerably offensive. Here, certainly was a fine opportunity for contagion to display itself. The physician in charge informed us that quite a number of cases had been received, but that the disease had not been communicated in a single instance.

Having stated our own opinions and endeavored to sustain them by facts which have come under our observation, we shall now proceed to allude briefly to the views of some of the most eminent and experienced authors and practitioners who have treated on the subject.

Dr. Cherrin, who, it seems, has given as much if not more attention to this disease than any other writer, has presented the following conclusions, to which his long and laborious investigations have conducted him. They are as follows:

1st.—That in Hospitals devoted to yellow fever patients, the attendants of every class have been invariably exempt from the disease, where these establishments have been situated beyond the source of sickness.

2d.—That, though according to the hypothesis of contagion, it might be imagined that persons frequently approaching patients within the range of infection might be more liable to contract the disease than those at a distance, yet this is not the case.

3d.—That, in fact, the nearest communication with the bodies of the deceased; the inoculating with the blood of persons affected; and drinking the black vomit, &c., has not propagated the disease.

4th.—That apparel used by patients has appeared to be equally inoffensive as their persons or corpses; and the separation and seclusion of the healthy from the sick, and prohibition of all intercourse, direct or indirect, has entirely failed in preventing its appearance.

In Dr. Fenner's history of the epidemic yellow fever, occurring in New Orleans in 1853, we find a number of conclusions expressed by the General Board of Health in relation to yellow fever. These conclusions are contained in a "Report of the General Board of Health of England on Quarantine in Yellow Fever," presented to both houses of Parliament, and signed by Dr. Southwood Smith, and other eminent authorities. The report is regarded as an extensive and careful in-

quiry into the subject, the data being drawn from authentic documents, received from all places where yellow fever has prevailed, both in America and Europe. There are fifteen conclusions in this report, the following of which bear directly upon the question now under consideration :

1st.—That so great is the success attending the removal from an infected locality, and the dispersion of the sick in a healthy district, that by this measure alone, the further progress of an epidemic is arrested at once.

2d.—*That there is no evidence to prove that yellow fever has ever been imported.*

In further support of the correctness of our views on this subject, we might add a great number of facts and arguments drawn from the most eminent and authentic sources, but this we do not deem necessary, and besides it would prolong our essay to a tedious and unreasonable length. Before closing the subject, however, we beg leave to introduce the following paragraph from the writings of Dr. McArthur, to whom we have before alluded, and who has had such ample opportunities of making correct observations on yellow fever. He says :

“Contagion, as a source of this fever is entirely rejected by those professional men who have the greatest opportunity of information, now resident in the West Indies. *No case occurred where the fever could be traced to a contagious source.* No place could be better adapted to spread contagion than the building appropriated to the sick in Bridgetown. From want of means of separation, fevers and other complaints were huddled together in the same ward. The officers and nurses lodged and visited in every part of the town, and lodgings were procured for sick officers wherever there was room in the town, when they were required, without hesitation. Yet, notwithstanding all this unrestricted communication, *no instance occurred where the fever could be traced to a contagious source ; and, surely, if it were contagious it would not be so generally confined to men recently arrived in the country.*”

Having thus, as we conceive, from a number of facts coming under our own observation, as well as those derived from the most authentic sources, established the position that yellow fever is not a contagious disease, we come now, in conclusion, to deduce some important infer-

ences, which indicate at once the great practical bearing of this question, and the importance of forming correct views in relation to it.

And, first, if yellow fever be not a contagious disease—and not imported from foreign sources—we infer that it must originate from local causes.

And, secondly, admitting the disease to be of local origin, its occurrence can only be prevented by the adoption of such sanitary and hygienic measures as experience may have taught to be most effectual in the removal of these local causes.

And, thirdly, that quarantine establishments, about which there has been so much controversy in the world, and which has placed so many imperious restrictions upon commercial intercourse, are not only useless, so far as the prevention of this disease is concerned, but are absolutely injurious, not merely to the interests of trade, but by calling off the attention from the adoption of proper sanitary measures at home, with the vain hope that safety may be found by restrictive quarantine regulations which have never been known to exert any good effect in the prevention of yellow fever.

We now take our leave of this subject, with the full consciousness of our inability to do it that justice which its interest and importance demand, but with the sincere hope that, from the facts and arguments which have been elicited during its discussion, we may at least have contributed an humble mite towards the attainment of the great principles of truth, and the formation of correct opinions and conclusions upon great and important questions, so interesting to the medical world, and involving as they do the health and lives of millions of our fellow beings.

MARCH, 15, 1854.

ART. VI.—CHRONIC INFLAMMATION OF THE BLADDER.

BY A. MAGUIRE, M. D.,

PARISH OF ST. MARY, LA.

“*Naturum morborum, curationes ostendunt.*”—Hippocrates.

The following lines contain the history of a case of chronic inflammation of the bladder, which, should you think worthy, you are at liberty to publish in the pages of your valuable journal. My attention

has been forcibly drawn, for a few years, to cases of this kind, and it always was my unfortunate lot to see the patients go through the tedious and long protracted ordinary forms of treatment, with little or no relief to their sufferings, until I attempted the method which I will describe; not in the view of claiming any priority in its use, but in order to draw the attention of the profession—having no doubt but that a few trials will produce the same conviction that I now entertain of its efficacy.

Mrs. B., of Cyprés Morts, in this parish, having imprudently exposed herself to cold and fatigue, a short time after confinement, was attacked with inflammation of the bladder. When I first saw her, she had then been suffering eight months without intermission. Her general appearance was that of a person reduced to the last stage of consumption. Digestion deeply impaired; diarrhæe distressing; her vesical symptoms of the most cruel nature; hardly able to retain the smallest quantity of urine, she was almost continually straining and passing a few drops of liquid, followed by a larger amount of pure blood; after which two or three strings of hard coagulated substance would make their appearance. A repetition of the same act taking place every five minutes. Her mind very much depressed; and, when she applied to me, after having subjected herself to many courses of unsuccessful treatment, it was with very little hope.

The first morning: I introduced a catheter in her bladder, and with the aid of a syringe, injected through it a solution of nitrate of silver, of fifteen grains to the ounce, letting it remain there a few seconds; straining and tenesmus considerable; relieved by a cold hip bath, and a small injection (in the rectum) with a tea-spoonful of laudanum. During those pains, a small quantity of blood, with long streaks of muco-purulent matter were discharged. After the space of six hours the pains ceased, and the urine passed only twice until next morning.

Second morning: another injection in the bladder, with twenty grains nitrate of silver, followed by the same symptoms; relieved with the bath and laudanum; quantity of blood still smaller, but muco-purulent matter passed in abundance. After the abatement of the pain, caused by the straining to discharge matter, urine retained till morning,

Third morning: third injection with twenty-five grains nitrate of

silver to the ounce of water; pains very severe, but relieved in a few hours by the cold bath and laudanum injection; during the day, very little matter was discharged, but a good deal of pale watery blood; after which, straining ceased, and the water was retained till morning. I thought then that the substitutive inflammation had been carried far enough, that the whole of the morbid lining (epithelium) had been destroyed and passed, I gave my patient a dose of opium to arrest as much as possible the secretion from the kidneys and induce rest, so as to allow cicatrization of a healthy kind to take place on the walls of the bladder.

I saw Mrs. B. three days afterwards, and was struck with the change in her appearance; her eyes, from the dull, glassy look, had assumed a bright and cheerful expression. She informed me that since she had passed her water in the morning, (twenty-four hours after the last injection,) naturally and without pain, all traces of blood and matter had disappeared, and the urine had assumed its natural color; that she found it necessary to empty her bladder only three times in the day, and then passed larger quantities each time, than for the last twelve months; said she felt remarkably well, strength and appetite returning; could ride and walk without difficulty, save from weakness, and was thankful, and in good spirits. The cure has lasted now for a month, and I have all reason to believe that it will be permanent.

Few occasions, unfortunately, occur to the physician in which he can test as palpably the *modus operandi* of his medicines and exertions. The satisfaction I have derived from the case, induced me to publish it, in the hope of securing for some of my professional brethren the same success and happy feeling.

ART. VII.—ASPARAGINE A SEDATIVE (?) ✓

BY ALLEN DEDRICK, M. D., A. M.,

OF NEW ORLEANS, LA.

“My hand upon my conscience, and my eyes upturned to heaven, I swear: 1.—To redeem the sufferings of the sick by the preventive sufferings of pure experimentation, which I shall make myself, or by means of persons animated by the like charity.”

The above is the first article of the profession of faith adopted by a school of disciples of the "divinely inspired sage of Meissen." That the homœopathic quantity of truth, expressed above, in words of allopathic rarity and size, is any more recommended, because coming from a sect zealous in profitable works, I do not venture to assert.

"Fas est ab hoste doceri."

In those sciences which have for their object the right investigation of the phenomena of nature, and the elucidation of recognized laws, the man who prompts a thought, or suggests a train of ideas, is as much a promoter of those sciences as he who carries out the idea to a certain admissible conclusion.

The development of knowledge is progressive; and, suggestions from sources apparently foreign or remote, often enable the human mind to arrive at conclusions, that, from an interruption in the chain of associated ideas, would probably never have been attained. Any one may become an observer of phenomena in the material world; but not every one is competent to investigate them, to point out cause and effect, and thence to announce a law of physics. Qualities of mind, and capacity of intellect, requisite for such a pursuit, are possessed by but few of our race. Such individuals the world names philosophers, or as it is better expressed in the German tongue—*Naturforcher*. Yet, there is a sphere for every one. The most humbly endowed intellect can note something.

But, can the method of investigation, employed in physics, be applied to the right study of therapeutics? To a certain extent it undoubtedly can.

How far one man may experiment upon the living organism of another; or, how tamper with the laws that govern for his well being, is a question that must, I imagine, be decided in each individual case by the experimenter's own conscience, with an eye always single to the operations of the civil law. Not so with his own body. In operating on it he has to fear only the penalties attached to a violation of the laws of life. To disregard these in the cause of humanity and sciences has been considered, in all ages, as meritorious. And, yet, to assume the part of a "redeemer," according to the tenets of Hahneman, involves so little risk, that it is truly surprising that more has not been added to our stock of therapeutical knowledge. Alas!

'tis to be feared that the stock of human ills is like the stock of a pawnbroker's establishment—much being left unredeemed.

Another question, which has caused a diversity of opinion, has done much to retard investigations after therapeutical truths by throwing doubts on the utility of certain kinds of experiments. This may be stated by giving the opinion of an eminent teacher of medicine. "It is not only wholly unsound," says Prof. Paine, "to reason from the effects of remedial or morbid agents upon man in health to man in disease, but especially so from their effects on animals, whether healthy or diseased." The above is merely an assertion conveyed in terms, to say the least, very indefinite. As an absolute truth, it is contradicted by daily experience. The venerable professor speaks the truth but in part. The effects of remedial agents are divisible into those of quantity and kind. His remark may apply to the former, but not to the latter. There are articles of the *Materia Medica*, which produce effects of a known kind, yet differ materially in the quantity or intensity of those effects. This difference is rightly attributed to modifying influences of constitution, in health and in disease. Now, this discrepancy is of but little consequence, compared with that knowledge of agents which constitutes the department of therapeutics, viz., the tendency to produce effects of some particular kind on some particular organ, or on some existing tissue or fluid. In other words: what a remedial agent *tends* to do, is of more importance to know than how much must be given to produce a certain quantity of effect.

Such an effect, repeatedly observed, adds a new fact to therapeutics, which is merely a collection of such facts conveniently arranged.

If we know that an agent tends to produce a certain effect on any organ in the healthy, living economy—whether that effect be one of excitation, or depression, it matters not—we unhesitatingly give it, whenever we deem such an effect of service in the treatment of disease.

Disease is not a destruction of parts.

To suppose that an organ—a gland for instance—when diseased is not amenable to the influence of those therapeutical agents which can influence it in health, is to suppose it insensible to the nerves which regulate its functions. If so, it is no longer simply diseased, but func-

tionally dead, and hence is of no use to the system, though nerves of organic life may still preside over its nutrition.

Again, organs whose functions are motion—the heart for example—are amenable to the influence of those agents that call forth nervous influence, (stimulants,) or of those that repress it, (sedatives.) Now, applying the same reasoning, as in the previous supposition, and we arrive at the same conclusion. The failure in case of mechanical difficulties, does not affect the question.

If the professor, however, has in mind the effects of agents called specifics, perhaps no one will gainsay his remarks. And, after all, these things are not matters of mere opinion, but are to be received in so far as they are demonstrated.

For the day when authority ruled everything in medicine is past. The sect of empirici are now the least prominent promoters of medical learning. A priori reasoning is admissible; and analogy suggests with a great deal of certainty where pathological and physiological knowledge is kept in mind.

For my part, it is sufficient to maintain that agents which affect any particular organ, tissue, or fluid in the sound economy, will tend to have the same kind of effect, when those parts are diseased.

With these preliminary remarks, I wish to proceed to the study of the subject of this article.

Asparagine is a peculiar crystallizable substance, first found in the root and stems of the *asparagus officinalis*. It is identical with althöin, found in the root of the *althæa*. Several other vegetables, as the potato, comfrey, liquorice-root, &c., also contain it. Later experiments have resolved it into an acid, which has been named aspartic, (s. asparamic,) and a base, ammonia. The chemical equivalents are thus expressed, $C_8 H_5 NO_6 -|- NH_3 = C_8 H_8 N_2 O_6 -|-_2HO$, (Liebig.) Hence, it is called an aspartate of ammonia, (s. asparamide.) The above decomposition is effected by acids or alkalis; the former taking away its base, and the latter substituting themselves for the ammonia, thus forming aspartate of potassa, soda, &c., as the case may be. Aspartic acid also unites with quinia, morphia, and some other bodies, (Berzelius.)

The specimen, which I have for experimentation, is composed of

beautiful transparent crystals, having no odor, scarcely a perceptible taste, but imparting to the tip of tongue a peculiar cooling sensation, with a slight numbness, similar to that caused by some acrid vegetables. Three grains dissolved in three drachms of warm water distilled, gave a clear, slightly acid solution, which, when caustic potassa was added, yielded a faint odor of ammonia.

It remains now to speak of its therapeutical properties; but, before doing so, a glance at the history of the uses of the plant, from which it was first derived, may not be unprofitable. Various ancient writers make mention of the asparagus, as an article of diet, though it is uncertain what species was meant, since the name—like our word “greens”—was, in common parlance, given to several plants, whose tender shoots were eaten. In medicine, the above named species was not so much used as another, the *A. aphyllus*, *L. μνάχανθος*, *corruda*, (Theophrastus.)

The Romans probably used the *a. acutifolia* as their culinary vegetable. Hippocrates, in his book on diet, speaks of it as an article tending to produce constipation, (*ξηρόν και στάσιμον*) and in his treatise on females, (bk. 1,) he says, the fruit macerated in wine aids conception. Celsus, on the contrary, (bk. 2,) commends it as an article of light food, and as having a tendency to a laxative and diuretic effect; and in (bk. 4,) he advises it in splenitis.

It is officinal in most of the Continental *Pharmacopœias*, classed among diuretics, and formulas given, (*Ph. universalis*,) some of which would now be considered as curiosities.

That it acts as a diuretic, has perhaps been inferred from the peculiar odor that it imparts to the urine, rather than from a positive observation of such an effect. This odor is not due to the asparagine, though that principle has been found in the urine of those who have eaten much of the plant. Lovers of this vegetable have also asserted that it possesses aphrodisiac properties, thus confirming the opinion of Hippocrates. Sedative properties have also been affirmed of it, particularly of the fruit, (Wood.) This has been denied by M. Gendrin, who tried it in numerous cases. Hence the properties, best established, are those which make it a diuretic; while its uses have been mostly confined to dropsical cases. The opinions of late writers are unfa-

avorable to its efficacy. Trousseau merely remarks, "La racine d'asperge est employée comme diurétique."

Æsterlen, in the fifth edition of his *Artzweimittlelehre*, recently published, says: "Ohne Zweifel nichts wirkt." And again: "Asparagin selbst soll wie Digitalis wirken (?)" He gives, as the dose, grains v—x. Hence it would seem that the properties of the plant, if any, are not exhibited by the asparagine.

Wishing to test its powers by "pure experimentation," à la Hahneman, I consented, for want of other resources, to turn "redeemer;" an office the more meritorious as the doses were of allopathic size and potency. For this purpose I made the following experiments:

First Experiment.—Three grains of asparagine were dissolved, as heretofore described, and swallowed. The normal pulse being seventy-five beats to the minute. A severe but transient frontal headache, with a fullness about the eyes, and a feeling of lassitude were experienced soon after taking the medicine. The pulse became irregular, and in fifteen minutes had fallen to seventy-one beats. After this it rose to seventy-four, and then sunk again to seventy-one, where it remained, with slight variations, for about an hour.

Second Experiment.—The next day the experiment was repeated with five grains, when the same effects followed, with a further reduction of the pulse to sixty-two, forty-five minutes after taking the medicine.

Third Experiment.—On a subsequent day in a third and last experiment, eight grains were used. The normal pulse was seventy-two, and the thermometer indicated 73° F. The pulse was counted every five minutes, for an hour and a half, with the following results:

Min.	Beats.	Min.	Beats.	Min.	Beats.	Min.	Beats.
5	64	20	60	35	64	50	56
10	64	25	70	40	66	55	60
15	62	30	60	45	60	60	56

At fifty-six, it remained for some time, half an hour, after which the effects seemed to pass off. The head symptoms were severest in the first, but very transient in each case. The feeling of "muscular prostration" was greatest in the last experiment. It required more than ordinary effort to exercise the muscles or fix the attention. No un-

easiness in the region of the stomach was experienced in either case, though the medicine was taken fasting.

Neither was any effect on the quantity of the urinary secretion perceptible.

The most marked effect was on the pulse, causing it to become very irregular; nay, even intermitting. For ten or more beats, it would sink so as to be almost lost, then, as if the heart rallied for an effort, several beats would follow in rapid succession. This would be the course until the agent had exercised its maximum of influence, when the heart would again resume its normal beatings. A peculiar softness of the pulse was also noticeable.

That asparagine tends to produce a sedative effect on the circulation, I think, can hardly be denied. Of its diuretic powers, I wish to affirm nothing, not having directed my observations specially to that end. The two effects are not incompatible; nay, Trousseau asserts that all sedatives are diuretics, and affirms a "communauté de propriétés" of them. However, the diuretic action of digitalin is not constant, even when it manifests its sedative powers. But of this relationship, I may speak after further observation.

Compared with digitalin, there is a great similarity in the collateral symptoms which they produce; while in intensity of power, they stand as 1:100, i. e. gr. $\frac{1}{10}$ digitalin is probably equal to 10 grains of asparagine. But then, is it not a safer and more manageable remedy? Judging from the known uses of the several plants from which they are derived, one would unhesitatingly pronounce it so.

Whether it is, like its kindred agent, cumulative in its effects, I have not yet been able to ascertain; nor do I know how much would constitute a poisonous dose. The same rule that is observed in administering all sedatives, should obviously be observed with this. For this end, grains iij—v may be repeated every hour or two, according to the effect desired. Like most newly extracted principles, it is still an expensive article, costing, at Powers & Weightman's laboratory, Philadelphia, \$1 12 a drachm.

ART. VIII.—HEMERALOPIA.

BY C. S. FENNER, M. D.,

OF MEMPHIS, TENN.

June, 1847, a servant boy, a field hand, 24 years of age, was brought me with hemeralopia. During the day his sight was perfect with no abnormal appearance of the eye; the irides acting freely to the stimulus of light. On examining his eyes by candle light, the pupils were found to be as widely dilated as they usually are after the continued application of stramonium or belladonna. The lighted candle was brought within two or three inches of the eyes without producing the least contraction of the pupils or causing the slightest perception of light. He remained in this condition until sunrise the next morning, when his sight began to return, and in a few minutes became perfect, continuing so through the day. The same symptoms had recurred nightly for a month previous to my seeing him, without interfering with his field labors during the day. As there seemed to be some slight determination of blood to the head, with an unusual fulness of the pulse, I took a few ounces of blood from the arm, prescribed a mercurial cathartic, and applied a large blister to the back of the neck; and the next day gave ten grains of quinine. The second night the blindness returned at the usual time and continued some four or five hours; after midnight the patient was able to see distinctly by the light of the moon. Continued a succession of blisters to the temples and mastoid. The fourth night the loss of sight lasted about an hour, after which time there was no return of the disease, and at the end of a week the patient was discharged cured.

Since the above mentioned patient was treated, I have seen and been consulted in a considerable number of similar cases, all them occurring among negroes working in an extended range of prairie plantations, commencing in Pickens county, Alabama, and extending up through Noxubee, Lowndes, Oktibbeha and Monroe counties, Mississippi.

The disease makes its appearance almost invariably in the spring, or early in the summer months, before the young corn and cotton

plants have attained sufficient size to shield the eye from the dazzling reflection of the sun's rays from innumerable small pieces of limestone, and fragments of shells that are thickly interspersed through the soil. These prairies constitute immense bodies of a peculiar black soil, lying on a strata of limestone. In many places the rock approaches the surface, and is entirely bare, reflecting the sun so powerfully as to be almost insupportable to the eye. To a person standing in the middle of some of these large prairies, the horizon, in many directions, seems to meet the surface without a tree or shrub to obstruct the view. I attribute the frequent occurrence of night-blindness through this section of country, to the exhaustion of the nervous power of the retina from continued exposure day after day to the powerfully reflected solar rays, without having any objects, even at a distance, (particularly trees covered with green foliage,) to relieve the eye, by frequently looking up and bringing a change of scenery within the field of vision.

All the cases of hemeralopia that I have seen, have readily yielded to treatment similar to that pursued with the patient above mentioned. In a majority of instances bleeding is unnecessary. Counter irritation from a succession of blisters, and repose within doors for a few days, are usually all that is requisite to give entire relief. I have been informed of several cases that recovered after the lapse of few months, without any treatment whatever; and, in no instance have I known any permanent injury to vision result from permitting the disease to take its own course. Cauterizing the margin of the cornea, with solid nitrate of silver, has been recommended in hemeralopia, but as this method of treatment is much more painful than blisters, and as the relief from the latter is speedy and effective, I give them the preference.

ART. X.—UTERINE CANCER.

BY BENNET DOWLER, M. D.

Much as I may dislike my former treatment of cancer I will neither criticise nor defend myself. This self-conceded amnesty is somewhat justifiable by the inconsolable doctrine, now paramount, that both medical and surgical treatment of any kind, even the most recent and

improved is wholly inefficacious in this malady: so that the *status quo ante medicamentum* herein setforth, can scarcely be worse than that growing out of the foregone conclusion in favor of the immedicable character of cancer. The fundamental principle, whether right or wrong, on which these cases were, many years ago, treated is that termed antiphlogistic, carried out to an extreme never practiced in any other malady whatever.

With this preface, I will proceed to copy the outlines of a few cases, not with a view to represent the present state of the pathology of *cancer uteri*; but in order to give cases, facts, and results as they occurred, in some instances, nearly a quarter of a century ago—and which produced convictions that later experience with remedial agents, and a good deal of reading have not greatly changed, notwithstanding the immensity of modern researches both chemical and microscopic into the pathological histology of cancers both hard and soft. Although these cases do not show a single cure in the ulcerated state, yet they afford a presumption, at least, that a cure is possible in the scirrhus stage of cancer and in cauliflower excrescence of the neck of the uterus, of long standing, and of an apparently malignant character.

Case 1.—Nov. 3d, 1832.—Mrs. Matheny, aged 46, mother of a large family, has been under treatment by regular physicians, steam-doctors, and old ladies for several years; during the last twenty-four months she has suffered much; sometimes it was thought that she would speedily die; occasionally she appeared, for a time, to regain her health, to a very great extent. Her mind was much depressed by reports which busy bodies circulated to the effect that she kept her bed unnecessarily, and was not by any means as ill as she represented herself to be.

Mrs. M. had suffered much from slight but oft-repeated uterine hæmorrhages—muco-purulent discharges mingled with sanguineous clots—lancinating pains radiating from the womb to the vagina, bladder, back, thighs,—pain in urinating, difficulty in walking erect, &c. Her medical attendants had not suspected the nature of her disease and had not hinted at the necessity of an examination *per vaginam*. I found the womb lower than natural, painful on touching, and nearly immovable;—its neck enlarged—its mouth open, admitting two

fingers, being irregular, notched, knobby, craggy,—having a gristly—almost a horny hardness. The mouth and neck of the womb contained spongy old clots, some of which adhered to the finger. It was found impossible to cause the womb to oscillate; its fundus and body appeared to be soldered to the surrounding tissues.

Mrs. M.'s pulse was hard, full, rolling or rather crawling under the finger like a worm—external veins prominent—feverish.

I directed the horizontal position—rigid diet—occasional blood-letting—(neither leeching nor cupping was practicable, as the former could not be obtained at all, nor the latter either without going twenty-five miles)—alterative doses of mercury—saline purgatives—opiates, &c.

December 7th.—Visited the patient a second time—twenty-five miles distant from my residence, in Clarksburg, Virginia—floodings and mucous discharges have nearly disappeared under the treatment which was doubtlessly carried out as recommended; the patient, her husband, and neighbors, all thought that recovery was proceeding at a most satisfactory pace. She walked erectly, free from a stooping or forward curving which had previously characterized her movements; she declared that her health was better than it had been for several years, and that her sleep was but little interrupted. Although the material condition of the uterus appeared to be nearly the same, the pain on touching was comparatively slight. While the hope of recovery was, to a medical man, evidently illusory, the improvement of the general health was indisputable. A slight emaciation was, however, noticed.

December 19th.—Has for several days discontinued medication; uterine pains and fœtid discharges are becoming troublesome again, with debility, arterial throbbings, general uneasiness, sleeplessness, low spirits; medical treatment abandoned.

January 1st, 1833.—Has abandoned all dietetic restrictions—uses wine, meat, and, in short, whatsoever she fancies—has occasionally a morbid, capricious appetite for articles which she subsequently vomits—copious water-brash, severe heartburn, flatulency, eructations, oft repeated vomitings; no marked alterations of the neck and mouth of the womb; bloody urine, which is passed with pain and difficulty; constipation; pulse hard, throbbing, crawling or vermicular. Her

sufferings have been constantly increasing recently, excepting a few days immediately after copious nasal and uterine hæmorrhages, which occurred simultaneous, and which produced a marked interval of relief, as upon former occasions.

February 7th.—Wholly confined to bed; unparalleled emaciation, being like a skeleton covered with harsh, dry skin; diarrhœa, tongue smooth and clean, vomiting almost incessant, pulse wiry, vermicular, throbbing; copious, offensive vaginal discharges; an alarming flooding, two weeks since, was followed by the most surprising relief from the burning pains of the uterus, &c.

March 8th.—Screams almost day and night until exhausted; ulceration extending upward into the body of the womb; the discharges from the vagina intolerably fœtid, producing deep external excoriations of the limbs and other parts, and acting like a virulent poison or caustic; the vomitings, of a ropy mucosity, obstinate; ulcerations of the mouth and throat; has taken no food for eight days; respiration accompanied with mucous rattles.

March 12th.—Died; having, to all appearance, suffered more than any person I had ever seen before.

Case 2.—October, 1834.—Mrs. D., aged 50, mother of seven children, long afflicted with asthma, of a sanguineous temperament and vigorous constitution; ceased menstruating five years since. For more than a year, pains have radiated from the womb to the surrounding parts; frequent and painful calls to urinate; fluor albus, together with small but frequent floodings; has frequent headaches; pulse excited and firm. The mouth of the womb is open; its margins irregular, serrated, and hard; the neck has a cartilaginous hardness, being uneven and knobby. The treatment of this case differed from the preceding, chiefly in extensive and repeated antimonial pustulations, vaginal injection made with infusions of oak bark, &c., the substitution of hyosciamus and other narcotics in place of opium. During the continuance of the treatment, a period of five months, the progress of the disease was arrested, the floodings were removed, the pain greatly lessened; but the hardness, the unevenness, and in some degree, the tenderness of the parts remained up to the time of my removal to New Orleans in the spring of 1836. During the same year, as I subsequently learned from one of her family who visited

this city, the disease returned with alarming severity, and soon proved fatal.

The above cases illustrate the progress of cancer uteri in the latter stage. Similar cases, all terminating fatally, might be multiplied from MS. books; but the patients refused to submit to examination by the touch, and consequently no satisfactory diagnoses could be made at an early stage, although the frightful sufferings and characteristic discharges attendant on the dying stage of this disease left no doubts as to its nature. The speculum was not then in use. Such an instrument, it may be asserted, would—*à fortiori*—have been deemed still more inadmissible than the touch.

A lady, aged 40, the mother of a family, having extensive connections, had suffered for several years from small, frequent, uterine hæmorrhages, mucous discharges, lancinating, burning, uterine pains, objected to an examination, as did her venerable family physician, until the patient was sinking from cancerous ulceration. She died.

Another case is alluded to in this place, in order to say that the patient having refused an examination, no medicine was, therefore, given; fetid cancerous discharges soon followed, ending in death more rapidly to all appearance, than in cases that had undergone medical treatment.

Case 3.—Miss * * * a young woman of surpassing beauty, having suffered or procured an abortion; I was called in two weeks after. I found her suffering from symptoms like those of typhoid fever of the gravest character, greatly emaciated, and so debilitated that she cannot rise from her bed; has severe throbbing pain in the womb and its connections. Discharges copious, and intolerably fœtid, beyond any odor I ever witnessed; the mouth of the womb is exceedingly tender and inflamed; from it I extracted a placenta weighing about four ounces. Her health improved under a general treatment. Supposing that the affection of the womb was not at all malignant, and would soon disappear, I paid but little attention to the case. Nearly half a year elapsed before I was called to see her again. She had latterly suffered much from uterine pain, which sometimes intermitted and returned, almost like parturient pains. She now had violent rigors, alternating with febrile symptoms; occasionally small clots passed from the womb, the mouth of which was thickened, and so open as to admit two

fingers, and was painful on being touched. By next morning reaction came on, together with general inflammation of the womb (metritis) and abdominal pain, tightness, and swelling, (peritonitis.) The treatment usual in these inflammatory affections subdued the disease in one week, whereupon I lost sight of the case until the following year. Her general health was now a good deal affected; she was sallow and dyspeptic; lancinating pains and burning sensations radiated from the womb to the surrounding organs. I found a gristly induration, with an uneven nodulated surface of the cervix upon its posterior or sacral aspect, with hypertrophy of the neck and the os tincæ—in a word, incipient scirrhus degeneration, as well as inflammation proper.

The treatment, which was antiphlogistic, was at first active so long as the uterine and peritoneal inflammation continued. The subsequent treatment, from which opiates were excluded, was similar in character, though not in degree. In six months the cure seemed complete. The patient was known to have continued well for four years, and is, so far as I know, still in good health.

Persons who make death the sole definer of cancerous disease, will of course, regard this case not as one of scirrhus, but as one of hard engorgement or inflammation of the neck of the uterus, simply because the patient recovered. Whether this patient would or would not have died of cancer, had no treatment been adopted, can neither be affirmed nor denied. There is, however, less difficulty in deciding as to the erroneousness of that pathology which assumes that the only test of cancer is the death criterion; that, because the last stage is incurable the first is incurable, as if the incurable stages of collapse in cholera, of black vomit in yellow fever, must be regarded as the criteria of the nature and curability of these diseases without reference to their inceptive stages.

Case 4.—April 26th, 1839.—M * * * aged 42, mother of two living children, miscarried twenty years since, at which time, owing to the mal-practice of a midwife, the womb was forced down, and finally brought out! An accoucheur was called who replaced the womb. She did not, after this, suffer any *prolapsus* or *falling of the womb*. In a few months after this event pains in the womb began. These pains were of a shooting or lancinating character, and extended to the bladder, the groins and the back. Though variable as to intensity,

they have gradually increased, especially when too much exercise has been taken; colds, and even a drink of cold water increase the pains. Two years since menstruation ceased, watery discharges followed, mixed with clots of blood. Though she has suffered more or less for nearly twenty years, she has not been confined to bed by her disease until lately. She has at present slight floodings having a disagreeable odor, headache, constipation, quick resisting pulse, skin somewhat dry, debility. The neck of the womb obliterated by enlargement; upon the right side, near the mouth, it is a mammalated or knob-like tumor, not much elevated from its deep broad base. The mouth of the womb has a kind of fissure or slit, one lip overlapping the other with a thick fold, having a cartilaginous or gristly firmness.

May 17th.—The mouth of the womb less firm and of a more natural shape. The nipple like tumor upon the right side of the neck, though hard and deeply set in the substance of the organ, is less prominent and less painful upon pressure.

June 7th.—Womb nearly of natural size; the nipple like tumor diminishing. She has kept the horizontal position; used a restricted vegetable diet; extensive pustulations with antimonial plasters and ointment, both upon the lower portions of the abdomen and back; mercurial alteratives, both internally and externally, with prolonged purgations, tincture of iodine and hydriodate of potash. The floodings and serous discharges soon ceased. In three weeks she had an exemption from pain, which she had not enjoyed for twenty years.

June 26th.—Takes free exercise, from which she sometimes feels a slight numbness in the back, otherwise she appears well. Uses no medicine internally. Has antimonial inflammation upon the back.

July 5th.—Says she is now perfectly free from the burning pain, weight in the loins, back, &c., which existed with greater or less intensity for nearly twenty years previously to the treatment.

September 20th, 1839.—This woman (now convalescent from a severe attack of yellow fever,) appears wholly free from uterine disease.

September 15th, 1840.—Continues well.

July, 1841.—Is quite well; since which I have heard nothing of her, as she left this city for Ohio.

Case 5.—Cauliflower Excrescence or Cancer.—May 1st, 1840.—Madame * * * aged 47, says she has for four years suffered almost without ceasing from sanguine emissions and watery and mucous effusions from the vagina; sometimes blood, sometimes water, passed in jets, and then subsided into oozings or drops; shooting pains in the thighs and hips; morbid appetites and longings, alternating with vomitings. Many regular physicians, not to name Thompsonian doctors, had prescribed remedies, as opposite as heat and cold, among which was the cold hip bath, which gave her, as she said, a stiffness of her lower limbs, but no benefit. She is very weak, excessively anæmic, and has an indescribably cadaverous, sallow, yellowish complexion. She has not been examined *per vaginam*.

May 3d.—The os tincæ obliterated; the cervix lost or blended with the enlarged and egg-shaped womb; upon one side of its cervical portion was found a dense implantation, rounded or convex on its outer surface, larger than an ordinary womb, consisting of a placenta-like mass, spongy like hæmatodes, though firmer, rougher, and more elastic, from which copious jets of red arterial blood took place, though no arterial pulsations were discoverable; the exterior surface felt uneven like cauliflower; touching or pressing it produced, as stated, a rapid hæmorrhage, but scarcely any sensation; the body of the womb engorged, hypertrophied, and very hard. She was anxious to be cured, and willing to submit to the treatment, that is to say, the horizontal position, a diet diminished to the lowest possible degree short of starvation; frequent and very strong vaginal injections of sulphates of zinc and copper, nut-galls, and the like, purgations, mercurials, preparations of iodine, antimonial pustulations, &c. The treatment was, with a few exceptions, carried out with a regularity altogether extraordinary. On the 3d of June, 1841, thirteen months from the commencement of my attendance, I noted her cure as complete. She became strong and very active for her age, and remained so nearly ten years, after which I lost sight of her. It would be tedious to enumerate the daily treatment. A few facts may be mentioned. Iodine produced troublesome intoxication or vertigo. Occasionally tenesmus abdominal pains, and a tendency to prolapsus uteri occurred.

May 18th.—The cauliflower mass is less indurated, as if wilted; the body of the womb less firm, though its symmetry is not restored, the

neck being obliterated or lost in the body, which is globular, not pear-shaped.

June 24th.—Uterine pains resembling labor; discharged about a dozen of reddish fibro-membraneous masses the eight of an inch in thickness, and of variable width and length. They were softened, some portions being like the pulp of grapes, having been macerated in water for twenty-four hours before I saw them. Beyond doubt they are parts of the excrescence. Portions of the same passed off several times, until the whole peeled off.

July 7th.—Much annoyed by loud puffing noises in the ears; the hæmorrhagic discharges have ceased; occasional discharges of serosity continue. These, however, slowly declined and finally disappeared, together with every vestige of the malady.

The treatment here alluded to, whether right or wrong, was founded on the principle enunciated in the following paragraph, from a note upon this case, which is copied verbatim, (or rather cut out of the MS. volume,) and will suffice without any further enumeration of details that might be deemed tedious.

[*Cauliflower Excrescence of the Uterus.*—This resembles placental structure, though less elastic and more uneven and more vascular. The treatment which I have adopted was based on the opinion that the capillaries of such a mass originating from the neck of the uterus, ramifying in this morbid growth, might be so collapsed, withered, or enfeebled by the withdrawal of nutriment as to fail in supplying the morbid structure, so that the latter would necessarily drop away by decomposition, or be absorbed, especially when the horizontal position is adopted. Being erect favors by gravity the vascularity, growth, &c.]

In several hopeless cases of cancerous ulceration of the womb, I have noticed a capriciousness of appetite as strongly marked as that which is a frequent accompaniment of pregnancy. In two cases, respectable and temperate ladies craved rum and salted meats, which, being given, seemed to aggravate the disease and accelerate its fatal termination. On the contrary, among other cases, that of a medical man might be mentioned, who, as I was credibly informed, began many years since to suffer from a scirrhus tumor of the skin, which was pronounced malignant and incurable, which, however, made no progress subsequently, though nothing was done to arrest its march

except the adoption of an extremely rigid diet. He will probably die of old age, not of cancer, though some of his friends expressed the opinion that they would prefer death to his extreme abstemiousness!

The unanimity with which the most enlightened and conscientious of the medical faculty denounce the so-called cancer doctors of the newspapers, as ignorant impostors, should not deter the honest practitioner from attempting to cure incipient scirrhus, or even the most deplorable form of cancerous ulceration, hitherto deemed incurable, although it may be strictly true that no real advance in treatment of the latter has taken place since the dawn of medicine.

The admitted unsuccessfulness of surgical operations for the removal of cancer, the certainty with which the disease returns, (after surgical interference,) often remote from its original site, the general pathological history of its phenomena, its coincidence and connections with what has been vaguely enough termed cachexia, or the cancerous diathesis, all indicate a constitutional rather than a local origin; whence, it follows that the fundamental principle of cure should be laid upon a broader basis than the edge of the scalpel presents. It is highly probable, upon the first expression of the disease, that a very rigid or restricted diet consisting of vegetables, or bread and milk, long persevered in, would cure more than the knife or the actual cautery. At all events, it will be found that wine, spirits, a full diet, motion, constipation, neglect of ablutions and injections will aggravate the disease. Dubiety, rather than demonstration, has hitherto characterized most explanations of the *modus operandi* of medicinal agents. Whether corrosive sublimate, calomel, iodine, and the other medicines alluded to in this paper, be any better than some other articles of the *Materia Medica*, I will not say. There may be other articles which act in the same way in dissipating scirrhous deposition, diminishing the general nutrition, and with it the local nutritive action of the part, arresting its growth, thereby favoring its absorption, and final elimination from the system.

In the early or nascent state of scirrhus of the mouth and neck of the uterus, (not to name similar degenerations found in other regions,) there is sometimes, if not always, a fine, very delicate capillary network which does not seem to enter the white indurated mass itself, which it seems to environ. This circumferential zone augments—*pari*

passu—with the activity of the central erosion, until finally it presents all the most intensified phenomena of inflammation, as pain, vascularity, turgidity, hypertrophy, hardness, redness, softening, together with gelatinous, purulent, ichorous, and sanguineous infiltrations, exudations and discharges.

The physical appearances and histological characters of scirrhus, as presented to the eye, with and without an artificial lens, are few but striking. It is difficult to cut it; it seems as bloodless and almost as white as a turnip; it is rather opaque than transparent; the induration is more apparent than real when tested with the knife, being somewhat yielding and elastic; it is whitest in the middle; bands, septa, or roots may sometimes be seen radiating from the centre, though the striated circumference blends obscurely with the surrounding tissue; it appears fibrous, rather than cellular, granular or fatty, and does not, at least in its early stage, discharge on pressure the albumenoid or milky liquid called cancer juice.

The granules, cells, nuclei, gelatinous exudations, cancer ichor, and so-forth, of which so much has been written by microscopical histologists, do not probably appear in the primary but only in the consecutive erosive degeneration.

Who can be sure that the histological characteristics drawn from the last, incurable, ulcerative condition of cancerous transformation of the womb exist also in the scirrhus stage *ab initio*? or that a hard knobby tumor of that organ is benign and never can be transformed into one that shall be malignant? or that a tumor originally fibrous cannot ultimately degenerate into one that shall be granular, cellular, gelatinoid, and erosive? Is it not illogical, as before remarked, to draw all the pathological and anatomical characters of this malady from its incurability as tests of its primary state, thereby accepting death as the sole test of malignancy. If a scirrhus tumor be cured, does this of itself prove that it was benign? or, that it never could have become malignant? “Most lame and impotent conclusion!”

On a careful examination of the reports which have been published by the most distinguished micrographers, it will be found, on the principle of internal evidence, that is to say, the evidence derived from these works themselves, that they contain so many qualifying exceptions, variations, discrepancies, disagreements, and even contradictions,

as to render it probable that this mode of research, alone, does not as yet enable the pathologist to pronounce with certainty what is, and what is not a cancer, so as to identify and distinguish its primordial, scirrhus state, in this form of degeneration, and point out beyond the possibility of mistake, its differential diagnostics as compared with other analogous alterations. Is there not a lurking scepticism, a mental reservation, a fear, or unwillingness to acknowledge ignorance of that which so many high authorities seem to know? Is there not a want of candor in those who publicly advance upon mere hearsay, as microscopic actualities, things which they know not experimentally, or regard doubtfully?

The cancer logic assumes that what tumor soever disappears, with or without medication, is not, never was, never could be malignant; eat it must! kill it must! in order that its character may be established, like some other doubtful characters, by an obituary.

The very men who are certain as to the certainty that cancer must kill, admit that they themselves, and their leaders, are wholly uncertain as to the primordial characteristics of cancer, physical, microscopic, or symptomatic; whether a tumor is benign or malignant; whether the former may not be transformed into the latter, is to them quite uncertain; but, nevertheless, they are certain of only one thing, namely, that death from cancer is malignant and incurable! so is death from old age—death from any malady. Cancer-cells are sometimes, as they say, found in the general circulation, sometimes in tumors, or in sanies, &c., being sometimes caudate or of some other form.

I regret that I have neither space nor leisure, at present, to give the numerical proportion or per centum of scirrhus transformations in the neck of the uterus among women, who, without previous subjective symptoms, or at least without anæmia, emaciation, &c., have died of acute diseases, as fever, dysentery, &c., and who have, nevertheless, presented perfect specimens of the early stage of this formidable, white, knobby induration deposited in this organ, forming, in some cases, irregular masses, involving the rectum. Those who will make a few hundred post mortem examinations, spending, when he has no assistant, four hours over each body, thoroughly examining each organ, will probably find as many cases of scirrhus in both sexes,

and in different regions, as I have unexpectedly found. I have even found cancer, in its ulcerated stage, in the womb, vagina, rectum, and bladder, with disseminated scirrhi in other organs—cases in which the disease had not been ascertained before death—cases wherein the parties were supposed to have died of consumption, fever, &c.

Case 7.—(Slightly sketched.)—September 14th, 1848.—Yellow fever prevailing. A negress of middle age, supposed by sundry physicians to have died of consumption; examined a few hours after death; presented, among many other lesions, not only all stages of pulmonary and mesenteric tuberculization, hard, soft, ulcerated, and excavated; intestinal hæmorrhage, as in yellow fever; hypertrophy of the uterus, so as nearly to equal the calibre of the ileum, together with well defined cancer uteri; the fundus of the uterus was enlarged and scirrhus; the whole organ fixed by scirrhus bands and masses, implanted into the surrounding tissues, as the bladder, rectum, &c.; the os tincæ, and much of the neck, eroded, jagged, and destroyed; a part of the vagina eaten away; the ulcerated edges firm and everted; the bases and circumferences of these ulcerations zoned with intense inflammation. The body of this woman presented what is usually, though vaguely, if not hypothetically, called the cancerous cachexia, as emaciation, anæmia, being a condition which applies to consumption as well, but is by no means pathognomonic of the scirrhus, or the primordial state of cancer uteri.

This case suggests what, indeed, many able pathologists have regarded as more than probable, namely, that tubercular and scirrhus transformations are essentially the same in nature; the parallelism, if not the identity, is certainly striking.

Scirrhus of the Neck of the Uterus; also, of the Liver, without emaciation, anæmia, &c.

Case 8.—July 21st, 1843.—Yellow fever prevailing: post mortem examination of A. P., an unmarried white woman, aged 33, whose disease had been diagnosed differently by different physicians. This woman was treated for jaundice, insanity, and so forth, for one month before death. Dead six hours—warm, except the limbs—robust appearance; muscles massive, slightly pale—fatty tissue largely developed both externally and internally including the mesentery, averaging more than an inch in thickness. Omitting the appearances not con-

nected with scirrhus of the uterus (though the liver was a mass of scirrhus) it is sufficient to say, that the body of the uterus was atrophied, and the neck transformed into a white, almost cartilaginous substance, apparently bloodless, which resisted the knife even more than the cartilaginous portions of the ribs—though the pharse, *creaking* or *crying* under the knife is rather strong. The ovaries had degenerated into two dense sacks of the shape of the gall-bladder—that on the right side contained about two or three ounces of black vomit, as it appeared—one part of the fluid darker than tobacco juice, the other dense black and flaky; the other cyst, the fallopian tube, and the womb contained about an ounce of the same kind of liquid.

This case is, however, far from being a good type of that primordial scirrhus which I have found after death in various organs in both sexes, as the stomach, pylorus, liver, rectum, mesentery, bladder, brain, thyroid gland, kidneys, and particularly in the womb and its appendages; the latter alone is now alluded to, not with a view to give special histories, but for the purpose of saying that the anatomical characters of scirrhus frequently mentioned in this paper are taken from cases in which death took place from acute, intercurrent diseases in no way connected with scirrhous degeneration, and before the so called cancerous diathesis had been established.

It is probable that the early state of scirrhus gives rise to few, if any well marked subjective symptoms, and still less to objective phenomena, except such as are derived from the touch. The patient in this early state, may, after death, from other diseases, sometimes present scirrhous tumors disseminated in other regions.

The configuration and magnitude of the scirrhous deposits vary greatly. In several cases an irregular almost angular mass of several ounces has been found implanted in the uterine neck having an isthmus by which it was conjoined and firmly fixed to a similar mass imbedded in the rectum, as already indicated.

A prize lately was awarded by the American Medical Association, to the distinguished Dr. W. J. Burnett, of Boston, for his Essay on "The Cell: its Physiology, Pathology, and Philosophy;" able as is this essay in all that relates to microscopic research, it amounts to little more than a profound negation, so far as the histology of cancer is concerned—not that he is unwilling to make the most that is possible

out of the microscope—not that he fails to magnify his speciality, but his love of truth forces him to confess, in *italics*, “that, both as to their *genesis and general aspect as cells, those which belong to the abnormal cannot be distinguished from those belonging to the normal conditions of life*,”—a weighty postulate which sometimes he inadvertently, but vainly, tries to invalidate; thus, he would put the cancer cell upon a foundation so very broad as to include tubercle, pus, or cancer! He says: “suppose, for illustration, that a small portion of a cancerous tissue has been sent to a microscopist for microscopic examination. He decides, in the first place, that the tissue in question is not a normal one, from the fact that its cell-constituents possess no *type* peculiarities. As a hetermorphous product, then, the diagnosis lies between its being tubercle, a pyoid form, or cancer.”

Part Second.

EXCERPTA.

Art. I.—*Surgical Operations*: By Prof. JOHN F. SANFORD, M. D.,
of Keokuk, Iowa. [From the Trans. of the Iowa State
Med. and Chir. Soc., 1854.]

Two cases of false joint have occurred in my practice—one within the last thirteen months—both successfully treated by sub-cutaneous scarification of the ends of the bones. The last was a case of un-united fracture of the humerus, in a boy 17 years of age, from Missouri, in which there was free motion between the ends of the bones after the removal of the primary dressings, and which continued, notwithstanding the re-application of a fracture apparatus, five months subsequent to the reception of the accident, at which time I saw him. By passing a strong tenotomy needle down to the ends of the bones, free scarifications of the surfaces was affected, after which the application of a retentive apparatus secured firm union. The object of this procedure is to break up the cartilaginous investment, which after a time cover the rounded ends of the fragments, and to excite effusions of plastic material to serve as the medium of union.

This operation, which I believe originated with me, is not urged as a substitute for the treatment recommended by Dr. Physiek in cases of pseudarthrosis; nor, do I think it will supercede other analogous operations for the same disease. Each proceeding has its appropriate adaptation to particular cases, and should be held in reserve by the surgeon.

Immobility of the Lower Jaw, of fourteen years standing, cured by extensive section of the Muscles.—I need not enter into a full description of the history of this case. My principal object is to give a brief detail of the operation, which it will be perceived presents some novel points. Miss B. was submitted to my attention about the middle of March, 1852. She had been profusely salivated in 1837, during an attack of bilious fever. The mercurial ulceration, which extended rapidly, destroyed the alveolar processes of the upper and lower jaws, and these processes, together with the contained teeth, separated by the ulcerative action, were taken away. A large portion of the cheek was also destroyed. After recovery, the lower jaw

was firmly fixed against the upper. The lost teeth had been replaced by others which had grown irregularly—some inwards and some outwards. The undestroyed portion of the cheek was firm and hard like cartilage, and the posterior angle of the open space was bound together by a band of similar substance. The patient had taken no solid food for fourteen years; she subsisted on fluids and alimentary substances, reduced to a pulvaceous mass, and forced between the teeth.

First Case.—On the 18th day of March, 1852, I performed the following operation in the presence of several medical gentlemen, assisted particularly by Profs. Hughes, Armor, and Hudson:

The patient was placed in a recumbant position, lying upon the right side. The incisions to separate the adherent lips and cheek were carried upward to the lower margin of the malar bone, backward and downward to the parotid gland and angle of the jaw; everywhere the parts were tightly adherent, and so dense as to oppose considerable obstacle to the progress of the scalpel. To avoid branches of the facial nerve these incisions were made close to the bone. All the tense and attached parts being free, an attempt was made to insinuate a wooden wedge between the teeth, but without success, as the jaw did not seem to be affected in the least by what had been done. A broad bladed tenotomy knife was then passed into the mouth, and carried to the posterior margin of the masseter muscle, (which was dense and rigid,) and the whole of this muscle freely divided. A further attempt was made to open the mouth, but in vain, and the knife was again introduced, and carried backwards and slightly upward into the temporo-maxillary region and turned against the temporalis. After the division of this muscle, the point of the knife was depressed, and carried still deeper into the pterygo-maxillary region, and again turned against the internal pterygoid, the complete division of which seemed to remove all the obstacles on that side of the face. It was found that the jaw would yield a little, and the wedge was with difficulty insinuated sufficiently to allow the application of a lever, which I had previously prepared to open the mouth. But, with all the force that could be safely applied, the mouth opened only to the extent of one-fourth of an inch. Passing my finger into the mouth to ascertain if possible the cause, I found that the masseter muscle, of the sound side, was extremely tense and forming quite a prominent ridge in the cheek. The knife was, therefore, passed beneath the mucous membrane a little anterior to the muscle, passed backwards, and a submucous division of it effected. The fibres gave way with a cracking noise, the ends retracted some distance, and upon the application of comparatively gentle force with the instrument, the mouth was opened to the extent of an inch and a quarter. The instrument was suffered to remain in its position for an hour, and lint passed into the cut parts, to stay the hæmorrhage. The margins of the open space in the cheek, were now found to be so far separated, that no attempt to bring them together

was deemed proper, and this part of the operation was postponed till a future day.

During the whole of this operation the patient was entirely insensible. She readily came under the influence of chloroform, and no disagreeable symptoms occurred during the operative process.

The instrument being removed, and some wedge-like blocks placed between the teeth, on either side, to prevent closure of the jaw, the water dressing was ordered to the face, and the patient requested to remain quiet.

We need not detail the course of the treatment during the subsequent six weeks. There was considerable swelling of the face during the week following the operation, but inflammatory action was not excessive. Pledgets of lint were kept between the gums and cheek to prevent adhesion, and, after two weeks, she was directed to move the jaw frequently, the blocks being left out during the day, and replaced at night. The dens sapientiæ, above and below, on the left side, which were cut, after the mouth became closed, were found as the swelling subsided, to interfere with the closure of the jaw, and the lower one was removed. This permitted the approximation of the teeth, and mastication was performed with facility. In six weeks all the parts were healed, and on the 20th of April we proceeded to close the opening in the cheek.

The extent of this opening, when the case was first presented, was so great as to demand, in our opinion, a transplantation for its closure, but the separation of the soft parts, which had been firmly bound to the bone, the stretching and lubrication which was constantly maintained during the healing, had almost brought together the margins of the opening in the cheek, so that it was now obvious, that a very simple operation would complete the cure.

Accordingly, on the day above mentioned, the patient having been placed under the influence of chloroform, the margins of the open space having been pared, and a slight band—which in spite of our efforts had bound the lower lip to the gum—separated, the edges were accurately brought together, and maintained in contact by the harelip suture. The principal care in these incisions, was to secure a proper symmetry of the mouth, and this was effected by previously marking in ink the course of the knife. Upon bringing the parts together, the deformity immediately disappeared. It was now thought proper, in order to prevent undue stretching of the parts, to keep the jaw closed and to remand the patient again to fluid diet.

Nothing unusual occurred during the subsequent progress of the case. The wounds healed rapidly, and on the 13th of May the patient having entirely recovered, prepared to go home.

The muscles regained their power, and the jaw was separated and brought together with great facility, so that mastication was entirely satisfactory. The space in the cheek was closed with accuracy, and the healing was so satisfactory as to remove every vestige of de-

formity. The speech became clear and perfect, and the disagreeable escape of saliva was at once arrested.

Second Case.—Deformity of the Face.—Gordon, infant, aged 7 days. Presented early in January, before the medical class, at the college. This was a most extensive deformity of the mouth, there being entire absence of the upper lip, of the hard and soft palate, and of the middle portion of the superior maxillary bone. The septum narium projected beyond the level of the face, and the portion of the maxillary bone, whose absence at the proper place seemed to allow this unusual development, was firmly fixed directly upon the point of the nose; passing over this bone, and projecting somewhat beyond, like a kind of proboscis, was the columna nasi, the whole presenting at first view a horrid resemblance to a miniature elephant. Four teeth fully developed, were loosely fixed in the upper jaw, on each side of the fissures above described.

The great extent of this deformity, and the horrible appearance which it presented, induced the parents to seek immediate relief. The child, in other respects healthy and vigorous, was accordingly presented, when only seven days old for this purpose. After consultation with Profs. McGugin, Armor, Hudson, and Hughes, who coincided in the propriety of an operation, the infant was brought into the amphitheatre, and delivered to an assistant, with its hands securely fixed. The process was commenced by dissecting up the integuments including the displaced columna nasi, from the protuberance on the nose. Carefully preserving the columna to be used in a subsequent step of the operation. The bony growth upon the nose was then removed, when it was found to be firmly connected by cartilage with the septum and alæ nasi. The projecting portion of the septum was also removed to the extent of half an inch, and was found quite ossified. A question now arose as to the proper manner of closing the large fissure of the upper lip, the margins being separated to the extent of an inch. The plastic and yielding condition of the parts finally induced me to bring the margins together, previously pared, with the common hair lip suture. A needle was, therefore, passed deeply through the lip at the prolabial margin, and approximation effected by a few turns of the thread. A second needle was then passed through the lips at the base of the nose, the pendent portion of the columna cut off and pared at the lower end, being pulled down, placed in proper position and included in the course of the needle. When the parts were all brought together by this means the deformity immediately disappeared. It was feared the degree of tension in bringing the parts together, and the feeble powers of nutrition, in so young a subject, would prevent a kindly union, and this fear was ultimately realized. The child seemed to suffer but little from this severe operation, but when the dressings were removed on the third day, it was found that union had not taken place. The parts composing this portion of the face had yielded and the

chasm was reduced at least one-half. Adhesive plasters were drawn across the upper lip, with the view of still further approximating the bones and soft parts, and the parents were requested to return with the child in the following April or May.

April 27.—Child was presented at the hospital. The constant traction upon the parts with the plaster had brought the labial margins sufficiently close to justify the ordinary operation for hare lip, which was accordingly performed. The needles were removed on the second day, but the coating of collodion, previously applied, prevented an accurate inspection of the part at the time. On the tenth day, the lip was fully exposed and found to be united in such a way as to perfectly remove the great deformity at first existing, and relieve the unhappy and anxious parents. The child has remained well and vigorous through these operations, and grown as rapidly as do children under ordinary circumstances.

Remarks—The extraordinary extent of the deformity in this case, seemed to call for an operation at an early age; also, the progress of development in the parts would have increased the difficulties had it been deferred. The yielding condition of the osseous structures at the age of six days, facilitated the closure and accurate opposition of the parts. The space to be traversed, rendered so much stretching necessary, that adhesion was prevented, yet the amount of necessary cutting was so great that the lateral parallel incisions through the lips were not deemed advisable, especially as it was known that remunerating benefit would be obtained, whether there was adhesion or not.

New Operation for Labial Fissures.—It is with great pleasure I am able to announce to the society a new operation for hare lip, and every variety of labial fissure, which I think obviates the chief difficulty of former proceedings. I had never been able, even by the procedure of Malgaigne's, to treat a case of hare lip without the disagreeable prolabial notch, which surgeons have made so many efforts to avoid, and this reflection induced me to resort to the following modification of the usual process.

Mr Shields, a printer, living in the city of Keokuk, presented himself to me on the day of the operation above described, with a considerable fissure in the upper lip, caused some years ago by mercurial ulceration. Adhesions had formed, as usual, and the teeth were exposed, causing a disagreeable deformity, for which, mainly, he solicited an operation.

In the presence of Dr. Elbert, I proceeded to separate the adhesions formed by the lip and to pare the margins, carrying the incisions a little outward at the middle of their course, so that after the paring was completed they presented a crescentic appearance. A needle armed with a tolerable strong ligature was then passed deeply through each lip near the lower margins, and the raw edges were brought together, accurately approximated below, as in ordinary

Glover's suture, but not so tightly as to strangle the included skin. A very fine needle, armed with a single fine silk thread was then passed *very superficially* through the contiguous margins, so as only to include the epidermis, and by very numerous stitches, from twenty to thirty, these margins were brought very accurately and smoothly together. These fine stitches were continued directly over the large deep stitch to the epithelial border, and, when completed, obliterated any vestige of the prolabial chasm. The parts were then well sustained by adhesive plaster. On the second day, the large stitch, which was designed merely for temporary support, was removed without difficulty, and the parts presented a beautiful appearance. No undue inflammation occurred during the cure—no deep seated ulceration in the course of the needles—no wrenching and pulling to get the needles away, exciting in some instances fresh and destructive inflammatory action. The stitches retaining the parts in contact until adhesion of the opposite surfaces had taken place, needed no attention, and becoming incorporated with the plastic exudation which came from the wound were finally thrown off with the scab, disclosing an even smooth, beautiful lip, without the indentations which usually mark the situation of the needle, and without the slightest imperfection of the prolabial margins.

Art. II.—*Medical Topography and Health of the Counties of Des Moines, Louisa and Washington, in Iowa.*

G. R. HENRY, M. D., Chairman of the Committee of District No. 2, comprising the counties of Des Moines, Louisa, and Washington, in the State of Iowa, in his Sanitary Report to the Medical and Surgical Society which met at Davenport, June, 1853, says:

The alluvial lands lying along the bottoms of the creeks and rivers, when they are not submerged by the annual overflow, are very productive; they are however, the hot-beds of disease, their inhabitants bearing upon their persons the marks of malaria, and on account of their frequent sickness they acquire an appearance by which they can generally be recognized as citizens of the *bottom*.

I have frequently heard it remarked, "that no one can grow rich in these localities," and have been told by the pauper residents of such neighborhoods, that they went there with some property and had constantly grown poorer, both in pocket and in personal appearance, until they were by necessity compelled to seek a more salubrious atmosphere. Much improvement may be expected, in respect to health, when a proper system of drainage is put in execution

among these low lands, and the pools of stagnant water taught to flow into the Mississippi.

In Louisa county there are twelve regular Physicians, two Thompsonians, and one Eclectic, to a population of five thousand. In Washington there are ten Regulars and one Thompsonian, to a population of less than five thousand.

In Des Moines county, outside of the city of Burlington, we have nine who profess to practice on the principles of the regular profession and one or two Root Doctors. In Burlington we have fifteen Physicians, one Eclectic, and four *Steamers* and *Botanics*—we have thus in the county, twenty-four Physicians and seven *Quacks*, to minister to the necessities of fourteen thousand persons, being one to every four hundred and fifty.

Besides this, we have frequent but transient visits from Homœopaths and Hydropaths, who create a brief and flitting sensation among that class of persons who are always looking for something new, and herald the advent of each last comer as the arrival of a savior who is to be infallible in his treatment of all diseases.

Art. III.—*Contractility of the Exsected Heart.*—*Tape-worm in the Cat-fish.*

Professor D. L. M'GUGIN, of Keokuk, reported a remarkable case of prolonged vital contractility in the heart of a fish :

In May, of this year, a fish, weighing a little over half a pound it is supposed, was caught in the Mississippi, sold at the wharf and purchased by one of our household, with others, for the table. At the time of the purchase the fish betrayed no signs of life, a small cat fish, bought at the same time, from the same fish-monger, did give repeated manifestations of vitality. It was brought to the house, a distance of a fourth of a mile from the place where purchased and during this time it evinced no movement betokening life. A youth in our employ, commenced the work of removing the scales, which when done, the small fish above alluded to was decapitated, and afterwards disembowelled. He discovered among the viscera removed, a small substance pulsating after the manner of the heart, and as he had paid some attention to Anatomy, having dissected during two or three of the past winters, he soon decided it to be the heart. It continued to pulsate, he supposed, from the first discovery of a movement for about twelve minutes, during which time it was wholly separated from the fish, or any part of it. He then called my attention to it, and it was found to be the heart, and the *systolic* and *diastolic* movements regularly and fully performed. It was watched for half an hour, during which it continued to contract and expand as regularly as at first

noticed. Being called away in the meantime, I requested Mr. Robert Adams, son of Dr. Adams, deceased, of Pennsylvania, to observe it and mark the time when its movements ceased. In one hour and a half I returned, and to my astonishment, I found it still dilating and contracting, but at somewhat longer intervals. It still continued its movements, each becoming more and more feeble, until at the expiration of two hours and forty minutes after its separation and removal from the fish, it ceased its action. Prior to this time some ten minutes, Dr. Haines, of this city, called, and upon mentioning the case to him, was invited to see it. Although very feebly, and at long intervals, it continued its movement, yet it was plainly perceptible to us all, and by Dr. Haines in particular, who assisted in weighing it and found it to be iv grains only. The heart is now in my possession. Here is furnished a strong corroborating proof of the postulate that "contractility is a property of muscular tissue, and inherent in it, and that the agency of the nervous system upon it, is merely to call it into active operation." In this case there was an entire separation from the nervous center and ganglia, nor was there any stimulus derived from the blood. The nutrition, which had been supplied to it and retained, appears to have been the only source of stimuli which, acting upon its special nerves, perpetuated its contractility. It is quite reasonable then, to suppose that stimuli act upon the nerves, and the nerves in turn, through their filamentous distribution, furnish to muscular fibres their capacity for contractility.

The Tænia Solium of the Cat-fish.

In the further prosecution of inquiries with other fishes, the small cat-fish, above alluded to, was opened and the heart examined, it was found to betray no signs of contractility, although the fish itself had exhibited by the motion of its fins and tail, symptoms of vitality. In removing the viscera of the abdomen, the vascular arrangement and distribution of what appeared to be the mesenteric vessels, struck me as being interesting and beautiful, and proceeding to make a preparation of the entire intestinal tube, it was relieved of its contents, mingling with which, when pressed out and removed, two of the above species of parasites were found as white as milk, and betraying evidences of active vitality, remarkable in this low order of organization. They were each about three inches long, would dispose themselves in full length, and perfectly flat, when they would measure the eighth of an inch in breadth, and then instantly shorten their length by disposing themselves into rugæ or frills all along their sides or edges, and their joints under an eye glass would nearly disappear, and then again as suddenly elongate themselves. These curious specimens are also in my possession. A few drops of alcohol destroyed them. Are these tænia peculiar to this species of fish, and if so, what purpose do they subserve in economy of this variety of the piscial tribe? These were found in that portion of the tube which might be called the colon.

Art. IV.—*Poisonous Effects of Soda Water from Copper Fountains and Lead Pipes*: By R. OGDEN DOREMUS, M. D.

Having, within a few days, had several friends relate their sudden illness after taking a single glass of soda water, and suspecting some poisonous impregnation to be the cause, I was induced to obtain several gallons of this favorite beverage, from different parts of the city, and to submit them to a chemical examination.

The substance which first attracted attention was *copper*.

This was very abundant in soda water obtained from several obscure shops, where it was presumed the traffic was limited, and consequently the acid water remained longer in the copper condensers. It was so evident that, on boiling off the excess of carbonic acid gas, a green scum made its appearance, which, on further evaporation, settled. This was carbonate of copper, previously held in solution by the carbonic acid.

The amount of metallic copper in a quart was one grain and a half!

Soda water obtained from the same establishment, on different days, was found to contain varying amounts of the poisonous carbonate.

The source of this copper, and the cause of these differences, may be accounted for in several ways.

The copper condensers purport to be tinned internally; but where they have been in use a long time, the tin, by chemical and mechanical action, has been removed, at least in part; thus exposing a surface of copper to the corrosive action of the carbonic acid, aided by sulphuric acid, which is occasionally found in the soda water.

Although the carbonate of copper is insoluble in *pure water*, it is capable of being held in solution in water highly charged with carbonic acid gas; for the soda water which yields this green scum after discharging the gas, is clear and colorless previous to the operation.

The soda water drawn shortly after charging the condenser, would necessarily yield less copper, on analysis, than that obtained from the same fount after having several days to exert its corrosive influence. Again, the tinning (for all are professedly thus lined) would be more perfect in some than in others, dependent not only on the length of time the condensers had been used, but also on the completeness of the original coating. I have been informed, that, in order to facilitate the flow of the tin, soft solder is at times resorted to, or the copper is washed with a salt of mercury. Under these circumstances, the chemical and electrical action would be rather complicated, and the soda water possessed of remarkable *medicinal virtues*.

The second poisonous compound, which, from its abundance, demanded investigation, was a white precipitate, the carbonate of lead. This was found, to a greater or less amount, in most of the waters examined.

In the quart whence the grain and a half of copper was obtained, 0.65 of a grain of metallic lead was found.

The chief source of this impregnation is the lead pipe used in many fountains to convey the carbonated water from the condensers to the jet.

It is an established fact, that the free carbonic acid found in spring waters, is capable of dissolving or facilitating the solution of many of the salts of lead, such as are found encrusting lead-pipes which have been used for conducting said waters.

By the investigations of Dr. Ellet, published in this city last year, it was clearly shown that even the trivial amount of carbonic acid found in Croton water, is sufficient to act upon the lead-pipes.

This lead may be readily found in any kettle which has been used for boiling the Croton water passed through a lead-pipe, by adding a little acetic acid to it. The acetate of lead will respond to sulphuretted hydrogen, by assuming a black tint (the sulphuret of lead,) or a yellow tint with the iodide of potassium, &c.

Since carbonic acid is possessed of such solvent powers, soda water, which is surcharged with it, must become poisonously contaminated by contact with lead, either in the pipes or the soldering; and, as much of the tin of commerce is alloyed with lead, even this metal, to which we look for protection, may be another source of evil.

Many are impressed with the belief that the first few glasses may be impregnated with lead to an injurious extent; and hence the custom, in the more respectable establishments, of discarding the soda water which is first drawn, and has lain in the tube over night.

Wherever lead-pipes are used to conduct the water to the jet, and, especially, where, in order to secure a cool draught, from thirty to sixty feet of lead-pipe are coiled in a tank and covered with ice, the highly acid liquid must necessarily dissolve the metal, and communicate the poison to all contained within the condenser.

These remarks are not applicable to pipes of pure tin, or of lead properly coated with tin.

I have examined the soda water obtained from a manufactory where it is bottled, but could discover neither copper nor lead.

The effervescent liquid which is at times "palmed off" upon the public, made by forcing atmospheric air into water, (most truly, "aërated water,") would, from the very want of the carbonic acid, be nearly free from these contaminations.

It might be asked, "If these poisonous bodies exist in soda water, why are not the effects more commonly known?" I would reply, they are more generally known than is supposed.

Since commencing these investigations, I have learned from several medical friends, that a coppery taste, violent vomiting, colic pains, purging, &c., have not been uncommon results from such draughts; and most with whom I have conversed, have experienced these effects personally.

In Dr. Mitchell's Therapeutics, mention is made that soda water from old copper fountains is strongly marked with the copper taste.

My assistant informs me that five years since, while in a drug store, he observed that vomiting and other symptoms of poisoning by copper followed frequently after drinking soda water, and that many thought it was cholera; and, after being similarly affected himself, he tested the water and found copper.

I am informed by a resident of St. Louis, that, while the cholera prevailed, most persons abandoned the use of soda water; it was a common remark: "Mr. — took a glass of soda water, and was immediately attacked with cholera."

Probably the syrups, which are the usual accompaniments of the soda draught, act in many cases as an antidote; for, although the efficacy of sugar in this respect, as originally proposed by Duval, was denied by Orifila, it has lately been re-asserted by Postel.

I regret that, for want of time, I have not been able to complete other experiments on this subject; yet, as I am convinced that in many cases this poisoned soda water has proved the exciting causes of cholera in those predisposed to this disease, and in others that it has by its inherent properties been injurious to health or destructive to life; and as at this time the cholera question is again agitating the public mind, I have thought it advisable to relate the result of this partial investigation.

With the knowledge of these facts, we may conclude that although soda water may be retained in a *well tinned* copper condenser, and discharged through a *thoroughly tinned* lead-pipe, without poisonous impregnation; yet, as any imperfection in the tinning of either, or long or careless usage, may expose the copper or the lead (or both) to the solvent powers of this carbonic acid, and thus render the beverage dangerous, therefore these vessels should be discarded or only permitted in the hands of trustworthy persons.

Condensers of stone, of iron, or of the purest block tin supported by iron bands or of gutta percha, aided in a similar manner, would be free from poisonous impregnation. Conducting pipes of these latter materials are likewise unobjectionable.

In another paper I shall present the results of more extended investigations, and shall be indebted to any physicians or pharmacutists who feel disposed to assist in this work of common interest, by favoring me with the reports of cases, or samples of suspected liquid for analysis. If those engaged in the fabrication of this article would afford an opportunity of examining some of the old soda fountains, it might aid materially, and perhaps result in the suggestion of better methods of protection.—*American Med. Monthly.*

Art. V.—*On the Diminution and Disappearance of Uterine Tumors.*

[Since the article on cancer uteri, by the Editor, in this number of the Journal, passed through the press, Braithwaite's Retrospect for July, 1854, came to hand, from which the following excerpted article is taken which strikingly coincides with the fundamental views in the first named paper, with the exception of what is said of diet.—EDITOR.]

By DR. SAMUEL ASHWELL, late Obstetric Physician and Lecturer at Guy's Hospital.

[There is something very extraordinary in the spontaneous diminution of some uterine tumors, without any coincident breaking down of their structure. A few striking cases of this kind are here recorded. The alarm on discovering the presence of large tumors of the uterus is by no means so great as it was some years ago.]

The exact pathology, however, of certain of these uterine tumors is still a vexed question—one certainly admitting of further elucidation—whether, for instance, a *fibrous* tumor is ever capable of being absorbed, is yet a matter of dispute; for, while Lisfranc regards the absorption of any tumor as satisfactory proof that it is not fibrous, there are, as I shall show, distinguished pathologists, both in this country and abroad, who believe they have witnessed the melting down and disappearance of both fibrous and cancerous growths. It is therefore right, where we can only form a doubtful diagnosis as to the nature of any uterine tumor, to treat it as though it were curable; for, if malignant, judicious treatment can do no harm; whereas, if it be of unspecific and benign character, the treatment will generally prove curative. I state, then, at once, that the pathology of these large and hard tumors, to the very occasional diminution and disappearance of which, *without* coincident breaking down and discharge of their structure, I am now soliciting attention, is not the object of this paper, my aim being to show that tumors of the uterus so large as to attract attention by their great size, so hard as to resemble, in some examples, the hardness of true carcinoma, and giving rise to alarming hæmorrhages and frequently recurring pain, do very occasionally, when all treatment has been abandoned, present the following results:

First.—A slow but progressive diminution of bulk and of hardness, unaccompanied by any appreciable breaking down of their structure.

Second.—A lessening, at least, and sometimes a cessation of the bleedings, profuse menstruation and discharges, and, where it has existed, of the pain.

Third.—A gathering of flesh, and restoration of the general health.

As tending to confirm these statements, I shall adduce the opinion of some eminent pathologists, to prove that the absorption even of scirrhus tumors may be effected by appropriate treatment.

Sir Charles Clarke (vide "Ashwell on Female Diseases," p. 297) mentions a case "where a tumor as big as a child's head could be felt through the parietes of the abdomen; just above the pubes, upon its surface, could be felt two small tumors—one the size of a man's fist, and the other twice this size. The patient had labored for some time under a very profuse discharge of blood from the vagina. A variety of means were employed for the relief of this case for two years. Upon examining the abdomen at the end of this period, the tumors could not be discovered, and after death the uterus was found as large as that of a woman at the end of the fifth month of pregnancy. Upon the anterior part of it, near the fundus, were found two tumors, *as large as peas*, which were probably the same tumors before felt, as there was no vestige of them. The tumors were of a hard and resisting nature, and were lying between the muscular part of the uterus and the peritoneum covering it."

Dr. Walshe (in one of the most invaluable text books of our profession, "On the Nature and Treatment of Cancer," p. 134) says, in reference to the termination of cancer, by *resolution and absorption*—"one of the most essential attributes of cancerous substance is an unswerving tendency to grow, or at least to sustain itself unimpaired by the influence of surrounding parts; its nature is opposed to resolution. Nevertheless, there would be temerity in denying the possible occurrence of this unfortunate change." Weller affirms that he observed a medullary fungus of the eye of an infant, which eventually disappeared, and was followed by dropsy and atrophy of the ball. Mr. Travers relates, that "the solution of chloride of lime effected the absorption of a large tumor, (in the course of some months,) regarded by competent authorities as scirrhus, in a lady whose other breast had been extirpated for that disease. Not long after she died of asthma, from diseased lungs; the scirrhus tubercle appearing, not only in the chest, but in several of the abdominal viscera." Whether the use of the chloride and the disappearance of the growth were a mere coincidence or not, I may at least adduce this case as confirmatory of the effects of treatment in my own cases; where, of course, the patients having happily recovered, the precise nature of the tumors could not be certainly ascertained.

Dr. Walshe (p. 207) remarks, that the external use of iodine, in the form of ointment, sometimes completely removes tumors possessing the character of scirrhus, (where it has been had recourse to at an early stage of their development,) is a fact of which we have witnessed some examples." Further on, the same distinguished author adds, "Nor does my present experience (1846,) allow me to conceive a single doubt that tumors *actually and truly scirrhus in structure*—tumors which would have run the common course of cancer—may be arrested in their progress, by early and judicious use of these agents.

Mr. G. N. Hill's cases ("Edinburgh Med. Surg. Journal," vol. xxv., p. 282, 1826,) afford very decisive evidence of the same effect; and Dr. Ashwell's statements on the subject (see "Cancer of the Uterus,") are strongly encouraging." The convictions of Mr. Travers on this subject (one of our highest authorities) are peculiarly strong; "By an indolent scirrhus," says the writer, "I mean an incompressible, permanent tumor, possessing for many years no distinguishing character of that disease; but in a deranged state of health assuming its general character, and at a particular period of life breaking up into actual cancer. I do not entertain a doubt that such a tumor *may be, and often is, absorbed in its first stage, and need, not therefore of necessity follow this course.*"

It may, then, be assumed that the diminution and disappearance of tumors, regarded as scirrhus, is not beyond the pale of a reasonable credulity. Of whatever nature the tumors to be described by myself may have been, they were morbid enough, if even only simple inflammatory indurations, to originate and keep up symptoms so distressing, as to destroy almost entirely the comfort of life. It cannot, for example, be a small amount of suffering which has to be endured, where a *hard* tumor, the size of an adult's head, has, for months, perhaps years, by its bulk and weight, pushed out of their place the contiguous viscera, and pressed severely on the ganglia, trunks, and branches of the neighboring nerves. Nor can the bleedings, frequently attendant on such growths, fail to alarm, not only from the syncope and long continued exhaustion, which is their immediate result, but also from the impaired health and dropsical effusions of which they are so often the precursors.

Many of my cases have been associated with severe, frequently recurring, and sometimes almost constant pain, for weeks and months together, in or about the growth. Emaciation, where the tumor was large, has not been an uncommon event. Nor ought the possibility of pregnancy (on which I have fully dwelt in my work on "Female Diseases,") and its dangerous and oftentimes fatal results, to be excluded from the long list of distresses occasioned by these tumors.

Prior to the detail of the appended cases, it is necessary to make the following observations.

That, in all of them, treatment (especially by iodine, in tincture and ointment,) had been long pursued; that nutritious, unstimulating diet, mild malt liquor, and light wines were allowed; that resort was occasionally had to *leeching* near, not over the seat of pain; and still more rarely to cupping on the loins; that *purgatives* and aperients were exhibited only when it was evident that the bowels required to be unloaded; that *setons* over the site of the tumor produced, in several instances, marked benefit; and, further, that in all the numerous cases which have fallen under my notice, the *recumbent posture*, and, as far as possible, the avoidance of sexual intercourse, but particularly the former have been *strictly* enjoined.

Case 1.—Large Hard Tumor of the Uterus, with Excessive Hæmorrhages, and Slow but Complete Diminution of Bulk.

April 22d, 1843.—Miss —, aged 48, resides near Hounslow, and has formerly been under the care of Dr. Blundell and her own medical attendant. First perceived a tumor, about the size of a small melon,* three years ago. It was then low down in the hypogastric region. Her health did not then suffer, but two years since menstruation became profuse, and there was also much uterine bleeding. Iodine was used, and various means were employed to arrest the hæmorrhages.

Now, the tumor is as large as a moderate sized adult's head, lobulated, and in several of its more prominent portions of *extreme* hardness. It reaches nearly as high as the umbilicus, and protudes the abdominal integuments, giving to the patient the appearance of a pregnancy of the fifth or sixth month. Has frequent cutting pains in and about the tumor, and is greatly inconvenienced by the weight, pressure, and tension. The growth is not tender to the touch, not even in those portions where there is constant pain. Walking is difficult. Internally the vagina is capacious, and there is much mucous discharge. The os uteri is patulous; and its lips, together with the cervix, are soft and swollen, but without any spots of induration. The most alarming symptom is the hæmorrhage; which, without any assignable cause, is sometimes so excessive as to induce long continued faintness. Cold applications are often employed for several days before the bleedings are arrested. She has lost flesh, and is very weak; her countenance is anxious and very pallid; pulse one hundred and ten; bowels constipated; appetite bad; she is restless and irritable, and often extremely depressed. Tincture of iodine, six minims, three times a day, in a little sugared water, and the iodine ointment every night over the tumor, were prescribed.

[For some years this patient was watched; frequently the iodine was suspended, and, on account of the exhaustion from the bleedings, other remedies used. In November, 1849, the patient's health was very much improved, and the tumor was the size of an orange. In May, 1851, Dr. Ashwell could scarcely make out the presence of any tumor. In March, 1852, the tumor could not be felt at all externally.]

Case 2.—Hard Tumor of the Uterus of Considerable Size; Diminution and Eventual Disappearance.

[Mrs B., æt. 48, when admitted under Dr. Ashwell, at Guy's Hospital, had a large hard uterine tumor of the size of a child's head. After being in the hospital several months, and having received great relief, she left May 15th, 1837, but continued as an out patient, and

*"As large as a small melon," is little better than "as large as a piece of chalk." A small melon in New Orleans is as big as a baby's head; and, a larger one, as big as a big baby, if not bigger.—EDITOR.

was taking six minims of the tincture of iodine in an ounce of water, three times a day.] Dr. Ashwell says:

For several months after June, 1837, I occasionally saw this patient, the tumor continuing slowly to diminish. From that time I lost sight of her till May, 1853, when she called to consult me about some affection of her lungs attended with cough. On examination externally, *no tumor was perceptible*, even when the fingers were pressed deeply down behind the pubes, and the cervix uteri is quite healthy.

Mrs. B. married in November, 1837, the tumor then being as large as a small melon. She had been pregnant only once, and aborted at two months; this was very soon after marriage. She had ceased to menstruate two years. The tumor decreased rather more rapidly for two or three years after her marriage, and she assures me that for the last four years it has been as imperceptible as it is at present.

Case 3.—Hard Tumor of the Uterus; Treatment by Iodine, and Eventual Disappearance.

[Communicated by Mr. Richard Wedd, of Cheshunt.]

Mrs. —, aged 46, had a tumor on the left side of the abdomen, occupying the space between the umbilicus, the anterior superior spinous process of the ileum, and the symphysis pubes. It was *hard*, not very painful to the touch, and about the size of the fœtal head. The os and cervix uteri presented no abnormal indications. The tumor could be balanced by the fingers placed on the cervix, and when pressure was made upwards, the tumor was distinctly elevated, and protruded the abdominal integuments, so much as to render its outline distinctly visible. External pressure above and around the growth forced down the whole uterus much lower in the cavity of the pelvis, and left no doubt that the tumor was really uterine. Her health was not at first much affected, although she suffered almost constantly, and at times severely, from shooting pains through the uterus, extending to the back, and coursing down to the inguinal region on either side. There were also frequent and large hæmorrhages during the catamenial intervals, and menstruation was profuse, lasting six or seven days. There was no interference with defæcation, and the bladder was easily relieved.

Mrs. — was treated by leeching and opiates for the pain. She was enjoined the recumbent position, and her diet was carefully regulated. Iodine was long taken in the form of tincture, and the iodine ointment was sedulously employed, with some intervals, for nearly two years. At this period the tumor was not diminished in size, and but little in hardness, although there was less pain, and, as her general health seemed giving way, she was advised to go for a lengthened period to the sea side. She removed to the South coast of Devon; and, although I did not see her during the absence of two years, I heard occasionally that the tumor was diminishing, and that she improved much in health. On her return to her former residence in Kent. I saw Mrs. —. She told me that in a few months after her

arrival at the sea, the bleedings at the menstrual period and during the intervals began to lessen; that her appetite and general health improved; that within six months from her removal the tumor had sensibly diminished, both in hardness and size, and that the pain had subsided in a marked degree. On careful examination, now four years from its first recognition, I could discover no traces of the tumor. The abdomen was soft, more flaccid than usual, even when, as in this case, the integuments had been distended by many quickly occurring pregnancies. The os and cervix uteri were entirely natural.

Case 4.—Hard and Large Tumor of the Uterus; Eventual and almost Entire Disappearance.

In November, 1840, I met Sir James Clark in consultation on the following case: Mrs. —, aged 40, has been married eleven years, and, although pregnancy has occurred, she has never borne a living child. Her health has generally been good, her temperament lively, and her habits active. During the summer of the present year, Mrs. — had a severe illness, primarily inflammation of the peritoneum, but followed by fever. In the latter part of September, 1840, she first discovered an enlargement in the hypogastric region, immediately above the brim of the pelvis and towards the left groin, which was tender, but not painful. Soon afterwards, however, it became larger; and, as there were dartings about the growth, and occasional paroxysms of severe suffering, a careful examination was made, (Nov. 20th,) when a tumor of considerable induration was discovered. It had risen three or four inches towards the umbilicus, and, although it passed a little to the right of the mesial line of the body, by far the greater portion was in the left hypogastric region. The cervix uteri was swollen, patulous, and indurated in several spots. There was no difficulty in relieving either the bladder or the intestines, but there was some emaciation, and the pain was sufficient to induce her to keep almost constantly the recumbent position, and confine herself entirely to the house. A light nutritious diet of fish, chicken, mutton, with some wine and water, was ordered; a mild aperient pill, and the following mixture were also prescribed: ammoniated tartrate of iron, one drachm; tincture of iodine, half a drachm; compound tincture of cardamoms, one ounce; distilled water, seven ounces; two tablespoonfuls to be taken three times a day. The local measures were leeches on alternate mornings to the tumor, the use every night of the hip bath, and the constant application, by means of linen compresses covered by oiled silk, of the poppy fomentation.

Such was the plan pursued for nearly five weeks, at the end of which time the growth had lessened considerably, and there was but slight pain. At this period (January, 1841) the weather was intensely cold, and to derangement of the health consequent on this state of the atmosphere, but more especially to excitement, an unfavorable change both in her health and in the tumor was clearly attributed.

Greater caution, however, was observed, and the morbid enlarge-

ment, by the end of February, did not exceed the bulk of a large Seville orange; it having, in November, equalled in size a foetal cranium at the full period of gestation.

During many subsequent months, iodine, internally and externally; mercury, to the extent of producing slight salivation; quinine, and various stomachics and cordials, were exhibited; and, in April, the tumor had subsided so much, and appeared to be altogether in so quiescent a state, that a seton was inserted over its site, in the left hypogastric region. Some weeks afterwards a large seton was made, nearly in the same situation, by Dr. Kirby, of Dublin. Sea-bathing was recommended, and when I last saw the lady, in August, 1841, her health was greatly restored; the tumor was so much lessened as to have sunk quite within the pelvic cavity, the pain having almost entirely ceased; and the cervix was much more healthy. In 1845, and subsequently in 1851, I was informed that not a vestige of the tumor remained, and that the general health was exceedingly good.

Remarks.—I could narrate several more examples of nearly entire disappearance of these large tumors, and a still greater number where, after having diminished very considerably in bulk, they have, for many years, remained entirely stationary. In both classes of cases the attendant symptoms have passed away, and there has been little or no interference with the comfort of life. All the cases which have fallen under my care prove the value, where any doubt exists of the precise character of uterine growths, of cautious and long protracted treatment. The continuance of pain, hæmorrhage, and emaciation, with a stationary condition of the tumor, although unfavorable, ought not to arrest treatment; while, on the contrary, any diminution of these painful accompaniments should be regarded as sufficiently auspicious to encourage a further continuance of remedies. In every similar case the means recommended may not be attended with like success; still, in by far the greater number, I am strongly of opinion their employment will be decidedly efficacious, if not entirely curative. We are not expected to determine what may be the final issue of many uterine tumors, nor to deny the possibility of eventual malignant development, nor to promise a cure; but, judging from the narrated cases, and many others which I am sure have occurred to different practitioners, I may truthfully declare, that in many such cases thus treated, entire disappearance, or, at least, an arrest of further growth will be the result.

Art. VI.—*Contributions to Operative Surgery*: By JOHN P. METAUER, M. D., LL. D., Prince Edward County, Virginia.

Extirpation of the Parotid Gland.—The parotid gland is liable to scirrhus and other malignant transformations of its structures, which if permitted to pursue their course, end in ulceration and its conse-

quences. Modern surgery, however, has, in numerous instances, arrested the progress of these transformations, and prolonged life; demonstrating that the extirpation of the gland, which was esteemed by most of the early surgeons as of doubtful propriety, nay by many as impracticable, is both justifiable and practicable.

In 1831, I extirpated the diseased parotid for the first time; and I was induced to undertake the operation, in a case of scirrhus of the organ, from reading an account of Dr. George McClellen's success in the case of Dr. Graham, a short time before, as reported in the 5th vol. New York Med. and Ph. Journal. I had, however, always considered the operation practicable and free from danger to life, although environed with difficulties. Since 1831, I have repeated the operation six times—in all, having operated in seven cases; and my experience leads me to regard it as justifiable in all cases of scirrhus of the organ, of increasing development, and sometimes after ulceration has taken place.

The first case in which I operated was for scirrhus of the entire gland, of more than three years duration; and it occurred with a man 28 years old, whose previous health had been good; but it had become somewhat impaired, since the enlargement of the parotid began to increase more rapidly. When I first saw this case, the tumefaction presented above the contiguous borders of the parotid depression more than two inches, and I supposed it to be fully eight times the size of the healthy gland. It was of a smooth convex appearance, and oval shape, and extremely hard and resisting under pressure. It extended from the base of the corresponding left mastoid process of the temporal, two inches down the neck, below the angle of the inferior maxillary bone, and was fixed and immovable. There had been occasional lancinating pain felt through the tumor, at nearly every stage of its development, but not sufficient at any time to cause much suffering. The enlargement had been exceedingly slow and gradual, until the last year, during which period the size of the tumor had more than doubled itself. No cause could be assigned for the tumor. There certainly was no reason to ascribe it to local injury. Its progressive augmentation, taken in connection with the long continuance of the tumor, left no grounds to hope for a cure by any other means than extirpation; and that expedient was resorted to on the 13th of November, 1831.

The following was the mode of operation I adopted: A perpendicular incision was formed from the base and forepart of the corresponding mastoid process of the temporal, along the middle of the tumor, and continued two inches upon the side of the neck below the angle of the inferior maxillary bone. This incision was carried boldly through the investing structures quite into the substance of the tumor, and fully an inch beyond each extremity of it. A flap was next turned off from each side, which exposed the surface of the tumor, and which was somewhat elevated above the parotid depression, by the division of its capsule. Care was taken not to wound the cartilaginous tube

of the external ear. The tumor now was forcibly drawn out from the parotid depression by strong double hooks, and the flaps held apart by an assistant, so as to enable me to dissect more readily beneath it. During the dissection, the edge of the knife was turned constantly to the tumor, and the incisions were short and rather scratching and exploratory. Where the connecting textures were loose, the handle of the knife was used. The extremities of the tumor were soon separated so as to enable me to pass my finger under them a short distance, and to render the diseased body somewhat looser. I next attempted to detach the tumor by forcing the left index finger under it, while the assistant made forcible and steady traction, with the double hook inserted deeply into its under side. In this effort I was partially successful, only by reason of the strong connections that confined the tumor; yet, I insinuated the point of my finger beneath it far enough to discover that the parotid was not in very close contact with the surface of the bottom of the cavity containing it. Finding that I could only loosen the tumor by this expedient, without detaching it, I determined to remove it by dissection; and, while it was forcibly drawn out from the depression, with the double hook, I carefully incised and lacerated the connections, using the knife and handle in alternation as became necessary. During this procedure, I found that the external carotid, at its temporo-maxillary portion, penetrated the tumor, but the artery was obliterated and presented as a cord-like transformation, and was divided below the border of the tumor a few lines, as well as the accompanying veins, which, however, were small, and effused but little blood.

I could now draw the tumor out from its bed, with the double hook, more readily, but still it was confined and comparatively immovable. Being held now by its middle and deeply-seated attachments, my aim was to dissect from below upwards, which would enable me more readily to reach them. The hook was inserted deeply into the lower end of the tumor, and pretty forcible traction made outwards and upwards, the flaps being at the same time held apart by an assistant. Insinuating the left index finger beneath the tumor thus held, and pressing outwardly so as to expose the connections, I cautiously divided them, using the point and handle of the knife for the purpose, I preferred to proceed by light applications of the point of the knife, rather than by incision, as already remarked. I found the handle of the knife of great use in tearing through the cellular attachments, so as to insulate the vessels, nerves, and, here and there, ligamentous shreds. In this manner I proceeded until the entire gland was finally removed, requiring from eighteen to twenty minutes time. The bleeding was slight. Only two small arteries required the ligature, and they were supposed to be branches of the facial and occipital. I distinctly saw, when the wound was sponged out and well cleared of blood, the internal jugular vein, the internal carotid, the styloid process of the temporal bone; and the margins of the masseter, sterno-cleido mastoid, and

posterior belly of the digastric muscles. The young man bore the operation with great fortitude.

After carefully examining the surface of the wound, to discover if any of the tumor had been left; and, finding that not a vestige remained, it was closed, using for the purpose sutures and adhesive plasters. A wet compress was then laid on and confined with a suitable bandage, and the patient placed in bed with his head considerably elevated. The succeeding morning the bowels were purged with oil; and, again, on the two following days, the cathartic was repeated. The wet compress was continued for three days, and then was superceded by a cerate dressing and a dry compress. There was very slight inflammation. The ligatures came away on the sixth day. Three-fourths of the wound united by adhesion, and the remaining portion healed by the 12th, and the patient left the infirmary, on the fourteenth day after the operation, perfectly well. There was very slight deformity of the face from the division of the facial nerve. I was informed, eighteen years after the operation, that the man remained well, and that there had never been the slightest re-appearance of the tumor.

The method of operating adopted in this case, and briefly described above, is the same that I have pursued in six others, with such variations, however, as the deligation of the external carotid, and other vessels, might require; or, when ulceration had taken place in the diseased parotid. Of the whole number of my cases, five were permanently cured, and their subjects are yet alive, now more than seven years since the last was treated. The two unfortunate examples were attended with ulceration of the tumors. Nevertheless, their wounds healed perfectly and in a very short time; but the disease returned, and finally ended in death.

Parotid tumors should be removed early, and, if possible, before they ulcerate. It is best, however, that they attain some magnitude, before their removal is attempted. When enlarged, the gland is more readily and safely extirpated, by reason of the partial displacement it undergoes as enlargement takes place, from wedging itself out of the parotid space against the borders of that space. Scirrhous and fungoid disease of the parotid, like those affections when attacking other organs, are liable to re-appear after extirpation, and the only plan of treating them successfully is to remove the re-appearing tumors as early after the return as possible; and, to repeat the operation again and again until the disease is eradicated, unless the constitution is profoundly contaminated. Even then, if the tumors are comparatively local, extirpation would be justifiable, as it affords the only chance for relief. The deformity of the face from the division of the facial nerve is very soon corrected. Before dividing the facial nerve, I uniformly aim to draw it out of the tumor, and to cut as few of the branches as possible, chiefly the central. In each of my cases, the amelioration was coeval with the complete healing of the wounds; and, after six months very little of it was discoverable,

and, only, when the individuals smiled or cried. In the two unfortunate cases, the operation was not repeated, nor have any of my cases required it except those two. Why these cases were not presented for a second operation, I have never been informed. That the cases operated upon were scirrhus of the parotid, I am positively certain; and, that the entire gland was removed in each case, I assert as an undeniable fact.

Scirrhus and Fungoid Affections of the Female Mamma.—The recent report in regard to “the results of surgical operations in malignant diseases,” of Dr. Gross, chairman of the committee appointed by the American Medical Association, to examine into and report upon that subject, does not accord with my experience in the treatment of those affections. I have extirpated forty-two mammae, twenty-eight of them affected with scirrhus, and fourteen with fungoid. Of the twenty-eight cases of scirrhus, fifteen were in a state of ulceration, and thirteen without the slightest ulcer, or disposition to it. Of the fourteen cases of fungoid, eight had ulcerated, and six had not. Of the forty-two cases, twenty-eight were perfectly cured, and at the date of this paper, continued well—more than three years now since the last one was operated on. Of the successful cases, nineteen were scirrhus and nine fungoid disease.

One of the cases alluded to commenced as scirrhus, and, finally, after three years continuance, ulcerated. When the patient came under my care, the ulcer had existed six months. I found the whole mamma implicated and somewhat enlarged, and three large axillary tumors, one about the size of an ordinary peach. The subject of this case was a negress, about 47 years old, and when she was placed under my management, had become considerably emaciated. Her previous health had been very good. She had borne several children, but for fifteen years had been sterile.

The mamma was naturally large, and, although the ulcer was quite extensive, a sufficient flap on each side of the incision was preserved, and of healthy integuments to cover the wound without much traction. The entire mamma, and the three axillary tumors were removed in the ordinary way, and without much difficulty. The wound was closed with sutures and adhesive plasters, as is common, and the water dressing, with a suitable bandage, was employed. The bowels were purged daily until all danger of traumatic fever had passed. The case recovered rapidly, and in fourteen days the patient left the infirmary entirely well.

In the course of ten months this woman returned to the infirmary with a second tumor, in the exact situation of the first, and fully as large. It was in an ulcerating condition, and, very much, in all respects, of the same shape and appearance as the one upon which I had operated. There were also two tumors in the axilla, one fully as large as the largest of the first. Had I not removed, in the first operation, the entire mamma, it would have been difficult for me to be convinced that the present was not a disease of the mammary gland,

so closely did it resemble the first tumor. The general health had become much impaired. There was considerable emaciation. This tumor was quite hard, but less so than the first. Its growth had been exceedingly rapid since it had attained the size of an egg. The disease re-appeared along the line of union in five distinct places—and nearly at the same time—as small tubercles about the size of large shot, unattended with pain, and were exclusively located in the cicatrix. As these tubercles enlarged, they amalgamated, involving the contiguous parts. The disease, as already stated, had existed ten months, when the woman returned, yet the axillary swellings had only been perceived two months previous to that time.

These tumors were again extirpated, not, however, without considerable difficulty, especially those of the axilla, by reason of their close connection with the axillary vein and artery. They were removed, and the extensive wound, resulting, being managed as in the first instance, speedily healed up. In eighteen days the cicatrix was perfectly formed.

For two months after the healing of the wound, everything went on most favorably. The general health had greatly improved. At this time, three small tubercles made their appearance, at different regions of the cicatrix, resembling, from the description I received of them from an intelligent individual, the first that appeared. Without delay they were extirpated, and I took care to remove, with each one, a liberal portion of the surrounding structures. The wounds were closed in reference to union, by adhesion and a speedy cure. The water dressing was also employed to prevent undue inflammation, and the bowels purged daily for three days as auxiliary to that agency. In twelve days the wounds healed perfectly.

After this operation the tubercles re-appeared, and were extirpated six times—the woman having submitted to *nine distinct operations*—two of which were exceedingly severe and painful. The ninth operation, however, seemed to remove the last vestiges of the disease; and the woman, now, (seven years since,) is well and entirely free from any suspicion of a return. The disease, in its first appearance, undoubtedly was scirrho-cancer, but in its re-appearance it assumed a fungoid appearance; and, such alternation I have thrice observed in cancer of the female breast. How far the want of the mammary gland may have influenced the disease in re-appearing, to assume the fungoid form instead of scirrhus, I am not prepared to state, but in the three instances, in which it did put on that form, it succeeded the removal of the mamma for scirrhus.

This case is interesting in several aspects. It certainly was interesting to the patient and surgeon, as it caused the woman great suffering and the surgeon not a little trouble. But its chief interest relates to the precedent it furnishes, in deciding upon or rejecting operative procedure, in cancer and fungoid diseases of the female breast. Had no operation been performed this woman certainly would have died. If the re-appearing tubercles had not been extirpated, there is not the

slightest reason to doubt that they would have resulted in tumors of the nature of those operated on, as they were of the same form and appearance, and originated in similar locations. The remarkable recovery in this case, was due, evidently, to the frequent operations, and from repeating them while the tubercles were small, and before the surrounding textures became implicated. And, it is probable, if a like mode of operating were adopted in the treatment of scirrhus and fungoid, located in other organs of the body, they would more frequently be cured. I ascribe my success in the treatment in these diseases only in part, however, to the mode of operating described, in the case in question. An important item with me, in the management of such patients, is the constitutional treatment generally pursued with them after recovering from the operation. And this treatment consists of aperients, the occasional employment of mercurials to improve the condition of the secretions generally, but more especially the biliary; the nitro-muriatic acid mixture; tonics, both mineral and vegetable; the cod liver oil; in some cases iodine and mineral tonics; a well regulated diet; narcotics, if needed; and free exercise in the open air, if it will not expose patients to catarrhal affections, or to exhausting fatigue; and the treatment should be perseveringly continued. I believe that these discouraging and too often incurable diseases have often proved fatal, by reason of an unwillingness with surgeons to meddle with them, and a disposition to advise unfortunate patients against active surgical measures; when, if an early resort to extirpation had been proposed, and carried out in practice, many lives might have been saved. A refusal on the part of patients, too, to submit to the repetition of surgical operations in these cases, more especially if discouraged by friends, and not unfrequently by their medical advisers, also, has, in many instances, consigned individuals to the grave who could have been cured; and, the case here referred to, is an instance in point. This woman was advised by her friends not to submit to a second operation, but I urged her to adopt the contrary course. She heeded my advice, and her life was saved. These tumors kill, generally, if left to themselves. By extirpating them, early, before the constitution becomes re-contaminated, and while the disease is in a great degree local, the only rational chance for a cure is afforded. My rule, now, is to operate in every instance, unless the disease is already manifesting itself as a pervading constitutional affection, or the patient is too feeble to sustain an operation, and, to repeat it as often as the disease re-appears, as early as possible after its return, and to continue to do so until the fell disease is hunted down and eradicated from the system.—*Virginia Med. and Surg. Jour.*

Art. VII.—*Remarks on Chronic Inflammation*: By DR. C. HANDFIELD JONES, F. R. S.

[When we have very little exact knowledge of the real causative condition of a morbid action, we are apt to class it under the term chronic inflammation. Cirrhosis of the liver, Bright's disease, white pericardial thickenings, &c., we are apt to regard as slow inflammatory processes in the respective organs.] Dr. Jones observes:

I would wish to name all the morbid changes in which the character of growth—of new formation—decidedly predominates over that of inflammation, according to their principal feature. I would call them simply fibroid thickenings, or fibro-cellular growths, and thus avoid giving them a name, either merely trivial, or of doubtful correctness. The matter is not one of nomenclature merely; it has important bearings on practice. Suppose a case intrusted to our care in which the liver was found considerably enlarged, projecting below the ribs, and that we have satisfied ourselves that the enlargement depends neither on engorgement with blood, nor on fatty change, nor on bacony deposit. The probability, then, is very strong that it is in a condition of cirrhosis, the fibrous tissue between the lobules being prodigiously increased in quantity. Can we hope by the administration of mercury or iodine to remove this deposit? If it were simply inflammatory, the attempt might be successful; but as it is a growth, one may confidently predict that it would be fruitless if not injurious. The same remark will apply more or less completely to any of the other instances before mentioned.

It seems to me that observers have not sufficiently held in view, although generally admitting as a matter of course, the vast predominance of that unknown force, call it what we please, which determined and still determines that here bone and there muscle, and in another spot nerve, and in another tendon, should be formed. There is some such force, and it is certainly liable to be deranged, and its derangements are just those which are most difficult to rectify. This perversion of the nutritive act (to borrow Andral's phrase) is that which determines the origin and growth of a cancerous, or a fibrous, or any other kind, even of a fatty tumor. It is the same, I believe, which causes the textural changes above enumerated, and some others which might be added, and not inflammation, which of itself and in its best marked form, produces no exudation which is capable of growing and enlarging. It is this property of vegetative increase which makes the removal of new formations and all allied productions so difficult: mere inflammatory effusions we can melt down and get absorbed, but these resist our "sorbefacient" remedies with the same persistence almost as the natural tissues.

The degenerative process which constitutes true morbus Brightii does not quite come under the above class of morbid changes. The

best observers are agreed that production of new interstitial tissue constitutes no essential part of the morbid action. Most writers incline to view it as a nephritis of more or less chronic character; but the great name of Prout can be cited in support of the view that the substratum of the process is something essentially different from inflammation. My own observation has always inclined me to adhere to this opinion. In the enlarged, often mottled kidney, we see simply a derangement of nutrition, producing hypertrophy of the epithelium concurrently with malperformance of its function. The albuminoid cell-growth does not undergo its normal changes; and instead of forming the secretion, it collects in the tubes, either choking them up and distending them, or getting washed out in the stream from the Malpighian tufts. In further stages, or in more rapid degrees of change, the tubes and epithelium break up into mere detritus. In all this process we see really nothing to justify us in considering it as inflammation. So, also, in the atrophic form, the small dwindled kidney, we find no satisfactory evidence of anything else than a gradually supervening impairment of the nutritive process. The epithelium wastes, and the tissue decays, and collapses; thus far is certain; but how little is there to show that this is dependent on previous inflammatory diseases? On the contrary, the frequency of recovery from acute congestion (as that of scarlatinal dropsy) shows that inflammatory and degenerative actions are widely different in their nature and results. The one exhibits the perturbation of healthy action by an intervening cause, the other is the decay of vital power from innate or induced weakness.

The subjoined scheme illustrates the transitions that seem to exist between two of the chief types of disordered nutritive action. In inflammation of acute sthenic character the hyperemia is a main element, and the exudation is non-vegetative. In cancer (say encephaloid) there is no apparent or necessary hyperemia, but the exudation is endowed with a vegetative power of most active growth. In simple deposit, as of tubercle or bacony matter, there is neither hyperemia nor manifestation of vegetative power; the sole morbid change is the deteriorated state of the plasma.

Inflammation.

Acute sthenic.

Subacute.

Chronic.

Tubercle—bacony matter.

Simple deposit.

Encephaloid cancer.

Fibrous tumor.

Fibroid thickening.

[*Braithwaite's Retrospect, July.*

["Bacony" or, "bacony matter" is a term not sufficiently precise, for a Baconian philosopher, as bacon is the hog's flesh after salting, smoking and drying, and may apply as well to the muscular as to the adipose tissue—to the lean, the fat, and the rind.—[EDITOR.]

Art. VIII.—*Morbus Brightii*.

The discoveries in medicine, within our day and generation, have been greater than at any other period of human history. In proof of this it is sufficient to name vaccination, auscultation, anæsthesia, and Bright's disease of the kidneys.

This disease has, doubtlessly, afflicted humanity in all ages, though its anatomical and pathological characters, whether pertaining to its stages of hyperæmia, hypertrophy, exudation, atrophy, or granular induration, never were ascertained, nor understood, until about a quarter of a century ago.

Bright's disease is not invariably an independent one—*ab initio*—so far as albuminuria serves to define it, but occurs consecutively to a few other diseases, particularly in general dropsy, although the latter is generally a sequence of the morbus Brightii. But, whether this disease be primary, or secondary, its diagnosis is highly important in a practical point of view. Although its anatomical characters, upon post mortem examination are striking, these, for curative ends, come too late. The differential diagnosis in the early stage is by no means easy. There is however, among others, a very simple test, requiring neither skill nor chemical apparatus, one easily applied in the fully developed disease, namely, that of heating the urine to the boiling point, by which the coagulation of the albumen, contained in the urine, is effected. Albuminuria, though occasionally found in other diseases, is highly characteristic of this, and has become a synonyme of the morbus Brightii.

In the case of a distinguished citizen of Mississippi, on whom Dr. Cartwright and myself attended, at the Arcade Hotel, for several weeks during the last spring, I found, on boiling, that about one-third of the urinary discharge consisted of albumen, which formed a white well defined coagulum, having the exact figure of the vessel which contained it, though the albuminous mass was not dense enough to bear removal without breaking into pieces. On being allowed to stand, it contracted toward the centre like the crassamentum of the blood. The appearance and the quantity of the urine deviated but little from the normal standard. The treatment adopted was followed

by a constantly decreasing quantity of albuminous matter, and an improvement of health wholly unexpected—a condition that did not continue long, as his death has been recently announced.

In the investigation of this disease, British pathologists have taken the lead, have pointed out the way, and have achieved important results; while, in this behalf, little has yet been done in America, particularly in the South.

It is believed that the following condensed articles on Bright's disease will prove interesting and useful to the readers of this Journal.—EDITOR.

Diseases of the Urinary Organs.—On Bright's Disease of the Kidney.

By DR. SAMUEL WILKS.

[Dr. Wilks' opinions are entitled to great consideration, as he commenced the study of this disease under those who labored with Bright himself.] He observes:

It may not be useless, in the first place, to enquire what is understood by the term Bright's disease. The answer to this question may be thought to give at once a solution to its pathological nature; but this is really not so. It must be remembered that Bright's name was given to designate certain cases of disease marked by certain symptoms and certain morbid conditions of the kidney; and, it must also be remembered that Bright included in his description a variety of forms of diseased organ, as well as a variety of symptoms. Among these were two very remarkable extreme conditions—the one, a kidney large and white, often double the natural size, and associated with a very considerable dropsy of the whole body; the other, a kidney hard and contracted, often only half the usual size, chronic in its character, and often destitute of symptoms. Bright's name, I say, belongs to one of these forms as much as the other, for he described both. No pathological theory, therefore, of Bright's disease can be admitted which does not include in its application both these forms of disease. If the two conditions be considered different, then more than one pathological theory is required. If all varieties are included under one pathological condition, as Frerichs writes, then the term Bright's disease becomes at once definite and scientific. If the conditions, however, are different, then Bright's disease has a mere nominal value, and has reference merely to a large class of cases which Bright described. In this general signification I think the term can alone be used. Thus, the expression is seen to have very little scientific bearing, and is nearly co-extensive with that of renal disease, so large a field of observation did Bright open.

Anatomical Characters of the Kidney in Bright's Disease.—[Dr. Wilks observes, that though his first two classes are characterized by the large white kidney and the small contracted one, yet these condi-

tions are merely to be regarded as the extreme conditions of two varieties of the affection]

The large white kidney is produced by inflammation of the secreting tubes and the exudation of an albuminous material, which, being constantly discharged and found in the urine, has been so fully described by Johnson and others. In a case which is fatal in a few days, as seen after scarlatina, often nothing is found in the kidney. If the case be more severe, or the malady has existed for a longer time, then the organ is found swollen, gorged with blood, and the cut surface often of a not uniform color, some parts inclined to be pale and others dark. If the case be still further advanced, evidence is gained of the essential pathological condition of the organ, an inflammation of the uriniferous tubes. The kidney is gorged with blood, some perhaps having transuded into the tube itself, and mixed with it is the peculiar inflammatory secretion. Beginning with a slight amount of exudation in the tubes, all degrees beyond this are found until we arrive at the extreme condition where the whole secreting structure is blocked up with it, and in some places destroyed by its coalescence in masses. The tubes become misshapen and distorted, the walls are thickened, and on their external surface and between them some amount of albuminous material is thrown out; but, while that in the interior is of a granular kind, that on the external is plastic or fibrous. In course of time the organ is much enlarged by the increase to the secreting part, the tubular portion is much diminished and encroached upon, and the organ has become exsanguine. Sometimes the veins may be filled with blood, and this gives the mottled or marbled look which some describe. The granular condition results from two different causes. The granules which some authors speak of are evidently little masses of deposit scattered all over the surface of the organ; while those, generally alluded to, are larger, and consist of bundles of tubes conglomerated together, and associated in this arrangement by the natural division of the veins on the surface.

Under the term *small contracted kidney*, I allude, as I before said, not only to the extreme condition found, but to all lesser stages of the degeneration: for example, a kidney may be found becoming granular, but not yet wasted. The onward progress of the disease results, however, in the organ becoming very small, hard, and contracted. By contraction, I mean the surface, instead of being smooth, is puckered, uneven, and often having indentations. It is rough, and covered with granulations, and the capsule generally tears the surface in the endeavor to remove it. The disease is seen by the naked eye to be one essentially of the secreting structure. This is wasted, and in some parts so narrow that the straight tubes nearly reach the surface. It is also wasted irregularly; so that the thickness varies, giving an uneven outline to the edge of the kidney. As a rule, no deposit is found in these cases; while a true degeneration of the secreting structure has taken place, and this generally by the addition of a large quantity of fibrous tissue. Here, I may state my surprise of the doubt which

some authors have expressed at the presence of a fibrous matrix in the kidney. Of this I should have thought the proof were easy in a healthy kidney, and of its development in the morbid organ there cannot be the slightest doubt. In many of the small kidneys a large quantity of the substance is fibrous tissue. A part of this no doubt is the remnant of shrunken and wasted tubes, but a part also is a new development. Besides the fibrous tissue, the microscope shows tubes of all shapes and sizes, much distorted, and many shrunken to mere threads. In some parts of such organs the function must altogether have ceased, as the tubes appear quite isolated from any healthy connections, are nearly full of granular matter, having lost their natural epithelium, and have undergone fatty degeneration. The Malpighian bodies may be also found wasted to less than half their usual size, and probably quite destitute of function. The granules on the surface are masses of tubes bound together by fibrous tissue, and thus marked out by the natural division of the veins. Thus, the organ often strongly resembles a cirrhosis of the liver, where a new fibrous structure, running in the course of the vessels, circumscribes masses of the secreting tissue. There seems no limit to the degree to which the kidney may degenerate. Together, we sometimes find them weighing only three ounces, and when from any reason one kidney is more advanced in disease than the other, it may waste until not a portion of healthy structure be left.

One of the most remarkable changes which this form of kidney disease undergoes is the *cystic degeneration*—a condition, according to the early essay of Simon, thought to be its natural termination. Some writers, however, as Frerichs, have almost ignored its existence, while Johnson appears to speak with doubt both as to its anatomy or even its reality. Indeed, I have failed to gather with precision the opinion of the latter writer concerning these cysts, as he speaks of their appearance as the result of an ocular fallacy dependent upon the section of tubes, and at the same time of their production from the dilatation of tubes.

Of the very common appearance of cysts in the kidney, I think there can be no doubt; I now speak of microscopic cysts, and not those which are plainly visible to the eye, and those are in proportion, as a rule, to the degeneration of the organ. I confess, for a long time, I accepted the theory of their formation as given by Simon, both because of the want of a better, and as it best explained the appearances, though the pathological theory of an exogenous growth of renal cells burst from their tubes was indeed not one to be readily received without proof. I had fully made up my mind, however, of their real existence before any doubt was thrown upon them. The palpable ocular delusion of mistaking tubes for cysts may with care be avoided. The faith I had in their existence has been fully confirmed by many further observations. In many particular specimens a doubt may exist whether a certain appearance may arise from an incised tube or a cyst, but an examination of various parts of the same organ can leave no

doubt. Occasionally I have met with a kidney where, from the circumstance of one being diseased in advance of the other, such a degeneration has occurred that nothing has been seen under the microscope but a mass of cysts, resembling very much in appearance a cluster of ova. These I have separated, detached, and rolled over in the field of the microscope, and have thus quite verified the fact of their real and independent existence. From this fact of being able to isolate them, and from their various forms and sizes, I formerly concurred in Simon's theory. Often can these cysts be traced from the ordinary dimensions of the renal cells to any size above. Since perusing Dr. Johnson's book, and his theory of their production from the walls of the tubes, I have not had an opportunity of examining many examples of the disease; but, that they are not formed from the whole calibre of the tubes is certain, from so many being smaller than the tubes themselves. If formed from these structures, it must be by small protrusions from the sides, which afterwards attain a separate existence. In favor of their formation from tubes, it must certainly be said that they are produced at the expense of the latter, for where they are formed there the true tube is wanting. They are also seen in a row, running in the direction of the tube, and in fact occupying its position. They are firmly adherent to the surrounding tissue, and with difficulty detach. They seem also to be lined by an epithelium very like that of the tubes—at least the larger are; the smaller cysts I have seen almost empty, and containing but a little granular matter. In a very degenerated kidney—not much above the size of a walnut—the whole organ was converted into cysts whose diameter was very uniform and a little larger than that of the tubes. They were merely collected into an aggregated mass.

It has been taken for granted that the larger cysts, found so often on the surface of the kidney, are of the same formation as the microscopic ones. Of this there can be no positive proof, and indeed I thought the contrary to be the fact for some time; for, after examining many specimens of morbid kidney, I found those with visible cysts on the surface had no smaller one detectible by the microscope, and, on the other hand, in the first few cases of the cystic degeneration I met with, there were no cysts visible to the eye. I thought it thus possible that the larger ones might be formed by a dilatation of the urinary tubes, while the smaller had a distinct formation. The contents of the vesicles, not being urinous, is no objection to the first named theory, when it is considered how soon a decomposition would take place. With reference to the formation from Malpighian capsules, I think I have seen some cysts undoubtedly produced in this way—at least I have found Malpighian bodies shrunken to half their usual size, only slightly connected with their tube, and evidently had lost their function, and so presented the appearance of isolated bodies.

That this cystic degeneration is the natural termination of the granular disease of the kidney, as Simon has hinted at, is not yet shown, though I believe it is a condition not uncommon, and generally

to be found in the most advanced cases. In fact, the more perfect degeneration the greater probability of the presence of cysts, and, as I before said, I refer to microscopic cysts. The larger and visible ones appear to depend in many cases upon other causes; as, for instance, lately I observed a kidney in a patient who had died of phthisis, scattered throughout with cysts, varying in size from that of a pin's head to that of a pea, and yet the intimate structure of the organ appeared quite healthy.

The *third form* of Bright's disease, which I call the coarse kidney, I have already referred to as being a subacute inflammation of the kidney, and having such an amount of deposit discoverable in the tubes as is attributable to the degree of morbid action present.

With regard to the *fourth form*, which I have mentioned, I am in doubt how far it resembles the morbid appearance mentioned by others. It is not altogether like the lardaceous kidney of Rokitansky, at least it is wanting in the transparent bacon-rind appearance of the liver, which is known by this name. A section of the kidney I speak of has the appearance of a dense piece of albumen, of a milky whiteness, and having no structure apparent or any masses of exudation. The kidneys are very large and dense, and the morbid deposit seems to have occurred quite at the expense of the secreting structure, the cortical part being enlarged, and the tubular wasted. The tissue is hard, with difficulty separated with needles for examination, and found to consist wholly of fibrous tissue, though in parts, by close examination, some few remaining urinary tubes may be found. As the ordinary white kidney of Bright's disease consists of an inflammation, with an exudation from the surface of the tubes, so here the inflammation seems to have been of a plastic character, an effusion having occurred amongst the tubes and bound them together. As one may be likened to a bronchitis, so the other may be considered in the light of a pneumonia where hepatization has occurred—the lung structure becomes infiltrated with albuminous material, and so converted into one mass in which all the natural tissue is lost to the eye. As in the small kidney of the second form of Bright's disease, a fibrous development often takes place, which is essentially chronic, and connected with the general wasting of the organ, so in these cases the same tissue seems more actively and rapidly produced, and forms in fact the principal disease. A nearer approach to this condition is found in the liver than in the lung—in fact, an exact counterpart of it is sometimes seen in cirrhosis, where the fibrous development of Glisson's capsule not only squeezes the tissues into little nodules, but sometimes destroys it so that no healthy tissue is left visible to the naked eye. Thus, particularly towards the surface, are seen occasionally in this disease of the liver, white masses of fibrous tissue of considerable dimensions, and in which only by the microscope can any remains of the normal structure be discerned. Apparently a similar change occurs in these forms of morbid kidney, and of a like chronic kind.

The same condition is found sometimes in the kidneys connected

with bladder disease, and where a chronic inflammation has taken place. Lately I examined the kidneys of a man who had died of stone. They presented an appearance very common in such cases. The pelvis was dilated, and full of muco-purulent fluid, the structure of the organ breaking down and in parts suppurating. A section showed some parts white others red. The former had a swollen appearance, and projected from the surface, and were advancing towards a state of suppuration. In the red parts the tubes could be made out, and were full of granular matter. The white portions to the eye were like pieces of hardened brain, and only on very careful dissection could any traces of structure be made out, the whole being in fact one solid mass of hardened lymph. Towards the surface the fibrous structure was disappearing, and suppuration was commencing. When carefully torn up, a piece presented nothing but fibrous tissue and fibre-cells in all stages of development. The nature of the inflammation, in this particular case, was shown by the condition of the capsule, which was closely adherent to the kidney and the parts around, very vascular, and three times its natural thickness and density. I allude to this case as showing the identity of the morbid process with the varieties of kidney I have just mentioned. In this case a plastic inflammation of the whole kidney had occurred, which had completely albumenised the structure, and the same had occurred in the organs I have described.

Morbid Anatomy of the Fatty Kidney.—The subject of fatty degeneration has of late very much occupied the attention pathologists, and the comparative novelty of its discovery has caused it to be looked for as a suitable explanation of many previously obscure conditions. Much valuable information has been gained by these investigations, but the full pathological interpretation of the condition has yet to be fully understood. It is sufficiently clear, that a fatty state of the body in general, and the viscera in particular, is not one of health. Age has a tendency to the production of fat and disease under particular circumstances. In my own note book, I have a great number of cases where this condition has presented itself, both in particular organs and in a general manner. Thus, as a rule, in all bed-ridden persons, particularly if advanced in life, fatty degeneration occurs; or where confined by some chronic disease, excepting always, of course, cases where a very rapid and exhausting malady is going on. Thus, it is mostly seen in long bed-ridden paralysed patients, in those long confined with scrofulous and cancerous affections. In the latter diseases, I have not time to discuss the question, why wasting at one time is produced, and at another a fatty degeneration; it is sufficient to remark, that this condition is constantly found. In patients who have suffered from any marked morbid state of the blood, it is particularly liable to happen, such as after perpetual fever or common fever. In two or three cases of sudden death, during convalescence from this latter disease, a fatty state of the heart has existed; one of the most marked causes, however, of fatty degeneration, is to be found in in-

temperance. This is mentioned by Bright in a former volume of these Reports, and seems to result from a constant imbibition of malt liquors rather than spirits. For this reason, in all those who have died from delirium tremens, induced by intemperate habits, I have found a fatty degeneration of organs. A fatty degeneration may be local, as in the liver of phthisis. I have alluded thus to the subject of fatty degeneration, because without its general consideration, that of the kidney cannot specially be understood, for I am sure that it has been by a too exclusive regard to one organ, that the true pathology of the particular case has been often misinterpreted. A fatty liver cannot be understood without a general reference to the body at large, much less be put down as a primary morbid condition; neither can a fatty heart be connected of necessity with sudden death, on account of the investigation of fatal cases where this has occurred, seeing that in the majority of cases where a fatty heart has been found, death has not occurred through the failure of that organ. In the same manner, a fatty kidney may indicate nothing of itself. When I speak of this condition, I use the term exclusively to designate those cases where the fatty state seems the only morbid condition. It may be found existing in diseased kidneys. Thus, the albuminous deposit in the large white kidney often undergoes fatty degeneration; so, again, we may find the small granular kidney very fatty. The state, however, I mean to refer to, is a simple fatty one, without any tendency to Bright's or other disease; indeed, the two conditions have nothing to do with each other. A fatty state may exist in an organ otherwise healthy, or in a diseased one. Bright's disease of the kidney may occur without the presence of any fat; and, on the other hand, a fatty state is often found, which can only be looked upon in the light of a degeneration. In certain subjects, particularly those under the conditions above mentioned, directly the body is opened, a general fatty state is recognised, both in the integuments and in the interior, about the intestines, &c.; in each case the viscera are found frequently to contain more than usual fat. I never found a kidney containing very much fat, whether otherwise diseased or not, without very visible traces of the same condition existing elsewhere. All this shows a general pathological cause for it, rather than a special one in the organ itself, and that when it occurs in a morbid kidney it is altogether secondary to the disease itself. It may be true that Bright's disease may indirectly be connected with a train of causes which have resulted in the fatty degeneration; but then other organs are seen to share the fat with the kidney. I do not speak of every slight or local change in a kidney where a tube here or there may contain some fat. This may arise from a degeneration confined to that part from a special cause. When I speak of true fatty kidney, I allude to an organ which is in an analogous condition to that of the liver, where we suppose that the mere superaddition of fat has produced the morbid state; in fact, an organ similar to the permanent state of that found in the dog or cat, where it presents an appearance healthy to the naked eye, but

yet containing an abundance of fat. So I have seen a remarkably fatty kidney in the human body pronounced healthy, because it was not otherwise structurally diseased. Thus, also, a very fatty liver may be overlooked, unless the fat be in such quantity as to alter perceptibly its color and consistence; and, speaking of this organ, as we look upon the fat so commonly found in the nutmeg liver or cirrhotic liver as a mere consequence or complication of a more serious affection, and the true fatty liver, where the organ only contains an excess of fat, so in an analogous manner do we speak of fatty kidney. In these cases, then, the cells are seen to contain many oil globules, and which sometimes burst, setting their contents free, which are found in drops throughout the tubes. In the large white kidney the fat is commonly found mixed with the exudation, as if this had undergone degeneration, and the separate granular corpuscles are found to have drops of oil within them. In the small degenerated kidney we often find the tubes very diseased, altered in shape, and in parts devoid of epithelium, and containing only granular matter and fat.—*Guy's Hospital Reports.*

On the Dropsy of Bright's Disease.

By DR. SAMUEL WILKS.

It was in the investigation of dropsy that Bright was first led to his discovery. As a symptom of renal disease, dropsy is found in a greater or less degree at some period, though not invariably so. In the first form of the disease, when the inflammation is going on, to the enlargement of the organ, dropsy is a uniform symptom. In bad cases it is more excessive than in any other malady, and much more general. This universal anasarca, or its existence in all parts of the body, constitutes the peculiarity of renal dropsy, and testifies as to its immediate dependence upon a general cause, that is, poverty of the blood. Thus we find among the first parts which become dropsical are the ankles, the face, with the conjunctiva, and the genital organs. Indeed, in the majority of instances, if we see the scrotum and penis included in a dropsical condition, we may safely conclude the presence of renal disease. The cause of dropsy, in many cases, as in obstruction to the flow of the blood through the chest in lung or heart disease, is simply mechanical, or in ascites from a cirrhosis of the liver; but in renal disease it depends upon the condition of the blood, and thus its universality, for a watery conjunctiva or œdematous scrotum could but rarely depend upon a local cause. This state of the blood is produced by two causes; first, by the scanty secretion of urine, by which much water is retained in the blood, and secondly, by the flowing away of its albumen, whereby also its specific gravity is much diminished. From the first cause a repletion of the vessels would occur, and, of necessity, a dropsy ensue. This would be in part mechanical, and allied to the other varieties of the affection. It is, however, from the poverty of the blood, arising from the drain of albumen, that the dropsy generally occurs, and for the same reasons as œdema of the leg is often found in chlorosis. Of the two causes

which operate to produce a watery state of the blood, a scanty secretion of fluid by the kidney, and a plentiful secretion of albumen by the same organ, I should say the latter was the more influential of the two. With a very scanty secretion two results follow, which are liable to produce a transudation through the vessels, a diminished specific gravity of the blood from the retained water, and an increased pressure from its superabundance: and yet I think the effect of these is less in the production of the dropsy than the discharge of albumen which is constantly taking place from the vital fluid. I think my experience shows that a highly albuminous urine, even with a considerable amount of fluid, is more likely to be accompanied by dropsy than where a less secretion occurs, and consequently less albumen is thrown off. Little, however, can be said on this point, as an accurate measurement of the albumen is difficult, and so many other cases operate to influence the result. Thus, if the urine be plentiful and its contained salts abundant, but albumen at the same time be taken from the blood, the specific gravity of the latter fluid is lowered, and a disposition to dropsy is produced: whereas, if the urinary secretion be scanty, or all but suppressed, generally a diarrhœa is set up, or some other vicarious action, which may relieve the blood, and so the immediate consequence in this respect is rarely seen. It is not common, however, to have dropsy to any amount in the chronic degeneration of the kidney, even if for days together the patient have his urine suppressed, and die from its toxæmic effect. On the other hand, dropsy is constantly seen to exist with a normal amount of urine secreted, leaving the abstraction of albumen from the blood as the sole cause of its production; remembering, however, always, a softened and relaxed state of the tissues, which may assist the transudation of fluid. It is clear, then, why, in the first form of inflammatory disease, with a scanty albuminous urine, excessive dropsy should exist, and why, in the second form of chronic degeneration, where a plentiful secretion exists with but a slightly albuminous state, none, or but occasional dropsy should be. In this latter form, if, from cold or other cause, a dropsy come on, the urinary secretion is at the same time seen to be much interfered with. We see, too, the reason why Bright, in the first instance, coupled dropsy with albuminous urine, and how it was subsequent investigation which discovered the diseased kidney where neither of these symptoms had existed, or at least been observed. That the dropsy is dependent immediately upon the watery condition of the blood, has been proved satisfactorily since the first analysis of Bostock, and it is thus made to be in a similar condition to that of chlorosis. The experience of the symptoms, and the beneficial result of the treatment by iron, all point to the similarity of the two conditions. There is one symptom, however, so remarkable in chlorosis, which is rarely found in Bright's disease, I mean the cardiac bruit de soufflet and the bruit de diable. I have constantly placed my ear to the heart of very anemic cases of Bright's disease, expecting to hear a soft, systolic murmur at the aorta, but have failed.—*Guy's Hospital Reports.*

On Bright's Disease.

By DR. H. BENGE JONES, F.R.S., Physician to St. George's Hospital.

[In this lecture Dr. Jones dwells upon the peculiarities of the urine in this disease, relating chiefly to prognosis and treatment.]

Congestion causes the diffusion of albumen, fibrin and blood globules, and a decrease of the specific gravity of the urine at the same time occurs, in consequence, probably, of the congestion hindering the escape of some substances out of the blood. By Bright's disease, precisely the same effect on the urine is produced. Between the urine passed by a patient after the cholera, or after scarlet fever, and the urine of a patient with Bright's disease, I see no difference whatever, either chemically or microscopically. The low specific gravity in Bright's disease is to me produced by exactly the same cause as the low specific gravity when the kidneys are inflamed after injury of the spine, or when they are congested in cases of chylous urine. Generally, the decrease of specific gravity may be taken as the index of the extent to which the kidney is effected; that is, the lower the specific gravity of the urine when albumen is present, the greater the alteration in the circulation through the kidney, the greater the congestion in chylous urine and after cholera; the greater the inflammation in spinal cases, and the greater the so-called degeneration (the effect of congestion and inflammation) in Bright's disease.

There are two opposite classes of cases of Bright's disease which require to be pointed out as exceptions to this law. In one class of cases there is considerable thirst, and very large quantities of urine are passed daily, and the specific gravity is consequently thereby reduced. In the opposite class of cases, the quantity of urine passed daily is exceedingly small, and hence the specific gravity rises sometimes very considerably above the healthy standard.

I shall illustrate both these classes by some examples.

A medical man was sent to me from the country, under the impression that he was suffering from diabetes as well as Bright's disease. He had no urine with him, and I asked him to come the next morning with the urine made at night. The following day he brought four quart bottles full, which he said was about the usual quantity passed in twelve hours. It contained no sugar; had a specific gravity of 1006, and was highly albuminous.

A woman, aged 43, who had œdema of the legs two months, was in the habit of passing three quarts of urine in twenty-four hours; the specific gravity usually was 1007 to 1010. She always complained of great thirst. On one occasion, the quantity of urine was three quarts in fourteen hours; the specific gravity was then 1004.

A farmer aged 50, was brought to me for diabetes. The urine was highly albuminous, contained no trace of sugar. The usual quantity was seven pints in twenty-four hours; the specific gravity was 1010. His only complaint was of head-ache.

Many similar cases I might bring before you; but these are sufficient to show that the quantity of urine must be taken into account before

any conclusions as to extent of the degeneration of the kidneys can be drawn from the specific gravity of the urine.

I pass on to the opposite exception, to the law that the specific gravity indicates the degree of disease of the kidney.

A woman, aged 38, had swelling of the legs for three months. She had a healthy, florid look. The dropsy was excessive. At one period of her illness, only three ounces of urine were secreted during one night; the specific gravity was 1036.2. It was intensely albuminous, loaded with urates, and contained many fibrous casts. For six weeks after her admission into the hospital, the quantity of urine was seldom more than half a pint in twenty-four hours; the specific gravity was from 1027 to 1033. In six weeks more, the urine became more plentiful, specific gravity 1017.4. She gradually became comatose and died.

A woman, aged 21, had swelling of the legs for twelve months. They were always most swelled when the catamenia were present. The face was flushed and full. In twenty-four hours half a pint of urine was made; it was exceedingly albuminous, loaded with urates, specific gravity 1035. She complained of sickness and drowsiness. By aperients and the vapor-bath, the urine became clear. The specific gravity fell to 1030, and the dropsy was lessened. After she left the hospital, I lost all knowledge of the progress of the disease.

Other cases like these I have seen, and from them I cannot but warn you from founding a favorable prognosis from urine of high specific gravity, even when urates are precipitated.

These exceptions lessen considerably the value of the knowledge to be derived from the specific gravity of albuminous urine; but by accurately observing the quantity of urine as well as its weight, you may approximately determine the amount of congestion in the kidney; and, if this be frequently determined, the existence and degree of the disease of the kidney may be foretold.

Having spoken of the quantity and specific gravity of the urine in Bright's disease, I will pass on to the quantity of albumen in the urine.

In my last lecture on chylous urine, I showed you how rapidly the urine may vary in the quantity of albumen which it contains; and in this, as well as in at least one other disease, (*mollities ossium*,) large quantities of albuminous substance may be thrown out by the urine without the existence of any serious disease of the kidney. The quantity of albumen is a test of the amount of congestion of the kidney at that time; and if at all times a large quantity is found in the urine, it indicates that the kidney is always highly congested, and it becomes most probable that this is produced by organic changes in the texture of the kidney. Thus the amount of albumen in urine is often considered as bearing a direct relation to the amount of degeneration in the kidney. If this were so, then little albumen in the urine would, on the other hand, indicate slight affection of the kidney; whereas there may be the most extensive disease of the kidney, and some cause may be in action which temporarily prevents the congestion of the kidney (as, for example, some determination of blood to

some other part of the body;) then the amount of albumen in the urine may be very much diminished, and sometimes none at all may be detectible.

A laborer, 55 years old, was admitted on the eighth day, of acute pleurisy of the left side. The heart was displaced, the cavity of the chest being full. There was a history of œdema some months previously, and as his heart was healthy, the urine was carefully examined, but no trace of albumen could be found. The effusion increased; the day but one after his admission the urine was again examined; it was quite free from albumen; specific gravity 1025; it was not nearly so deep colored as it usually is found to be in cases of pleurisy. A week after his admission the difficulty of breathing increased; he had an epileptic fit; the urine was still not deep colored, contained a little urate of ammonia; specific gravity 1022; it gave a precipitate of earthy phosphates by heat, nearly clearing with a drop of dilute hydrochloric acid; nitric acid alone gave a slight cloudiness. On account of the difficulty of breathing, paracentesis was performed; eighteen ounces of purulent fluid were drawn off. Shortly after the operation, the breathing was free; two hours after, the pulse was irregular and unequal, and the difficulty of breathing was nearly as bad as before. During the night he had three attacks of convulsions; in the last he died.

Feb. 9, 1847.—On examination, both kidneys were highly granular.

Sometimes the albumen is absent only at the commencement of an acute inflammation of another part.

A youth, aged 16, was in St. Georges Hospital, under Dr. Chambers. He had some œdema and much fever, with rigors. In a day or two he complained of excessive pain in the left side; the urine did not coagulate with heat alone, and very slightly with nitric acid. In four days the heart was pushed to the right of the sternum; the fever was diminished; the urine was more coagulable by heat and nitric acid; one specimen had specific gravity 1015, another 1010. At the end of another fortnight the urine remained still coagulable by heat and acid. On account of the difficulty of breathing, paracentesis was performed, and about four ounces of pus were drawn off from the chest; the difficulty of breathing increased, and he died forty-eight hours after the operation.

When the capsules of both kidneys were removed, the surface appeared quite smooth; the vessels were injected; the substance looked more white than natural. On section being made, there was an increase of cortical structure, which was much more mottled than natural.

These, and many such cases, prove that the quantity of albumen does not indicate the amount of disease in the kidney, but it does show the amount of congestion at the time of the observation. A highly diseased kidney may be very slightly, or not at all congested, and a highly congested kidney may be very slightly diseased. The quantity of albumen may be very small in the first case, and very great in the last.

The general rule that the quantity of albumen is proportioned to the state of the congestion of the kidney, is of great importance for treatment and prognosis; and the exceptional cases mentioned above only confirm the general rule, that the quantity of albumen indicates the degree of congestion, and not the degree of degeneration. Such cases are also instructive, because they show the necessity for making more than one or two examinations for albumen before it is concluded that no disease of the kidneys exist.

In the first of the two last cases, I have mentioned the pale color of the urine at the time when acute inflammation of the pleura was present. This paleness of color may be very frequently observed when blood is not present to discolor the urine. When the kidneys are healthy, acute inflammation causes the urine to become deeply colored, almost proportionately to the intensity of the inflammation; but when the kidneys are diseased this proportion ceases. In Bright's disease the most intense inflammation of a serous membrane may exist, and yet the urine throughout may show no alteration of color or appearance.

Among many examples of this, perhaps one of the most striking that I have seen was in a medical man, who for many months had pale, straw-colored urine, highly albuminous, specific gravity about 1010. He was attacked by acute pericarditis, with delirium and loss of sleep. His symptoms exactly resembled those which occasionally occur in the pericarditis of rheumatic fever. He died about the tenth day of the inflammation; the urine throughout presented no change whatever in color or specific gravity; it had no resemblance to the urine of acute inflammation.

Such, then, are some of the facts which may be derived from observation of the specific gravity, the coagulability and the color of the urine, in cases of disease of the kidney. You will see from this, that chemistry cannot tell the exact state in which the kidney will be found after death, but it does give much help in determining the state of the circulation through the kidney during life.

That which chemistry is unable to effect, the microscope professes to make clear. According to the microscopic appearances of the urine, the appearances observed in the kidney after death have been subdivided into many different diseases; and instead of a single disease in different states of congestion and stages of deposit, we are asked to recognise as many different diseases as there are different microscopic appearances in the urine. Thus, in urine may be found: 1st, tube-casts, with recent gland cells; 2dly, waxy tube-casts; 3dly, fatty tube-casts; 4thly, casts with fibrin; 5thly, casts with pus or blood. These are said to be the distinctive signs of nearly as many distinct diseases. First, of acute or chronic disquamative nephritis. The second disease is, waxy degeneration of the kidney. The third, fatty degeneration; and this is divided into two perfectly distinct diseases—granular fatty degeneration, and mottled fatty degeneration. The fourth disease, corresponding to the fourth microscopic appearance, is

acute and chronic non-desquamative nephritis. The fifth disease is, suppurative nephritis. This subdivision can scarcely fail to remind you of the proposed subdivisions of some cutaneous diseases founded on the appearance of the eruption. There is no practical gain in subdividing scabies or erysipelas into different diseases, according to the appearances of the eruption. What, think you, was gained by assuming as many different syphilitic poisons as there were different forms of secondary syphilitic eruption? I shall keep to the simpler view, which appears to me to be truer, simpler and more practical—a single disease, in different states and stages of congestion, deposit and wasting, not arising from the elimination of any peculiar animal poison, but produced by a diseased state of nutrition, leading to the deposit of fibrin, fat and pus, and ultimately to the wasting of the structure of the organ.

Such are the broadest outlines of the different views of Bright's disease. There are, at present, four cases of this disease under my care in the hospital. You will see how little assistance in regard to their treatment, can be derived from the microscopic examination of the urine. Nevertheless, with the hope of arriving at more certain knowledge, the microscopic appearances of the urine should be carefully observed. You may thus help to determine whether there is a vital process of desquamation; a rapid formation and shedding of renal gland-cells; whether the so-called large granular fat kidney and the small contracted kidney are the result of two essentially different morbid processes; whether the epithelium in this last state is disintegrated and swept away, and in the former state transformed into fat; whether the smooth, mottled, fatty kidney, or whether the deposit of fat is not unessential in both states of kidney, appearing as pus, may appear in any stage or state of the disease.

Those of you who may have the opportunity of seeing much of scarlet fever will, by careful microscopic examination of the urine, be able to add importantly to our knowledge. The late Dr. Miller, than whom I know no one who had so carefully watched the sequelæ of scarlet fever, and from whom, if his life had been spared, more valuable knowledge would have been obtained, told me that he had seen in the urine every variety of microscopic appearance attributed to Bright's disease, and that in *post-mortem* examinations no distinction could be made, for all forms of deposit were to be found after scarlet fever. The microscopical questions regarding the state of the urine after scarlet fever will repay the minutest and most extended research. Whatever you observe note it carefully, and publish it slowly, for premature publication leads only to corrections and ends in disputes.

You will say, what is to guide me in my prognosis and treatment? I reply, do not trust alone to the microscopic appearances in the urine, but take the case as a whole. In a case of consumption, it is rarely that you can determine by the stethoscope alone the course which the disease will follow, or the best treatment to be adopted. The stethoscope may give most important assistance, but the history, the

general symptoms, the special circumstances, will still more correctly determine your judgment as to the duration of the case and the most suitable treatment. So, also, in renal diseases. The microscopic examination may give most valuable knowledge regarding the state of the kidney, *e.g.*, the pus may indicate suppurative inflammation; the blood, a loaded state of vessels; the fibrinous casts, the degree of recent congestion; the fatty matter, the duration, perhaps, of the evil; but it is far more important to take all the features of the case than to make the microscope the sole foundation for your prognosis and treatment.

In albuminuria, as in other diseases, take the history first. If you can trace the complaint to scarlet fever, to sudden cold, or to pregnancy, the chance of recovery is far more favorable than if the disease has insensibly approached. I could give you many instances of recovery, where the disease commenced from such causes; but I know of no perfect recoveries, where a bad state of health has given rise to disease of the kidney as a secondary consequence of a previous cachectic state.—*Med. Times and Gazette.*—*Braithwaite's Retrospect.*

Part Third.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

- Rev. I.—1. *A Treatise on Venereal Diseases*: By A. VIDAL (*De Cassis*) Surgeon of the Venereal Hospital of Paris; Author of the *Traité de Pathologie externe et de Médecine Opératoire, &c. &c.*, with colored plates; translated and edited by GEO. C. BLACKMAN, M. D., Fellow of the Royal Medical and Chirurgical Society of London; formerly one of the Physicians to the Eastern and Northern Dispensaries, New York. New York: Samuel S. & William Wood, 261 Pearl street. 1854. 8vo. pp. 499.
- 2.—*Letters on Syphilis*: By PH. RICORD, Chirurgien de l'Hôpital du Midi, &c. &c.; translated by D. D. Slade, member of the Massachusetts Medical Society, &c.; formerly House Surgeon at the Massachusetts General Hospital, &c. Boston: David Clapp. 1853. Pp. 404.
- 3.—*The Modern Treatment of Syphilitic Diseases, both Primary and Secondary*: comprising the Treatment of Constitutional and Confirmed Syphilis, by a safe and successful method; with numerous Cases, Formulæ, and Clinical Observations: By LANGSTON PARKER, Surgeon to the Queen's Hospital, Birmingham. From the third and entirely re-written London edition. Philadelphia: Blanchard & Lea. 1854. 8vo. pp. 311.

It is M. Guizot, the statesman, historian and *savant*, who says that all the great principles of civilization either originate in France, or necessarily receive the sanction of that country before they are adopted as genuine by other nations; that is to say, France is the source and centre of civilization. Whether this be indisputable or not, there can be no doubt that Paris is the centre of syphilization, syphilism, syphilography, syphilographers, syphilizators, syphilitic inoculators and experimenters. Since the days of Hunter, syphilitic literature has flourished most in that capital, Ricord and Vidal having become its chief oracles, as will more fully appear in the sequel.

Declamation, rhetoric, moral suasion, and even the decalogue, have hitherto proved unavailing, as preventives of the venereal disease. *What ought to be*, is not so much within the scope of physio-

logical research as what *is*. The eternal conflicts and contrasts between *ought* and *is*, have characterized the great and the small—the salient and the secret transactions of humanity ever since

"Man's first disobedience, and the fruit
Of that forbidden tree, whose mortal taste
Brought death into the world, and all our woe."

The moral phases of humanity are legitimate objects of medical research, in so far as they influence health, longevity and the physical well-being of society.

There is not in the whole nosology a disease in which society is more interested than that under consideration. If anatomy, physiology, and therapeutics, are to be popularized and taught in the elementary schools and from books, as too many medical men recommend, the venereal disease, replete as it is with whatsoever is repulsive to delicacy, should claim the preference for many weighty reasons. Parents and children, matrons and the intending brides are deeply interested in knowing the dangers of a contaminating and often an almost incurable malady, which is transmissible to children, as well as ruinous to the health of parents.

It is, doubtlessly, very shocking to modesty to place a manual on the venereal disease, in the parlor or boudoir; but not more so, however, than the usual practice of receiving and reading the daily journals, which contain numerous quack advertisements, not describing the dangers and incurability of venereal diseases, but containing false accounts of infallible cures, &c. Is the universality of this pollution due to the press more than to the public? Does the press represent, rather than create—reflect rather than inaugurate—public opinion, both in ethics and æsthetics? Is not the press inclined to follow rather than lead? Be the cause with the press or the public, no one can deny that newspaper readers, that is to say, all civilized people—from the virtuous Victoria Regina, the head of the Church, Franklin Pierce, the head of the Republic; from the Right Reverend Father in God, the Lord Arch-Bishop of Canterbury, to the fashionable world; from the saintly matrons and the pious virgins who kneel at the sacramental altar, to servant maids in the kitchen; from the fashionable saloons to the log-cabin—all receive

and read journals graced with clap, pox, gleet, stricture, impotency and secret diseases, ad infinitum.*

The fashionable world, however, is not always consistent in its decencies, tastes, æsthetics and codes. It would taboo and utterly exclude from its pale, not so much the man who has seduced half a dozen of virgins, and proved false to as many concubines—white, yellow and black—as he who would dare to utter a vulgar word—such as shirt, shift, belly, thigh or stocking; stranguary, lax or costiveness, menstruation, pregnancy, abortion, stricture or impotency, clap, pox or flatulency, buttock, anus, vulva or louse.

How much soever such modesty and tabooing may be commendable, there is a higher and purer standard attainable, namely that which would guard not only against vulgar words, but vulgar and ruinous actions; which would lead parents to guard against the bestowal of their daughters to infected and infecting husbands; which would restrain the infected from fraudulently entering into matrimonial connections; which would persuade young men—on the principle of self-interest, including public and private hygiene, if not from any higher motives—to avoid the dangers which await their constitution, not to mention their consciences and the body politic, from syphilization, since with few exceptions, (if reliance can be placed in not a few great authorities,) a thorough syphilitic contamination must disqualify the party for honorable matrimony—unless it be lawful, honest, and praiseworthy, to poison wife and children for life! At least honorable men would not venture to marry until they had passed through the most complete therapeutic purification, fortified by the lustration of years of health and the assurance of the phy-

* *Disgusting Medical Advertisements*—A society has been formed in London and Manchester, called "the Union for Discouragement of Vicious Advertisements," which, by the circulation of tracts, calls upon the public to set their faces against papers admitting such advertisements as "Manhood," "the Silent Friend," "Nervous Debility," &c. We heartily wish the society may succeed in excluding advertisements of the kind in question, and in putting down all papers that insert them. We should feel surprised that the proprietors of any newspapers could be found sufficiently vile to publish such offensive, indecent announcements, but that we know that no work is too dirty or disreputable for some people to do; and that life is clung to so tenaciously, that a continued existence in a fœtid atmosphere of moral corruption is preferred, by some degraded specimens of humanity, to decent death and burial.—*Boston Med. and Surg. Jour.*, for August, 1854.

sician that the cure is complete; though some of the most eminent writers say that this never occurs, the disease being incurable; as will be seen in the sequel.

Why should not constitutional syphilis be a valid cause of divorce as well as impotency? or adultery? or willful desertion? or fistulæ? or cancer of the genitalia? Is not the contamination of a wife a greater evil than any of these, visiting as it probably does "the children to the third and fourth generation."

The pox and the clap are not confined to the poor and the vulgar; they flourish equally in the rich and the fashionable world. Beardless boys, not yet in their teens—the sons of merchants and others—have been led by sorrowing fathers into the writer's office, infected with both of these diseases in the same person at once, while neither parent nor child seemed to be aware of the Iliad of woes which hung over his future, from constitutional syphilization. This, however, is not one of the cases to which the maxim of the poet refers, when he says—

"O, blindness to the future! kindly given;"

for the future must be anticipated, and its evils must be averted by timely medication, from month to month, perhaps, from year to year.

A knowledge of the dangers of the constitutional infection, of the insidious, masked, relapsing character of that infection, after an apparent cure—a knowledge of the permanence of the syphilitic diathesis, (as many affirm,) and its transmissibility under an almost infinity of phases, would do much by way of prevention; more, indeed, it may be, than M'me. Guyon's doctrine of the disinterested love of God—which Fenelon advocated; more, because penalties possess a potency which conscience does not, with many individuals. Now were it known, that some of the ablest writers on syphilis declare, most positively, that the syphilitic diathesis is incurable; and, that where it does not destroy life it poisons its fountains, few would incur its risks.

What physician ever saw a syphilitic patient who had the remotest idea that his disease was incurable? Indeed, the latter not only expects a perfect cure, but often demands to know the period; that is, how many days will be required to effect a cure;

and, if the physician will not agree to guarantee that happy result, he turns away indignantly and seeks one who will.

It may be allowable to pause a moment, in this place, in order to give one or more authorities on this point :

In the British and Foreign Medico-Chirurgical Review, October, 1852, Mr. Erasmus Wilson's book on syphilis is quoted as asserting as a fundamental proposition, "that the poison of syphilis being once introduced into the blood, it remains there certainly for the rest of existence; and that this poison infects both the wife and the children during life; that though masked during life, it reappears after the lapse of several generations—showing itself as scrofula, lupus, kelis, lepra and psoriasis; that a man perfectly free from primary symptoms communicates syphilis to his newly married wife."

"Some very distinguished practitioners," says M. Vidal, "maintain that confirmed syphilis is incurable."

Prof. Erichsen, of the University of London, in his new work on surgery, admits that both primary and secondary syphilis may exist simultancously in the same person. "The period at which constitutional syphilis declares itself usually varies, from six weeks to six months after the occurrence of the local disease. Occasionally it shows itself earlier, about the second or third week, when it may be coincident with the existence of the primary syphilis; in other cases again, it does not manifest itself for years after the primary disease."

Prof. Erichsen says, "when syphilis has become constitutional, it is extremely difficult to say when it can be eradicated from the system; certain it is, that if neglected or improperly treated, it will affect the system for an indefinite time, declaring its existence by exciting and modifying local inflammations years after the original absorption of the poison. Daily experience tends to show that in many constitutions syphilis cannot be eradicated; and, that in most others when once it has occurred, it is apt, even when apparently cured, to modify certain cutaneous and other affections in a remarkable manner, after the lapse of many years; showing clearly, that if the poison no longer exist in the system, the constitution has received a peculiar impress from it, which it is long in losing."

"It is remarkable," says this same author, "for how long a time

the syphilitic poison will continue dormant in the constitution without producing local manifestations of its existence, until this is developed under the influence of a broken state of health. I have at present under my care, an extremely severe case of constitutional syphilis, in which twelve years have elapsed since the occurrence of the primary disease, during the whole of which time no secondary affections occurred until the patient's health gave way from other causes." He mentions a similar case in which five years passed before the secondary affection took place.—424.

Now, if manuals and tracts were written in a popularized style by honest and competent persons, revealing "the truth, the whole truth, and nothing but the truth," this malady would be one of the best beginnings for the non-professional public, provided the people are to become educated or indoctrinated into the science of medicine. The indecency of syphilographic literature, it has been seen, can be but little objected to, judging from the syphilitic advertisements daily accepted, while good manuals might prevent, to a great extent, the evils of syphilization, by a dread of its deleterious consequences, truly stated by reliable persons. Anti-venereal tract societies and associations might thus find employment not less useful than that of advancing dogmas and abstractions of a sectarian character. To save the constitutions as well as the consciences, thereby preventing disease and crime, is a mission worthy of the benevolent—a platform altogether catholic.

A sanitary diploma from a reliable family physician, certifying that the party intending matrimony, is free from syphilitic diseases, is of higher import to the intending bride, than any degree that the University can grant—whether it be M.D., LL.D., or D.D.

But, in all seriousness, manuals of physiology and of syphilis, anti-venereal societies, tracts and diplomas will be tabooed; and the eternal contrast between what *ought to be* and what *is*, must be dealt with as a finality, without reference to sentimentalism.

The syphilized *blasé* will, in spite of tracts, at length fall back upon matrimony; the fairest, the purest, and the best, will often accept him "for better, for worse"—sound or unsound.—The wife, after a time, needs a doctor; skin, mouth, throat, eyes, ears, nose, head, glands, bones suffer—children inherit these; and the physician

will sometimes hear what no one else will; what father, mother, brothers and sisters, will never hear; what the gay world and former companions never will know—pains and griefs ineffable. Dose follows dose—symptom, symptom. The husband often equally suffers, physically and morally. Before he ventured upon matrimony his physician had assured him that he was cured; that the contamination of a wife need not be feared, and in the confident belief of all this, he married * * *. The sequel—veiled to the public, often the physician—is not for that reason less *triste*.

But as real griefs and sufferings are seldom so interesting as those of fiction, this part of the subject will be passed over, in order to notice another, before entering upon the chief topic of this paper. Mr. Ricord, in the work named above, enumerates many unexpected circumstances by which the venereal disease may be contracted, as he affirms most positively from his vast experience: among which, the following are the chief:

M. Ricord says that, in drinking out of a glass, or eating with a spoon that has been used by the infected, the disease may be communicated; also by the lancets of a scarificator; by leech-bites. M. Ricord says infection has taken place from pots, water-closets, tobacco-pipes, &c. “In the case of a financier, thirty leech-bites became as many chancres; which, afterwards took on the serpiginous form. The primary affection cost ten thousand francs; the cure was not dear, although the treatment lasted more than six months.”—Letter xxvii.

If syphilis be thus easily contracted, people cannot be too careful. The putting of coins in the mouth; omnibus tickets in the lips; the washing in everybody's wash-bowl; the wiping on a towel after others, &c., “should give us pause.”

M. Ricord charges the nurses in Paris with infecting many infants; he says that they contract syphilis whilst they are nursing, and generally by the genital organs. While in this condition they infect the child by means of their fingers, contaminated by the virus; then the evil is always referred to the *rotten* Parisian children.

One quite common mode by which inoculation is effected in the cases of nurses, is the conveyance of the virus by their fingers, soiled with the secretion from the parts affected, to the nipple. They pull

up the nipple, which is more or less cracked, and implant there a chancre. Thus the infants become inoculated. Again, the individual who may draw off their milk by suction, may have a chancre upon the lip, and thus communicate the disease.

An infant at the time of parturition may contract chancres from the mother, and then communicate them to the nurse. Or the child may become affected by strangers, upon whom no suspicion falls; a curious example of which fact our author gives. In all cases, with care and perseverance, we can discover the source of the syphilitic symptoms. At any rate, we see how much reserve and prudence is necessary.

M. Ricord recommends that nurses should be carefully examined, in order to ascertain whether they have chancres on the neck of the womb, &c., before entering upon their duties.

One of the most exceptionable doctrines advocated by M. Ricord is that of voluntary inoculation, not that he urges it upon the well, but upon those already syphilized! As to the prophylactic power of repeated inoculations, this must be worse than useless, seeing that upon his theory syphilis never can be taken more than once! It may be satisfactory to the experimenter to test the genuineness and virulence of the existing disease, by multiplying the number of the chancres by repeated inoculations, but there seems to be but little amusement or use in so doing. Great men may have odd notions!

M. Ricord says (letter xv.) that he assisted at the inoculation of M. Robert de Welz, an adjunct professor; the pus taken from the chancre of an inoculated monkey, produced the disease in the gentleman named. This is probably the same individual alluded to by Prof. Flint, in his recent letter, mentioned in another page of this article. This assistant professor remains to this day uncured and incurable, in consequence of the above experiment!

Surely, sanitary police, legal medicine, the common law, the Justinian Pandectæ, the Napoleon Code, or statutes against felonies, ought to prescribe summary punishment wherewith to restrain such an offence as that of willful syphilization, being far more criminal than the marrying of any of the issue of her present Majesty under the age of eighteen, without the consent of Parliament, which marrying is treason and punishable with death! The law punishes with death all aiding in such a marriage; and, yet, it does not punish those who give others by inoculation, or matrimonial intercourse,

delapidated noses, rotten throats, cutaneous diseases, ulcers, carious bones—not to name a pair of horns, which one of the writer's venereal patients was blessed with for months.

Self-inoculation, and, still more, the willful inoculation of others, should be prohibited by law, under severe pains and penalties, one of which should be incompetence to contract matrimony; or, if the party be married, voluntary syphilization, should—*ipso facto*—render void the marriage, thereby saving the innocent woman and her children from a poisoning which M. Ricord himself has known to continue thirty years, not to name the residue of life, which may be thirty years to come, should the disease fail to prove fatal.

The Western Journal of Medicine and Surgery, for July, 1854, contains a letter from Prof. A. Flint, just received, dated at Paris, April 19th, in which it is stated that “artificial syphilization, as a mode of protecting the system against the syphilitic virus, was agitated here a few months since. The subject is now only referred to as one of the curious by-gone medical novelties that have produced a transient excitement. The practice was condemned by the Academy of Medicine, and interdicted by the Prefect of the Seine. A young prosector of anatomy, who had submitted to experimental inoculations for syphilis, is now laboring under constitutional effects of the disease thus induced, which resists medical treatment, and will be likely to ruin his health permanently.”

M. Ricord speaks of *his* school, and of the school opposed to him. As it regards a number of his peculiar doctrines, it appears, that but very few of his medical compatriots accept them unconditionally. His great fame throughout the world, particularly in America, gives to his opinions an importance which perhaps no other syphilographer enjoys. The many who rely upon the authority of a great name will regard his decisions as final. It is fortunate, however, that M. Vidal and many other of the school opposed to M. Ricord, do not accept his dicta as conclusive, and appeal to incontestable facts and reasonings, which nullify many of his fundamental views, as set forth in his latest and therefore most authoritative work, that is to say, his Letters on Syphilis, as will more fully appear in the present examination.

M. Ricord regards the following proposition as one that cannot be overturned, namely; “The chancre (primary ulcer,) at the period of

progress or the specific *statu quo*, is the only source of the syphilitic virus, ("morbid inoculable poison," 152.) He asserts that syphilization can happen but once; that there is but one syphilitic poison; that the cure of syphilis once, after it has been introduced by inoculation or otherwise, leaves the party no longer susceptible to future attacks, and that inoculation is prophylactic!

M. Vidal, on the contrary, maintains that the virus of the venereal disease is of two-fold character—a duality: namely, the virus of blennorrhagia (clap,) and the virus of chancre; and that these primary poisons, both produce consecutive or constitutional infection; and that in both of these constitutional infections or secondary conditions are contagious, or communicable and transmissible to the children.

"Patients may contract a new chancre," says M. Vidal; "a fact doubted by no one; they may afterwards have consecutive accidents, symptoms of syphilis which every one admits, but with a different explanation: thus, the *unicists* do not attribute the last accidents to the last chancre, but to the first: that is to say, to the chancre the most remote. Clinical facts, in unison with experiments, do not permit the least doubt as to the possibility of one person having several attacks of the disease. The inoculation performed by M. Bouley, physician of the *Lourcine*, produced a second syphilitic affection in a woman laboring under genuine tertiary symptoms. We no longer place any value on syphilitic vaccination (inoculation;) in other words, that the pox will not be given to patients, to prevent the pox: an individual may have repeated attacks."—291-2.

The writer of this paper begs leave to refer to a recent case, illustrative of this important subject:—Mr. * * *, after a long treatment for syphilis by some of the most eminent physicians in Europe, considering himself well, married, more than twenty years ago: his children have all suffered from secondary syphilis—particularly from throat diseases; in the mean time, he has suffered but little himself until about two years ago, when the writer was called to treat him for a recent well-defined chancre; which, in spite of every effort to destroy it, was followed by constitutional syphilis—as sore throat, cutaneous eruptions, an ulcer on the leg, &c.—just as in the first syphilis. This man acknowledged that he had recently wandered

from home, but was not sure whether he had incurred the new infection from white or black females. The wife had suffered from secondary syphilis, for nearly a quarter of a century: but her narration was so interrupted by cryings and sobbings, that a connected history could not be obtained. Other cases of syphilis after syphilis; cases of the contagiousness and transmissibility of secondary syphilis might be here enumerated, but most practitioners have doubtlessly often witnessed similar examples. Here the testimony of the family physician is likely to be more satisfactory than that derived from hospitals. The former has the family under his eye for life; the hospital physician soon loses sight of his patients.

M. Vidal says, "To justify prophylactic syphilization, we must first establish the unity of the syphilitic virus, and the necessity of subjecting the organism to this saturation; it is necessary, in fine, that the analogy between syphilis and variola should be complete.

* * * We find the experimental and logical proof, the possibility of several attacks of constitutional syphilis"—47. Blennorrhagia (clap) without chancre, produces constitutional syphilization, &c., "being capable of giving rise to consecutive syphilitic accidents"—64-5; "pus, secreted by the glands and prepuce (in balanitis,) which is (are) not ulcerated or even excoriated, may produce chancres. Now, in this case, even the pretext of the *chancre larvé* cannot be invoked, for it is one of external blennorrhagia"—54-5-6.

Again, M. V., says, "Balano-preputial blennorrhagia, in its ulcerated and non-ulcerated forms may be contagious, inoculable, and the cause of consecutive accidents, in the same manner as certain urethral blennorrhagias; there is this difference, however, viz: that matters here are more easily detected, since it is an external blennorrhagia, a disease which is completely under our observation." The translator, Dr. Blackman, quotes from Mr. L. Parker, "who states, that in this form of disease, he has seen eruptions, accompanied by a node on the forehead, loss of the hair, and other symptoms of constitutional syphilis produced in the wife, where this species of abrasion with thickening, were the only symptoms in the husband."—170.

M. Ricord's classification of syphilis, into primary, secondary, and tertiary, is utterly rejected as fanciful by M. Vidal, who substitutes one which he divides into primitive and consecutive.

Constitutional or secondary syphilis, says M. Ricord, never occurs if the chancre be destroyed before the fifth day; that inoculation with the matter of clap is negative, not being inoculable; and that constitutional symptoms always take place within six months after the infection from chancre. These dicta of M. Ricord are opposed by a weight of authority, observation, and experience, which, according to M. Vidal, cannot be gainsaid.—47-8, 290 to 293.

M. Ricord asserts, that a chancre once cicatrized, never returns; a new chancre is from a new infection. How can this be possible, at least as regards an indurated chancre, which, according to him, can happen but once in a life time?

M. Ricord's criterion for chancre, is the inoculability of its pus! This, even in France, will rarely be tried, with a view to settle the diagnosis. He denies, in the most absolute manner, the existence of what has been called *bubo d'emblée* or primary bubo, without antecedent, symptoms or chancre.

M. Ricord maintains that indurated chancre alone, which never happens but once, always produces constitutional syphilis, which, however, M. Vidal emphatically denies. The latter maintains that, while all chancres are more or less indurated, the induration is not the cause or antecedent, but the effect or proof, and even may be the sole evidence of constitutional infection.—206.

M. Ricord contends, that even the bubo of indurated chancre never yields inoculable pus; never produces constitutional symptoms. That there can be no constitutional syphilis, without previous indurated chancre, open or masked, (*chancre larvé.*) He maintains with great pertinacity, where constitutional infection occurs without having been preceded by visible chancre, that in the genital passages or elsewhere, concealed chancre (*chancre larvé*) must have existed; that chancre may have been overlooked on the surface of the body—may be communicated by the fingers of nurses, to which the virus adheres, and by which the nipple is inoculated and the child as well: all of which must occur from the pus of indurated chancre alone; thereby exonerating simple, inflammatory, gangrenous, and phagedenic, non-indurated chancres of all inoculable or communicable properties.

This is most incomprehensible upon M. Ricord's theory, of one sole venereal poison, he being a strict "*unicist.*" It is difficult to

understand how the unitary virus should produce so many kinds of chancres which yield no virus at all, the indurated alone excepted, though the phagedenic is the most virulent and dangerous form. The perfect harmlessness of simple, inflammatory, gangrenous, and phagedenic chancres, is a proposition doubtful as it is dangerous, and should never be allowed to arise into credit, nor be adopted in practice. The mælstrom of syphilitic phagedæna is not a safe place!

“What connection,” asks M. Vidal, “exists between the indurated and the constitutional disease? Certain it is, that the general state of the system modifies the form of the chancre; and wherever we find an action which can be called *syphilitic*, the reaction must be greater. For example, when the chancre is obstinate, when, from being moderately it becomes much indurated and seems to constitute a separate variety, I believe that the syphilitic virus has then for some time already infected the system. The cause of the constitutional disease, therefore, exists; and the system is already affected. This degree of induration proves only that the diathesis is established: for the indurated chancre is really a consecutive accident, an expression of confirmed pox. But the effect of the diathesis may be confined to this point.”—206.

While M. Ricord, with infinite pains attempts to show, but with no great success, how concealed chancre (*chancre larvé*) is at the bottom of all the constitutional affections not comprehended in visible chancre, M. Vidal wholly dissents from this assumption; and holds that, in the female, the speculum enables the practitioner to decide whether chancre exists or not. He says, “as I do not believe in the deep-seated chancre, and admit only that of the meatus and the fossa navicularis, my mind is less prepossessed with the difficulties of diagnosis.”—69.

M. Ricord says, that an experience of twenty years authorizes him to “advance with certainty the following propositions:”

1. Every bubo furnishing inoculable pus, is never followed by constitutional symptoms.

2. The multiple indolent bubo, the consequence of the indurated chancre, is a further and sometimes the only proof of the constitutional infection, when we have not been witness to the induration of the chancre.

Six months never pass, without manifestations of the syphilitic infection supervening.

The bubo of the indurated chancre never yields *specific* pus—it does not inoculate.

He says that he has inoculated the same individual, and this, too, many times, with a variety of pus, and that the pus of the chancre, alone gave positive results.

If the chancre still exists as a chancre, *its pus is inevitably inoculable.*

There is really no difference between the natural contagion and the artificial inoculation of syphilis.

M. Ricord denies, and M. Vidal asserts, the contagiousness of secondary syphilis, a question of paramount import. In his work on surgery, Prof. Erichsen, of London, holds the following language: "constitutional or secondary syphilis is not contagious. This point, which I look upon as one of the fundamental doctrines in syphilis, has, in my opinion, been incontestably proved by the observations of Hunter, and more recently by Ricord, who has shown that the pus from secondary sores is never inoculable; observations that are fully carried out by what may often be observed in practice. It is, however, hereditarily transmissible, under certain circumstances, from parent to offspring," 422. The writer of this article has seen, in his own limited practice, examples of infection of the wife from the secondary syphilis of the husband, and has been informed during a recent conversation with Prof. Warren Stone, M. D., of the University of Louisiana, (a higher authority upon this subject need not be desired,) that his experience justifies him in saying that secondary syphilis is contagious and transmissible. A great and increasing number of reliable observers regard secondary or constitutional syphilis as communicable to the wife, or the child, or to both, and that, too, without an existing chancre on the part of the husband, and without previous chancre having occurred to the mother, or to the child! so that in fact, secondary syphilis becomes in one important respect more contagious than the primary or chancreous form; for, even the primary chancre affecting the mother is not transmitted to the children, and the children's children as constitutional syphilis is! If the great number of the witnesses—their experimental knowledge, their ability and integrity, their agreement and their motives—be considered with reference to the affirmative of the communicability of secondary syphilis, M. Ricord's *ipse dixit* to the contrary is futile, notwithstanding the *Io pœan* of his school.

As to the statements of the infected and infecting party, who has an interest in deceiving, lying is the rule, truth the exception. After making this concession, it appears to the writer, from what he has seen, and from statements by parties who had no interests to subserve by withholding the truth, that clap, after an apparent cure, even where marriage has been deferred for some months or a year, may sometimes infect the constitution of the wife—giving rise to sore throat, eruptions and so forth; causing the same to appear upon the children, which are often born prematurely or die soon after birth; or, should these children live, they will probably have a succession of masked or open syphilitic symptoms—a conclusion, which, though contrary to my former belief, and based on a very limited number of cases, has been received as valid by others who are better qualified to judge—among whom is M. Vidal.

In the treatment of syphilis, adopted by MM. Ricord and Vidal there is nothing new. The anti-mercurialists are giving way and falling back upon mercury, which, however, is used with a precision, caution, and judgment, little known to the ultra-mercurialists of the last generation. Even the new preparations of iodine, lately so popular, are receding to their proper level, as auxiliary agents in the cure of syphilis.

It is scarcely possible for the young physician to judge from his individual experience as to the completeness and permanency of the cure of this malady, since he is obliged to wait long years before he can say, Sir, you are cured; you will stay cured; and may marry, without infecting wife or children. Adieu!

M. Ricord recommends the destruction of the primary sore as soon as possible: he says that, when he has an infecting chancre to treat, he has recourse without delay to the specific treatment, that is to say, the mercurial. That this treatment may prevent or simply retard the constitutional manifestations during a period which is difficult to limit, between months and years.

There are no practitioners who have not seen patients, who, after having been specifically treated, have enjoyed excellent health for a period of ten, twenty and thirty years, and who, either for the first time or as a relapse, have presented the characteristic symptoms of syphilis. Thus we are forced to admit the persistence of the diathesis, compatible with apparent good health—and how can we conclude

upon the absolute destruction of the syphilitic disposition when once acquired?

To continue the treatment only until the disappearance of the symptoms, is a method which is most likely to be followed by future trouble—neither does the continuation of the treatment after the cure of these symptoms, for the same length of time that it occupied to obtain the cure, give any more satisfactory results. Neither is salivation any gauge. But, the author says, six months treatment at a daily dose which has a marked effect upon the symptoms that we have to combat, and which effect, we are convinced, still continues after their disappearance—constitutes the most rational and satisfactory method. He then gives his ideas upon how and when, mercury and the iodide of potassium should be given; and finishes his letter by summing up the therapeutics of syphilis.*

Syphilis is the cause of much dishonesty; both patient and doctor are apt to be deceived; and the more so, as each is biassed by wishes and hopes which too often prove illusory; not that the venereal disease is absolutely incurable, as some contend—but doctor and patient must have patience. From month to month; perhaps, from year to year, should the treatment be renewed at intervals, as the symptoms may indicate. Facts, of apparently indubitable character, go to show that in the first instance the cure is complete, though subsequent experience will generally prove otherwise. Perhaps ignorance, rather than dishonesty should bear the blame. The patient, in a few months after having been cured, as the parties had fondly thought, complains, calling the secondary symptoms by the wrong name—as rheumatism, mercury in the bones, &c., &c.

A knowledge of the natural history and prevention of venereal diseases is becoming more and more important, particularly in the maritime districts of the South—upon the plantations and in the towns. These maladies augment with the density of the population and the facilities of commercial intercourse, not to name *civilization*, wealth and luxury!

While writing this, a gentleman from one of the maritime parishes of Louisiana, informed the writer that he had noticed very generally among the slaves on a sugar plantation, a peculiar gait in walking, which led him to suspect the prevalence of clap; which, on examina-

* In quotations from "The Letters on Syphilis," sometimes Dr. Slade's summary has been preferred as being briefer than the author's statements.

tion, proved to be true. Every slave, except eight or nine out of a large number, having contracted the disease. The irruption of the disease was attributed to white sailors, recently from the centres of infection, who had resided temporarily on the plantation while taking in a cargo of sugar.

In remote inland situations, where the population is sparse, prostitution rare and secret—as, for example, in Western Virginia, remote from the maritime and river cities—venereal diseases scarcely exist, excepting isolated cases, derived from a distance.

These diseases, once introduced into the maritime plantations of the South, must spread with great rapidity, owing to the promiscuous intercourse to which the blacks are much addicted—and the more so, as the population grows dense and commerce extends. An illustration from the April number, 1854, of the British and Foreign Medico-Chirurgical Review, will serve to show the dangers of syphilization in the denser portions of the British Empire. In that work, Dr. Holland reviews eight elaborate, statistical works on prostitution in the old world; giving the following tableau of his own country. Let philanthropists, planters, and the rural population ponder on this hideous picture of city civilization—among the whites, in a Christian land:

	Number of Prostitutes.	THE PROSTITUTE T		
		Males.	Females	Population.
London	10,000	104	121	225
Liverpool	2,900	43	45	88
Manchester	700	156	169	325
Leeds	700	70	75	145
Edinburgh	800	106	130	236
Glasgow	1,800	87	97	184
Dublin	1,170	101	119	220
Cork	350	113	134	247
England, Wales & Scotland	50,000	202	213	415

If we take into account the geographical position of these cities, as well as the proportion of the population to the number of prostitutes in each, it will, we believe, be evident, to all at least acquainted with the statistics of this subject, that these figures fall far short of representing the actual number of these women; and, if allowance be made for the paucity of prostitutes in country towns, there is every reason to believe that to represent the public prostitutes in England, Wales, and Scotland, 50,000 is an estimate too low. Further, we

presume there will be no objection made to the assumption, that unless each of these 50,000 prostitutes submitted to at least one act of intercourse during every twenty-four hours, she could not obtain means sufficient to support life.

Though the result of the evidence contained in the first Report of the Commissioners on the constabulary force of England and Wales, was, that at that time about two per cent. of the prostitutes of London were suffering under some form of venereal disease; yet, we will descend even lower, and presume, that of one hundred healthy prostitutes, taken promiscuously from England and Scotland, if each submits to one indiscriminate sexual act in twenty-four hours, not more than *one* would become infected with syphilis: an estimate which is, without doubt far too low; yet if admitted to be correct, the necessary consequence will be, *that of the fifty thousand prostitutes, five hundred are diseased within the aforesaid twenty-four hours.*

If we next admit that a fifth of these five hundred diseased women are admitted to hospital on the day on which the disease appears, it follows, *that there are every day on the streets four hundred diseased women.* Let it be supposed that the power of these four hundred to infect, be limited to twelve days, and that, of every six persons, who at the rate of one each night, have connection with these women, five become infected, it will follow, *that there will be four thousand men infected every night, and consequently one million four hundred and sixty thousand in the year.* Further, as there are every night four hundred women diseased by these men, *one hundred and eighty-two thousand five hundred public prostitutes will be syphilized during the year; hence one million six hundred and fifty-two thousand five hundred cases of syphilis in both sexes occur every twelve months.*

If, then, the entire population had intercourse with prostitutes in an equal ratio, *the gross population of Great Britain, of all ages and sexes, would during eighteen years have been affected with primary syphilis.* Be it remembered, we do not assert that more than a million and a half of *persons* are attacked every year, but that that number of *cases* occurs annually in England, Wales and Scotland, though the same individual may be attacked more than once. Although it is evident that all the estimates used for these calculations are (we know no other word that expresses it) ridiculously low, yet we find that more than a million and a half cases of syphilis occur every year, an amount which is probably not half the actual number. How enormous, then must be the number of children born with secondary disease! how immense the mortality among them! how vast an amount of public and private money expended on the cure of this disease!

As the means whereby this fruitful source of disease and crime can be confined to certain limits, we hope we have proved that it can be accomplished by means of a registration, examination and control of prostitutes; this at least has been the object for which these particulars have been thus prominently brought before the profession, and

we trust the authorities may see the paramount necessity for speedy legislation on this subject.

We might dwell upon the fact that prostitutes are often thieves or their abettors, the receivers of stolen goods, &c; crimes of which only a part, probably a very small part are discovered. One of the most important services rendered to society by a control of prostitutes is, that while it enables the authorities to watch over the outgoings and incomings of this class, to learn their histories, and thereby discover how so much crime escaped detection; it at the same time renders apparent the weak points in our criminal code.

We cannot avoid expressing also our strong conviction, that apart altogether from the prevention of syphilis or of crime, a proper control over prostitutes would be attended with an incalculable benefit; it would assist in the reformation of those women. How many unhappy creatures go down to their graves uncared for and unpitied, we tremble to think; how many drag on a base and hopeless life, may be guessed at by any one who will penetrate into their foetid dens. A police control would at once insure the lowest prostitutes, what we give to our felons, cleanliness, pure air, and some sort of order and discipline. It would give them also, at all times, an outlet from their wretched life, if they wished to avail themselves of it; and it would afford benevolent individuals incalculable aid in their attempts, now, alas! most feeble and ineffectual, to withdraw these women from the inevitable consequences of moral and physical decay.

We have avoided entering into any details as to the kind of control which should be put in force in this country; the time for such a consideration will not arrive until it has been enacted, that the reception of money for sexual intercourse is a criminal act, which places the woman under control; then, with the aid of the experience derived from the working of the different systems in other countries, and assisted by the conclusions arrived at by the "Congrès Général d'Hygiène," we may be enabled to answer in detail this most important question:—"What are the measures to be taken for arresting the progress and diminishing the dangers of prostitution, and for reclaiming, as far as may be, the unhappy women whom circumstances have forced into a life of debauchery?"

Prostitution—which exists; which has always existed; and which, ever will exist—presents, independently of its moral bearings, questions of hygiene, of prophylaxis, and of pathology, of the utmost importance to the well-being of society; questions, which have been ably and boldly examined by some of the most enlightened and benevolent physicians and philanthropists of the continent of Europe; men, who have done for brothels what John Howard did for the prisons of Europe, and have merited the everlasting gratitude of the civilized world. They have sought to remove or lessen actual evils; their

mission, though a sanitary one—virtually “calls, not the righteous but sinners to repentance;” whom the sanctimonious and hypocritical ignore or despise—while affecting the deepest concern for heathens, infidels, and heretics, advocating visionary schemes for ameliorating the human family.

Prostitution, at once originates and propagates the most loathsome of maladies which “flesh is heir to;” and the more so, whenever it is abandoned to itself: though, in fact, it is far more amenable to sanitary legislation, and to the control of medical police than epidemics—as cholera, plague and yellow fever, not to name sundry impracticable objects, reforms and unachievable projects pertaining to sanitary legislation.

The inherent repulsiveness and indelicacy of prostitution afford no valid excuse, any more than the filth and nuisances of the streets do, for that moral cowardice which skulks from an existing evil—which is great, increasing, and ought to be checked and controlled—not that legislation can prevent it; not that the law should seek to authorize and sanction it. The law can make prostitution known by registration, which is better and safer for society than to allow it to proceed uncontrolled, in a clandestine manner. The law can hold the keepers and the inmates of houses of prostitution, to a strict accountability; can place them under the strictest *surveillance* of police commissioners and medical officers, under heavy penalties; as well as subject them to visitation, examination, registration, and supervision, as rigidly as if their inmates were incarcerated in prison. According to the most reliable authorities, houses of prostitution in some of the great cities of Europe are, in many respects, subjected to prison discipline.

The law, while tolerating and controlling what it cannot prevent, has, with a benevolent purpose, provided facilities by which those who desire to reform and return to their friends and families may have the necessary means.

Although medico-legal jurisprudence and hygiene, in the English language, ignores *tolerated prostitution*, yet the voluminous works and official reports, emanating from enlightened minds on the Continent, have at length awakened the attention of the British writers, as may be seen in the two last numbers of the Review above quoted. The

doubters should at least inform themselves as to the actual results achieved by sanitarian legislation abroad; and, if they should be convinced that the fifty thousand or even the smallest fraction of that number, who visit New Orleans every year—from seas, lakes, rivers, mountains and plains—could be protected by medical police from infection, and could be returned home safe and sound—well and good.

Moses, the Hebrew law-giver, recognized and described clap—"a running issue;" and enacted sanitary laws to prevent its extension among his compatriots; as, for instance, in the fifteenth chapter of Leviticus:—"And the Lord spake unto Moses and Aaron, saying—speak unto the children of Israel, and say unto them: When a man hath a running issue out of his flesh, &c.; or, whether his flesh be stopped of his issue, &c." A lengthened account is given, applicable to both sexes.

A very competent observer, now in Paris, Prof. A. Flint, in a letter dated in May last, says:

The subject of legalized prostitution involves important moral considerations which I do not design to discuss. With us the law recognizes the evil only to enact penalties which do not prevent it; if, indeed, they tend to diminish it. In France the evil is recognized as one that cannot be prevented by law, and hence that it falls within the scope of legal enactment to mitigate some of its terrible results. It may be doubted whether the system pursued in France and some other countries of Europe operates in any measure as a bounty, for the increase of prostitution. The influence is perhaps the reverse. But as regards the physical consequences to both sexes it is unquestionably true that the regulations so rigidly enforced at Paris tend to mitigate and diminish them in no small degree.

This subject is one of interest and importance to the moralist, and the political economist, as well as to the medical philosopher. It is one of those subjects pertaining to health and happiness which, from a false delicacy, it has been tacitly understood to be either improper or useless to discuss,—*West. Journ. Med. Surg.*

M. Latour, editor of *L'Union Médicale*, who idolizes M. Ricord, says in a letter to the latter, that the number of diseased public prostitutes in Paris at present, as appears from recent official reports has so greatly diminished, as scarcely to amount to one in four hundred, owing to the wise and efficacious measures of the administration—both hygienic and medical; while, on the other hand, clandestine prostitution alone tends to swell the number of the syphilitized. The

former, he says, "is a social evil incomparably less than that which results from prostitution, free and without shackles." M. L. adds :

For, like myself, you think that the noblest mission of our science and art, consists not in curing diseases by therapeutics, but in preventing them by hygienic measures. I therefore deposit these ideas with confidence and as in a propitious soil, in your mind and heart. You owe to syphilis, to the pathological and therapeutical study of it, the finest part of your legitimate renown ; it is to you, especially, that reverts the glory of having almost extinguished the poison in public prostitution, by your intelligent counsels upon the use of the speculum in the researches for the virus. You must complete this truly humane trilogy my friend ; pursue and cause this frightful malady to be tracked even into the perfumed boudoirs of our modern Lais. The poison incessantly pursued, tends to disappear in the Venus of the cross-way ; having taken refuge in the libidinous and covetous *alcove* of unpunished courtesans, it thinks itself safe from the investigations of the *bureau des mœurs*. Prove that syphilitic virus ought not to enjoy the right to an asylum, any more than robbery and murder, and the public morality will be grateful to you.

It may be thought that the strong governments of Europe are alone able to control prostitution ; and that the model Republic, governed by majorities, cannot restrain nor circumscribe the inalienable rights of syphilization. There is, however, one class of the Southern population, now nearly four millions, which, without any legislation except that of the plantation police, enacted by the planter, may, with care, be protected from syphilitic maladies—namely, the negroes.

The importance of the subject dwelt upon in this paper must justify its length.

Before concluding, it is hoped a few additional remarks upon the works, whose titles precede this article, may be acceptable to the reader.

A fault common to MM. Ricord and Vidal's books, mentioned above—a fault chargeable to French literature generally—is the absence of indexes. The omission of an index in a book of reference, is a very serious evil. A medical book is seldom read continuously, or even once. It is obtained for reference and examination, as called for by emergencies. It is consulted, in the hurry of business, just as the dictionary is for information on a particular subject, and like the dictionary should be arranged alphabetically by an index. The French, however, excel in constructing *tables des matières*. These are poor substitutes for alphabetical indexes.

Whatever errors, if any, M. Ricord's venereal logic may be chargeable with, his experimental opportunities for acquiring correct information, and the zeal, industry, and talent displayed in the investigation of his great specialty (syphilis) are unrivalled, and cannot fail to inspire confidence on the part of the reader of his works.

These witty, anecdotal, colloquial, and instructive letters, originally addressed to the chief editor of l'Union Médicale, are the latest emanations from a towering intellect, and forms a chatty, epistolary edition of his more ponderous works, but do not constitute a formal treatise; they abound in graceful, yet often pungent satire, which his immediate cotemporaries must understand and feel much more than the foreign reader, who may not be informed in the polemics of Parisian syphilographers.

Dr. Slade deserves the thanks of the profession for his excellent translation of this valuable series of Letters on Syphilis.

The word *accident* is very often used in M. Vidal's work, "as primitive venereal *accidents*, consecutive venereal *accidents*," &c., *ad infinitum*. French dictionaries and works on the synonymes of that language, show that this word has the same fundamental signification as it has in English. Thus Crabbe and Boinvilliers give the same definitions and illustrative examples: "It is an *accident* when a house falls," says the former: "Les chutes d'édifices sont des *accidents*," says the latter. The former says: "*Accidents* cannot be prevented; *accidents* may sometimes be remedied; we do not expect what is *accidental*."

The fundamental idea in both languages, therefore, relates to unforeseen, unexpected, unpreventable events—as railroad collisions, steamboat explosions, earthquakes, a fall down stairs, an abortion, a crevasse, &c. The usual phenomena which follow inoculation or vaccination are not called *accidents*, though a mortification of the arm, or death from such cause might be so termed, being unlooked for and contrary to known probability.

In the French there may be, possibly, a little more latitude in the use of the word *accident*, and, by a somewhat forced meaning, it may signify symptom. M. Ricord in his very first letter (see l'Union Méd. Jan. 22, 1850,) uses the word *accident* repeatedly; but Dr. Slade always renders it by the word *symptom*, which is an improvement.

M. Vidal's treatise is one of inestimable value to the practitioner, both for its principles and practice, combining as it does upon a large scale, controversial questions and suggestions, special details, and analyses, generalizations, pictorial illustrations, methodical arrangement, and rational modes of treatment.

Dr. Blackman has enriched his translation of this able treatise, by adding a large amount of valuable matter from the latest standard authors, intercalated in the text.

MM. Ricord and Vidal being rival writers of the same hospital, (*du Midi*,) and opponents in doctrine worthy of each other, ought to be studied together. Hear both sides.

Dr. Parker's work on syphilitic diseases, the third in the enumeration at the head of this article, was received too late to permit of a thorough examination at present. An hour devoted to the perusal of this author has produced the conviction that his work is the first in pathology, the first in therapeutics, and the first in value in the English language since the days of Hunter. The author appears to be an able observer and practitioner. He has devoted twenty years to his specialty, and has treated eight thousand cases of syphilis, and having condensed his researches into a thin volume, comes before the public with claims which the reader has but to examine in order to be convinced as to their high import.—EDITOR.

Part Fourth.

MEDICAL INTELLIGENCE.

Art. I.—*Injection of the Tincture of Iodine as a Means of Diagnosis in Discovering the Internal Fistula in Ano*: Translated for this Journal from the Rev. *Thérap.*, with remarks: By M. MORTON DOWLER, M. D., of New Orleans.

In a case of anal fistula, the internal opening of which could not be discovered by the ordinary mode of exploration, M. Limange adopted the idea of injecting the pure tincture of iodine in the fistulous tract by the external orifice, after having first introduced the finger into the anus. When the finger was withdrawn it was discovered to be stained with an indelible spot, produced by the iodine tincture; and, on comparing the situation of this spot with the depth to which the finger had penetrated into the intestine, he obtained the proof, not only of the existence of the internal orifice of the fistula, but a precise idea of the height of this orifice in the rectum. M. Limange very reasonably observes, that this mode of diagnosis is infinitely preferable to all other colored liquids that have been recommended to be injected into the fistulous tract; because, in the first place, if the internal aperture is very small, it may happen that the quantity of the liquid forced into the intestine may be so minute that it will sometimes be difficult to verify its escape from the anus; and, in the next place, because the stains which other liquids may produce on the finger introduced into the rectum have not the permanence of the stains occasioned by the tincture of iodine.—[*Archives Belges de Médecine Militaire.*]

REMARKS.—The method of M. Limange might, no doubt, be improved upon, so as to apply to cases in which the external orifice is situated beyond the reach of the finger. A silver or silver-plated tube of the size of the finger, might be passed high into the rectum,

and would receive a permanent stain at whatever point it might be touched by the least particle of the iodine tincture: moreover, it would appear, that so strong an irritant as the tincture of iodine would scarcely be required as a mere diagnostic means. Starch is so delicate a test for the presence of iodine, that a very small quantity of the latter rendered soluble in water by adding a little of the iodide of potassium, and injected through the fistulous tract would imprint a deep blue color on any instrument that might previously have been introduced, covered with decoction of starch. The tube might be covered with fine muslin saturated with the decoction. We should say *à priori*, that a very weakly ioduretted solution of the iodide of potassium forced into the fistulous tract would, under these circumstances, not only give "la preuve de l'existence de l'orifice interne de la fistule, mais encore un donnée précise sur la hauteur de cet orifice dans le rectum," if a single drop of the fluid passed into the gut.



Art. II.—*The First Death from Yellow Fever in New Orleans,*
in 1854 ?

June 16th, 1854. Was invited by Dr. Meighan to see a patient—Johnson—born in Scotland, aged about 28; of good physical conformation—a sailor—resident chiefly in New Orleans since 1848; often absent on river voyages; remained in the city during the epidemic of 1853; boards in a brick house on the west side of New Levee street, near Benjamin street; sick, about six days; Dr. M. has attended him since the 12th instant. Dr. Meighan's account, confirmed by that of Drs. Harvey and Desmond, and that of the woman who boarded and nursed the man, with others, proved that the early stage of his disease corresponded with yellow fever; the man had been working in the sun, upon the hurricane deck of a steamboat; was taken with a transient chill; frontal, spinal and muscular pains; flushing of the face, neck, &c.; hot skin; vomitings; hæmorrhages, from the nose and gums; copious black vomitings. Just before my visit, as well as in my presence, the patient threw up two varieties of black vomit—that is, a homogenous liquid darker than an infusion of tobacco or coffee,

with a heavy, flakey and granular sediment mixed with a dense mucosity, sinking to the bottom of the vessel. The eyes were prominent, staring, and somewhat injected; expression of the face dull, approaching stupidity; answers questions coherently; abdomen, tender and tympanitic; retention of urine, probably, for several days; catheterized by Dr. Desmond, at my request.

The man died the following night. Next morning I attended with Dr. Meighan and several other physicians, to make a post mortem examination; but the acquaintances of the deceased refused permission with vehemence.

I learn that no one in the house or neighborhood where this man died, suffered from yellow fever, before or since.

This statement having been read to Drs. Meighan, Harvey and Desmond, who saw the case before I did, is approved by them, as correct.

B. DOWLER, M. D.

[Since the above article was put in type, Moses Morton Dowler, M. D., has addressed the following note to the Editor:]

John Frederick Lassing, wife and family, arrived here on or about the 28th day of April last, from Germany, in the ship Heinrich Von Gagern, from Bremen. The family took a house on Tchoupi-toulas street, between Philip and Saraparu, where Lassing now resides. The family all continued in good health 'till on the 5th day of June last, Joanna, aged 45 years, wife of Mr. Lassing—nervous temperament and delicate habit of body—was taken sick of yellow fever. I saw her on the second day of her sickness, and attended on her two days. There appeared to be quite an improvement in her case on the evening of the 9th; and it was understood that I was to return only on special call. On the 10th, I was called to see the patient again, she having suddenly and dangerously relapsed, and died on the morning of the 12th.

Art. III.—*On the Proper Position of the Woman during Labor :*
Translated for this Journal, from Rev. de Thér., of June, 1854; By M. MORTON
 DOWLER, M. D.

Does the dorsal *decubitus*, which is usual in France; or, the left side position, which is preferred in England; or, the pronation of the body, supported on the knees and elbows of the patient, involve merely a question of habit, an affair of fashion or of national manners? Does not each of these attitudes, on the contrary, fulfil a real indication? M. Hubert thinks it does, and very well demonstrates his position.

According to him, in order to operate in a case of version, if the child, with shoulder presenting, has the belly turned backward, it is better to leave the woman on her back. The operation can be effected with greater facility. But, if the fœtus has the belly turned forward, and its pelvic members are placed against the anterior parietes of the uterus, then, allowing the woman to rest on her back, the accoucheur can only reach the feet by carrying the hand in the prone position, and strongly forward; or, if the *liquor amnii* has escaped and, especially if the belly be projecting, the arch of the pubis compressing the fore arm, would soon cause it to be benumbed, and render it unable to act, and would prevent it from penetrating as far forward as is sometimes necessary,

On the contrary, if you place the woman on her elbows and knees, and apply the hand in a state of supination, you have but to follow the anterior parietes of the uterus and pelvis in a line almost straight and horizontal from behind, forward, which is done with the greatest facility. But this, position, besides wounding the modesty of the woman, being tiresome to maintain, the following is the method by which M. Hubert succeeded in realising all its advantages, without subjecting the patient to these inconveniences. He allows the woman to remain on her back till the right hand has cleared the cervix uteri; he then flexes the right thigh and leg of the patient, and then, while she turns or some assistant turns her on her left flank, he passes the flexed limb above his right arm, and he is thus placed opposite to the patient's back. He can then attain the antero-lateral left, and even the ante-

rior parietes of the womb, in order to find the feet. Having brought them to the vulva, he replaces the woman on her back.

By this ingenious proceeding, M. Hubert draws from the pronated attitude of the body, in delivery, all the advantages of which it is capable of rendering, without imposing it on the patient. He cites numerous cases, thanks to the advantages of this manœuvre, which many of his confrères, who not being otherwise able to touch the feet of the fœtus, have been able easily to reach them when the hand, seconded by this change of position, has been enabled to penetrate further forward.

To complete the delivery, if the power of traction is lost by the resistance resulting from the anterior parietes of the cervix, and the placenta cannot be with facility drawn forth, the woman must be laid on one of her sides, and the accoucheur standing behind her, must simply draw towards himself. If there be no abnormal adherence these tractions, being very near parallel to the axis of the uterus and that of the superior strait, the delivery will be easily effected.

Art. IV.—*Sir Walter Scott on Calomel.*

In July, 1819, the great Novelist, thus expressed his views of the therapeutic value of calomel:—It is all a joke to talk of any other remedy than that forceful, unpleasant one, calomel. I cannot say I ever felt any advantage from any thing else; and I am perfectly satisfied that, used as an alterative and in very small quantities for a long time, it must correct all the inaccuracies of the biliary organs. At least, it has done so in my case more readily than I could have believed possible.

Art. V.—*Sir Walter Scott's Ideas of Psychological Medicine—Good Nursing.*

Sir Walter Scott, in his Diary, Dec. 16th, 1826, says:—Another bad night. I remember, I used to think a slight illness was a luxurious thing. My pillow was then softened by the hand of affection, and the little cares put in exercise to sooth the langor or pain, were

more flattering and pleasing than the consequences were disagreeable. It was a new scene to be watched and attended; and I used to think that the *malade imaginaire* gained something by his humor. In my better days, I had stories to tell; but death has closed the long avenue upon loves and friendships, and I look at them through the grated door of a burial place, filled with the monuments of those who were once dear to me, with no insincere wish that it may open for me at no distant period, provided such be the will of God.

Art. VI.—*Psychology—Diagnosis of Sorrow.*

Mr. West, the American painter, while painting Lord Byron, said to him: "Notwithstanding your vivacity, I think myself correct at least in one estimate which I formed of you, for I still conceive you are an unhappy man. Byron inquired, what reason I had for thinking so? I asked him if he had never observed in little children, after a paroxysm of grief, that they had at intervals a convulsive or tremulous drawing in a long breath? Wherever I observed this, in persons of whatever age, I had always found that it came from sorrow.—*Moore's Life of Byron*—ii., 415.

ADVERTISEMENTS.

MEDICAL COLLEGE OF THE STATE OF SOUTH CAROLINA.

The Annual Course of Lectures in this Institution will commence on the first Monday in November, and terminate on the first Saturday in March, upon the following branches:

Anatomy, by J. E. HOLBROOK, M. D.

Surgery, by E. GEDDINGS, M. D.

Institutes and Practice, by S. HENRY DICKSON, M. D.

Physiology, by JAMES MOULTRIE, M. D.

Materia Medica, by HENRY R. FROST, M. D.

Obstetrics, by THOS. G. PRISLEAU, M. D.

Chemistry, by C. U. SHEPARD, M. D.

Demonstrator of Anatomy, F. T. MILES, M. D.

Prosector to the Professor of Surgery, J. F. M. GEDDINGS, M. D.

Clinical Instructor, D. J. CAIN, M. D., Physician to the Marine

Hospital and Clinical Instructor, lectures twice a week on the Diseases of that Institution.

H. W. DESAUSSURE, M. D., Physician to the Hospital of the Alms-House, at which lectures are delivered twice a week on Diseases, the diagnosis discriminated, and the student indoctrinated in their treatment.

Demonstrative Instruction in Medicine and Surgery, at the College Hospital, by the Professors of the Medical College.

The Anatomical Rooms will be opened in October and dissections conducted daily.

The Faculty of the College take pleasure in calling the attention of the friends of the Institute to its present prosperous condition—the class of the past year exceeding any former years.

They have been enabled by the liberality of the Legislature, at its last session, to make such alterations in extending and improving the college buildings as will promote materially the comfort of those in attendance on the Lectures.

The Anatomical Theatre has been enlarged and completely renovated, and such changes made as will secure free ventilation with a pleasant arrangement of the seats. They confidently believe that it will not suffer in comparison with any like structure in the United States; the edifice with its appurtenances being as commodious and attractive as any such establishment in our country. They have been enabled also to make considerable additions to the museum.

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HENRY R. FROST, M. D., Dean.

WORKS OF THE SYDENHAM SOCIETY, (London.)

The undersigned having received the appointment of "*Local Secretary*" of this Society, for the Southern States, would respectfully invite attention to the valuable *Standard Works* which it publishes each year, and furnishes to subscribers, at extremely low rates. The annual subscription is only *Five Dollars*, for which, usually *three*, but sometimes *four*, valuable volumes of the best Medical Works, are furnished. The works of several of the last years are still to be had.

For further information, apply to the undersigned, who will receive subscriptions, and have the works delivered with the utmost despatch. The attention of Medical Colleges is particularly invited to the rare opportunity here presented, of supplying their libraries with *Standard Works*, both ancient and modern.

E. D. FENNER, M. D., Local Sec'y, Sydenham Society,

December 16, 1851.

No. 5 Carondelet street.

THE PRACTICE OF MEDICINE. WHAT IS IT?

The regular Practitioner of Medicine has almost insurmountable difficulties to contend with, in the fact, that his prescriptions are necessarily little better than experiments, more particularly the Physician of the South and West from the fact that the purity and strength of Medicines vary so very materially.

It is well known that Laudanum is usually made from the most inferior and unsaleable pieces of Opium, which seldom if ever contain half the proper quantity of morphine.

Rhubarb is powdered from roots varying in price, from 20 cents to \$1 00 per pound; the lower prices, of course, more or less decayed and worm-eaten; and thus, being unsaleable, are powdered and colored to sell 'cheap.'

Instead of Jalap, large quantities of the "Spurious Jalap," and also of a variety known as "Jalap Tops," are sold at about one-quarter the price of the true Jalap.

For Peruvian Bark, at least a thousand pounds of the worthless, inert Carthagena and Maracaibo Bark, are ground and sold for every pound of the true Peruvian Calasaya Bark.

Now, *if* these are facts—and they certainly *are* well-known facts, and very serious facts too—how is it possible to prescribe with any certainty? Is prescribing with such Medicines anything else than experimenting.

And that they are facts, is abundantly proven by the Report of the Custom House Inspector of Drugs and Medicines, and also by the Report of the Special Commissioner to the Secretary of the Treasury, on Adulterated and Spurious Drugs; which Reports, shocking and humiliating as they are, do not show a tithe of the facts in regard to the wholesale adulteration of Medicines.

The Report of the U. S. Examiner says:

"Such sir, are the fruits, thus far, at this port, of the wise and eminently beneficial sanitary measures, so heartily approved of by every friend of humanity; that measure, too, which met from its inception, the open, determined and unremitting hostility of a God forsaken portion of our trading community. From the moment the question was first agitated, and during the progress of the bill through Congress, intense excitement and ill-feeling was manifested among certain importers and speculators who had long made the murderous traffic not only a source of profit but of wealth, and no means were left untried by them calculated to defeat the measure.

"Most persons, we admit, can judge very correctly, by sight, of the quality of most articles of food and clothing; but where is the man who can, by simply looking at the almost countless number of medicinal preparations, chemical and otherwise, say whether they are pure or adulterated? or by looking at the various preparations of morphine, say whether they do or do not contain five, ten, or twenty per cent. of *amygdaline*? or can detect by sight, *corrosive sublimate*, *prepared chalk*, *gypsum*, and other impurities in calomel? or can by sight say whether blue-pill mass contains its full equivalent of mercury, or only one-fourth or less of the requisite quantity? or can say whether hydriodate or iodide of potash is pure, or is adulterated by the admixture of *sal acetosella*, *sup. tartrate* and *sulphate of potash*? or can in the same way detect *salicine*, *mannite*, *sulphate of barytes*, and *oxide of zinc* in sulphate of quinine? or can say whether Croton oil is, or is not, adulterated by the admixture of inert *fats* or whether it is, not, in fact, an entirely *fictitious article*? or by looking at the powdered cinchona bark, say whether it is genuine powder of that species which affords the largest quantity of quinine and some cinchonine, or whether it contains thirty or fifty per cent. of the powdered *Moracaibo* or *Carthagena* bark, which affords but a trace of either of these important alkaloids, and is consequently worse than worthless for medicinal purposes; or whether it is not, in fact, composed entirely of the latter worthless variety? or can say, by looking at powdered rhubarb, whether it is of that prime quality which

affords from sixty to seventy per cent. of soluble matter, and some twelve per cent. of pure resin, or whether it is an article produced from the decayed and worthless root, (the color and smell having been heightened by artificial means,) which affords not to exceed fifteen per cent. of soluble matter and no resin at all?

"The several barks before alluded to, although differing in physical appearance, are those generally known in the trade as the red and yellow Maracaibo and Carthagenia barks; and as they resemble the true officinal bark in color, they have long been used in a powdered state for the purpose of adulterating those barks, or sold to the unsuspecting as the genuine article. This fact shows very clearly why it has long been almost impossible to find on sale in the country, or even in our minor drug and apothecary establishments in town, one pound of the red or yellow cinchona bark, of the requisite strength and purity; or, in other words, that will afford, on analysis, a per centage of alkaloids corresponding with that produced by the genuine barks. Some samples that have been obtained afforded neither quinine nor cinchonine in any perceptible quantity! others less than one-fourth part of the alkaloids found in the true and pure barks; and, so upward, according to the extent of the adulteration. From the quality of samples that have been forwarded to me from a distance, I am satisfied that the country is filled with such base mixtures and worthless trash.

"The question now very naturally and properly comes up, will prime crude drugs, after having been powdered and prepared, be found on sale in town and country in as pure a condition as when imported; or, in other words, be found free from adulteration? I fear not, unless a strict watch is kept over the operations of the unprincipled portion of those among us whose mission it is 'to buy, sell, and get gain,' honestly if they can; if not, get it.

"It has heretofore been too frequently found that drugs become astonishingly reduced in strength and purity during their transition state from root, bark, gum, &c., &c., to powder. Prime fresh drugs are no doubt (as well as worthless) sent to the drug-mill; but somehow or other, 'by falling into bad company,' I suppose, they are apt, during their stay, to lose their virtue; and as a matter of course are returned to their owner, and sent out into the market, with a character decidedly tarnished—an article fair to look upon, but whose touch is death. Badinage apart—the business of drug-grinding or powdering requires a searching and thorough reform.

"I have already alluded to the mysteries and trickery of the laboratory when in skillful but dishonest hands; but be assured, sir, its conjurations and diablerie if I may so express myself, in the preparation of adulterated chemical medicinal compounds, hardly exceed in ingenuity, deception, and iniquity, the frauds committed under the roof of the drug-mill.

"I have in my possession the voluntary confession of a drug-grinder, who has retired after amassing a fortune in the business; but I will not swell this report by entering at this time into an extended detail.

"This is a very important subject; and one, too, which the profession throughout the country, as well as the medical staff of the army and navy, whether on duty at a distance or at home in hospital practice, should lose no time in investigating; for how is it possible for the physician to do justice either to his patient or himself, however judicious and correct his prescriptions may be, as long as there is so much uncertainty as to the strength and purity of the curative agents he may recommend? I cannot but believe that many, very many valuable lives have been lost, owing to this lamentable condition of things."

Ought not the whole Profession to feel that their reputation, their success, and the lives of their patients should rest on a surer foundation than "guess work" or experiments?

Are not Pure Medicines far safer for the reputation of a Physician, and far more economical, taking success in view, than the cheap Medicines, which are entirely unreliable, even when they have any virtue?

THEREFORE, your earnest attention is requested to a branch of business intimately connected with success in the treatment of disease.

It is well known among dealers, and yet not generally known by the profession and the public, that pure and genuine medicines, particularly pure powdered drugs, from the first quality of gums and roots are scarcely procurable in this

country, and therefore physicians often prescribe medicines to meet certain indications in the disease of the patient, without obtaining the desired and expected beneficial result. To enumerate the articles of adulterated medicines that are daily sold in market would be to name almost the entire list of the materia medica. From the finer and more important chemicals and pharmaceutical preparations, such as Morphine, Quinine, Hydriodate Potass, Calomel, Blue Pill, &c. &c., down to the most common, and those of daily use, such as Cream Tartar, Rhubarb, Ipecac, &c., the adulterations are so adroitly made, that (without analyzation) even the closest inspection will fail to detect them. Quinine is often found largely adulterated (in some instances more than half) with mannite and other substances. Blue Mass and Calomel have been found much more than half adulteration. A gentleman at one time connected with an extensive manufacturing establishment, informed us, that just before he left England, the factory turned out more than four thousand pounds of Blue Pill, containing Barytes, instead of Mercury; and it was all designed for the American market.

Knowing this matter to be worthy the first and earnest consideration of the practitioner, we would respectfully ask attention to the accompanying

CIRCULAR.

We wish to call particular attention to our Extra Powders, which are pulverized from selected roots and gums of the very best quality; and when necessary, every piece is broken and examined under our own immediate supervision, and consequently possesses a purity hitherto unknown in this country, and a uniformity of action upon which the physician may rely with perfect confidence.

Our powdered Ipecac, extra, also will be found much superior to the usual article of commerce, being made from the true Brazilian Ipecacuanha, and consisting solely of the active outer coating of the root, carefully separated from the ligneous parts, and from all other inert matters. We pulverize only the true Mexican Jalap. In pulverizing Colocynth, extra, we retain only the active pulp of the apple, rejecting the seeds, which latter constitute the principal part of the weight of the fruit and are nearly inert. Powdered Rhubarb, extra, we prepare from the best East India Rhubarb, which is culled over with great care, every root being broken to detect any unsoundness. The Powdered Resin Guaiac, extra, is the pure Resin collected in tiers entirely from the dross and dirt usual to the ordinary Guaiac of commerce.

The Scammony also is powdered from an article differing in appearance and very much superior to what is usually sold for Aleppo Scammony. Blue Pill, bearing our label, will always contain one-third part of mercury, and our Hydrosublimed Calomel will be found to be of superior and regular quality.

Many of the roots from which the Extra Powders are prepared, are sifted and washed, and so many extraneous roots &c. are culled out, that the loss is often from one-quarter to one-third of the original weight, making, consequently, a very different article from the ordinary powders of commerce.

PARTICULAR NOTICE.

Having repeatedly heard that it is asserted that the superior quality of our "Extra Powders," and the unusual care in preparing them is all pretence, and that no one would take so much trouble and time, we merely state that we have on hand to show to any one doubting the facts, the stems culled from cubebs, the seeds taken from the pulp of the Colocynth, the woody, inert parts of the Ipecac, the extraneous roots culled from Pink, Senega, Serpenteria &c., and various other tangible proofs of the difference between our "Extra Powders" and the ordinary powders of the trade.

Although many Druggists denounce the Extra Medicines as all humbug, yet they have imitated our style of bottles and put in them the ordinary inferior powder of trade.

Powders can be imitated so easily by coloring, those wishing the pure would do well to compare them; such for instance as Rhubarb, Gum Arabic.

Care is taken to have these "Extra Medicines" not only pure, but of the best quality procurable.

When required, any of these articles can be obtained of us in their original state as some may desire a superior article to use unpowdered.

The life of the patient as well as the success and reputation of the physician and apothecary, depend so much upon the prompt action of the medicines used in sickness, that we feel every confidence in any effort to furnish them with pure and superior drugs will be fully appreciated.

To preserve the preparations from being injured by the air and moisture, they are generally put up in bottles and jars containing one pound each; and also in five and ten pound canisters. They should be kept as much as possible from the light.

It will be observed that the prices of these superior articles are necessarily higher than those of the ordinary kind; and physicians and merchants at a distance, when they wish this quality sent them, are particularly requested to write for the 'Extra Medicines' of Philip Schiffelin, Haines & Co., Chemists and Druggists, New York.

EXTRA POWDERS.

Aromatic Powder, U S P	Pulv. Blood Root	Pulv. Lobelia
Pulv. African Pepper	Bitter Root	“ Seeds
Alum	Black Root	Mandrake
Allspice	Borax	Marsh Rosemary
Aloes, Cape	Buchu	Mace
“ Socot	Caraway Seeds	Mezereon
Assafœtida	Cantharides	Nux Vomica
Antum Ref. Blk. Sulph	Banella Alba	Nitrate Potass, pure
Agustura Bark	Cardamon (Seeds)	Nutmegs
Ariseeds	Cicuta	Nutgalls
Elecampane	Cranesbill	Pleurisy Root
Digitalis	Cloves	Rhubarb, Turkey
Ergot	Cinnamon, ordinary	“ East India
“ 1 oz. phials	“ Ceylon	Rhatany Root
Extract Colocynth C'd	Cohosh, Black	Sarsaparilla, Para
“ Licorice, Calab	Columbo	Sage
Fœmngrek Seed	Colchicum Root	Summer Savory
Gentian Root	“ Seed	Sweet Majoram
Ginger, Jamaica	Colocynth Pulp	Sabina
Golden Seal	Cubebs	Senaa, Alex.
Opium, Turkey	Guaiaic Resin f. Tears	Seneka Root
Orange Peel	Gum Arabic, Turkey	Scammony, Alp. No. 1
Orric Root	“ Gamboge	“ Virgin
Pepper, Cayenne, A.	“ Mastich	“ . “
Pepper, Black	“ Myrrh	“ . “ 1 oz.v.
Peruvian Bark, Loxa	“ Tragacanth	Snakeroot, Virginia
“ “ Red	Hellebore, White	Squills
“ “ Calasaya	Hycosyamus	Sulphate Copper
Prickly Ash Bark	Ipecac	“ Iron
Pink Root	Ipecac and Opium	“ Potass
Bayberry Bark	Jalap	Uva Ursi
Belladonna	Kino, Trne	Valerian, English

BOTTLES AND CANS AT COST,

When put up in Quarter and Half-Pound Bottles additional ten cents per pound.

As many of the Gums, &c., are of unusual purity, for instance Guaiac, Aloes, Assafœtida, &c. they are very liable to run together and become solid. Even the ordinary common Gums of commerce are so apt to run together that Drug-Ginders usually grind with them some woody substance, such as Licorice root, Gentian root, &c.

Some roots that are rich in resin, such as Rhubarb, Jalap, &c., are also apt to agglomerate. The Extra Powders being perfectly pure and free from all foreign substances, are therefore more liable to become solid than the common qualities, but where they do so, we will replace them with others that are freshly powdered.

PURE CHEMICALS PREPARED AT OUR LABORATORY.

Ammonia, Aqua	Mercury, Bin Iodide	Spirits Nitri Dulc
“ Liquor Fort	“ “ “ crystals	Syrup Iodide Iron
Ammoniated Alcohol	“ “ Protoiodide	Sulphuret Potassa
Argent Nitras	Morphine Sulphate	Vallets Ferruginous Mass
“ “ Crystals	“ Acetate	Zinci Acetas
Lunar Caustic, Nos. 1, 2, 3	Granvilles' Lotion	Zinci Sulphas
Ferri Carb, precipitated	Gallic Acid	Zinci Chloride
“ “ Sulphas, pure	Oil Capaiva	Chloride Soda, Labarraque
“ “ “ Exsiccate	Oil Cubebs	Confection Rosas
Iodide Arsenic	Precipitated Chalk	Confection Senna
“ Lead	Prussic Acid	Blue Pill Mass
“ “ Crystals	Potasse Nitras, pure	Ung. Mercurial
“ Iron	Strychnine	

Many of these chemicals differ decidedly in appearance from the chemicals of commerce as well as in their valuable properties, and bear externally the style of our manufacture, being in crystals and having the peculiar crystalline characteristic of each article; while those usually sold are in the powdered state, in which form it is difficult to judge of purity.

The Crystals of the Iodines of Lead and Mercury, and the Sulphate of Morphine more particularly, are much esteemed by all who have tried them, and our Soluble Precipitate Carb. Iron, Nitrate of Silver, Extra Blue Pill Mass, hold the first rank among choice chemicals.

Our Hydro Alcoholic Extracts are prepared by steam process, in a patent Vacuum Apparatus, and at a low temperature of nearly an hundred degrees below the boiling point. so that the valuable properties of the plant are preserved uninjured, and at the same time a consistence, color and taste are obtained, which are sufficient evidence of their superiority. Among them are

Extract of Belladonna	Extract of Digitalis	Extract of Quassia
Butternut	Gentian, opt.	Rhubarb
Buchu	“ Ordinary	Sarsaparilla Simplex
Bloodroot	Hyoscyamus	“ Compound
Boneset	Hops	Para Sars. Alcoholic
Conium	Jalap	Hon. Sars. Alcoholic
Chamomile	Lupuline	Stramonium
Colocynth, Ordinary	Mandrake	“ Seeds
Colocynth, Opt.	Nux Vomica	English Valerian
Dandelion, Alcoholic	Opium	Dutch “
“ Inspissated	Pinkroot	

Observe that it is a well established fact that many plants which in their native conditions are possessed of very active medicinal properties, lose by cultivation their peculiar characteristics and become nearly inert. Some vegetable which belong to poisonous families of plants, by cultivation, are made innocuous, and are freely eaten as food, as the potatoe, parsnip, cellery, &c., &c.

To insure to our extracts, such as Hyoscyamus, Belladonna, Conium, &c., all the active therapeutical effects which they should possess, we take pains to procure the herbs, from which they are prepared, from places where they are indigenous to the soil, viz: from Germany, France, England, India, &c., &c., and they are consequently much superior to extracts made from the cultivated plants of American growth.

The difference is very apparant between these extracts and those of any other make, not only in appearance but also far more in flavor and in the peculiar aroma of the herbs; so much so, that even without the labels they are easily distinguishable by the taste and smell. These are facts of so much importance, that they should be remembered by all who have any occasion to use medicinal vegetable extracts.

These Extracts together with our pure Chemicals and Extra Powders have received the unqualified approbation of various Medical Associations, and of Phy-

sicians and Apothecaries, and have elicited many complimentary letters on their good qualities. The following token of approbation is from the Ohio Medical Convention:

"Resolved, That the thanks of the Medical Profession are due to the house of Philip Schieffelin & Co., of New York, for their efforts to furnish the community with pure Drugs; and we recommend their Extra Medicines to the confidence of Dealers and Practitioners."

And also from the following eminent practitioners :

PHILIP SCHIEFFELIN, Esq :

Dear Sir—It is with pleasure I add my commendation to that of other physicians as regards the superior quality of the Extracts and Chemicals, prepared and sold by your house. The extraordinary care and assiduity shown by you in obtaining and putting up Drugs free from adulterations, and Chemicals prepared perfectly pure, deserve the highest commendation, not only from the physicians, but also from the public, whose safety is eminently concerned in the employment of articles of the *Materia Medica*, free from inert or injurious additions. Although the public generally has been warned by the publications of "Inspectors of Drugs," and by the medical press, that such adulterations have been made by dishonest vendors and speculators, still the imposition continues to be exercised, and, in many cases, almost without reserve. The Cod Liver Oil, manufactured for your house, I find far superior to any other, being, I believe, perfectly pure, and yielding to the tests the absence of the oils generally employed in the adulteration of this really valuable article. My patients also find it far less disagreeable, and more readily digestible than the kinds I have formerly prescribed.

Wishing you the success you so fully deserve. I remain yours, very truly,

H. P. DEWEES.

SEAMANS' RETREAT HOSPITAL, STATEN ISLAND, June 21st, 1850.

Messrs P. SCHIEFFELIN & Co.:

Gentlemen—Having used your Drugs and Chemicals in this Institution, as well as in private practice, for the last twelve years, it affords me much pleasure to bear testimony as to their quality and purity; for without these, no physician, be he ever so skilful, can calculate the result to his patients.

Trusting that the public, as well as the profession, will appreciate your endeavors to furnish them with pure articles in medicine. I remain, yours respectfully,

JAS. R. BOARDMAN, M. D.,
Resident Physician and Surgeon.

Their superior efficacy, in all prescriptions, will be at once apparent to every one who reflects upon the difficulty oftentimes experienced in the administration of the common Drugs of commerce, and the *loss of life* and of reputation, consequent upon the use of inert remedies.

COD LIVER OIL.—The great and increasing demand for Cod Liver Oil, and the difficulty of procuring the oil in its pure state, and such as we can guarantee to our customers, have induced us to send our agent to the fisheries for the purpose of having the best article that can be offered in the market. This article will also bear our label when put up in bottles, and be warranted pure, when ordered in bulk.

TO DRUGGISTS.—In addition to the Extra Medicines, we also keep a large and well assorted stock of the ordinary Drugs and Medicines of commerce, carefully selected, and the best that can be procured. Our Essential Oil, and other Liquids, we obtain from the most reliable sources, and are submitted to every known test for impurities; and we avoid purchasing any kind of Drugs in the powdered state.

Our arrangements and facilities are such, that we can offer inducements to dealers, which must influence all, who, not only like to have a fair equivalent for their money, but at the same time to have goods that are what they purport to be, and such as will bear the strictest examination and analysis.

PURE CHLOROFORM.—Much of the Chloroform of commerce being very impure, and its use having in some cases been attended with unpleasant consequences, we have been repeatedly urged to make some at our Laboratory, of a quality superior to that generally for sale in this market. We would, therefore, inform the Medical Profession that we have prepared an article, the purity of which can be implicitly relied on.

NITRATE OF SILVER can also be obtained from us perfectly *pure*, either in sticks or crystals, manufactured at our Laboratory.

MORPHINE.—Our Morphine having acquired a reputation superior to any

other, those who have occasion to use the article will be satisfied of its excellence by giving it a trial.

☞ We also prepare the SYRUP OF IODIDE OF IRON U. S. P., now so highly esteemed as a remedy in Scrofulous Complaints, and also Dupasquier's Syr. Iod. Iron, which is a much milder preparation, and better adapted for Ladies and Children—These articles [which it is the greatest consequence to physicians to have of reliable quality,] are, with our other preparations, offered to the notice of those desiring pure Drugs and Chemicals.

☞ N. B.—Letters directed to "Schieffelin & Co.," intended for us, have frequently gone to other houses, there being several firms of that name; therefore please be careful to write our names in full.

PHILIP SCHIEFFELIN, HAINES & CO.,

September, 1853-1y.

107 Water Street, New York.

ORTHOPÆDIC INSTITUTION,

FOR THE CURE OF DEFORMITIES,

Nos. 457, 459 and 461 Pacific Street, South Brooklyn,

Afford to in-door patients, afflicted exclusively with bodily deformities, domestic accommodation, Orthopædic Apparatus, and a superior and attentive medical treatment hardly procurable at a private home.

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THE
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EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia; Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresponding Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c. &c.

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BOOKS AND PAMPHLETS RECEIVED.

- I.—*Auscultation and Percussion*: By DR. JOSEPH SKODA. Translated from the Fourth Edition, by W. O. Markham, M. D., Assistant Physician to St. Mary's Hospital. Philadelphia: Lindsay & Blakiston, 1854, pp. 380. From Mr. T. L. White, Bookseller, 105 Canal street, and Mr. J. B. Steel, Bookseller, 60 Camp street.
- II.—*Healthy Skin*: A Popular Treatise on the Skin and Hair; their Preservation and Management: By ERASMUS WILSON, F. R. S., Second American, from the Fourth and Revised London Edition: with illustrations, pp. 291. Philadelphia: Blanchard & Lea, 1854. From Mr. J. C. Morgan, Bookseller, Exchange Place.
- III.—*Principal Forms of the Skeleton, and Structure of the Teeth*: By Professor R. OWEN, F. R. S., pp. 329. Philadelphia: Blanchard & Lea, 1854. From Mr. T. L. White, Bookseller, 105 Canal street.
- IV.—*Principles of Animal and Vegetable Physiology*: A Popular Treatise on the Phenomena and Functions of Organic Life. To which is prefixed an Essay on the Great Departments of Human Knowledge, &c. By J. STEVENSON BUSHMAN, M. D., 102 illustrations; pp. 234. Philadelphia: Blanchard & Lea, 1854. From Mr. T. L. White, Bookseller, 105 Canal street.
- V.—*Principles of Comparative Physiology*: Intended as an Introduction to the Study of Human Physiology, and as a Guide to the Philosophical Pursuit of Natural History; By WILLIAM B. CARPENTER, M. D., F. R. S.; Author of "Principles of Human Physiology," &c. New and much improved Edition; 300 illustrations, pp. 752. Philadelphia: Blanchard & Lea, 1854. From Mr. J. B. Steel, Bookseller, 60 Camp street.
- VI.—*Report of the Sanitary Committee of the Board of Health, upon the subject of Slaughter-Houses and Soap-Boiling Establishments in*

- Cities—their Effects upon Public Health; together with the Location of Cow-Stables in Cities, and the Effects of Feeding of Still-Slops upon the Milk, &c. Made to the Board, August 22, 1854. Pp. 15. Philadelphia.
- VII.—*Poisoned Wounds*: Their Distinctive Features, Classification, with Remarks upon the Classes, and a Special Treatise upon the Nature and Treatment of the Wounds resulting from the Bites of Venomous Reptiles. Experiments, &c. Being a Report of a Committee to the Medical Association of Missouri: By A. F. JETER, M. D., of Palmyra, Mo. Pp. 32. Quincy, 1854.
- VIII.—*Observations on some of the Remedial Properties of Simaba Cedron*, and on its Employment in Intermittent Fever: By S. S. PURPLE, M. D. Pp. 16. New York, 1854.
- IX.—*Charge to the Graduates of Jefferson Medical College of Philadelphia*: Delivered March 11, 1854, by Professor ROBLEY DUNGLISON. Pp. 15.
- X.—*Illustrated Catalogue of Blanchard & Lea's Medical, Surgical and Scientific Publications*. Pp. 64.
- XI.—*Elkoplasty, or Anaplasty applied to the Treatment of Old Ulcers*. Also, A New Mode of Treatment for Delayed or Non-Union of a Fractured Humerns: By FRANK H. HAMILTON, A. M., M. D., Professor of Surgery in the Medical Department of the University of Buffalo, Surgeon to the Buffalo Hospital of Sisters of Charity. Pp. 19. New York, 1854.
- XII.—*A Monograph on the Fatal Circulation*: With three illustrations on wood: By E. R. PEASLEE, A. M., M. D., Professor of Anatomy in Dartmouth College and in the New York Medical College, and of Surgery in the Medical School of Maine. From the American Medical Monthly, May, 1854. Pp. 26. New York.
- XIII.—*Report of the Special Committee of the Board of Regents of the Smithsonian Institution*. Pp. 21. 1854.
- XIV.—*Quarterly Summary of the Transactions of the College of Physicians of Philadelphia*. From May 3, 1854, to July 19, 1854, inclusive.
- XV.—*Medicinal Properties of Bailey's Spring, Lauderdale County, Ala., and Cooper's Well, Hinds County, Miss.* By S. C. FARRAR, M. D., of Jackson, Miss. Pp. 24.

- XVI.—*Observations on the Asiatic Cholera, as it appeared in Cincinnati, in 1849-'50.* By THOMAS CARROLL, M. D., reprinted from the *Western Lancet* for June, 1854. Pp. 75.
- XVII.—*Difficult Labors and their Treatment:* By M. B. WRIGHT, M. D., of Cincinnati. For which a Gold Medal was awarded by the Ohio State Medical Society. Pp. 32. Cincinnati, 1854.
- XVIII.—*Proceedings of the New Orleans Academy of Sciences, 1854.* Pp. 71.
- XIX.—*Constitution and By-Laws of the same.* Pp. 22.
- XX.—*Proceedings of the Boston Society of Natural History.* January, 1854. Pp. 16.
- XXI.—*Proceedings of the Academy of Natural Sciences of Philadelphia.* January to August, 1854. Pp. 44.

COMMUNICATIONS RECEIVED.

Communications have been received from Drs. WOOTEN, BOZEMAN, FENNER, POWELL, PALMER and J. R. DOWLER.

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THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL,
FOR NOVEMBER, 1854.

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Part First.  
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ORIGINAL COMMUNICATIONS. ✓

ART. 1.—ON THE TREATMENT OF YELLOW FEVER, AS IT OCCURRED
IN MOBILE, IN THE FALL OF 1853.

BY N. WALKLY, M. D.

Mr. Editor:—In the last number of the transactions of the Alabama State Medical Association, I notice several reports on the yellow fever of last year, in Mobile and the State of Alabama; each of them detailing the treatment usually pursued; and no one of them willing to adopt any other treatment than that which they had found so successful. If these gentlemen had given any statistics, whereby we might ascertain the relative mortality of those treated by them, we might make a comparative estimate of the value of the modes of treatment pursued. The reporter from this city, Dr. Wm. H. Anderson, (an extract from his report is published in the July number of your Journal,) states that himself and his associate in business treated, by the quinine treatment, as it is called, eleven hundred cases; but gave no account of the fatality attending that mode of treatment.

In the early part of the epidemic, I pursued the identical course of treatment detailed by him, and was much pleased with its effect in the early stage of the disease; but after the fever left, there was a tendency of the patients to sink, and their stomachs were so irritable that no

mode of stimulation that I could adopt, would keep them up; and I lost forty-two per cent. of all thus treated. I soon concluded that the treatment was not the best that could be adopted, and fell back to the expectant plan of treatment, which I had used in previous epidemics, and continued it through the balance of the season. I will now give the details of that treatment, as pursued by me, and the result.

Every one who saw the late epidemic, must have been convinced that the disease was not identical, nor had any affinity with our ordinary endemics, (supposed to be occasioned by malaria,) but that the disease was "*sui generis*," and was not caused, or much modified, by atmospheric or other changes, except that of temperature, (it is usually extinguished by a temperature beyond 40° Fahrenheit,) and that it would run its course in spite of all violent treatment, except that which caused death. It was therefore evident, that until we could find an antidote for the specific poison occasioning the disease, the indications were to adopt such a plan of treatment as would not obstruct, but rather assist nature in her efforts to throw off the disease.

In most of the cases attacked, it was found that the action of the stomach was suspended for several hours before the manifestation of the disease, as an emetic would usually unload the stomach of the food taken at the one or two previous meals, undigested. Consequently, in those cases where a loaded state of the stomach existed, an emetic was indicated, followed by a mild purgative; these, if given at all, were administered during the first hours of the fever, before the gastric congestion was set up; and if not administered at first, were dispensed with altogether. The reasons for dispensing with the evacuant, if not administered in the first four or five hours, are these: The stomach soon become so much congested that nothing would lie on it, or very little, except calomel, and that would frequently be thrown up; and if it was not ejected, the stomach was so paralyzed by the disease, that the medicine was not acted on, and remained in the stomach to increase its irritability, until after the fever had left; and then it prostrated the patient by salivation or hyper-purgation. If a loaded state of the bowels existed, an enema was sometimes administered if the first dose of medicine did not operate in five or six hours. Its object was merely to relieve the bowels, so as to render it unnecessary for the patient to get up until he was strong enough to do so without fainting; and no operation was deemed of advantage

until after the expiration of the fourth twenty-four hours from the onset of the disease.

The excessive pain in the head and back, was temporarily relieved by a hot mustard foot bath. A deep tub or bucket containing it, was placed on a chair beside the bed, and the patient turned round so as to let his feet hang in it, and remain in the bath for twenty or thirty minutes, or until the head and back were relieved. This usually afforded relief for three or four hours, and then was repeated, during the febrile stage of the disease.

In addition to the diaphoretic effect of the bath, it acted as a derivative, by withdrawing from the head to the lower extremities the excess of circulation, thus temporarily relieving its congestion, and affording great comfort to the patient. As I have before remarked, the stomach would retain but little if anything without becoming irritated; but it usually bore cold water or ice better than anything else; yet, if much of that was given it was soon ejected. Some of the physicians who pursued the general treatment pursued by me were partial to warm teas, orange leaf and sage teas, given in small quantities at a time, and found them of advantage as diaphoretics. But it was of great disadvantage to load the stomach with much of anything, as, if that organ was distended by anything, vomiting was induced, which was hard to check. As the action of the salivary glands, together with all the other glands of the system was suspended during the febrile stage of the disease, the mouth and larynx became dry and parched, and a sensation of great thirst would be complained of, which would seem to call for the special action of mercury, to increase their secretions; but experience taught that no advantage was derived by its administration, for reasons expressed before, viz: it remained inert in the stomach, and was not absorbed until there was no use for it, that is, until after the fever had run its course. The thirst was best appeased by wetting the mouth frequently with iced water, swallowing as little as possible.

No nourishment whatever was administered in the first or febrile stage, and nothing else was administered during the 30 or 40 hours that the fever lasted, except as above specified.

After the fever had left, the patient was watched, and if it was craved, light nourishment was administered in small quantities. If any irritability of the stomach existed, it was quieted by the internal administration of

carminatives, an infusion of serp. Virginiana, water slightly acidulated with aromat. sulphuric acid, &c., and the external application of spice plasters, mustard, and other rubefacients over the epigastrium.

On the third night, seldom sooner, between eleven o'clock at night and four in the morning, the patients, if they did not commence convalescing before that time, showed a tendency to sink, commenced sighing, and their pulse showed a tendency to diminish in frequency and volume. This stage needed to be watched for, and met with appropriate stimulants. Usually brandy and water set best on the stomach; some liked mint juleps; some could not bear either, and could take champagne, or cider, porter, or ale, and frequently we would have to run through the whole catalogue of stimulants to get one that would stick; and then failing, would have to resort to sponging with tr. cinchona and tr. capsicum, equal parts, or some other external stimulant, and failing with them, resort to blisters. I made it a rule to see every patient between midnight and morning of the third and fourth nights, and I have no doubt but many were saved thereby, as at that hour nurses are apt to be sleepy and careless, and most of them are disposed to think their patients are out of danger when the fever leaves.

In administering stimulants care should be taken in not administering too much, as that would induce gastritis, which is not desirable. Usually convalescence was established by the close of the third twenty-four hours, and they were, in a majority of cases, discharged on the fifth or sixth day.

Considerable care was necessary in convalescence, as a little imprudence either from over eating, or exposure, would cause a relapse. If in convalescence, the liver failed in regaining its functions, a little blue mass was exhibited, or if needed, the kidneys were assisted by a diuretic.

I give no mercurials, myself, in these cases, except the small amount of calomel contained in an ordinary dose of the "compound cathartic pills" of the Pharmacopœia, which was the purgative administered in nearly all the cases.

I used no depletion by bleeding; did not apply a leech, or a cup during the season; nor did I see but three patients bled by others, and they all died.

In the commencement of the season it was necessary to give neutral mixture, in teaspoonful doses, as a placebo, to keep the

patients' friends quiet, until the patients themselves had time to get well. By the first of September, however, this could be dispensed with, as most were willing to trust and obey their physicians' orders without question.

In making the comparison below, I would not intimate that my want of success militates against the so called quinine treatment, when properly administered in yellow fever, but that I did not know the secret of administering it properly, or that my cases were those in which it was not applicable.

The disease, in the first stage, evidently is not inflammatory, and does not call for depletion, and will not become inflammatory except from complications, or overloading the stomach. In those cases where black vomit occurred, I found very dilute aromat. sulf. acid, or sour butter milk, in teaspoonful doses every few minutes, of more value than kreasote, matico, or anything else that I could use; but cases treated as above were very rare.

By this simple plan of treatment, between the 12th of August, and the 1st of November, 1853, I treated three hundred and thirty-six patients consecutively, and out of that number ten died, or a little less than three per cent.

The average duration of the febrile stage, in adults, in the above, three hundred and thirty-six cases was thirty hours; the longest forty hours, and the shortest twenty-six; of children under twelve years, the average was twenty-six hours; the longest twenty-eight hours, and the shortest twelve; the younger the children were, the shorter the fever ordinarily.

In the twelve quinine cases, nine were adults, and three were under twelve years of age. The duration of fever in the adults was an average of thirty-nine hours; the longest seventy-two hours, and the shortest twenty-four.

The average of children was twenty-four hours; longest twenty-eight, and shortest twenty hours.

I am well convinced that excessive medication by inducing gastritis, extends the febrile stage much longer than it would naturally run, if let alone, and by prostrating the patient, diminishes greatly the chance of recovery.

ART. II.—REMARKS ON THE ADMINISTRATION OF CHLOROFORM.

BY C. S. FENNER, M. D.,

OF MEMPHIS, TENN.

It is now seven years since Dr. Simpson, of Edinburgh, published the results of his experiments, showing the anæsthetic powers of chloroform. These experiments were repeated by others, with results so satisfactory, that in the brief period of a few months, the use of chloroform became common throughout the entire civilized world, and met with such universal favor, that it was generally resorted to preparatory not only to the severer, but also to the most trifling surgical operations, such as the opening of an abscess, or the extracting of a tooth. Very soon, numerous instances were reported, in which death resulted from its administration; a majority of these cases occurred in the hands of dental surgeons, and those who were not regular members of the medical profession; but the same unfortunate result occasionally happened to men of the highest professional skill and learning, who had had extensive experience with its administration, and whose standing was a sufficient guaranty that every precaution had been used to prevent unpleasant results. These cases became so numerous that many eminent physicians entirely abandoned its use; while others administered it with great caution, and only to relieve the pain attending the more severe surgical operations, or to quiet convulsive muscular contractions.

At the present time, there exist a great contrariety of opinions in regard to the safety of chloroform as an anæsthetic agent; all, however, agree that there are some peculiarities of constitution to which it is decidedly inimical. To detect such idiosyncrasy, in all instances, before trial, is a desideratum, the present state of our knowledge of the subject has failed to accomplish. M. Baudens published in the London Lancet, of October, 1853, some very judicious rules for the administration of chloroform; they are the best I have seen, and are well worth a careful perusal by every one in the habit of using this agent; and if strictly adhered to, will, I have no doubt, considerably diminish the dangers attending its use.

M. Bauden's rules are republished from the Lancet, in the January number of the American Journal of the Medical Sciences, page 208.

I have used chloroform, principally as a remedy for the pain attending surgical operations, since 1848, and generally with the happiest effect; two or three instances, however, have occurred that have considerably shaken my confidence in the safety of the remedy, and have induced me to administer it very slowly, and largely diluted with atmospheric air; at least at first, and closely to watch its effects, always keeping the finger of one hand on the radial or temporal artery while the inhalation is going on.

In August, 1851, I visited a negro woman about forty years of age, whose perinæum had been ruptured during parturition, throwing the rectum and vagina into one opening, for some three or four inches; she had been in this condition for several years, constantly confined to her bed—had suffered much, and at the time I saw her was in feeble health. She, as well as her master, was anxious that an effort should be made to relieve her, which could be done only by denuding the ruptured surface, bringing the parts together, and confining them, so as to facilitate union by the first intention. She inhaled chloroform from a towel; anæsthesia was speedily induced, without material change in the pulse or respiration. When the operation was about half completed, she returned to perfect consciousness, conversed freely and intelligibly, and asked to be put to sleep again, which I declined doing, and proceeded with the operation. In some ten or twelve minutes, her pulse began to fail, and soon became extinct; immediately after which, respiration ceased. I dashed several dippers of cold water in her face, without reviving her, and then resorted to artificial respiration, by placing my mouth to her lips, inflating her lungs, and with the hands on her chest forcing out the air. This was continued regularly, by myself and others, for half an hour; but to no purpose. I, at the time, attributed her death to the shock of the operation on her system, debilitated and worn out by long suffering; and was sustained in this opinion by several highly intelligent medical gentlemen, who honored me with their presence and assistance. Still, she had taken chloroform, and it may have exerted a deleterious influence.

In October, 1852, an active healthy negro girl, 12 years of age, was brought to me from a distance, with a long and deep cicatrix on the side of her neck, disfiguring her person, and greatly impeding the natural motions of the head. I intended to dissect out the cicatrix, and if possible, to draw the edges of the sound skin together. She was placed on

a table, and a folded towel on which were thirty or forty drops of chloroform, held within three or four inches of her nose, one edge of the towel resting on her chin; after four or five inspirations, her pulse suddenly sank so as to be scarcely perceptible; her respiration became convulsive; her extremities assumed an icy coldness, and her face presented an exceedingly pallid appearance; the towel was immediately removed and cold water dashed in her face; she gradually revived, but respiration continued embarrassed for several minutes. I have no doubt but death would have ensued, had she inhaled the chloroform a few moments longer.

The next case was still more marked, the patient recovering after suspended animation had lasted several minutes.—July 5, 1854, a gentleman of dark complexion, and decidedly bilious temperament, about 28 years of age, called at my office, requesting me to perform an operation on him for phimosis, with adhesion of prepuce to the entire surface of the glans, and to the lips of the urethra. As the operation would be attended with considerable suffering, owing to the extreme sensibility of the organ, I suggested the propriety of his inhaling chloroform sufficient to deaden the pain, but not to produce its full anæsthetic effect. He was pleased with the idea, and procured an ordinary half ounce prescription phial, nearly full of chloroform. His health was good, and he had never suffered from cerebral, cardiac, or pulmonary disease. He was placed on his back, and a few drops of chloroform, largely diluted with atmospheric air, inhaled from a folded towel. As soon as I discovered the first symptoms of anæsthesia, I began the operation, the patient at the same time conscious of suffering considerable pain. As I was dissecting the prepuce, from the glans, my attention was arrested by the extreme slowness of his respiration; I placed my fingers on his wrist and found him pulseless, and breathing not more than four or five times in a minute; I dashed cold water in his face which produced two or three rapid, convulsive inspirations, when he ceased to breathe, having every appearance of being dead; I instantly resorted to artificial respiration, and kept it up for several minutes; directing an attendant to procure a bottle of ammonia, but before he returned, the patient revived, and although he had been apparently lifeless, was conscious of every thing that had been said and done; after he revived I completed the operation. The quantity of chloroform inhaled could scarcely have exceeded one fluidrachm, for the phial still re-

mained two-thirds full ; a very large part of that used must have evaporated without having passed to his lungs.

The quantity of chloroform required to produce anæsthesia, varies greatly in different individuals ; some become insensible in two or three minutes from the inhalation of thirty or forty minims, while others require three or four fluidrachms, or even more, and have to breathe from fifteen to twenty minutes to produce a similar effect ; indeed I have had some few cases (generally persons of sanguine temperament, of light complexion, and light or red hair,) that I could not carry beyond the second period, or stage of excitement.

April, 1853, I attempted to induce anæsthesia in a gentleman of strongly marked nervo-sanguine temperament, preparatory to an operation for fistula in ano, and utterly failed, although I continued the effort for half an hour, and used more than a fluidounce of chloroform ; it produced great arterial excitement, with determination of blood to the brain ; the patient complaining of a peculiar sensation of pressure at the top of the head, but was perfectly rational all the time, and acutely sensitive to the touch of the knife. It is sometimes the case, that when it is difficult, or deemed unsafe to go beyond the period of excitement, an operation may be performed, and although the patient may complain bitterly of pain, yet when he comes to himself, will have no recollection of having suffered at all.

September, 1853, I gave chloroform to a plethoric, obese, old gentleman, but could not induce full anæsthesia, and removed a large steatomatous tumor from his back ; he struggled violently, requiring three or four assistants to keep him on the table during the operation ; but when the effects of the chloroform passed away, he had no recollection of having suffered any pain, and was not even aware that the tumor had been removed.

There seems at the present time a growing disposition to substitute some other article for chloroform, and sulphuric æther seems to be more generally preferred, as its administration thus far, has, as far as I am aware, been unattended with fatal results, which is certainly a powerful recommendation in its favor. But if no absolutely safe substitute for chloroform could be found, I still should regard it as a great boon, and should feel justified in resorting to its use, in cases where it is evidently not contra-indicated, preparatory to severe and painful surgical operations, such as are likely to produce a powerful shock to the system.

I regard the risk as partially counterbalanced by the lessening of the

danger attending the severe shock to the system, and the quieting of mental excitement usual in such cases. Persons have died from the extraction of a tooth, and not unfrequently, from bleeding; yet the risk attending these operations is too small to be calculated. No one would withhold the use of the forceps or the lancet for one moment, in cases in which they are indicated, from any fear of fatal hæmorrhage or tetanus in the former, or phlebitis in the latter. So with mercury; the cases are numerous in which it has been productive of the worst results, if not proving fatal; still, from its local action on the jaws and face, causing caries of the facial bones, immobility of the lower jaw, sloughing of the lips, cheeks, &c., often leaving the patient in such condition that death would be preferable; yet, notwithstanding these unfortunate effects, the benefit derived from mercury is so evident that I trust very few educated physicians would wish to banish it from the *Materia Medica*, because it cannot be prescribed with absolute safety. The same may be said of many other medical agents, and chloroform forms no exception. When administered by an experienced hand, and proper judgment exercised in selecting suitable subjects, the danger attending its administration will be so small as scarcely to be calculated, when we take into consideration the benefit derived from its use, in relieving an immense amount of human suffering.

ART. III.—CASES OF CONGESTION OF THE BRAIN.

BY B. BROOKS, M. D.

OF WHEELLOCK, TEXAS.

Case 1.—On the morning of July 9, 1854, was requested to see a stout, robust negro man, about thirty years of age, who had been taken ill on the preceding afternoon. Had had a slight chill, after which he soon become comatose, and remained in that condition until I saw him, the following morning.

His extremities were cold; pulse feeble, beating 100; profuse perspiration; bowels constipated; involuntary discharges of urine; head hot; pupils a little dilated; carotid arteries throbbing violently:—Treatment—bled him twenty-four ounces from the temporal artery; applied mustard to forehead, back of the neck, along the spine, and to the wrists and ankles; gave brandy and carbonate of ammonia freely;

had his legs and arms rubbed with a strong decoction of red pepper, though all without any good effect, for the patient sunk and died in about four hours after I first saw him.

Case 1.—On the 23d of August, was called to see T. B., 15 years of age, a stout, healthy young man, who had been attacked about three hours previously. He had gone in to the watermelon patch, for the purpose of getting a watermelon, and when found, an hour afterwards, was speechless, and entirely insensible. Found him exceedingly restless, requiring force to hold him on the bed; extremities cold; pulse feeble, and beating 90; skin dry; pupils natural; jaws almost immovable; bowels constipated; involuntary discharge of urine. Applied mustard to the forehead, back of the neck, spine, wrists and ankles, keeping it on until blisters were drawn. The jaws were so firmly set that it was with considerable difficulty they could be forced open sufficiently with a spoonhandle, to introduce medicine into the mouth. Gave five grains carbonate ammonia every hour, until the circulation was equalized. To quiet restlessness, dissolved extract hyosciamus in water, and gave a dose every hour. In about ten or twelve hours after I first saw the patient, there appeared to be a slight remission in the disease, at which time commenced giving quinine in five grain doses every two hours, continuing it until four doses had been taken. Upon visiting my patient next morning, found him still speechless and insensible, though his pulse was reduced to 80, and had more volume than on the preceding day. I should have given calomel on the day he was attacked, had it not have been so difficult to get him to swallow anything, even in a fluid state. This morning, (24th,) his bowels had not been moved; I, therefore, ordered twenty grains of calomel, with two drops of croton oil, to be taken at once. Probably not more than half the dose was swallowed, though sufficient to produce, in the course of two or three hours, four or five large bilious evacuations, which were discharged involuntarily. Continued the quinine in five grain doses, lengthening the interval to three, and finally four hours. In the afternoon, after a short, but refreshing nap of sleep, the patient asked for water, being the first time he had spoken since his attack. Upon handing him the water, I perceived that his jaws had become movable, and that he could drink without difficulty. I now commenced giving calomel, in two grain doses, every four hours, which, together

with the quinine, was continued a couple of days. At the end of four days, the patient was able to sit up in bed, and from this time on, convalesced without an unfavorable symptom.

The first remedy that suggested itself to me, upon seeing this patient, was the abstraction of blood from the temporal artery, or one of its branches, but having seen this remedy tried in several cases, with doubtful effect, I determined upon the course above indicated, and put it in requisition at once. If congestive fever or congestion of the brain be a disease of the nervous system, as suggested by some writers, it occurs to me that the loss of blood has a tendency to increase the prostration that exists, thereby rendering the disease more dangerous. I combined the croton oil with the calomel in order to facilitate its action, thereby hoping to produce a revulsive effect upon the alimentary canal, and I was not disappointed in my expectation, for it certainly exercised a very salutary influence over the disease.

ART. IV—A REPLY TO DR. BOLING'S EXPERIMENTS

With Phosphorus, and his Remarks upon its Dose and Action when given in the form of Alcoholic Solution or Tincture.

BY S. AMES, M. D.

An account of these experiments was published in the May number of this Journal for this year (1854.) They were instituted, it seems, while Dr. Boling was in search of a cardiac sedative equally prompt, powerful and reliable as the veratrum viride, but without the objections which, in his opinion, pertain to the properties of that remedy. After trying by experiments on healthy persons whether the yellow jessamine would not furnish the kind of sedative required, and being disappointed, he was induced by my paper on the treatment of pneumonia, published in the last January number of this Journal, to make similar experiments, also on well persons, in order to test the sedative properties of phosphorus in an alcoholic solution, the remedial operation of which is there ascribed to a sedative action. The experiments thus prompted led Dr. Boling to three principal conclusions, viz :

1. That phosphorus is not a sedative ;
2. That it is not a stimulant ; and

3. That it is not poisonous when given in an alcoholic solution or tincture.

These conclusions, it seems, were derived exclusively from these experiments. Dr. Boling expressly disclaims having any experience of the properties of phosphorus in acute diseases in the concluding paragraph of his paper, which I here copy :

“In conclusion,” he says: “I would here remark that though prior to these experiments I had taken and given a good deal of the alcoholic solution of phosphorus, I have never in any instance given them (it) in a case of acute or dangerous disease.”

The first and third of these conclusions, broadly as they are expressed in Dr. Boling's paper, including the action of this elementary substance both in health and disease, are so directly opposed to the results of my own experience of its action in inflammatory affections of the lungs, but especially in pneumonia, that there is a seeming obligation on my part, in the absence of any attempt of the kind on the part of Dr. Boling, to account for the discrepancies, apparent or real, between the results of his experiments on healthy subjects and of my experience at the bedside.

Dr. Boling has, indeed, invited such an investigation in so many words; but I am influenced in undertaking it less by any desire to detect and point out, to adopt his language, any fault or deficiency in the mode of conducting the experiments, though this easy task becomes necessary for the purpose of explaining the difference between his facts and mine, than to establish or confirm in the minds of others a just confidence in the remedial powers of phosphorus, which, in some acute affections of the lungs, are, in my opinion, equaled by but one other remedy. In carrying out this object, I expect to show that the experiments neither justify the conclusions based on them, I allude especially to the first and third, nor in any material respect affect the correctness of my own observations, made in a different field of inquiry, and in altogether different circumstances.

It is important to keep in view these several conclusions, in the order here set down, an arrangement not adopted in the paper under review, as for the sake of perspicuity it is required to examine the experiments and the remarks on them in reference to each conclusion separately.

The first question before us then is as to the proof afforded by these experiments that *phosphorus is not a sedative*.

Dr. Boling selected two subjects for his trials with this medicine: one a

mulatto child, seven years old; the other an adult black, under treatment by means of the bandage and a recumbent posture, for an ulcer of the leg, but otherwise in good health. Besides these, Dr. Boling submitted himself to some experiments, but, as they are noticed by him only in a general way, and more in reference to its poisonous than sedative action, their consideration is deferred for the present.

Before proceeding to the immediate examination of the experiments, I wish to bring into notice a matter of some interest incidentally connected with the subject now before us. From the mode in which the experiments were conducted, as well as from a portion of the comments thereon, I am induced to think that Dr. Boling had misconceived my opinion in regard to the power of phosphorus over the action of the heart, or as a "cardiac sedative." The experiments seem to have been begun and carried on in the expectation, or the hope, of finding in this article a sedative prompt and powerful in health, as well as in disease, like veratrum viride, and, consequently capable of overcoming any of the ordinary physiological influences over the pulse. Now, if this be true, and the opinion was derived from any thing I have said, it is certainly a misconception of my views of its properties which requires to be corrected. By referring to my paper on the treatment of pneumonia it will be found that I have nowhere spoken of phosphorus as an active sedative agent over the action of the heart. In all that is there said of the *dose* of phosphorus no mention is made, even of its sedative powers; and this it may be seen is in striking contrast with what is said of the saturated tincture of aconite in speaking of the dose of that article. As regards the latter a great deal of pains was taken to point out that its remedial and poisonous action is the same, namely, *sedative*, and that on this account great caution was required in giving it; while in regard to the former, no notice is taken of its sedative action in connexion with any possible injurious effects that might arise from it on this account. The only reference to its sedative properties occurs in attempting to account for the mode of its curative operation in diseases of the lungs, and here its sedative action is spoken of only in connexion with its medicinal action, or its action in medicinal doses, the latter being contrasted with its poisonous action which is *not* sedative. My opinion of the extent and kind of sedative power it exerts in disease, is expressed in the following extract, from page 431, (vol. 10th of this Journal.) "Its action on the lungs seems from its effects to be

espeially directed to the minute bronchial tubes and the air cells; and in inflammation to the capillary vessels rather than to the heart;" that is, though showing some direct sedative influence on the action of the heart, its chief action is indirect, through the capillary vessels which are the seat of the inflammation. At two or three other places I have spoken of its sedative influence over the general circulation, in medicinal doses in contrast with its poisonous action in larger doses, which is said by all authors to be stimulant, but always of course in reference to this opinion as to the mode of its sedative action, but have never represented it as existing to any poisonous extent. All the warnings that I gave, and they might with the greatest propriety be repeated here in view of the experiments of Dr. Boling, in regard to the danger of too large doses, founded more on what I had read than of what I had seen its effects, refer exclusively to its poisonous action which is of another and different kind.

A misconception of this kind might possibly arise, not from any direct remark of mine attributing to phosphorus great powers as a direct sedative in disease, and much less in health, about which I expressed no opinion at all, but as an inference from the following passage: "There is a point at which it (phosphorus) ceases to be medicinal, or sedative, and becomes poisonous, or stimulant. Thus it is not possible to produce with it the extreme depression which follows large doses of aconite; for when the dose is enlarged for this purpose beyond a certain point a new and opposite action is set up in which the (sedative) power is lost or merged in the local inflammation it excites and its concomitant influence on the nervous system and the general circulation." This is strictly true in fact, but the inference which the words may bear, for the opinion does not seem carefully worded, that its poisonous is the only limit to its sedative action, is certainly erroneous. But then, such an inference is contradicted, it seems to me, by everything I said on the subject elsewhere.

Having set this matter right, so far as any misconception of the degree and kind of sedative power of this article, for I am satisfied that its direct power of controlling the action of the heart is very limited, may have arisen from any inadvertence of mine, I take up now the examination into any sources of fallacy to be found in Dr. Boling's experiments, so far as they relate to the sedative operation of phosphorous on persons in health and disease.

In all experimental trials of this kind, it is to be remembered that the state of the pulse is the thing to be observed, and is alone to be looked to for any indication of the effect of the medicine given; and consequently, that all collateral inducements to changes in the action of the heart should be carefully avoided. Such being the case, it is not without some surprise that one observes in the record of these experiments an absence of any especial care to avoid those normal causes influencing the state of the pulse, which, being common to all persons, of whatever age, are almost constantly in operation during our waking hours. Indeed, their possible influence seems to have been overlooked, except in the simple instance of the recumbent posture, which is mentioned apparently in view of its effect on the pulse; though the difference between that state and an erect position or active exercise in connection with such effects is not alluded to. The presence of these causes is mentioned, but not in this connection. Thus, it is found in several instances, that nearly an entire day is consumed in an experiment; the pulse being counted almost every hour, without any mention of the time of taking food, though it is said generally, that the subjects took their meals as usual; so, too, we are told that the experiment was begun sometimes immediately before, at others immediately after eating; and again in the middle of the intervals between the meals; and on one occasion it is especially stated that the medicine was given soon after a hearty breakfast; so, too, in regard to posture and muscular exertion; one of the subjects was generally called in from play, when the pulse was to be counted or the dose given; or was riding, in the course of the experiment, in his master's buggy, and attending to his horse at the different stopping places; and in one experiment he was asleep a part of the time and awake at another. In all these instances the facts are spoken of without allusion to the certain influence over the pulse, of alternations of fasting and repletion, of active muscular exercise and rest, and of sleeping and waking. Yet these are well known to be active sources of perturbation in the action of the heart, which if not voidable, ought, it would seem, to have been carefully noted, and, as far as possible, taken into the estimate in counting upon the results.

It is plain that under such circumstances, so certain usually to produce considerable changes in the pulse, moderate powers in the medicine, whether sedative or stimulant, might not be manifested at all; or at least not so manifested as to afford reliable proof one way or the other. It is

well known, in fact, that the influence of digestion and muscular exertion over the action of the heart is frequently sufficient to overcome, or render insensible, the power of some pretty active medicinal agents.

But the influence of these common physiological causes, though requiring to be noticed as one of the elements of fallacy, is of much less moment in this discussion than they would otherwise be, on account of the obvious presence of another cause, operating more powerfully to disturb the pulse, and, consequently, affecting more decidedly the results of the experiments. The evidence of the presence of such a cause, and of its controlling influence in both subjects, not only over the pulse, but over the action of the ordinary physiological causes, is found in the record of the experiments; and to the details necessary to point this out, I wish for a brief space to call especial attention.

The two first experiments on the little boy, "Sam," are very significant of the singular fact just referred to:

"On the 25th of February, having kept him (Sam) in a recumbent posture some time, his pulse being at 102, at one o'clock, P. M., I gave him seven drops of the saturated tincture (of phosphorus); at two o'clock, P. M., pulse variable, from 90 to 108 (eighteen beats while counting it); three, P. M., (subject asleep) pulse 94; the dose of seven drops repeated; four, P. M., (subject asleep) pulse 104.

"February 26th, ten o'clock, P. M., pulse 100; one and a half o'clock, pulse 114; two o'clock, pulse 100" — (a rise and fall of fourteen beats at intervals of half an hour.) "No phosphorus given to-day. The experiments already given were conducted with the subject in the recumbent posture. In the following, he was generally called in from play, when a dose was given or the pulse to be counted."

In these two experiments the subject appears to have been, more than in any of the subsequent ones, freed from those ordinary incidents which operate to derange the action of the heart in health. For example, he was lying down all the time, so that no influence could have been exerted by changes of posture, or of more active muscular exertion. Again in the first experiment he was asleep at two consecutive countings; the pulse still exhibiting the same tendency to change as when awake; it being found at the last counting, ten beats quicker than when counted while asleep one hour before. Thus while the tendency of sleep is to reduce the frequency of the pulse and give it uniformity, it is found to be not

only faster in this subject, when asleep at one time than another, but faster by eight beats when asleep at four o'clock, than when awake at one o'clock. The cause of these changes was therefore sufficient to overcome the usual sedative influence of sleep—a very powerful one, too. We are not told whether "Sam" took any food shortly before or after one o'clock, on either day, nor is it material here to know; for if so, its effect could have been only to give a uniform acceleration to the pulse while the process of digestion was going on; not, certainly, to produce this kind of vacillation. There is still another appreciable physiological cause, whose operation would be more likely than any other to bring about this kind of changes, and that is mental agitation; but we have no direct evidence of its presence. If present, it would probably have shown its presence by other signs, which could hardly have failed to attract the attention of Dr. Boling, who does not, however, allude to it. Nor could the medicine given have had any influence to derange the pulse in this way; for seeing that the changes in these and other experiments, occurred equally when the phosphorus was or was not given, and equally when given in comparatively small or large doses. Dr. Boling, himself, very properly excludes it from any agency in producing them.

Here, then, we find in two experiments, one of them occupying three hours, and the other one hour only, an extreme variation in the pulse of eighteen beats in the minute in the one, and of fourteen in the other; while in both the pulse rose and fell so suddenly as not to be really the same at any two consecutive countings; and this when all the ordinary incidents affecting the rhythm of the heart's action, were either absent or obviously inoperative to produce such effects. Excluding, therefore, all these ordinary incidents, what other conclusion is left, the subject being in good health, than that there was in him some inappreciable peculiarity or idiosyncrasy of considerable force, operating to produce these striking anomalies in the heart's action.

In the subsequent experiments on "Sam," the circumstances were less favorable for the manifestation of this peculiarity; one of the ordinary physiological influences, namely, muscular exertion; and one extraordinary one, not physiological, namely, large doses of alcohol, were present in these and absent in the others. The influence of the alcohol may be estimated with some assurance of accuracy, for, being the vehicle for the doses of phosphorus, the time of giving it, and the quantity, are noted; but

we have no means of estimating the probable effect on the pulse, of the additional physiological cause for its changeableness. We are not told, for instance, in which of the experiments he was called in from play to take the experimental dose, or to have his pulse counted; nor whether, when called in from play or at other times; the pulse was counted while standing, or in some other position. All the information we get on these important points, so sure ordinarily to derange the pulse, is that he was *generally* called in from play. So too in the experiments which were partly conducted while the subject was riding in the buggy with his master, no especial reference is made to the circumstances in which the state of the pulse was noted. Nor have we any information in this, any more than in the other experiments, at what time "Sam" took his meals, nor in which he did or did not take food, though we are assured that both subjects took their meals as usual, and with their usual appetite and relish, and of course, in the usual quantity and variety; but, as we do not know the hours of breakfast, dinner, or supper, we cannot apply this general assurance to the particular experiments. These incidents, I hardly need say, are of a kind to have had a decided influence over the pulse, but whether they did so in this instance; or when, and to what extent; are things that we are not permitted to bring into the estimate, for want of these essential details. Nevertheless, an analysis of the experiments as we find them recorded, brings into view, in despite of all counteracting causes, the manifestation of the same controlling powers that is so plainly exhibited in the former experiments.

The first evidence of this that I shall notice is, that the pulse took a *lower* range, and was not more variable, in these subsequent experiments, when the pulse should have been accelerated by an erect posture, or the more active muscular exertion of boyish play, than in the two first, when the subject was lying down all the time, and asleep a part of the time. Thus the average of the two first experiments is respectively, leaving out the fractions, ninety-nine and one hundred and four beats to the minute; while the highest average in the subsequent ones, excluding the effect of overpowering doses of alcohol, was one hundred, and the lowest ninety; while the general average in the former, the subject being at rest, is one hundred and two. The general average in the latter, when he was engaged in active exercise, actually falls to ninety-six. Why did changes of posture, and the alternations of rest and muscular exercise, fail to

have their usual influence in quickening the pulse? The pulse ought to have been very much accelerated, but we find it slower. How could this happen unless there had been some other cause at work more powerful than they, in its influence over the action of the heart.

Secondly: the suddenness and extent of the changes in the pulse. Thus: March 10th, the pulse at 12, M., was one hundred; at 1, P. M., it varied from one hundred, to one hundred and six. This means, probably, that the variation occurred while the pulse was being counted at that time. At 2, P. M., pulse one hundred and four; 3, P. M., pulse one hundred and eight; 4½, P. M., pulse ninety-two; 6, P. M., pulse eighty-eight,—a fall of twenty beats in the minute, in three hours; and *five minutes* later, ninety-six.

Thirdly: similar changes occurred while the subject was under the influence of large doses of a powerful stimulant. Thus: March 12th, at 5, and at 5½ o'clock, P. M., "Sam" took, at each time, two hundred drops of anhydrous alcohol,—the vehicle for the doses of phosphorus then given,—that is about four drachms of anhydrous alcohol in the course of half an hour. The result as regards the pulse is thus noted: 5, P. M., pulse one hundred—two hundred drops given; 5½, P. M., pulse one hundred—two hundred drops given; 6¼, P. M., pulse one hundred and seven; 7½, P. M., pulse ninety-four.

That is to say, the pulse rose seven and *fell* thirteen beats in the minute, within two hours after taking the last dose of alcohol; and of course, while the subject was still under its influence. Now, it would seem hardly possible that the pulse could have fallen so much, for it is to be noticed that two hours and a quarter after the last dose, it was six beats slower in the minute than just before the doses of alcohol were taken; or at all, while acted upon by a quantity of this prompt and powerful stimulant, which is equivalent to about an ounce of good brandy, given to a child seven years old; unless there had been some unusual cause of perturbation in the action of the heart.

Another significant fact, leading to the same conclusion, is exhibited in the succeeding experiments. Thus: March 13th, 8, A. M., pulse ninety-two—five hundred drops (more than half an ounce of anhydrous alcohol, the menstruum for the dose of one-tenth of a grain of phosphorus) was given. One hour afterwards, namely, at 9½, A. M., the pulse was still at ninety-two; ten beats *below* the average in the other

experiments, in which the subject took neither an artificial stimulant, nor muscular exercise.

These experiments, if they give us no clue to the nature of the cause of these singular interruptions to the normal rhythm of the heart's action, afford some decisive evidence of its power. We have seen that the vacillations in the pulse occurred under all circumstances, with two exceptions, in both of which a large dose of an active stimulant was operating to control it. The pulse rose and fell almost equally while under the influence of about half an ounce of alcohol, as when none was given; while a larger dose, more than half an ounce, served only to make the pulse regular, and apparently to *reduce* its frequency, for it is found below 92 in but three countings in all the other experiments on this subject. It required a larger dose still to overcome this physiological tendency, to change so far as to exhibit the usual effect of a stimulant on the pulse. Thus: March 17th, 4 P. M., pulse 104, nine hundred and ten (910) drops, "being," says Dr. Boling, "exactly one ounce (of an hydrous alcohol being the menstruum for the dose of phosphorus) given at a dose. And here is the result: half after 4, P. M., pulse 116; 5, P. M., pulse 120; 7, P. M., pulse 104, and the subject feels very well." The dose of alcohol, it is seen, is a very large one, if we take into consideration the tender age of this subject. Good brandy contains less than fifty-three per cent. of alcohol; the dose, therefore, was nearly the equivalent of two ounces of brandy. The age of the subject being seven years, the dose for him should be about one-third of that for a man of middle age; so that the quantity of alcohol taken by Sam, at one dose, was equivalent to nearly six ounces of good brandy for a man of middle age. Yet the pulse was quickened only for the space of something over two hours; at the third hour it was found to be the same as at the time the dose was given. The pulse fell *sixteen* beats in the minutes between the second and third hour. Was this owing to any narcotic effect from the alcohol? It seems not. "The two hours immediately succeeding the two last doses," Dr. Boling tells us, "Sam spent riding in my buggy, and attending to my horse, at the different stopping places, and at the end of the experiment, the subject feels very well."

What estimate, then, shall we make of the power of that cause

perturbing the heart's action, in this subject, when half an ounce (four hundred drops,) of anhydrous alcohol failed to affect it at all, and five drachms (five hundred drops,) served only to steady it, and, apparently, rather to reduce than to augment its frequency, and a full ounce, equivalent to nearly two ounces of brandy for the child, or six ounces for an adult, quickened it but for a short time, something over two hours? May we not at least conclude that a cause, so resisting to such active stimulating influences, may have been sufficient to prevent any manifestation of a sedative power not absolutely poisonous?

Abundant evidence of the same peculiarity is exhibited in the adult subject, in whom this vacillating tendency in the action of the heart, was not less striking than in the other, if due allowance be made for the difference in age. For example:

“March 4th, eleven o'clock, A. M., pulse 80; half-past twelve, P. M., 72; three, P. M., pulse 68. No phosphorus given to-day.”

March 7th, twelve, M., pulse 78; half-past twelve, pulse 68; 80 drops (of the saturated tincture of phosphorus) given; two and a half, P. M., pulse 76.

March 10th, half-past seven, A. M., pulse 76; half-past eight, A. M., pulse 70; ten, A. M., pulse 80; eleven, A. M., pulse 80; twelve, M., pulse 82; half-past one, P. M., pulse 76; two, P. M., pulse 68; three, P. M., pulse 74; half-past four, P. M., pulse 80; six, P. M., pulse 82. No phosphorus administered to-day.

March 11th.—No phosphorus was given, but the pulse being counted at intervals, was found to vary about as it did yesterday.

The extreme range of the pulse is here seen to reach ten, twelve and fourteen beats to the minute, suddenly, and without any apparent cause. The same thing is true generally, of the other experiments on this subject; the pulse in this, as in the other subject, being very seldom the same at any two consecutive countings, no matter how short the time between them. Thus the changes occurred equally when the phosphorus was or was not given; the man was all the time recumbent, so that neither posture nor exercise could affect his pulse; there seems to be no reason to believe that mental agitation operated in this, any more than in the other subject; and finally the pulse did not, in a single instance, exhibit the regular diurnal ebb of the heart's

action from morning to night. The taking of food, by this subject, is particularly spoken of, in connexion with one experiment only, and in this it is worthy of notice, that the pulse was more steady, on this than on any other day, though we find the usual order of the diurnal change reversed, the pulse increasing in frequency, instead of diminishing as the day advanced. Thus :

March 15th, nine o'clock, A. M., pulse 72; 200 drops of the saturated tincture (of phosphorus, holding in solution about one-fourth of a grain,) given; ten, A. M., pulse 72; eleven, A. M., pulse 74; one, P. M., pulse 76. A short time before he took the 200 drops, he ate a hearty breakfast, and at two, P. M., with decided relish, a substantial dinner of bacon, cabbage, potatoes and corn bread; four, P. M., pulse 80.

The unusual regularity of the pulse in this instance may have been accidental; nevertheless, it is singular as a coincidence, that this is the only day in which it exhibited a similar regularity in its variableness; the effects of digestion seeming like the stimulus of the alcohol in the other subject, rather to steady the pulse than to increase its frequency, which is known to be its usual tendency.

Excluding, therefore, in both subjects all known and appreciable causes from any agency in producing these remarkable and peculiar physiological changes in the action of the heart, or making all proper allowance for their effects, we find remaining a degree and kind of eccentric action, whose cause, it is evident, must have had a predominate influence over the result of these experiments. In such a condition of the heart's action, so perseveringly and actively variable, arising from a cause capable of resisting not only the usual physiological stimuli when known to be present, but also the power of a very active medicinal agent, administered in large doses, may we not suppose that even a pretty active sedative influence would be as powerless over the pulse of these subjects as the stimulants proved to be? In truth, this source of fallacy seems to be of that precise kind which would necessarily preclude any other than the negative results actually obtained by Dr. Boling in these experiments.

I have spoken of the variableness of the pulse in these subjects as remarkable and peculiar. Dr. Boling, however seems to view the matter in a different light. "The pulse," he remarks, "as most per-

sons know, and as every one may satisfy himself of, will be found to vary in many persons, if not in every one, somewhat in frequency in the twenty-four hours, and often even when felt at short intervals, will be found so to vary from trivial or inappreciable causes. Now, this is no doubt true within certain limits, and as a general remark, but not true, I think, in its application here. There is a regular diurnal variation in the pulse of all healthy persons, and this is liable to interruptions from several sources, all of which, however, whether trivial or not, are, I believe, well known, and consequently appreciable if the cause be inquired into; and their influence may be estimated ordinarily, in all questions in which the issue may depend on the state of the pulse with sufficient accuracy for all practical purposes, when they cannot be altogether avoided. The error here, therefore, is not so much in the general truth affirmed as in its application to the cases before us. If it was meant that such changes in the pulse as occurred in these subjects are common to persons in health in like circumstances, or indeed, in any circumstances, then the remark, I believe, is not justified by the experience of physicians generally—certainly not by that of professed physiologists who are universally silent on the subject of any changes in the pulse of persons in health occurring independently of those physiological influences, already so frequently referred to, which constitute the recognized sources of interruption to the normal regularity of the heart's action. So far as my individual experience goes it coincides with all the authorities I have consulted on this point. Indeed I do not remember to have seen even in disease such a kind of perturbation in the pulse, so continuous, abrupt, and wide in its range, except in some acute affections of the brain, and, though I am not sure, in some forms of hysteria.

We turn now from the fallacies in the experiments to a more important fallacy in the conclusion from them. The latter is not dependent on the former, but inheres in the conclusion itself, and would be the same if there had been no fallacies in the experiments. The conclusion, which is a general one as to the sedative action of phosphorus, is so stated as to cover two points not necessarily connected with each other, viz: *First*, that phosphorus does not act as a sedative on persons in health; and, *secondly*, that it does not

act as a sedative in disease. The latter is a mere inference from the former, inasmuch as Dr. Boling has not tested the effects of phosphorus in acute diseases in a single instance. It is, therefore, strictly an *a priori* inference, the truth of which has not been distinctly tested by any experiments. The fallacy in the conclusion lies in this inference, from the negation of physiological effects to the negation of therapeutic ones.

The inference is fallacious, because there is no necessary connection between it and the facts on which it is based. It might be true that phosphorus is *not* a sedative in health, and yet it might be true that it *is* a sedative in disease. Medical experience has furnished innumerable examples of the general truth, that negative facts concerning the physiological effects of a medicinal agent afford no reliable proof as to its therapeutic action—and this because the facts *are* negative. This rule is reversed as regards positive facts obtained in the same way. If, for instance, phosphorus had proved to be sedative in these experiments, then the conclusion would have been legitimate and proper that it is also sedative in disease, and this without any other proof. And why? Not certainly in virtue of an *a priori* inference like that of Dr. Boling, from the facts in one set of circumstances to the truth of the same facts in other circumstances, or of any evidence abstractly to be derived from the former facts, but solely in virtue, that *experience* has long ago established the general truth of such influences from positive facts; the law being subject, however, to many exceptions, that the manifest effects of a medicinal agent are equally shown in disease as in health. But no such law applies to negative facts, and hence it is that while in the one case, the experience of medical men has furnished beforehand the necessary experimental evidence to establish the truth in many instances of such an influence from positive facts; such an inference from negative facts, lacking this stored-up proof, requires to be supported by an especial and direct experimental proof in every instance. In the one case the facts indicate a certain truth; in the other but a probable one at best. To establish the latter it is required that the indication from the facts obtained in experiments on well persons, shall be verified by experiments on sick ones. But this Dr. Boling has not, it seems, in this instance thought it necessary to do.

Instances illustrative of this argument, and of the fallacy of a *a priori* inferences of the kind we are examining,—in which it is shown that decided and even powerful curative effects are brought about by the aid of medicines which have either no poisonous action at all on well persons, or are given in doses too small to affect them sensibly, or in which the curative action is apparently the very opposite of their sensible physiological action, are equally numerous and familiar. I shall select for illustration here only two such instances, which, I think will suffice on account of their especial bearing on the example before us.

Lemon juice, it has recently been discovered, exercises a speedy and very efficient curative agency in acute rheumatism,—an inflammatory affection in which contra-stimulants or sedatives are necessarily the only effective remedies. Now is there any thing in the physiological effects of lemon juice which would indicate this therapeutic action? Is it a cardiac sedative administered to persons in health? I believe not; no mention is made of any sensible physiological effects from it by either of the few authorities I have consulted, though among them is the elaborate work of Dr. Periera, the last edition, where one might expect to find them, if any where. Certainly it has no considerable power in this respect, if any at all; and yet it is said to reduce the force and frequency of the pulse in this disease with a degree of power not equaled by the most active sedatives known to the *Materia Medica*. Let us suppose, then, that when this discovery was first announced,—a discovery of such value that it has been said on high authority in England, to mark an era in the history of practical medicine,—physicians, instead of testing its value in the circumstances of disease in which its sedative power was affirmed, had set about to determine whether this was true or not, by giving it to well persons, to try in fact, whether it would cure rheumatism by giving it to persons who had no rheumatism to cure,—what the *a priori* inference from such experiments would be, as well as its value when made, is obvious enough. Would they not, in fact, have necessarily come to the same general conclusion in regard to the sedative power of lemon juice that Dr. Boling was led to by his experiment in regard to phosphorus?

Again: It is well known to medical men practicing in the Southern and Southwestern States, particularly in the newly settled parts there-

of, that quinine acts more efficiently as a sedative in a certain class of diseases than any other medicine. The pulse may be—every one must have seen it—reduced by a single dose, but always by a few doses, if the circumstances be suitable, twenty, forty, or even fifty beats in the minute in a very short time. And yet if the same medicine be given to a person in health in the same doses, or even larger ones, it may be that the pulse will not be effected at all, and is just as likely to be made faster as slower. An example in point, remarkable on account of the unusual size of the doses, has been recently related to me by Dr. Reeves, of this city, who gave to a man with chronic hypertrophy of the spleen, but without fever, the enormous quantity of an ounce of quinine in the course of four days, in doses of forty grains, given three times daily. Dr. Reeves did not observe any material change in the man's pulse during the whole experiment, for such it was. What change there was, indicated an increase rather than a diminution of its frequency. Numberless other instances of its inaction on the pulse of well persons, as also in some forms of febrile disease* will occur to the memory of every physician who has made much use of it. My own experience with it as regards its action when I have taken it, or given it to others, is to the effect that quinine, like lemon juice, does not so effect the pulse in health as to give any intimation of its extraordinary sedative power in some forms of disease. Such, no doubt, is also the result of the experience of physicians generally, if not universally. But if so, how false a guide to the truth would be experiments made with quinine like those made by Dr. Boling with phosphorus?

But not to fatigue the reader with further illustrations of an obvious truth, we here close the review of these experiments, so far as they relate to the sedative action of phosphorus, by summing up the conclusions from all that has been said, which, I think, are embraced in the following propositions:

First: That the fallacies in the experiments were of such a kind as to preclude any other than the negative results actually obtained from them,

*This fact, which I had frequently observed before, was very forcibly brought to my notice last fall in the first case of yellow fever that I treated. In this case it was apparent that quinine aggravated all the febrile symptoms proper—singularly enough too, the case presented the features of a quotidian remittent, exhibiting, the three first days, decided morning and evening remissions and exacerbations. I had never seen quinine act in that way, that is as a stimulant, in a remittent fever before—nor indeed in any fever, nor in the absence of fever—but I had never before treated yellow (remittent) fever. Supposing that this stimulating effect was only apparent, I gave it in several other cases; but they only served to confirm the opinion I had formed of its effects in the first case.

and consequently, that they afford no reliable proof as to the sedative power of phosphorus in health; and,

Secondly: That if the experiments had been so conducted as to insure accurate results, they would furnish no reliable evidence as to its effects in disease.

The sedative action of phosphorus in health is one question; its sedative action in certain forms of disease is another question. Whether it is sedative in its action on persons in health I do not certainly know, and as I have expressed no opinion about it heretofore, so neither do I intend to express one now. There are, however, some positive facts relating to this point, which I wish to put on record here, to be used, if desirable, for reference, if more of the same kind should be brought to light hereafter. At present, they are too few in number, and the circumstances attending them have been too little varied to justify anything like a general conclusion. A part of them, which are referred to by Dr. Boling, is contained in a note in my paper on the treatment of pneumonia, which I copy:

“Two young gentlemen, my personal and professional friends, have recently been engaged in some experiments on persons, in order to test the effects of phosphorus on persons in health, they themselves being the subjects of the experiments. These gentlemen found (in several experiments) that the force and frequency of the pulse was invariably reduced by a single dose of two drops of the saturated alcoholic solution; the changes in frequency ranging in the number of pulsations to the minute between eight and twelve beats.”

To these may be added three experiments recently made on myself, while convalescent from an attack of sickness, but still confined to bed, without appetite, and taking food, only in a very small quantity, of the lightest kind, at a time, and this at long intervals, circumstances highly favorable for such a trial. The dose, (for I took but one in each experiment, in two of them,) was three drops of a saturated alcoholic tincture, and in the other thirty drops of the diluted tincture used by me in practice, the last being equivalent to either of the others. I ought to mention, as a fact of some importance in experiments with either of these preparations, that I dropped the doses into water cooled by ice to about 40° of Fahrenheit, from which almost no vapor is given off, though it makes the water a little turbid; whereas, if dropped into a tumbler or

other open-mouthed vessel like it, into water at 70° and upwards, it gives off a volume of phosphorus vapor so large, as to render the quantity remaining very uncertain; there can be no doubt that the dose is in this way frequently very much reduced. The results of my experiments corresponded with those of Dr. Pollard and Dr. Oliver. The greatest depression in the pulse was twelve beats in the minute. This effect was evanescent; the pulse always returned to the same state as before the dose was taken within an hour and a half after the greatest depression was reached. The longest time required to produce this effect was one hour and twenty minutes; the shortest one hour; the change in the pulse was perceptible in twenty minutes in two experiments, and in thirty minutes in the other.

A question is raised by Dr. Boling relative to the proper size of the medicinal dose of phosphorus, which being incidentally connected with its sedative action as a therapeutic agent, may more properly be discussed here than elsewhere. This question, implied rather than directly expressed, is as to the possible efficacy of this medicine in the doses recommended by me in the treatment of pneumonia. The implication is, that given in doses so minute as they were estimated to be by Dr. Boling, there must have been some mistake in attributing any efficacy to them; especially when it was shown that it could be given in a dose many hundred times greater to a person in health without any appreciable effect of any kind. This last fact, which is dwelt on with some emphasis, is exhibited in the experiment on Sam, 10th of March, in which he took at one time nine hundred and ten drops of a diluted tincture, which held in solution about one-tenth of a grain of phosphorus. "Here," says Dr. Boling, "a child, seven years old, took at a single dose, one thousand eight hundred and twenty (1820) of Dr. Ames' doses for an adult."

The phraseology here is somewhat peculiar; it is not said that the child took one-tenth of a grain at one dose, but eighteen hundred and twenty of Dr. Ames' doses; so that the objection would seem to be founded more on the relative quantity that could be given without effect than to the absolute size of the dose. This supposition is confirmed by a subsequent remark: "Yet not only did my subjects take it in doses so immeasurably greater than the doses with which such effects are said by Dr. Ames to have been produced by it," &c. Now, if such

is the meaning, this instance is not the best that could have been selected for the contrast. A still stronger one could have been found in the record of cases, hereafter to be noticed, in which three, six, and even twelve grains of phosphorus has been given at a dose, with no more sensible effect than Dr. Boling witnessed from his dose of a tenth of a grain. But phosphorus does not stand alone in this particular. The books furnish some stronger contrasts in regard to other poisons, whose medicinal doses are better known. I have space but for one example. I suppose an eighth of a grain of calomel, repeated like the doses of phosphorus every three or four hours, must be an efficient medicinal dose for an adult in certain circumstances—in fact, an adult may sometimes be speedily poisoned with it. A case is related by “Taylor, on Poisons,” in which a child eight years old was badly poisoned by three doses of two-thirds of a grain each, given at intervals of twenty-four hours. In contrast with this I give the following case, which came within my own knowledge while a student of medicine: A child between two and three years old got hold of a vial of calomel by accident, which she broke while playing with it; and, liking its sweetish taste, ate up a full ounce of it before she was discovered. But the child showed no sensible effect from this enormous dose, except that the next day a single free evacuation of the bowels took place. Thus, a child less than three years old, took, almost without sensible effect, thirty-eight hundred and forty efficient doses of calomel for an adult person, or six hundred and forty such doses as proved sufficient to poison a child three times older. This is an extraordinary case as regards calomel; but it is well known that this remedy has often been given to adults in doses of two, three, and four drachms—that is, from nine hundred and sixty to nineteen hundred and twenty efficient medicinal doses, without any sensible effect. The books on poisons are full of similar contrasts. It may be concluded, therefore, that the quantity of a poison which can be taken with impunity, relatively to the proper medicinal dose, is no measure of the efficiency of the latter. Hence, the medicinal dose of phosphorus might be relatively as small as that supposed by Dr. Boling, and yet be an efficient one—a kind of question, however, which may always be easily enough decided by experience, but never by *a priori* conjectures.

But the more important question which I suppose was also intended to be made, is an objection to the absolute quantity employed by me and others medicinally. And here I wish to remind the reader, or rather to request him to continue to bear in mind, that this objection is in the nature of a conjectural inference, merely, from the supposed size of the doses given by me, and from what Dr. Boling saw of its action, or rather want of action, in a much larger dose given to a healthy subject. Dr. Boling had not tested its medicinal efficacy in any dose in any acute disease.

How large the dose really is, I had not made a subject of inquiry before the appearance of Dr. Boling's paper; nor, perhaps, should I ever have done so but for the estimate there made of it. The object of all my trials with phosphorus in varied doses, which had been confined to diseased subjects, but especially to those affected with pneumonia, was to ascertain the smallest quantity of an alcoholic solution capable of producing decided medicinal effects, and yet be without danger of producing, in any condition of my patients, those disastrous consequences which are said by authors to have resulted from doses that in any other substance, except, perhaps, aconitine, would be too small to produce any sensible effect. Having secured this object, the actual quantity of phosphorus contained in a dose, became a matter of perfect indifference.

Dr. Boling's estimate of the dose is as follows: "In a vial containing an ounce of anhydrous alcohol, I placed four grains of phosphorus, in another two grains, and in another one grain. At the end of fourteen days—the time usually necessary for the preparation of tinctures by maceration, the time recommended by a majority of the pharmacopœias—of the four grains, about one-fourth or less was dissolved; of the two grains, about one-half or less; and of the single grain there remained a portion undissolved. It is fair, I think then, to say that the saturated alcoholic tincture, instead of containing four grains to the ounce, contains in reality but about one grain to the ounce. It may possibly be a little more; it would seem as likely to be a little less." Proceeding on this assumption, the dose is estimated to be about the one-sixteen thousandth part of a grain—a quantity, as he remarks, almost inconceivably small. Let us see how nearly correct this estimate is.

Seeing that there was one obvious source of fallacy in Dr. Boling's experiments to ascertain the quantity of phosphorus that alcohol will dissolve, I requested and obtained the favor of Mr. Williams, principal assistant in the drug store of Messrs. B. S. Thiess & Co., a gentleman remarkable for the care and accuracy of his pharmaceutical manipulations, to institute some experiments, of which the following is an account:

1.—Twenty grains of phosphorus were put into an ounce by measure of alcohol not anhydrous, sp. grav. 812 (at 70 deg. Fahrenheit) and allowed to digest nearly twelve days. The phosphorus was then removed from the vial and weighed. The loss was found to be three grains. A small part was wasted in transferring the powder* to the scales, which Mr. Williams estimated at about one-eighth of a grain.

2.—Two grains were put into a measured ounce of anhydrous alcohol (sp. grav. 794 at 60 deg. Fahrenheit) on the 9th of June. This was all taken up in eleven days.

3.—Four grains were put into an ounce of alcohol, (sp. grav. as in the preceding experiment,) on the 9th of June. Twenty-one days afterwards there remained undissolved, according to the estimate of Mr. Williams and others, not more than a quarter of a grain.

4.—Thirty grains were put into a measured ounce of alcohol (sp. grav. as above) on the 13th of June. Twenty days after, namely, on the 2d of July, the remainder was carefully removed from the vial, and weighed. The loss was found to be five grains. There was no perceptible waste in this experiment.

5.—Twenty grains were put into a measured ounce of alcohol (sp. grav. as above) on the 13th of June. On the 2d of July, the remainder having been carefully transferred from the vial to the scales, weighed thirteen and seven-eighths grains. The loss, consequently, was six and one-eighth grains. There was no perceptible waste in this experiment†.

*In this, as in the succeeding experiments, the phosphorus was first reduced to powder, after the manner of M. Leroi, in order to save time by facilitating the solution.

†An excellent mode of preparing solution of phosphorus for use has been adopted by Mr. Hudson, of the house of E. Fowler & Co, druggists of this city. It is to put ten or more grains of good phosphorus into an ounce, by weight, of absolute alcohol, contained in a strong vial, of the capacity of three or four ounces, which is to be carefully stopped and sealed. The alcohol is then heated in a water bath to 150 degrees, Fahrenheit, when the vial is taken from the bath and violently agitated; when cooled to 80 degrees or less, it is again heated as before; and this process is repeated a third time; after which it is rapidly cooled to 60 degrees, and the clear liquid decanted. The whole process requires hardly an hour, and the result is a perfectly transparent solution, ascertained by several trials to contain one grain of phosphorus to one hundred grains of alcohol. The exact strength of the solution, as determined by its specific gravity is 4.84 grains of phosphorus, in 480 grains of alcohol. This preparation has these advantages, that its strength is definite, and when dropped into water it gives off very little vapor.

The difference in the quantity taken up in the two last experiments, is to be attributed, no doubt, to the circumstance that the phosphorus used in the last, was more minutely divided. It is probable that neither solution was a saturated one, the time employed being too short to complete it. The pharmacopœias recommend the digestion in either to be continued four weeks; but I am satisfied that a solution in alcohol, to be saturated, requires a much longer time, but less, in proportion, as the particles presented to the alcohol, are smaller.

Care was taken, in the four last experiments, to see that the alcohol was really absolute, and the phosphorus pure; that it had the characteristics said to indicate a pure article, being translucent of a light amber color, and melting at 108 degrees of Fahrenheit. These precautions are indispensable in such experiments, for the reason that phosphorus kept in a bottle, in water, and exposed to the light, undergoes important chemical changes, among which is known to be the oxydation of a part of it. The same change occurs when it is put into alcohol containing water over that chemically combined in it. The oxyde being insoluble in alcohol, and the phosphorus insoluble in water, it is seen how much these circumstances might affect the strength of the solution.

The solution or tincture that I first employed medicinally, on the visible effects of which my opinion of its medicinal action was founded, was made with all necessary care, to secure a saturated solution of pure phosphorus in absolute alcohol. Both the phosphorus and alcohol were tested for this purpose. It was allowed to digest more than four weeks before any part of it was diluted for use; but only a small part being used at a time for this purpose, the digestion of the remainder was continued much longer. Now, if we suppose the solution made in this way was equivalent in strength to that obtained by Mr. Williams, after only three weeks digestion, about which I think there cannot be a reasonable doubt, it must have contained at least six grains to the ounce, and the diluted tincture six-tenths of a grain, instead of one-tenth, as estimated by Dr. Boling; and, consequently, that the dose recommended by me, was at least six times larger than his estimate of it.

Still the dose remains a very minute one; so much so that physicians who are in the habit of measuring the curative power of a

remedy, chiefly by its quantity, might find a difficulty in according any efficacy to one so small as this. The objection to it on this account occasions no surprise when made by those who have neither employed the drug medicinally, nor become acquainted with the peculiar activity of its physiological manifestations, in a much more minute quantity than is contained in the dose recommended.

If a solution containing one grain to the ounce, which is the strength of that used by Dr. Boling, be diluted with nine additional parts of alcohol, so that the dilution shall contain one-tenth of a grain to the ounce, and this be dropped into water, in the dark, it gives up a luminous flash with every drop as it touches the water. Supposing eight hundred drops to be contained in an ounce, the one-eight thousandth part of a grain thus becomes palpable to the sight. Again: If sixteen drops of this very weak solution be put into four ounces of water, a teaspoonful of this mixture, containing half a drop of the tincture, or the one-sixteen thousandth part of a grain becomes palpable to two other senses, having a decided smell of phosphorus, and an aliaceous taste. Thus, it is seen that the dose estimated by Dr. Boling, though almost inconceivably small, as an arithmetical quantity, is really more appreciable than many large and very active doses of other medicines. But more than this.

If eight drops of this diluted tincture be mixed with one drachm, or one hundred drops of water, a single drop of this mixture, which contains but the one-hundred thousandth part of a grain, has a taste and smell of phosphorus strong enough to be easily recognised by those who are familiar with its sensible qualities.

But if four drops of a diluted tincture containing six-tenths of a grain to the ounce, which I suppose is the strength of that recommended by me, be put into an ounce of water, a teaspoonful of this mixture, which contains about the twenty-six hundredth of a grain, has a taste and smell of phosphorus strong enough to be disagreeable to most persons; and half a drop of this tincture, that is a drop of tincture of half the strength, will give off a luminous flash if dropped into water in a dark place.

The inference from these facts is obvious, but still like the opposing objection, is merely hypothetical. Let us for the better understanding of the matter reduce the argument of both hypotheses to a brief form

of expression, thus: on the one hand the implication is, that it is not reasonable to suppose, *for all former experience contradicts it*, that a dose of any medicine so small as the one-sixteen thousandth of a grain can have any medicinal power; on the other it is urged, that it is reasonable to suppose, *for, all former experience confirms it*, that a dose of an active poison large enough to be tasted and smelled, may have some power as a remedy. If phosphorus is an exception to all other known medicines in exhibiting its sensible qualities in a quantity so minute, so it may be, and is likely to be, an exception, in having a medicinal effect in a quantity in which such an effect could not be expected *a priori* of any other known substance. But if this is a reasonable supposition applied to the dose of a sixteen thousandth of a grain, to which only the implication in Dr. Boling's paper applies, it is much more likely to be true of a dose which is appreciable to the sight, as well as to the taste and smell, and whose measure is the quantity estimated by Dr. Boling many times multiplied.

Still the question remains unsettled. If the inquiry stop here one is placed in the dilemma of having to decide between two opposing hypotheses, neither of which establishes a truth. One may do very well as an offset to the other, but this is all; neither proves any thing, unless it be that the other is not true. Something more therefore is required on the one side or the other; and this is the test of direct observation.

After all, then, that may or can be said in arguments of this kind relating to subjects of physical inquiry, the matter has always to be brought for settlement to this arbitrament at last. And consequently the only efficient, perhaps the only proper reply to the hypothetical objection under review, as applied to the doses recommended and employed by me and others is that the point in question had already been tested over and over again by the actual experience of competent observers; not for a brief period; by a single individual; on a few subjects; or in the same place; but for a series of years, and by a number of physicians, and in a considerable number of subjects, some of them living in places widely separated from the others; all tending to give an assurance as strong as is usually obtained in regard to new remedies or to old ones introduced under new auspices, that phosphorus in the doses recommended by me, varied no doubt by others and

by an occasional want of proper care in the preparation of the tincture; but whether in doses larger or smaller, has exhibited medicinal qualities of the highest value in inflammatory affections of the lungs, but particularly in pneumonia, as they appear in this climate. Before publishing the results of my own experience with this remedy, I consulted several physicians as to whether their experience of its efficacy generally accorded with mine, but especially as to its sedative action in pneumonia, and found there was no discrepancy of opinion as regards either. Some were consulted verbally, others by letter. Among the answers to the letters there is one which so well expresses the uniform opinion of those who have had much experience with it, that I will take the liberty to extract a portion of it, though I have not asked permission to do so. The letter was written by physicians associated in practice, which will account for the occurrence of the plural pronoun, and was dated May, 1853. After saying that "no death from pneumonia has occurred in our practice since we adopted this treatment," they add: "as regards the effects of phosphorus in such doses as recommended in your treatment, so far as our experience goes, it fully concurs with your own. In almost every case in which we have administered it there has been a uniform and steady diminution of the frequency of the pulse with a marked quietude of the whole system. At first we were disposed to attribute this sedative influence to the aconite and quinine, but on suspending these and continuing the phosphorus, we found the same condition of things continued; and in no single instance have we found the pulse increased in frequency, or nervous excitement produced by the proper administration of this remedy." Among the other physicians of my acquaintance, some thirteen or fourteen in all, who have treated this disease by means, partly or chiefly, of this remedy in similar doses, I believe there is none, whose experience of its efficacy might not be properly expressed in the words of the latter paragraph of this letter; nor do I think there is one who entertains any more doubt about its curative power in the doses spoken of, than he does of the curative power of any other remedy that he may have ever employed in this disease. I ought not to omit, in this connection, a reference to the recorded statistics of the results of the treatment of pneumonia by this as one of the chief remedies, in which the mortality is found in sixty-eight consecutive cases,

occurring in the course of a little over four years, reduced to less than three per centum, including one case in which the patient evidently died of a brain affection, which complicated the attack from the beginning. The result of the treatment has been the same or even more favorable, as I have been informed, in the practice of other physicians.*

2.—The second conclusion of Dr. Boling from his experiments, in the order I have adopted, namely, that phosphorus is not a stimulant, I pass over, with the single remark, that my observation of effects in disease had led me to the same conclusion, as regards its medicinal action.

3.—We come now to the third and last conclusion, from these experiments, viz : that phosphorus is not poisonous given in an alcoholic solution.

This conclusion is derived from several experiments, in which doses of this medicine, dissolved in alcohol, of various sizes, were given to the two principal subjects, and were taken by himself. To *Sam*, the largest dose given was nine hundred and ten drops of tincture, containing less than a tenth of a grain to the ounce ; the quantity of the tincture given being about an ounce, the dose was about the tenth of a grain. The largest dose given to the other subjects, was two hundred drops of a tincture, containing less than a grain to the ounce, or about the fourth of a grain. Dr. Boling took, himself, five drops of this tincture, three times daily ; allowing eight hundred drops to the ounce, each of these doses contained about the one-hundred-and-sixth of a grain. As it is not material whether these doses are estimated with entire accuracy, we may leave out of consideration the probable waste in dropping them out, by the conversion of a part of the phosphorus into hypo-phosphoric acid, which probably escaped in the form of vapor, or may have been taken in the place of phosphorus proper.

In reference to the dose of two hundred drops, or one-fourth of a grain, given to the adult subject, we find the following comment :

*I do not wish to be understood here as intending to give an undue importance to this remedy, or to attribute to it alone the favorable results of the treatment of pneumonia with this and other remedies ; I believe that aconite is equally valuable, and that quinine is frequently indispensable, while opium and blisters are valuable adjuncts, and tend very much to secure a favorable result. What I mean to say, is that in the treatment of pneumonia in this climate, it deserves to rank as a leading remedy, and one more valuable than any other single remedy I have used except aconite. Nor do I by any means intend in what I have said in defence of the dose I have been in the habit of using, to intimate that there is any specific dose which is peculiarly efficacious : What I mean to say is that, in my opinion, formed after much experience with larger doses, this is large enough for ordinary purposes in the treatment of pneumonia, while it is not too large to be perfectly safe. I have to day, however, conversed with a physician, who doubts the propriety of giving it even in this dose in cases where there is a gastric complication.

“How much further the dose might be augmented with safety, and without appreciable effect, I am at present unprepared to say; but reasons, I think will appear as we proceed, that will render it not improbable, that the quantity of alcohol, rather than any suppositious quantity of phosphorus, the preparations, as prescribed and given, may contain, should form the only necessary limit to the dose.”

It is evident from this, as well as another instance in which Dr. Boling has expressed a similar confidence in its harmlessness dissolved in alcohol, that he had not studied either the medical or toxicological history of phosphorus very thoroughly. The confidence derived from his own very limited experience with it, in this form of administration, would not, I presume, have been entertained, if the larger experience of others in other modes of giving it, had been sufficiently consulted. The medical history of this article shows that it has been given with a like impunity in ether, or almond oil, or in substance made into pills, with conserves, in larger doses, and for a longer time than Dr. Boling gave it to either of his subjects, or took it himself; yet its poisonous properties in either of these preparations, does not seem on this account to have been questioned.

It is an established fact that phosphorus, in any mode of using it, exerts its poisonous power very unëqually at different times, even in the same person, and in circumstances apparently alike in other respects; and this eccentricity is more striking, when its effects in disease are compared with its effects on persons in health. Thus, while it has been given occasionally or frequently in very large doses without doing any harm, it has been known to prove fatal in comparatively very small doses. M. Magendie quotes a number of authorities, showing that at one time the common dose given by physicians was two or three grains, and upward, even to twelve grains. Dr. Mentz, it is said, gave to one of his patients a dose of two grains made into a bolus, and repeated the medicine, three grains at a time, the next night and morning with the most happy effect; “quietude was immediately produced, with sleep and gentle perspiration.”* In another case he gave six grains, also in two doses, with benefit. Wolff reports twelve cases,

* I had thought that the opinion of its exclusive stimulant operation, was universal among the older, as well as the more recent authors who treat of phosphorus, but I find, besides the example in the text, that its sedative action is affirmed in this way by several others. Thus M. Leroi, speaking of the luminous pills, first prepared by Kunkel, each an eighth of a grain, says: “They are endowed with a sportive and calming property.”

extracted from the case book of his father, in which phosphorus was given in doses of two or three grains, from which "the results were so extraordinary that the author was induced to call phosphorus a divine remedy." M. Magendie also refers to Alphonse Leroi, who, having seen the German physicians give it in the doses of six, eight, and even twelve grains per diem, took three grains himself; but not with the same impunity he had seen others give it. In speaking of its effects, M. Leroi says it is a wonder it had not killed him.* Indeed, this kind of dosing could not be long continued; and we find accordingly, that the false security engendered by it was sadly dispelled by the homicides, reported or otherwise, committed in its use; or by its dangerous effects in a less degree. "In the midst of all this success," says M. Magendie, "Wickard, in the second part of his writings, related cases and experiments which ought to put people on their guard against the rash employment of this remedy." So, too, a society of physicians in London, in a report on the properties of phosphorus, in the London Medical Review, while commending its medicinal powers, add that, "this powerful and active remedy should be employed with the greatest caution." And this caution was so often repeated, as the disastrous effects of its employment in such doses became more and more frequent, when, at last, the eyes of the medical men were opened to see the danger, that it was soon banished from practice. After a time it was introduced into medical use again, in small doses, but was soon given up again. It was introduced a third time in still smaller doses, and a third time banished; and it is curious to observe, in tracing out the medical history of this potent drug, how the doses grew successively smaller and smaller as the experience of its pernicious effects, in doses still diminishing, was developed in the two last periods. In the first the dose was two grains, and upwards, as high as twelve; in the second, it was from a grain to the tenth of a grain; while in the third it fell from a fourth to a twentieth, fortieth, and even to the one-hundredth of a grain.† At the same time we find the warnings of its unruly and dangerous nature as a poison, quite as urgent and impressive in the writings of those who employed it in smaller doses, as in

* Eds. Dic. Med.

† From two to five or ten drops of a solution in ether, or almond oil, containing four grains to the ounce. The ethereal solution is the officinal preparation of the French codex; the dose is from five to ten drops, or from the fortieth to the twentieth of a grain.

that of those who gave larger ones. In all this time the various sized doses were not only given with frequent impunity, but with the highest commendations of their success as a remedy; and yet we find it no more able to maintain its place in medical practice in the smaller than in the larger doses. The doses had been reduced from six to twelve hundred times, and were still so much too large that physicians were compelled to stop prescribing it. The smallest quantity reported to have produced death in a single dose is about the tenth of a grain; another case is reported in which one-third of a grain was fatal: in another case, a grain taken in the course of four days, produced death. Dr. Chapman poisoned the only patients, three in number, he ever gave it to, though fortunately he did not kill them, with doses of one eighth of a grain. These are *reported* cases. How much harm may have resulted from giving it in doses from the twentieth to the one hundredth of a grain, frequently repeated, we have no especial report of; but that there must have been a good deal is evident, from the urgent cautions which are mingled with or follow the commendations of its curative effects.

The causes which thus render phosphorus harmless at one time, in a given dose, and at another a deadly poison, in the same or a much smaller quantity, have not elicited much inquiry. Its frequent disastrous effect in any dose that had been used, was supposed, by many, to unfit it for medical uses altogether, and there inquiry seems to have stopped. My limited experience has led me to believe that this eccentricity is connected more with the condition of the stomach when the medicine is taken, than with any difference in the modes of administering it; it is more apt, for instance, to show its deleterious qualities in case of abstinence, as in the experiments on myself, and as is ordinarily the case in febrile diseases, but more certainly, when to abstinence is added an irritable stomach from disease. A careful study of the cases I have been able to find reported, tends to confirm this opinion. However this may be, the fact that phosphorus is thus irregular or eccentric in the exhibition of its poisonous properties must, necessarily, derive the experiments of Dr. Boling of the weight they otherwise might be entitled to as proof that it is not poisonous in alcohol. A greater number of experiments, it appears, have been performed with other preparations of it, known to be poisonous, in which it proved equally harmless, in doses from four to

forty-eight times larger, than the largest given to either of his subjects ; its administration has also been continued a much longer time than in his experiments with a like result. Alibert, quoted by Dr. Eberle, gave to some epileptic patients a grain a day for two months without any bad effect ; on the other hand, a dose not larger than the largest, given to one of his subjects, has been known to prove fatal, while its bad effects, in doses from two to ten times smaller, but frequently repeated, served to banish the medicine not merely from medical practice, but to a great extent from the list of officinal medicines. Hence the impunity with which it may be given at times in certain doses, so far from being held as a proof that it may always be given with impunity in the same, or smaller doses, is rather looked to as a warning against trusting a poison so subtle and treacherous at all; or, at least, in any doses that had been employed, even to the one hundredth part of a grain. What is thus known to be true of phosphorus in any other mode of giving it may be, and I have no doubt is, true of its solution in alcohol ; nor does it seem to me that the very few experiments of Dr. Boling tend at all to invalidate such a conclusion ; for it is evident enough, from the preceding abstract of its history, that if they had been made with phosphorus in any of the modes of prescribing it heretofore in use, the results might have been just the same as they were when made with an alcoholic solution.

There remains to be said a few words about Dr. Boling's experiments on himself. I copy the greater part of the paragraph, containing his comment on them, and the conclusion they led to, as the best mode of presenting both fully and fairly before the reader. He says :

“ In connection with this experimental practice on myself, I will again call attention to the views of Dr. Ames, in regard to the effects of phosphorus in their (his) doses. Thus, he says, that it cannot be continued in the smallest quantity mentioned, half a drop, for any great length of time, without inducing considerable disturbance of the stomach, shown by nausea, or vomiting burning heat, and a feeling of oppression at the epigastrium. Though he admits, that in the quantity of two drops, a single dose, or perhaps a few doses, may be given with impunity, yet he would evidently regard a lengthened use of it, in such a dose, as a very serious matter, and tells us of one instance in which dangerous effects resulted from the administration of three doses, of two drops each, at intervals of twenty-four hours. It is most desirable that we should yet

be able to discover and explain the cause of these discrepancies. While Dr. Ames tells us that doses of half a drop cannot be continued for any great length of time, without the most serious results, I have myself taken it in doses of five drops—just ten times the quantity—a long time, and for eight days, without omission of a single dose, without effect. While under his observation, for a cumulative action, dangerous effects resulted from three doses, of two drops each, administered at intervals of twenty-four hours, being in all six drops taken in the course of three days, yet I have taken, for eight successive days, three doses, of five drops each, or fifteen drops per day without effect. Indeed, unless I should discover something in its action which has never been manifested in any of my experiments, from my own experience with the article, and from all the lights at present before me, I should not hesitate, were it not merely for the trouble of the thing, to continue it in the same manner for years." These emphatic antitheses are continued in the succeeding paragraph, which I also copy.

"In the healthy subject, at least, any effect of the article resulting in nausea and vomiting, could be easily appreciated, and not readily mistaken; yet, not only did the subjects of my experiments take it in doses, as mentioned, so immeasurably greater than the doses with which such effects are said by Dr. Ames to have been produced by it, but they took it under circumstances that were well calculated to favor the production of such an operation. Thus, while they sometimes took it in the intervals between the meals, they also took it at times immediately before eating, and at others immediately after eating. On several occasions, I myself, having forgotten my dose, which I usually took just before eating, until I had partly finished my meal, have called for my phial, taken the dose and proceeded with my meal without disrelish, or any subsequent manifest effect."

We shall find, I believe, no greater difficulty in discovering and pointing out the cause of these discrepancies, which Dr. Boling thinks is most desirable, than was found in discovering the discrepancies themselves. The first step in this process is to point out some errors in these extracts which in themselves go a good way towards effecting this desideratum.

The first error that I shall mention is, that what I said of two drop doses is applied, inadvertently of course, to the half drop doses. The latter is spoken of by me only as liable to produce considerable disturb-

ance of the stomach when long continued ; while the former are said, in effect, to be unsafe, *in the treatment of pneumonia*, if continued for any great length of time. The "most serious consequences" therefore, should properly refer to the effects of the larger doses only.

Another error arises from a wrong construction of the following passage. "Its effects are cumulative ; that is to say a dose which singly is not large enough to produce any sensible effect may become very troublesome or dangerous after several repetitions at intervals of *three or four hours*. This *quality* was developed in one instance by repeating it in a dose of two drops of the strong alcoholic solution three times at intervals of twenty-four hours." Dr. Boling construes this to mean that dangerous effects resulted from the three doses given at intervals of twenty-four hours ; but the reader will see that the troublesome or dangerous effects refer only to the dose repeated every three or four hours, and that the cumulative quality alone is referred to in speaking of its repetition once a day. Such I believe is the proper grammatical construction ; it was at least what was meant. In point of fact, no dangerous effects were produced in this case. The subject took the two first doses without any sensible effect, but a short time after taking the third, symptoms of a pretty severe stranguary came on, which lasted several days. It is altogether probable, however, that if the dose had been repeated several times more, dangerous effects would have resulted from it. A third error, the source of which is in part explained in the two preceding paragraphs, is in the comparative estimate of the quantity of phosphorus taken by Dr. Boling and that given to my patients, to which the danger of serious consequences was ascribed, if continued any great length of time. Dr. Boling took five drops of a tincture containing less than one grain to the ounce three times a day, supposing it to be a full grain—each dose was about the one hundred and sixtieth part of a grain—my patients took two drops of a tincture which, as we have seen, there are the best reasons to believe, contained at least six grains to the ounce ; each dose, therefore, allowing as in the other case, eight hundred drops to an ounce, was about the sixty-seventh part of a grain, given every three or four hours, more than double the quantity of the other, and repeated twice as often. The doses taken by Dr. Boling, therefore, instead of being ten times greater, were less than half the size, or more than twenty times less than the estimate. The aggregate taken by him in twenty-four hours, was about the fifty-third part of a grain ; the daily ag-

gregate given to my patients was from the eighth to the eleventh of a grain; that is, nearly from five to seven times greater. The half-drop doses, to which danger of exciting considerable disturbance of the stomach was ascribed by me, if continued long, being of the same tincture, contained, of course, one-fourth the quantity of phosphorus in the two drop doses, or the two hundred and sixty-eighth part of a grain; each of these doses, therefore, is somewhat more than half (as one to one and two-thirds) the size of each of the doses taken by Dr. Boling, and their daily aggregate in twenty-four hours, compared with that of Dr. Boling's doses, is larger by from a fourth to a third. The largest quantity given to either of his subjects in one day was two hundred and seventy-two drops of a tincture having less than a grain to the ounce, or about the third of a grain of phosphorus. The daily aggregate of the half-drop doses given to my patients, if repeated every four hours, is about the forty-fifth part of a grain; so that the difference, instead of being "so immeasurably" great as supposed by Dr. Boling, is only as one-third of a grain is to the forty-fifth of a grain; and the measure of the difference is almost exactly as fifteen to one.

But these are minor matters. By far the most important source of these discrepancies is in the different circumstances in which the medicine was taken by Dr. Boling, and given by me. Dr. Boling took it in health; I gave it in disease; an essential difference, which in the inception and progress of these experiments seems to have been entirely overlooked. It must be remembered that in all I had said of the effects of phosphorus, in the part referred to by him, I spoke of its effects in the treatment of pneumonia only. In this disease, in this climate, the stomach and bowels are usually in a condition to be most unfavorably affected by irritant *ingestæ* of any kind. This condition I took some pains to point out in my paper on the treatment of pneumonia, in rendering a reason for the more frequent poisoning by calomel and tartar emetic in its treatment with these remedies, in this than in higher latitudes. There is, in fact, with us a peculiar and decided tendency in this disease to take on gastric or gastro-enteric inflammation from even mild irritants. In this condition, phosphorus, an irritant poison, whose action as such is always directed especially to these organs, is much more likely to show its poisonous effects than in a healthy condition of these organs. Here, then, was an empty and irritable stomach, prone to take on inflammation from slight causes, to invite a development of the poisonous action of phosphorus.

On the other hand, Dr. Boling took the medicine in health and at his meals. The importance of this last circumstance as regards its probable influence over the action of the medicine is shown by the fact that some authors recommend it to be taken only on a full stomach, for the purpose of avoiding its poisonous effects. It is equally true of other irritant poisons, that a full stomach renders them less active as poisons, *cæteris paribus*. Dr. Boling, it is true did not take his doses, literally, on a full stomach, but taking a dose *just* before eating is, practically, or so far as concerns the irritant action of a poison whose action is slow in developing itself, the same thing as taking it during or immediately after a meal. Now this was not only the time at which Dr. Boling took doses of the one hundred and sixtieth part of a grain, but the time also when he gave to one of his subjects the largest dose that either of them took, namely, a quarter of a grain. The influence of health, exercise and the regular meals, in modifying its effects I have frequently witnessed. In the experiments made on myself in whom nearly all such influences were absent, each dose of three drops of a saturated solution gave rise to disagreeable and even painful effects on the stomach, as I have seen it do in smaller doses in the treatment of pneumonia; the effects on myself were severe enough to prevent me from carrying the investigation, as I intended to do, any further than the three experiments mentioned. Nevertheless, I have often prescribed in doses of one and two drops in chronic affections of the bladder or urethra, and in seminal weakness, twice or three times a day when the patient took his usual meals, and attended to his ordinary business without any such effects, though continued many days. If therefore Dr. Boling took the medicine, as seems to have been the case, in a condition of the stomach unfavorable to the development of its local action on this organ, and my patients took it in circumstances the most favorable for the development of such an action, we may find in this difference alone a sufficient explanation of the difference in its effects, even if the doses had been equal.

But leaving these minute, and probably, to the reader, tedious details, let us sum up, in a general view, the principle sources of the discrepancies between Dr. Boling's experience and mine; separating the apparent from the real. If I have written with sufficient perspicuity, it has been seen in the progress of this paper, that all the discrepancies are included in, *first*: the different circumstances in which the facts have been observed

on either side; *secondly*: in the difference between facts observed on one side, and inferred on the other; in other words, between facts of experience, and facts obtained by *a priori* reasoning.

In the first, the discrepancy is apparent, not real, even if we admit that the experiments gave accurate results. It may be true under this admission, as I have said before, and I think, shown, that phosphorus does not act as a sedative on sick persons; but does so act in certain forms of disease. The difference in the circumstances, health on the one hand, and disease on the other, in which the experience was obtained, might alone make all the difference apparent in its effects. So, too, as regards the poisonous action of this remedy in certain doses, its eccentricity, and other circumstances connected with the condition of the subject to whom it was given, may very well account for the difference in its effects. So far, then, there is no real discrepancy; a difference in the facts there is, because both were not looking at the same thing exactly; but there is no clashing or incongruity, as might be supposed on a superficial view of the matter. Either set of facts might be true, without in any respect impeaching the integrity of the other.

Not so, however, as regards the second. Here the difference is not between facts observed on either side; but between experimental facts on one side, and *a priori* inferences on the other. When Dr. Boling concluded, from his experiments, that phosphorus dissolved in alcohol neither acts as a sedative nor as a poison, in certain doses in disease, he overstepped the bounds of legitimate induction, and the real discrepancies began to appear. The utmost limits to which the experiments permitted him to go, is, that phosphorus is neither sedative nor poisonous, in the doses in which he gave it to persons *in health*; there was no legitimate warrant for the supposition that it does not so act in certain forms of disease. Dr. Boling saw correctly, in the circumstances in which he placed himself; but reasoned incorrectly from what he saw. His reasoning brought him to conclusions directly opposed to the experience of others; and, curiously enough, he supposed that the former are more reliable in proof of the matters in question than the latter; and affirms, in effect, that the naked inferences, shorn of an attempt, even, at verification, are the true facts; and the direct observations of others, made in the very circumstances about which he reasoned, but did not observe, are the false ones. Here, then, is a real and irreconcilable discrepancy, not,

however, difficult of explanation. The truth is, as we have just seen, Dr. Boling explored one field of inquiry, and I another; and seeing nothing where he was, concluded there was nothing to be seen where I was. He did not come over into my field, in order to see with his own eyes, on the spot; but chose rather to look from afar off, and through an *a priori* telescope, which hardly even permitted any thing to be seen as it really is. Looking thus from his position, he thought he saw better what was going on where I was, than I did on the spot; but in the nature of things this was impossible; and with the chances so largely against his seeing right, through this medium, it is no impeachment of the natural accuracy of his vision to suppose that he saw wrong.

The facts obtained in these different modes are incompatible; on one or the other side they must be wrong. The reader, who has now seen both sides of the argument, must determine the matter for himself. But if still in doubt as to which are the true, and which the false, and the subject is of sufficient interest to induce him to take the trouble, let him bring the questions at issue to the arbitrament of his own direct observations in the circumstances, in which, only, the sedative and curative, or the poisonous action of phosphorus, in certain doses, has been affirmed by me.

ART. V.—RESEARCHES UPON THE ANATOMY, PHYSIOLOGY, NATURAL History and Cure of the Tape Worm or *Tænia Solium*.

BY BENNET DOWLER, M. D.

General Remarks.—Anatomy and physiology of *tæniæ*.

An erroneous opinion has prevailed in regard to the study of parasitic worms, namely: that the physician's business lies solely in the treatment, and not in the natural history of these animals. But a knowledge of their origin, causes, laws of development, habits and natural history, promises more than anything else to lead to the best means for their prevention or removal. Here the physician, more than the mere zoologist, has a motive which should impel him to study. Although, on the part of the physician, the mere hope of enlarging the boundary of his knowledge of nature is laudable, yet, he has a higher purpose in view, namely: that of relieving suffering humanity.

Ignorance, empiricism and accident are less likely to lead to useful results than systematic and well directed investigations, and the more so, as the subject is profound and difficult. As a proof of the value of anatomical knowledge in therapeutics, it is sufficient to allude to the cephalic extremity of the tapeworm; for, whether there be a head or not, there is a minute, very long, threadlike portion, supposed to be the anterior end of the worm, whence the numerous large joints are, doubtlessly, derived or developed. Thus the discharge of a hundred yards of fully developed tapeworm from the caudal end promises little if any permanent relief, while the discharge of the cephalic and almost microscopic end guarantees a cure. This subject will be resumed hereafter.

In C. Bonnet's works, (7 vol., 4to.,) published more than a century since, containing an elaborate description, with plates, illustrating the tapeworm, microscopic views of this animal's head have been given. These have been, as I venture to think, servilely copied by his successors, though some have deviated so far as to give a sharp, spear-pointed figure to the head, as in Gregory's Practice, (1831) and in other works.

In his pathological anatomy, Prof. Vogel, (tapeworm, cestoidea) describes the head of this parasite as being "very minute, only becoming evident by the application of magnifying glasses. It then exhibits four lateral mammillary suckers, and between these, in the middle of the head, an arched eminence, in whose centre a minute, scarcely perceptible opening exists, upon this are sometimes seated hooks in double rows." 423. The internal evidence derived from this and many similar statements, as well as from physiological anatomy, shows that the functions ascribed to the microscopic head and cephalic microscopic hooks of this animal, are imaginary or rather impossible. No one pretends that he has seen the animal feeding by its supposed microscopic mouth, or microscopic suckers, or holding fast to the bowels by its microscopic hooks, as will more fully appear in another part of this paper.

Prof. Vogel continues: "the growth of the worm takes place in this manner: from the neck, new joints are continually evolved, which push those behind still further back, and develop themselves in proportion as they regress. The hindmost joints are, therefore, the oldest, and at the same time the most perfect; yet it appears that new joints are developed not only from the neck, but may be introduced between

the perfectly developed articulations, even at the posterior end of the animal"—423.

Sir A. Carlisle, says that the head of the tapeworm, when accidentally lost, is regenerated—an assertion difficult to disprove, but still more difficult to prove or believe.

The compilers of the Cyclopædia of Practical Medicine say, "authors who were ignorant of the animal possessing a head, or who thought that it was made up of mutually adhering cucurbitine worms, asserted that new joints were formed at each end to replace those passed by stool. From the time that the head was detected, Andry and others began to believe that it was only to the hinder extremity of the body that such addition was made. At length Pallas and Muller showed that the increase did not take place at either extremity, but that the elongation occurred chiefly in the filiform portion of the animal next the head. The joints of the young tapeworm are so very close and small as to give it the appearance of being merely wrinkled rather than articulated; but on inspection through the microscope it is found to consist of innumerable articulations. The received opinion of the present day is, that the animal get no new joints, but only develops those which existed from the first in a rudimentary state." Prof. Vogel admits, as above mentioned, that new joints are developed at both extremities of the worm, as well as "between the perfectly developed articulations." Developmental analogy in comparative physiology rather favors the doctrine that the new joints are evolved from rudimentary forms which are multitudinous.

Several of the most eminent writers persevere in the statement, that the *hindmost* articulations are the *oldest*, the others being developed in *front*. To the same effect is the account given by Gluge in his excellent work on Pathological Histology, (Prof. Leidy's translation, 1853.) Thus in Plate X, Entozoa, fig. 9, *a*, the small end, which is the cephalic extremity, is called "*the youngest and anterior part of the body;*" and that part represented by fig. 9, *b, c*, is termed "*the posterior and older part of the same.*" Is not this account wholly erroneous? Have not micrographers and naturalists shown, and does not analogy teach, that the anterior or cephalic extremity consists, not of newer, but more crowded rings, the rudimentary elements of all the subsequent articula-

tions—which latter on being fully developed and matured, ultimately drop away? Minuteness does not prove seniority.

Prof. Vogel says: “when the last joints have attained to their perfect development, and are filled with mature ova, they detach themselves, and either uninjured, or in a state of decomposition, are evacuated by stool. Since the joints which are thus thrown off, are continually being replaced by new ones, it is absolutely necessary, in order that the annoyance caused by the worm shall cease, that the whole animal, including even the extremity of its head, should be evacuated.”—424.

Bremser, a high authority, in helminthology, contends that no one has ever seen an entire tapeworm, because the caudal, or posterior articulations, impregnated with ova, detach themselves and pass off, while those next the head develop themselves unceasingly.

There has been much blundering about this worm both in and out of the profession. If the animal is developed from the head, backward, it is an important point in the cure, to ascertain whether the head has been discharged, for the frequent discharge of the posterior joints may be natural, not an indicant of the death or decay, but of the growth and activity of the worm.

Bremser mentions, as an important practical fact, “that of the many hundred persons cured by him of the tapeworm, not a single individual has seen the head come away.” Now this is not at all surprising, because the general opinion represents the head as a large and terrific one, whereas, it is a microscopic object, and, for this reason, likely to escape the most careful observation.

As a proof that the anatomy of the head of the tapeworm has not been satisfactorily demonstrated, it is only necessary to read the different and conflicting statements by microscopists of the greatest eminence.

The hooks which surround the reputed mouth of the tapeworm, are variously described, both as to shape, number, and function. Invisible to the naked eye, they are, nevertheless, said to be used by the animal for the purpose of laying hold of the mucous membrane of the intestines. That an animal, often many yards in length, should anchor itself firmly in the bowels by these microscopic hooks, is against all analogy. A Man-of-war might as well use for its anchor a feather.

In the Museum of Animated Nature, (vol. II, 423, fig. 3906,) these hooks are represented as amounting to thirteen, while many authors

mention only four, or an indefinite number. In the Cyclopædia of Practical Medicine it is said that "the interior of the animal is made up of amorphous cellular tissue, without any abdominal cavity, and that it is often extremely difficult to detect the head of the tapeworm. The head, which is a minute tubercle pisiform body, terminating the thread-like neck, has on its most prominent and anterior part a circular aperture, or depression, surrounded by sharp, curved processes or hooks, and supposed by some to be the rudiment of an alimentary canal. These processes are, however, not constantly found; being supposed by Bremser to disappear by age. On the somewhat angular projection on the sides of the head, are situated four equi-distant apertures, or suckers, which seem to be two mouths. The depression in front, by some called the mouth, is so minute as occasionally not to be visible without the aid of a microscope. The head is a third or a fourth part of a line, or even less in width."

This account is inconsistent with itself; for if "the animal is made up of amorphous cellular tissue without any abdominal cavity," (which is the true exposition) the assumed suckers and mouth could not perform the suctorial operations like the leech. It will be shown in this paper, according to the latest and best authorities, that these organs and functions are imaginary.

The Cyclopædia already quoted, continues,—"it is by the suckers on their head that they (*tæniæ*) stick to the walls of the intestines so firmly, that if force be applied, they break rather than lose their hold,"—a hold by means of the microscopic suckers of a microscopic head! while "prone—long and large, full many a rood," this intestinal "leviathan with fixed anchor, lies stretched out huge in length," holding fast by invisible suckers! Milne Edwards, says that the *tænia* has no distinctly formed mouth—(*la tête ne présente pas de bouche bien distincte.*—(Zool.)

M. Beaupe, (*Méd Dict.* 2 vols., 1849,) described the head, suckers, and hooks of the tape worm as indiseernable by the unassisted eye; they can only be seen (*appréiables seulement,*) by a lens.

Prof. Cruveilhier, (*Entozoaires, Diet. de Méd.*) says that the head of the *tænia* cannot be seen without a strong magnifying glass—"le tête est très-petite et ne peut être vue qu' à l' aide d'une forte Loupe." vii, 325.)

Microscopic suckers which take a hold so firm that the body will break before the animals will let go their hold! By some writers tænia are mentioned as being more than half a mile long! Thus, Pliny mentions one three hundred cubits long; the Copenhagen Transactions, one of eight hundred ells; Rosenstein one of three hundred feet! Robin, cited by Bremser, relates, that a patient who had died after voiding several feet of tapeworm, there was found a remainder, extending from the the pylorus, to within six or seven inches of the anus. Prof. Stone, of the University of Louisiana, informed the writer of this paper, that on making a post-mortem examination of a Russian, in this city, he found a tape worm, which from his description must have been much longer than that last mentioned.

“Van Doeveren mentions a peasant who, having taken an emetic, vomited up forty Dutch ells of tapeworm, and might have got clear of more of it, ‘if he had not been afraid of puking out all his guts, and for that reason bit the worm off.’”

Physiological or teleological anatomy, often a most reliable guide in such investigations, must repudiate these microscopic hooks as instruments by no means adapted to fix so huge an animal to the intestines. It must equally reject the four microscopic suckers as organs unsuited to the alimentation, for so large an animal, which has not any structural adaption for this suctorial function.

Comparative physiologists, who assign to the tapeworm the functions of a colossal leech, ought to point out a suctorial apparatus, an intestinal canal, and anatomical adaptations thereunto belonging.

Distrusting my own microscopic and anatomical researches on the tapeworm, I beg leave to call the student's attention to a late learned and reliable authority, namely, Professor Siebold, whose work on the Anatomy of the Invertebrata has recently been translated and annotated by a name dear to science, now no more, the late Prof. Burnett, who died in Boston a few weeks since, and who had already acquired, at the early age of 26, the reputation, at home and abroad, of a profound naturalist.

In this able work, Prof. Siebold's sixth class, helminthes is divided into six orders—the second of which, cestodes—includes tænia. He thus describes the latter:

“The panenchymatous body is ribbon-like, having often incomplete transverse fissurations; often it is wholly divided transversely into rings.

Digestive organs are wanting. The genital organs of both sexes are combined in the same individual; copulative organs are present; the four cup-like cavities at the cephalic extremity are *imperforate, and not so many oral orifices leading into an alimentary canal.*"

In his first order of helminthes, the cystici, and in the second cestodes, (including tænia) and in the fourth order, the acanthocephali, "*there is neither mouth nor alimentary canal* ; in the first two orders there is, however, a system of vessels which may be regarded as a digestive apparatus; but these are designed for circulation, rather than for digestion, since their walls are complete throughout, and have no openings as has erroneously been supposed, which communicate with the suckers of the head; and their contained nutritive material is received by them *through the skin in an endosmotic manner.* It has already been observed that the four suckers of tænia, regarded by Nitzsch as oral orifices, are imperforate at their bottom. Owen (Cyclp. Anat. II. 131) has fallen into a similar error in regarding these organs as mouths,"—III. 112.

"The system of vessels designed for the circulation," alluded to in this passage, is supposed to relate to what has been called the *water vascular system*, not blood vessels proper.

Muscular System of Tæniæ.

Prof. Siebold says there is a distinct subcutaneous layer of longitudinal muscular fibres in the rings of the tænia, forming also sucking cups, cavities and septa.

Bremser, who has seen the tapeworm live for some time after it was passed from the bowels, says the head and neck were in continual movement, and that the body was alternately elongated and contracted.

Prof. McGugin's account of the tænia of the catfish (published in the September number of this Journal) agrees with Bremser's as to the alternating elongations and shortenings of the body of this animal during muscular contractions. These do not, however, constitute the sum of its powers in locomotion; they doubtlessly give rise to the pains and disturbances of which patients often complain, and describe as pinching, gnawing, crawling, &c. These animals can execute the most complex and extraordinary motions. A lady for whom I prescribed koussou, discharged a living tapeworm, which had doubled up a considerable portion of the larger part of its body, forming a symmetrical duplication, around the middle of which it threw a single transverse band, tying therewith a very firm, beautiful bow

knot, which, probably, cannot be untied without breaking the tape-like bundle to pieces.

Nervous System of Tænia.

In the order to which the tapeworm belongs, no nervous system has been found. The late Prof. Burnett, in a note to Prof. Siebold's *Invertebrata*, (1854) says: "Blanchard (1848) appears to have distinctly made out a nervous system in *tænia*. With *tænia serrata*, there are, directly behind the proboscis, two small medullary nuclei united by a commissure; from these pass off on each side a nerve which is distributed to the lateral parts of the head, and connects with a ganglion situated at the base of each sucker, which sends filaments to the muscles of the last.—Posteriorly there are given off filaments which run parallel to the intestinal tubes. This, however, has not been confirmed by other observers, and Agassiz has made a statement in a private letter to me, which is worthy of notice. He says: 'I believe the nervous system described by Blanchard to be bands of muscular fibres which cross each other between the fossæ of the proboscis; at least, this is so in the new species of *tænia*, from *Amia calva*, which was observed alive for several hours; and I could discover no nervous threads, but only muscular fibres, which had exactly the arrangement of Blanchard's nervous system.'"—109.

Circulatory Apparatus in Tænia.

Prof. Siebold says, "a circulatory system has not yet been found with certainty in the helminthes"—117. The vessels, canals, and capillaries which exist in them, are not blood vessels, but belong to what he has termed "the water vascular system," "and contain some times water only, sometimes a peculiar secretion."

Generation of Tænia.

All experience seems to warrant the conclusion that no species of worms which are found out of the body can live long when introduced into the human intestines, while, on the other hand, it is equally true that the parasitic worms which inhabit the alimentary canal, die when they pass out of it.

The late Prof. Charles Caldwell, of Kentucky, maintained that, inasmuch as the parasitical worms, round and flat, are found in the alimentary canal, and as he affirms, no where else, they must be "produced by abnormal secretion," "equivocal generation;" "nature having the power to produce living beings without the instrumentality of specific parentage

all the result of settled laws;" "external nature does not produce these animals; from without they cannot be received; with neither our food nor drink are the animals or their eggs swallowed. Both the *tænia* and the *ascaris lumbricoides*, and, I believe, the *trichuris* also, have been found in the alimentary canal of the foetus in utero. The *tænia* moreover has been often detected in the livers, muscles, and other inaccessible parts."

* * * "A worm was found in the liver of Mrs. H., a lady of some note who died in Philadelphia, of an unknown disease. The worm was so large as to occupy a great portion of the viscus that contained it, and was of a species unknown to every naturalist who examined it. The dissection was witnessed by the late Prof. Rush, who spoke of it in his lectures; and the fact is recorded in the American Museum."

Although the experiments of Dr. Crosse, with galvanic electricity, were, as some persons thought, favorable to the doctrine of spontaneous generation, yet, the microscopic *acarus crossii* thus generated does not seem to be at all conclusive in this behalf, as has been argued in this journal by the late Dr. Hort, not to name others; these experiments by no means prove spontaneous generation, because the microscopic germs, or ova of the aforesaid insect, may have been introduced into the solutions, or into the air contained in the apparatus used by Dr. Crosse during his prolonged experiments.

The theory of spontaneous generation, of which the tapeworm has been supposed to be an indubitable instance, is wholly inadmissible. All analogy, all research, all zoological and physiological discoveries, tend to disprove or repel this theory, and to reduce, more and more, the number of supposed examples which have been brought forward to sustain it.

The tapeworm is doubtlessly subject to a series of metamorphoses, alternate generations, the phases of which are to the uninitiated little short of incredible, and to all extraordinary, though recent researches have thrown a brilliant, increasing, and even an unexpected light upon these hitherto obscure transformations of this parasite.

Whether the tapeworm be, as some contend, polyzoic, that is, an aggregation or colony of individuals conjoined to each other; or whether it is monozoic, that is, a single animal, will not be here inquired into.

Recent researches seem to show, that the tapeworm undergoes a series of transformations, called alternate generation, before it is developed in

the human intestines as a squatter; where only it gets the name and form, and assumes the functions now attributed to it. With respect to several other animals, at least, naturalists have ascertained that the offspring does not resemble the parent until after several generations shall have elapsed. Thus naturalists have ascertained that some insects pass through numerous metamorphoses, some of which take place in the bodies of other animals, and yet the offspring bear no resemblance to the parents until after from three to eight generations, whereupon they complete their double cycle of metamorphoses and alternate generation; returning to their original type or form, so that the great-grandchildren alone, and not the intermediate generations, resemble their ancestral primordial types.

To the casual, uninitiated observer one of these animals will seem, in its different stages of parent, child, grandchild, adult, &c., as so many separate and distinct animals.

Metamorphosis and alternate generation, are not antagonistic, but may co-exist, or co-incide in the same individual, so that a series of different metamorphoses through a series of generations may be necessary to reproduce in a gradual and progressive manner the original and perfect animal like that which was its progenitor—each change thus becoming a step towards the perfect finality, though the mode of generation may have been for each stage different—the first may have been by ovulation, the next by gemmation, another by fissuration, and so on. It would be too tedious to quote, on this occasion full illustrations of these principles from the ablest and latest authorities.

“At certain seasons of the year, the Sculpins of the Baltic are infested by a particular species of tænia or tapeworm, from which they are free at other seasons. Mr. Eschricht found that, at certain seasons, the worms lose a great portion of the long chain of rings of which they are composed. On a careful examination he found that each ring contained several hundred eggs, which, on being freed from their envelope, float in the water. As these eggs are innumerable, it is not astonishing that the Sculpins should occasionally swallow some of them with their prey. The eggs being thus introduced into the stomach of the fish, find conditions favorable to their development; and thus the species is propagated, and at the same time transmitted from one generation of fish to another. The eggs which are not swallowed are probably lost.

All animals swallow, in the same manner, with their food and in the water they drink, numerous eggs of such parasites, any one of which, finding in the intestine of the animal favorable conditions, may be hatched. It is probable that each animal affords the proper conditions for some particular species of worm; and thus we may explain how it is that most animals have parasites peculiar to themselves." (Agassiz & Gould. Prin. Zool—171-'2.)

The cause and the condition—the creation and the development of animal life and form belong to different categories. The seed enveloped for thousands of years in the cerements of an Egyptian mummy vegetates when it finds the essential conditions of development, as heat, moisture, earth; yet these do not create or produce the seed itself. Thus the ova of the tapeworm may be taken into the stomach in drink, or in animal or vegetable food, and on reaching the intestinal canal, may find the sole and essential conditions in which they can be developed into the form of a *tænia*, all other ova being lost or remaining undeveloped.

Prof. Siebold says:

"Although most of the Helminthes propagate by means of genital organs, yet there are a few species which multiply by *fissuration* and *gemination*.

"The *fissuration* is always transverse, and differs from that of the protozoa and zoophytes in the fact that complete individuals are not produced, there being only a separation of certain organs from the perfect animal, as, for instance, that of the segments of the body in cestodes. This *fissuration* is complete or incomplete. In the first case, occurring in the *tænia*, the segments are detached from the body, and continue to live independently, without, however, ever forming a new individual."—119.

"In the eggs of *tænia*, the envelop is colorless, and of a very variable, and sometimes quite remarkable form."—123.

Prof. Burnett inclines to the opinion that their development is not from eggs, but buds.—129.

"The entozoa," says Gluge, "must cease to form an especial zoological order, and several of them are now known to be only different stages of development of the same animal. The *cysticercus* is only a young *tænia*, and this is only a trematode worm without a digestive apparatus. The *acephalocysts* and *echinocci* also appear to be only steps of development of *cysticercus*."

"Some years ago, Dr. Kuchenmeister, of Zittau, had made use of the *cysticercus pisiformis*, a species of encysted parasite very common in the peritoneum of hares and rabbits, for a series of experiments, in which he caused these parasites to be swallowed by dogs and cats; in the expecta-

tion that they would become developed into tapeworms in the intestine of these animals. The trial succeeded perfectly with dogs."

In 1852, "Professor Von Siebold made a report on the experiments which were undertaken some months previously, in the Physiological Institute under his direction, for the purpose of showing the possibility of a transformation of the cystic parasites into tapeworms." The following results were obtained from the experiments "with the *cysticercus pisiformis*: These entozoa, which are usually about the size of a pea, were given to young dogs, mixed with milk, still inclosed in their peritoneal cysts, and in quantities varying from thirty to sixty." These dogs were subsequently killed, at various periods from three hours to eight weeks:

"After eight weeks, the cysticerci in the intestine of the dogs had attained a length of many inches. The largest were thirty-six to thirty-nine inches in length, and their posterior articulations were provided with a perfectly developed sexual apparatus, and contained many mature ova. Several of those a yard long had already thrown off their posterior articulations, with their mature sexual products. Von Siebold was now able to recognize, in this tape-worm, developed out of the *cysticercus pisiformis*, the *tænia serrata* of the dog. The cephalic extremity, the form of the articulations, the structure of the generative organs, and particularly the mature ova of this tapeworm, corresponded in the most perfect manner with the same parts in the *tænia serrata*. There was no longer any doubt that the *cysticercus pisiformis* of the hare and rabbit bore the same relation to the *tænia serrata* of the dog, as the *cysticercus fasciolaris* of rats and mice to the *tænia crassicolis* of the cat. Furthermore, the *tænia serrata* is rarely met with in the intestines of parlor and house dogs; but is, on the contrary, very abundant in hunting dogs; no doubt because the latter are often allowed to devour the entrails of hares killed in the chase, swallow at the same time the *cysticercus*, and so become infested with the *tænia*; a circumstance that would naturally be less frequent with parlor and house dogs."

Professor Siebold had already, in 1844, expressed the opinion that the parasitic *cysticercus* (*C. fasciolaris*), found in the liver of rats and mice, was nothing else than an abnormal, dropsical tapeworm; and that it was, in reality, identical with the tapeworm of the cat. (*Tænia crassicolis*.) He maintained further, that the *cysticercus fasciolaris*, like all cystic worms, was invariably destitute of sexual organs, and could not multiply its species by generation, unless it were transferred to a favorable locality, where it might lose its dropsical condition and develop its sexual organs. These changes actually take place when a rat or a mouse, with a *cysticercus fas-*

ciolaris in its liver, is devoured by a cat. The parenchyma of the liver, according to Siebold, is digested in the stomach of the cat; but not so the entozoon. The parasitic animal loses only its dropsical appendage, and passes, with the digested food, from the stomach of the cat into the small intestine. It then finds itself in a favorable locality, and becomes developed into a perfect tapeworm, with articulations and sexual organs, (*tænia crassicollis*.) This idea had been first suggested to Prof. Siebold by the perfect resemblance between the cephalic extremity of the *cysticercus fasciolaris* and that of the *tænia crassicollis*; and by the fact that there are often found, in the intestine of the cat, several specimens of the *tænia crassicollis*, in different stages of development.

Although Siebold's experiments with the other species of encysted entozoa, mentioned above, were not entirely finished, he had yet carried them so far with the "*cœnurus cerebralis*" as to convince himself that this worm also, which is so much dreaded by sheep breeders, becomes developed, in the alimentary canal of the dog, into a tapeworm. (Cited by Prof. Dalton, Buffalo Med. Journal, 1853.)

"M. Blanchard has investigated the habits and mode of development of the *fasciola hepatica*, an entozoon, very common in the livers of the ox, sheep, &c. In all his researches, M. Blanchard never found any but *adult animals* in these affected livers. In spring, myriads of their ova could be detected in the biliary canals, and ductus choledochus, and throughout the whole length of the intestinal canal; the nearer these ova approached the extremity of the intestines, the more fully developed did they appear. M. Blanchard considers it evident that the ova undergo incubation in their progress, are cast out of the body with the fæces, and again taken into the body with the food in a farther stage of development; on this latter point, however, he had no data for speaking decisively. In the *batrachia*, whose intestines swarm with entozoa, M. Blanchard has never found any but adult, or very nearly adult trematodes. He considers it very probable that in the human subject, the eggs of *tænia*, &c., are swallowed with the food; in some countries *tænia* seem endemic. M. Blanchard has never seen worms in the foetus—their existence is, however, placed beyond all doubt by M. Grätzer."*—Ranking's Abs.

The British and Foreign Medico Chirurgical Review, (Oct., 1852,) in a critical notice of Van Beneden's work on cestoid worms, holds the following language:

"The labor of such investigations must have been very great. A cestoid worm is not to be found in all its stages in a single animal. Dis-

*Comptes Rendus, p. 355, 1848.

covered in a cartilaginous fish in an adult state, for instance, the prey of that fish had to be found out, and in these animals the search was carried on for the earlier forms; perhaps, again, the worm was to be found, in its very earliest state, only in the food of these;—so that the youngest form of the parasite of a ray might be ultimately traced to a crustacean, to a mollusk, or to an annelid.”

“Those alone,” says Van Beneden, very truly, “who have worked far from their study, know what labor and trouble these researches cost; how wearied one gets in soul and body, when one has to look to everything oneself; to seek out fish, then to hunt for their parasites; when these are found, to examine, to dissect, to draw, to preserve them; to make one’s notes, and to do everything in a short time,—for, generally, they die very soon, and then undergo immediate alteration.”

“In whatever manner the young cestoid reaches its proper locality—the intestine—the succeeding changes are the same. The tail-end rapidly lengthens and enlarges; it becomes divided, in most cases, into well-marked articulations, in each of which a complete set of generative organs is formed. The development of the joints takes place, so that the hindermost is the oldest—the others being developed successively *in front* of one another; differing in this respect from the development of new segments in annelids and myriapods, in which the new formation of segments takes place between the last segment and the last but one. To this state, which is that under which the cestoid worms are commonly known, Van Beneden has given the name of strobila, from a just perception of the close analogy between it and the strobila condition of the medusæ observed by Sars.

Finally, the segments become detached and free, the posterior segment usually dropping off very early; and for these detached segments, to which the old writers gave the name of ‘ *cucurbitary worms,*’ from their resemblance to gourd seeds, Van Beneden retains the name of proglottis, first applied to them by Dujardin. The proglottides become rounded off at each end, and take on a certain rough resemblance to a distoma; never acquiring, however, any trace of the complicated organization of the latter, though they exhibit independent contractions. They are, as Van Beneden himself expresses it, but ‘a sort of sheath to the sexual apparatus.’

How long they are capable of maintaining this form is wholly unknown; but it seems probable that, discharged among the *feces* of the animal in which they were formed, they become the food of some of Nature’s many scavengers,—of some mollusk, or osseous fish, for instance, in whose intestine the eggs are set free, and the embryos developed into their ‘*scolex*’ form. In this state, it would seem that they remain until the mollusk, or osseous fish, is devoured by one of the carnivorous plagios-toms, or some other higher animal, when the cestoid worm starts from its wholly buried and half-dead state, into new life.

By the discovery of these extraordinary facts, the problem of parasitism among carnivorous animals seems to be pretty clearly explained.”*

Topography of Tapeworm.

The British and Foreign Medico-Chirurgical Review, in an article on the endemic diseases of Sweden by Dr. Magnus Huss, recently published at Stockholm, quotes thus:

“The inhabitants of the coast of the Baltic, at the head of the Gulf of Bothnia, from Tornea to Pitea, are grievously infested with *tænia-lata*, or tapeworm. Dr. Wretholm states that in Haparanda, the suburb of Tornea, there is scarcely a family, whether rich or poor, of which some of the members are not afflicted with this disease. He has observed it in children at the breast, who had never imbibed anything but their mother’s milk. From the coast the disorder spreads inland, following the course of the largest rivers; and it is ever most prevalent where large surfaces of water are found.

Dr. Wretholm observes:—‘As the result of the attention I have paid to this subject for several years, I may observe that the prevalence of tapeworm is in a direct ratio to the nature of the soil, and of the water used for drinking. In the higher tracts of land, where the drinking water is derived from springs or from the mountain brooks, *tænia* is hardly known; but as soon as we descend to lower ground, and especially to the banks of the larger lakes and rivers, where the houses are often built on the former beds of the lakes, and where the water, coming from morasses and bogs, is impure, and derived from wells sunk in the soil, and filled with extractive matter rapidly passing into decay, there tapeworm prevails among persons of both sexes, and in every condition of life.’—(p. 5.)

In Gestrikland (lat. 60°, 61°) *Tænia-lata* is, again, common as in Lappmark, and the reporting physicians ascribe its prevalence to the great consumption of salmon, a fish much infested with *tænia*. Is, then, the *tænia* of the salmon identical with that found in the human species?

The district of Halland is one of the poorest of Sweden; and here, consequently, uncleanness is seen in its highest degree. *Ascarides* are likewise common here; and strangers, who have never before been affected by these parasites, soon complain of their presence when they have settled in the country.

In the district of Smaland, children are constantly infested with the *ascaris lumbricoides*, while tapeworm but seldom occurs. Children of all ages, and all conditions of life, are equally subject to these tormenting parasites; and females often suffer from them during their whole lives.”

Helminthologists of the West, in their views of the topography of the tapeworm, seem to attribute too much importance to rivers, swamps, humid-

*“We are heartily sorry to find that such labors have seriously impaired the Professor’s health, and that we must not hope for a continuation of these researches from the same learned and conscientious observer. We trust, however, that his prognosis is not so trustworthy as his observations, and that we may yet have a renewal of the long series of contributions with which he has enriched the Comparative Anatomy of the Invertebrata.”

ity and low lands. In the East, in the very land where kouso flourishes, tapeworm is immeasurably more prevalent than elsewhere. These lands are high, dry, and to a great degree devoid of rivers, swamps, and depressed basins, as in Arabia, Abyssinia, and the coasts of the Red Sea.—Mr. Surgeon Vaughn, at Aden, Arabia Felix, in a letter to the London Lancet, says almost all, if not the entire population of those regions, are more or less affected with tapeworms, the kouso being a special blessing of Providence—a medicine within the reach of the poorest shepherd, and infallible in its effects.

Tænia is prevalent in Egypt, Russia, Poland, Switzerland, in most departments of France, and in some other countries of Europe, both insular and continental. In America its topography does not seem to excite, as yet, the attention of helminthologists.

The writer can affirm from personal observation that the hilly districts of Western Virginia are incomparably freer from tapeworm than the low lands of New Orleans.

Diagnosis of Tænia.

The diagnosis of tapeworm anterior to the discharge of a portion of its joints, per anum, is often obscure, seldom amounting to more than presumptive evidence. Among the numerous symptoms indicative of the presence of this worm in the intestines, the following are the most common: Convulsive motions in sleep; epileptiform fits; headache; disturbed vision; dark areolæ around the eyes; noises in the ears; itchings in the anal and nasal passages; capricious appetite; morning sickness; water-brash; eructations; vomiting; diarrhœa; intermittent colicky or gnawing or pricking sensations in the stomach and bowels; palpitation; sharp features; debility; emaciation; swooning; febricula.

Tapeworm produces, as might be expected from its enormous size and length, great central irritation, which is speedily radiated to the whole system. It produces irritation, without doubt, as a foreign, inert body in the bowels, and, also, as a living, moving parasite. Not that the animal bites and hooks into the bowels, as credulous pathologists gravely assert. When its large body, with angular serrated margins and articulations, distributed through twenty or thirty feet of the intestine, are all put into commotion, alternately contracting and elongating themselves, and even forming convolutions and dense knots, it is by no

means wonderful that the patient should suffer gnawing pain and constitutional disturbances. In one of my patients, who discharged a tapeworm after taking koussou, the worm tied a part of its body into a very artistic not.

The terrible effects attributed to the microscopic mouth and hooks of the tapeworm, are doubtlessly due to the violent contractions and motions of the animal. A late reviewer accounts for the patients' sufferings as follows :

“Consider the common tape-worm, for instance, with the general structure and habit of which troublesome parasite we have been long tolerably well acquainted. It is known to be found in the intestine of vertebrate animals, and of these only. The head with its four suckers and its proboscis, armed with strong hooks, is fixed in the mucous membrane of its unhappy host, whilst its jointed body ‘lies stretched for many a rood’ in the intestinal cavity.”

The following statistical ætiological statement, by a physician who practiced in the Danubian districts, indicates certain precursory and co-existent diseases, with which tapeworm is, by him, supposed to be connected.

“During the period of twenty years, Dr. Wawruch treated two hundred and six persons affected with tænia; seventy-one men, and one hundred and thirty-five women. The oldest person was aged 54; the youngest, 3½ years. Twenty-two were under fifteen years of age; and among these were six girls who had not menstruated. Most of the patients were from fifteen to forty years old. The patients belonged to the middle and lower classes; they nearly all inhabited the district lying along the Danube, or lived in low or damp dwellings. They followed very different occupations, and their mode of life was various. It is remarkable that the same conditions which give rise to scurvy in Vienna also produced tapeworm. *The articles of food which seemed chiefly to engender tapeworm, were bad bread, meals of milk, butter, cheese, potatoes, pork and mutton, and bad water. The diseases which preceded the formation of tænia were gastric and cutaneous affections, but especially gastric and intermittent fevers. There were observed forty-three cases of intermittent fever, twenty of gastric fever, sixteen of typhoid fever, ten of ring-worm and herpes, forty-two of itch, eight of scarlatina, thirteen of measles, and two of chronic urticaria. Scurvy, syphilis, chlorosis, and other diseases which affect digestion, were also observed to precede tænia; but, in general, few of the individuals affected with tape-worm, had not had lumbrici when young. The influence of hereditary predisposition is very doubtful. Dr. Wawruch only saw

two instances; in one, a mother and daughter; in another, a father and son, had tænia. Irregularity of the menstrual function seemed common; thus, thirteen females had only menstruated at the age of 16 years, twelve at 17, nine at 18, seven at 19, and one at 20. The duration of the disease was from a few months, to ten, twelve, fifteen, twenty, twenty-five, and, in one case, even to thirty-five years. Of the two hundred and six patients, three only, who were foreigners, had *bothriocephalus*; the others had *tænia solium*."—(Gaz. Méd.)

Therapeutics of Tænia.

It is not intended on this occasion to enumerate the various remedial agents usually employed in the treatment of tapeworm, one or two excepted.

Anterior to the actual discharge of one or more joints of the tapeworm per anum, as has already been stated, the diagnostics of its presence in the intestinal canal seldom rise beyond a strong probability. Hence the value of such vermifuges as kousoo and pumpkin seed, which are never deleterious to the patient, but to the worm only; while almost all other remedies of reputation necessarily disturb the patient's health, often severely, at least for a time. Hence, upon the empirical or experimental principle, an innocent vermifuge is admissible where the indications are even slight and would not warrant the exhibition of pink root, spirits of turpentine, &c.

A careful analysis of the facts connected with the successful administration of kousoo for the last six years, notwithstanding its occasional failure, would clearly establish on a firm foundation, the propriety of the class termed anthelmintics, which some writers on the *Materia Medica* have called in question or openly denied, so far at least as one agent can serve as the type of a class, kousoo, for its peculiar power of destroying tapeworm without causing, at the same, time any marked perturbation in the human economy, presents a valid claim to the rights of classification, as being not arbitrary, or spurious, but fundamentally scientific.

The evidence in favor of the unequalled efficacy of kousoo as a vermifuge, derived from oriental travelers and still more from the occidental doctors of Europe and America, is strong, increasing, and incontestable. The *Materia Medica* scarcely affords a parallel to kousoo for the efficacy and certainty with which a given indication is fulfilled.

A concentrated view of the evidence to this effect would prolong this paper too much, but would show that this remedy, unlike most of the new ones, has not been overpraised.

Many of the so called anthelmintics expel worms by their violent purgative action, which is often injurious to patients, who, for the most part are already debilitated from the intestinal irritation and consecutive diarrhœa occasioned by these parasites. At the same time, this class of worm medicine is altogether uncertain, very generally failing to dislodge worms.

Kousso.

Kousso, or Brayera, Anthelmintica (called after Dr. Brayer, who made its virtues generally known in Europe) as met with in the apothecary's shop in New Orleans, appears to consist of broken down calyces, petals, leaves, &c., or of a coarse powder of a dull brownish hue, which, when closely examined by the naked eye, and still more with a magnifying lens, presents many purplish particles, supposed to be disintegrated petals or flowers; the coarser and more predominant portion seems to consist of leaves, their ribs, stems, fibres, &c. This powder* has no well-defined scent, or taste; at first, there is a faintly saltish taste, which is soon replaced with one slightly bitterish, astringent, and acrid, somewhat like rose leaves. The tree belongs to the order rosaceæ, and is said to be about twenty feet in height, occupying the elevated table lands of Abyssinia, Arabia Felix, and the shores of the Red Sea.

Mr. R. Kirk, surgeon to the British Embassy to the Kingdom of Shoa, a few years ago, gave an account of the anthelmintic properties of Kousso, representing the tree as being one of the most picturesque in appearance. The Kingdom of Shoa, extending from 8° to 10° North latitude, consisting of two great platforms of table land; the lower, two thousand five hundred, and the higher, ten to twelve thousand feet above the level of the sea, has, in the day time, a range of temperature from 70° to 75°, with a yearly mean of 55½°. In the colder months ice is occasionally seen.

The Kousso grows on the table lands of Tigre, and other parts of Abyssinia, as well as in Shoa.

*Will Kousso deteriorate by age? The affirmative is, it is said, maintained in the East. Hitherto the article is said to have retained its virtues for years, in Europe.

Dr. Beke, who in 1841,-'2 passed a whole year in these regions, little known, says that, "This tree is found throughout the entire table land of Northeastern Abyssinia, but appears to require an elevation of upwards of six thousand (perhaps of seven thousand) feet for its growth. Where I found it most luxuriant, was in the vicinity of the source of the river Abai, (Bruce's Nile,) at an elevation of close upon nine thousand feet. Tigre, the Northern portion of Abyssinia, being, on the whole, of lower elevation than the rest of that country, the tree is only found there in a few places."

There can be but little doubt that this tree would flourish on the elevated lands of Southern Mexico, and in Central America.

M. d'Hericourt, of Paris, appears to have enjoyed the monopoly of the kousso trade, until lately. The price, originally about ten dollars per ounce, or five dollars per dose, has greatly declined. This article can now be had in New Orleans at \$2 50 per ounce, in phials near the size of the ounce phial of quinine.

It is believed that no kousso has ever been imported into New Orleans direct from Egypt, or Aden, where it is delivered by the caravans. Nothing could be easier than to adulterate this medicine in the state of a powder or broken down petals and leaves. It would, therefore, be advisable to get it in packages of the dried capsules and flowers, as put up in skins, where it is gathered, dried, and forwarded.

The dose is from half an ounce to six drachms, infused for an hour in a pint of boiling water; one-fourth to be taken every fifteen minutes, or half hour, first stirring the mixture, so as to take the substance as well as the liquid.

It should be borne in mind, that the discharge of a portion of the small thread-like anterior extremity of the worm, affords a strong presumption that the head has also been discharged. It is highly probable that the anterior undeveloped end of the worm, when broken into pieces, is not, like the posterior, capable of maintaining an independent vitality in each part; moreover, the head, with the adjoining portion, detached from the main body, in a great majority of cases, owing to its minuteness, would altogether escape observation.

During four of the trials with kousso, which I carefully watched, I was not able to discover any marked physiological, or rather patho-

logical effects, due to the remedy itself. The testimony is very strong, going to show that its effect on the economy is either null, or extremely slight. It sometimes acts gently on the bowels, and has been supposed to occasion nausea, though rarely, if at all.

Case of Madam H.—Kousso.

Madam H., aged about 35, married, mother of a small family, enjoyed vigorous health until about three years ago, at which time she began to emaciate; her digestive functions became impaired; her bowels disordered, and finally, joints of tapeworm were often passed, per anum; whereupon, she consulted some of the medical faculty, and passed through a long course of vermifuges without any advantage. In the winter of 1853,-'4, I was called to see her. She had just finished taking a great bottle of spirits of turpentine. I found her suffering from extreme debility, hot skin, abdominal pain and tenderness, diarrhœa, tenesmus, nausea, headache, muscular disturbances or irritability, quick pulse; symptoms much resembling typhoid fever. She had intermitting pains, of a dull gnawing character, in the right lumbar and iliac regions, particularly in and near the ileo-cæcal valve; from this deplorable condition she gradually, but slowly recovered her former health or *statu quo* as before the administration of the vermifuges, having used opiates, quinine, bitters, wine, and a good diet.

Here recovery from the fever, which latter was probably in a great degree, if not wholly due to the inflammation of the intestines from the vermifuges she had unsuccessfully used, left her still an invalid. For several months her general health, the tone of the alimentary canal, and the muscular forces, were in a more healthful condition. But at length, headache, intestinal pains, debility, and many anomalous and indescribable symptoms prevailed in various degrees from time to time. A course of diet, tonics, opiates, iron, bitters, combined with the milder vermifuges—also Carolina pink root, were tried, but emaciation with intermitting intestinal pains and griping continued. Kousso had been directed, but none could be obtained until after considerable delay; the article was found at Messrs. Sickles'. About six drachms were administered, in infusion, in divided portions during two or three hours, in the morning. No appreciable physiological or morbid action

resulted from the medicine. Early in the afternoon she passed a large quantity of broken up tapeworm—the longest portion of which was tied in a bow knot, and was alive, as was also, the cephalic extremity or head and neck. On the following day two drachms of kousso were given without any effect. After one month an ounce of kousso was again given in two doses on successive mornings. No worm was discharged. From the time that the first dose was given, now more than six months, no symptom of tapeworm has been felt. The lady's health has constantly improved, with the exception of an alarming uterine hæmorrhage three or four months since, which for several weeks reduced her strength. Her health is now restored.

The Pumpkin Seed Cure.

The seed of the pumpkin (*Cucurbita Pepo*) appears to be next to kousso as a therapeutic agent in removing tapeworm, being a true vermifuge, like kousso, expelling the parasite without producing physiological disturbance in the patient's health.

In the use of this remedy I have no experience. But the testimony in its favor is strong and increasing, though by no means equaling that in favor of kousso. As the medicinal virtues of the pumpkin seed emulsion, or powder, is ignored in the most recent American medical formularies, it is proposed to give a part of the clinical evidence extant, in relation to this and several other articles, in the *Excerpta* of this Journal, as its introduction here would extend this paper beyond the limit intended.

ADDENDA.

Galen (lib. iv, c. xvii) recognizes broad or tapeworms as being more pernicious than the round worms—"atque interdum latos eos, qui pejores," &c.—a distinction he alludes to in the treatment—"si lati sunt," &c.—if they be broad, &c.

Although a great number of the vertebrata are infested with tapeworm, it appears that the alligator is not; at least, I have never found the tapeworm in this animal, in which, however, the long thread-worm (*tricocephalus dispar*) abounds, both in the stomach and intestines.

ART. VI.—WORMS IN THE URINARY BLADDER.

BY BENNET DOWLER, M. D.

April 19, 1828. Mr. B. and his lady, persons of good moral character, eye witnesses of the facts here related, and the parents of the boy whose case follows; at separate interviews stated that their son, aged 13, had been in bad health for a long time, and had undergone several courses of treatment under different physicians without relief. His disease appeared obscure—its seat uncertain. He had been treated by one physician for worms in the alimentary canal, but none had been discharged.

About four months since, Mr. B. having assembled his neighbors for the purpose of "*a house raising*," at which whiskey was drank as usual; some of the men succeeded in getting Mr. B's boy to drink until he became quite intoxicated. A painful and obstinate retention of urine followed, which lasted four days, and which did not give way until he discharged several living worms from the urethra, some of which were fifteen inches long. He has since discharged from the urethra a number of similar worms—in all about twenty; three of which were delivered to me by the parents, inclosed in a phial of whiskey. One of these exceeds ten inches in length and is nearly one inch in circumference for the distance of three inches from the head backward. A longitudinal band which is smooth constitutes the belly.—One fifth of the body next the head has numerous well defined semi-circular rings, which are obliterated on both sides as they approach the abdominal band. The middle third of the worm is nearly smooth; the tail becomes rough, wrinkled, and annulated like the undeveloped joints of the tapeworm. The anterior portion of the body is nearly uniform in size for more than one-third of its length; it then abruptly diminishes to less than two lines in diameter; two considerable enlargements occur in the posterior third, or caudal extremity, which latter has a thread-like termination. These enlargements divide the tail into three unequal parts.

The head consists of numerous well defined rings, closely set, which compress the cephalic extremity more and more for a quarter of an inch—from the centre of which projects a proboscis, which at

its emergence is more than a line in diameter, and about two in length formed of rings, which become smaller until they terminate in the blunt proboscis which is studded with strong fangs or hooks. In the interior of the animal, adhering the back or dorsal aspect, two large parenchymatous columns, varying in size with the body, extends its whole length, being like lard in color, but a little firmer in consistence.

The other two worms appear to be of a different species, resembling *tænia* in structure, being flat and having numerous short annulated articulations with acute margins, like the joints of the tapeworm before development; one worm is five and a half inches in length—the other something less, both being almost uniform in size, scarcely averaging a line in width. All of these worms are white.

These worms, still in my possession, have not been examined microscopically.

Whiskey, for once, seems to have acted beneficially, having doubtlessly, in this case, proved detrimental to this nest of worms. The boy recovered his health rapidly after the discharge of these worms.—He removed, not long afterward, with his parents to the West. About seven years later I met with him in New Orleans, (in 1836,) in good health, pursuing his trade, in the saddlery establishment of Mr. John Hoey, on Tchoupitoulas street.

ART. VII.—REPORT OF A CASE OF VESICO-UTERO-VAGINAL FISTULA,
As treated successfully by M. Jobert, of the Hotel Dieu, Paris.

BY E. M. BLACKBURN, M. D.,

OF NATCHEZ.

Mme. Ponvard, aged thirty-four years, chair woman, was admitted into the ward Saint Maurice, on the 14th March, 1854. This woman, of lymphatic temperament, appears to be of a weak constitution. She has never been sick, however, with the exception of some rheumatic pains at the age of twenty-four years, which readily yielded to proper treatment. She began menstruating at the age of fifteen years, the menses re-appeared, regularly, until she arrived at the age of twenty-four years, at which time she became enceinte for the first time. The pregnancy was attended by no accident, her accouchement was easy.

The infant was born healthy, but died at the age of nine months. The menses re-established themselves five months after this delivery. Since this delivery, Mme. P. has become enceinte four times; her pregnancies and deliveries have always been easy, without any complicated accident whatever, although her fourth labor lasted longer than any one previous; why it did so, the patient can assign no cause. In 1852, the patient became enceinte for the fifth time. This pregnancy progressed happily; labor commenced at the proper time. This one (in March, 1853,) was long and difficult. The labor had already lasted some ninety-six hours, when the forceps were applied with success. The child was delivered dead, and from the statement of the patient was very large. It was not until the fourth or fifth day that Mme. P. perceived that the urine flowed entirely by the vagina, and that she had no inclination to urinate whatever. This infirmity has persisted since that time, and it is to disembarass herself of this that she now enters the Hospital. The menses re-appeared five months after this delivery; she has menstruated regularly every month since. The menstrual fluid flows out mixed with urine. This accident appears to have greatly affected the mind of the patient; her gaiety has disappeared, and her *embonpoint* is diminished.

May 6th.—Although this woman has been in the Hospital since March, M. Jobert has not yet attempted an operation, wishing her to become accustomed to the wards of the Hospital. She has been employing baths and emollient injections. To-day she was examined before us for the first time.

We find first an erythematous state around the anus, also around the vulva; second, vagina red and erythematous; third, the urethra is a little shrunk; fourth, by the touch we cannot recognise the fistula, we find only that the neck of the uterus is very irregular, and that the anterior lip above all, is formed of two tubercles, which are easily separated, one from the other; fifth, the direct examination discovers an opening at the union of the neck and the vesico-vaginal partition. This opening is irregularly rounded, surrounded by inodulary tissue, and bordered on the central walls by two hard tubercles, which are nothing else than the remains of the neck of the uterus. Its diameter is considerable, enough to admit, easily, the introduction of a female catheter.

The urine flows always into the vagina, no matter what position the patient occupies.

May 6th.—*The Operation.*—The patient is placed in the ordinary position, the posterior wall of the vagina is thrown back by the aid of the speculum-univalve; the labia being held aside by levers, M. Jobert implants there a strong pair of forceps on the vesico-vaginal wall, immediately before the opening of the fistula; then by the aid of a narrow bistouri, he pares the edges of the orifice, that is to say, he takes away the two tubercles, of which we have spoken, and which represent the remains of the neck of the uterus. Now we can appreciate the extent of the fistula; it presents the aspect of a cleft or gutter, formed, on one side, at the expense of the anterior lip of the neck of the uterus, and on the other by the vesico-vaginal partition. When M. Jobert had finished trimming the edges of the fistula, it presented a diameter of almost two centimetres. He proceeded then, by aid of a *Porte-aiguille* (needle bearer,) to introduce two points of suture—one in the middle, and one at the extremities of the fistula. As soon as the sutures are terminated, M. Jobert makes three incisions, two lateral and parallel to the orifice, and the third posterior on the grand vaginal cul de sac. He then made two injections of cold water into the vagina, and introduced a tampon of amadon. The patient was then carried back to the wards and placed in bed, with the pelvis slightly elevated, by the aid of pillows. A catheter was then introduced, to remain. Diet; two pots de tisane. At two o'clock in the evening, a small clot of blood was expelled by the vagina. The urine passes entirely by the catheter. Skin hot; abdomen a little tender. The tampon of amadon has been removed. May Seventh, light febrile movements; abdomen soft, not painful on pressure. The patient has felt, during the night, a desire to urinate. The urine is mixed with blood. Two bouillons, two pots de tisane.—Eighth, same state; no fever; the urine passes by the catheter.—Tenth, the catheter became obstructed during the night, and the patient is wet, as before the operation; probably the urine passes out by the vagina.—Eleventh, state satisfactory; no fever.—Twelfth, the urine passes off by the catheter, but the linen of the patient is stained with blood. We see, furthermore, a small clot of blood at the entrance of the urethra, and another more voluminous on the sheets of the bed; these clots are probably produced by the men-

strual fluid. For the patient says it has been five weeks since she menstruated.

May 13th.—The patient was examined to-day; the reunion almost complete. M. Jobert removes the sutures; no accident.

In the evening, the patient feels herself wet by the urine; it has probably passed by the vagina; the menstrual flow has completely disappeared. Fourteenth, the urine passes by the catheter; none has passed by the vagina; the catheter remains. The patient complains of some slight pains in the bowels—*Diete*. Fifteenth, nothing worthy of remark in regard to the urine. The night has been less favorable than the preceding. Light febrile movement; two liquid evacuations.—Sixteenth, the diarrhœa continues; tongue dry, red on the borders and at the point; abdomen somewhat swollen and tender on pressure.

The catheter is found obstructed during the day; the urine has accumulated in the bladder; none has passed by the vagina. The introduction of a new catheter is a little painful; it brings off a considerable quantity of urine. *Diete*, portion gommeuse.—Seventeenth, the diarrhœa seems to have disappeared, although during the day the patient had two liquid evacuations; no fever; no accident in regard to the bladder.—Eighteenth, diarrhœa; four operations during the day.—Nineteenth, the diarrhœa persists. In the night preceding four liquid evacuations. M. Jobert removes the catheter. During the day the patient has two or three times had a desire to urinate. The desire is so pressing that she scarcely has time to take the vessel. In spite of this she is found wet. In the evening the diarrhœa appears to have diminished.—Twenty-first, some colic; the laxity has diminished.

The urine flows involuntarily, which renders the patient always wet. It is impossible to know whether this urine passes by the urethra or the vagina. The catheter is again introduced.—Twenty-second, the patient can no longer support the catheter. M. Jobert orders it to be taken out.—Twenty-third the diarrhœa and colic have almost entirely disappeared.—Twenty-fifth, the catheter is again introduced. During the day she has slight colic; furthermore she has two serous evacuations.—Twenty-sixth, the catheter is again withdrawn.

The diarrhœa disappears; general state excellent.—Twenty-seventh, the patient is again wet, but the urine flows out of the urethra by regorgement. The desire to urinate is very frequent, about every fifteen or

twenty minutes, and always pressing.—Twenty-ninth, nothing new; the patient although wet, is much less so than before the operation.—Thirty-first, the patient examined. The cicatrix is complete, with the exception of a small opening about the size of the head of a pin, through which some drops of urine pass. M. Jobert cauterises it with the *nitras argenti*. A catheter introduced into the bladder does not penetrate more than four or five centimetres, which represents the length of the urethra and bladder, showing how much this organ has shrunk.—June third, the slight diarrhoea which showed itself three days ago, has completely disappeared.

The urine continues to wet the patient, but it passes out by the urethra.—Sixth, the patient is examined again; we see on the lateral walls, two cicatrices, very apparent, under the form of two white lines. In the centre we see another cicatrix formed by the rudiments of the neck of the uterus. This central point appears well cicatrised, although there still remains a small opening through which the urine oozes. We must recollect that the bladder of this woman is extremely narrow. Cauterization with *argenti nitras*. During the day the patient was a little wet. Ninth, the catheter is again introduced; the urine often passes out between the instrument and the walls of the urethra. General state excellent.—Thirteenth, we still observe a small opening which has not yet cicatrized.

One might say that the fistula was cured; but the patient cannot retain her urine more than an hour. Cauterization with *argenti nitras*. Fifteenth, cauterization with *argenti nitras*. She was not found wet during the day.—Sixteenth, the patient is wet. We might remark that the patient is never found wet during the day of cauterization, but always on the day following.

The catheter remains.—Twentieth, we see at the bottom of the vagina two or three drops of urine. In the place where the fistula existed, we find an inodulary tissue, perfectly drawn on all the points. In raising the vesico-vaginal partition, we see again a small tubercle, in the centre of which exists a small opening, which, probably, gives passage to the urine. Cauterization with *argenti nitras*. In the evening the patient is not wet.—Twenty-fourth, wetting by the vagina. M. Jobert thinks the urine passes through the urethra, and falls back into the vagina. Cauterization with *argenti nitras*. Catheter retained. Twenty-fifth, the

patient is again wet.—Twenty-eighth, cauterization with argenti nitras; about every hour the patient has a desire to urinate.

The urine sometimes passing involuntarily, after an effort at coughing. July sixth, we still see the small opening at the end of the vagina. The catheter is still retained. Cauterization with argenti nitras.—Seventh, the patient is wet.—Eighth, same state, cauterization.—Ninth, the patient still loses her urine.—Tenth, one small point is only to be seen, where exists an opening almost imperceptible, by which the urine oozes. Cauterization with argenti nitras.

In the evening no urine escapes by the vagina; 13th, cauterization with argenti nitras; since the 11th, the introduction of the catheter has been painful; evidently the mucous membrane of the urethra is becoming inflamed. To prevent this accident, M. Jobert withdraws the catheter.—14th, the patient is wet again.—17th, M. Jobert cauterizes the little opening with the nitrate acid of mercury. No catheter.—18th, the patient is wet.—20th, the patient is left to repose.—22d, cauterization with argenti nitras.—23d, as we find that the different cauterizations have been of little service, the urine still running into the vagina, M. Jobert thinks it necessary to practice immediately, one point of suture on the small opening before mentioned.

This he performs in the same manner as in the previous operation. Immediately after he introduces a catheter to remain. In the evening no urine has escaped into the vagina. The urine flows abundantly through the catheter and is perfectly clear.—24th, during the preceding night the catheter was found obstructed twice; in spite of this the patient was not wet. The desire to urinate was strong enough to awaken her twice during the night.

To-day no urine is found in the vagina; we cannot appreciate the length of time she is able to retain her urine as the catheter remains in the urethra.—25th, the patient is a little wet, but we are convinced that the urine flows through the urethra. The catheter is still retained.—27th, the urine flows freely through the catheter; during the night the patient had slight colic. The urine is somewhat mixed with blood—M. Jobert thinks that this blood is that of the menses, flowing through the urethra; as the neck of the uterus not existing, the menses must pass through the bladder.—28th, the hæmorrhage has almost disappeared; the urine passes freely. At the entrance of the vagina and opening of the urethra we see

two or three clots of blood.—29th, the patient is examined to-day; after the second operation, we see at the bottom of the vagina a lineary cicatrix, in the middle of which we see the loop of the suture thread. M. Jobert extracts it, and remarks that not one single drop of urine passes out.

The cure is now complete, nevertheless the catheter is continued.—31st, the patient is perfectly dry, but during the preceding night she was seized with considerable diarrhoea. The catheter is withdrawn. To-day the diarrhoea has diminished; abdomen somewhat tender.—August 2d, diarrhoea has completely ceased. The urine is retained without difficulty for nearly two hours.—3d, we see at the extremity of the vagina a surface perfectly smooth and dry; at the place where the fistula existed is an infundibuliform cavity.

The patient retains her urine more than two hours which proves that the bladder has developed itself.—5th, the patient is examined, and we find a streak of inodulary tissue where existed normally the neck of the uterus. Complete disappearance of the neck; no traces of fistula; around the point where the neck existed we see a radiation of the walls of the vagina. The width of the vulvo-uterine duct does not appear to have diminished; the length is a little less. The recto-vaginal wall makes a slight bulge, having the appearance of a prolapsus. The patient retains her urine at least four hours.—8th, M. Jobert examines the patient for the last time and finds the vagina perfectly dry. The catheter introduced into the bladder brings off one hundred and fifty grammes of urine perfectly clear and limpid. This shows that the bladder has taken great development at the bottom of the vagina, and to the right of the lineary cicatrix the remains of the neck of the uterus make a small bulge on the ancient fistula. Finally the patient can retain her urine a little over four hours. She makes her exit *completely cured*.

PARIS, Aug. 10th, 1854.

ART. VIII.—LETTER ON YELLOW FEVER.

BY M. MORTON DOWLER, M. D.

To the Editor of the "New Orleans Medical and Surgical Journal."

Argreeably to promise, I sit down to write you a word or two, in relation to the health of the Fourth District, for the last two months. I had intended, in the coming number of the "Journal," to have given,

somewhat at length, my views of the ætiology, pathology and therapeutics of yellow fever; but I have been prevented by business engagements. I hope to have my article prepared for the January number.

I may set out by saying that the front of the Fourth District has been scourged by the yellow fever, in its most malignant form. In every direction, the unacclimated have been indiscriminately smitten. Our District, is the great resort of the German and Irish immigrants, particularly of the former; and in my ample experience, which extends from 1837, to the present, I can safely say that, considering the comparatively small number of persons, who were subjects for the disease this season, I have never seen a more ghastly display of deaths doings. In the square bounded by Jackson, Tchoupitoulas, Josephine, and the Mississippi, I am informed that no less than twenty-five persons have died, besides which a number have been sent off, in a moribund state, to the Charity Hospital. There have been about thirty cases, several resulting fatally, in four houses, near the corner of Tchoupitoulas and First streets. From one house on Josephine street, near Tchoupitoulas, in which the German immigrants were literally piled and crammed, no less than twelve persons were carried out dead; and from the same house, a number were sent to the Hospital. From the German boarding house, at the corner of St. Andrew and Tchoupitoulas streets, a favorite stopping place for German immigrants, I learn that a great number have been sent to the Hospital, not a solitary immigrant escaping the disease. In the Irish districts of St. Mary and St. Andrew streets, none of the unacclimated have escaped. In the German portions of the District, the same thing is seen. Amongst these destitute people, black vomit, hæmorrhages and death have constantly occurred, since about the first of August. Those amongst the sufferers who could command the comforts and necessaries of life, and who early underwent treatment, have generally passed safely through the disease. The cases I have taken charge of, have been principally of this class. The filthy and destitute immigrants, to which I have been summoned, I have caused to be sent to the Hospital.

There is no change in the character of the disease. It is what it was last year, what it has ever been, and probably ever will be, one and identical; a pestilence, peculiar, specific—distinct from all other diseases, as non-contagious as the colic, and as little liable to importation or exportation. It stands out in bold relief, and alone, amongst fevers. In

many cases there is no "fever" whatever, but mere simple apyrexial debility, the morbid agency having acted so lightly that the organism is not aroused, and there is depression instead of excitement. I saw last year numerous *walking cases* of yellow fever, and this year I had a remarkable one. Yet some pretend to say that yellow fever is but a high grade of bilious remittent! I will give you some idea of the general course of treatment, I have pursued. I hold that yellow fever requires a treatment different from that of any other disease with which we are acquainted.

It should never have the treatment of an acute inflammatory disease, under whatever form it may appear; it should not be treated as a bilious disease, nor as a congestive disease. Some morbid agency has disturbed the functions of the body. That agency is a something which amongst other conditions produces disturbed vascular action, painful innervation, morbid temperature. A man is apparently in perfect health, and in less than an hour he is laboring under the severest symptoms of yellow fever. Here there is nothing but functional derangement. There are amongst other things three grand indications to be fulfilled, namely: to allay and relieve pain, to reduce the temperature if excessive, and to quiet the tumult of the heart and arteries. We must, at the same time, bear in mind that the organism has been assaulted with a noxious agency which has but a few hours of potency, which if left to nature must either give way to convalescence, or destroy the patient. All three of these indications, if the disease be taken at its very onset, can be fulfilled by a single agent and that is opium in sedative doses. In the epidemic of 1847, I satisfied myself of the incalculable value of large doses of opium at the very onset of yellow fever. The first case in which I administered the remedy was in that year in the case of a stout fat plethoric German woman, *æt.* 22. She was most furiously assaulted with the disease. There was burning heat, panting, surging of the carotids, universal pain, red eye, thirst, restlessness, delirium. In such cases I had in former years tried the lancet, I had caused my patients to be marked with the lived rings of the copper, I had used purgatives and refrigerants, I had used foot-baths, I had used warm-baths, I had sweat my patients with air-tight bed-clothes, and all had too often proven unavailing. In that year I had used the sulphate of quinine in doses of from twenty to thirty grains, in many cases repeating the same, and though the remedy has a very marked

salutary influence on the disease when administered early, it took but an imperfect hold on the symptoms in which there was but a treacherous pause. Quinine and relapse too often go together. I at once gave this patient five grains of opium; in two hours I revisited her; she was sleeping, but touching her wrist awakened her; she was covered with a single blanket and was literally flooded with perspiration, her pulse at first 98, was now 70; pain entirely gone. The heart and arteries were calmed; the nervous system was relieved, the proper temperature restored. The case went rapidly on to a favorable termination. During that year, I treated numerous cases of yellow fever with sedative doses of opium, when called early in the disease, with entirely satisfactory results. In the epidemic of 1853, I attended an immense number of cases of yellow fever. Either alone or in combination with quinine, I have as a very general rule, in the early stage of all cases requiring treatment, given powdered opium in decidedly sedative doses. I generally give from three to five grains in from twenty to thirty grains of quinine at a single dose, in a table spoonful of water, repeating the dose in six or eight hours if necessary. The quinine though it acts well in combination with the opium, acts a subordinate part. If the opium fall short of its full sedative effect, it produces injurious stimulation; hence in stout, robust, plethoric subjects when the onset is severe, four or five grains should be given at once, in combination with the quinine; in such subjects I have frequently repeated the five grain dose of opium. It is one of the most powerful of all sudorifics; it puts an end to the pain, it quiets the throbbing, lowers the exalted temperature. Its effect on the mind is one of its precious advantages.

The fear of death, which harrasses the panic-stricken patient, and often renders his case intractable, is assuaged under the calming influence of the medicine. Fear is a deadly debilitant, and should never be lost sight of in the treatment of disease, but should be met by some remedy which modifies nervous excitement. It is a most remarkable fact, that opium, thus administered, at the onset, in the febrile state of the disease, is very rarely objectionable on the score of these idiosyncracies, which so frequently show themselves under its use in other diseases, and in the latter stage of yellow fever.

But, perhaps, on mere theoretical grounds, I may be told that I am

endangering the life of my patient by administering sedative doses of opium in the furious excitement of yellow fever; and that I have nothing to expect but apoplexy, fatal coma, effusion, &c. If practical facts, exhibited in several hundred cases, in 1853, and not a few cases this year, show that no such accidents happened, in my hands, I leave antagonistic theories to their own worth. One of Molière's doctors was informed by the servant, that a certain patient of his, was dead. The doctor said it was false, and quoted Hippocrates—the time for the crisis not having arrived. The servant replied that Hippocrates might say what he pleased, but the man was most definitively dead. Theoretically, the man was living, but for all practical purposes he was done for. If the furious pumping of the heart and arteries be allayed and softened down, if the agonizing pains of the head, back, limbs and joints, &c., be relieved, if the skin becomes flooded with perspiration, if tranquil and non-comatose sleep be produced, then we have a right to infer that the patient has taken a salutary agent, a powerful febrifuge. But the theorist, cannot, from anything which can be said of opium, make out even a *prima facie* case against the treatment.

In yellow fever I have laid aside the lancet; I have dismissed the cuppers and leechers, *in toto*; I allow no hot mustard foot-baths, or body-baths, as there is generally more than enough of heat. I never make an effort by piling bed-clothes on my patient, to force a perspiration; on the contrary, if the patient is heavily covered, and there is a hot sweat upon him, I prefer to throw off the clothes, and cover him merely with a sheet, and allow the skin to dry and cool. A mortal injury is constantly done to patients by the *bed-clothes diaphoretic*. If the *calor mordax* persist, as it often does, under every known mode of treatment, we must be unremitting in our efforts to get rid of the heat. The patient must be stripped naked in the bed, and diluted alcohol, cooled to the utmost with ice, must be applied *à capite ad calcem*, the patient turned again and again, till the skin is effectually cooled. A couple of blankets should then be thrown over the patient. The hand of the nurse should frequently be thrust under the blanket to mark the returning heat, which should be the signal for renewing the application. Some patients will require two or three days, of this kind of nursing. There are cases in which the patients' life depends upon it. But the opiate generally produces an approach to the natural temperature, and a free

perspiration, without the forcing aid of bed-clothes. In the excessive perspiration, the patient is generally clamorous to get rid of the saturated under-clothes and sheets. I invariably allow these to be changed frequently. Shirts and chemises may be thrown off two or three times a day. Under all circumstances the air should frequently be admitted under the bed-clothes. The foul custom of allowing a patient to lie for five or six, or more days, in the same sheets and under-clothes, for fear of relapse, cannot be too strongly reprobated.

The opium itself, in powder, I prefer to any of its preparations. It far exceeds any of them in sudorific and febrifuge power. I have observed the sedative doses sometimes cause the urine to be retained in the bladder. In some instances the urine appeared to be increased in quantity. Retention of urine, however, often occurs in cases in which there has been no medical treatment, opiate or otherwise. The state of the bladder requires strict attention.

I wish here distinctly to say, that in a very great number of cases, to which the physician is called, the use of sedative doses of opium is wholly inadmissible. The same may be said of quinine or any other sedative agent. The opium must be employed early in the disease, or not at all. If the case has gone beyond the second day, all idea of such treatment must be in most cases abandoned. A dose or two taken on the day of the attack, which would produce the happiest results, might, if taken two days later, destroy life. There may be equal hazard in the latter case, whether the febrile symptoms have continued or subsided. In such case, if the fever has persisted there may be complications and lesions already existing which would render opiatism pernicious to the last degree, and if the pyrexial symptoms have subsided, whether the case put on a favorable aspect or otherwise, opium as a sedative, is out of the question.

When the case is, to all appearance, going on favorably and safely, and the febrile symptoms rapidly disappear, there is often great uncertainty of the patient being out of danger. In an hours' time there may be a total change in his case. The fever may again flare up; or in the midst of a most insidious and treacherous *apyrexial* state, in which he makes little or no complaint, the fatal black vomit may be lurking in his stomach. Even in cases in which no formidable symptoms arise, after the subsiding of the febrile phenomena, the patient has often a

great deal of uneasiness and suffering on entering into convalescence. There is very generally an uneasy painful sensation in the pit of the stomach, accompanied with tenderness, on pressure. This sensation frequently extends up the sternum, producing a disagreeable sense of constriction in breathing. The latter affection throws the patient into a perfect night-mare during sleep. A light stimulant dose of the *liquor opii sedativus*, a free dose of the ammoniated tincture of valerian, or what is perhaps better, a dessert spoonful of brandy, given occasionally, relieves the patient. Mustard may be applied for the abdominal tenderness, or in case of nausea; but I have entirely discontinued the use of fly blisters in yellow fever. If applied early they produce injurious stimulation, and whether early or late, they induce a vile hæmorrhagic appearance, excessively annoying and irritating to the patient, and sometimes discharging blood. Last year I saw a case in which even a mustard plaster produced a bloody oozing from the skin.

Fly blisters, in fact, are extremely objectionable in yellow fever, and in all hæmorrhagic diseases, on account of their action on the kidneys, ureters and bladder. The strong tendency to hæmorrhages, from every mucous surface in this disease, should, of itself, banish their use. Cantharadism in the urinary organs, in yellow fever, very generally produces hæmaturia, and it no doubt serves the renal and cystic epithelium in the same manner it does the external cuticle.

The administration of quinine, in large doses, at the outset of yellow fever, has often been found to produce such modification of the organism as to induce a sudden apyrexial crisis and rapid convalescence. Opiatism brings about the same condition with greater certainty. This sudden arrest of the febrile symptoms by quinine, has been called by the absurd name of *abortive treatment*, showing obviously, how we may bamboozle ourselves and others with a mere shibboleth. We will carry out the obstetrical idea. In some cases there is simple apyrexial debility from first to last. Here there is no abortion but sterility. In other cases the febrile symptoms spontaneously disappear on the same day, and here is an abortion without an abortionist. The same patient may relapse into a conception, or when the foetus is at first expelled by the ergotism of quinine, and the patient deemed effectually delivered, a new conception may cause him to die in a subsequent parturition. No rational physician should ever think of attempting to cast out devils in this manner. I have

never thought of striking at the cause of the disease, because that is unknown. I look to the prevention, repair, and moderating of injuries at the hands of an unknown enemy. Words often weigh heavily in medical science. Broussais treated his patients with gum water, leeches, and acid slops, and called his treatment the "physiological system." The leeches and gum water were but a small affair, but the term, physiological treatment, conveyed an idea of science, and turned the honest heads of physicians who had grown gray over calomel and jalap.

Whatever plan may be adopted in the treatment of yellow fever, (and our *Materia Medica* is abundant in resources,) our remedial means ought to be such as will be attended with the least possible expenditure of the patient's strength. It is not a disease which admits of severe and exhausting medication. It is not a disease for blood-letting, cupping, leeching, mercurialization, vesication, antimonials, or rattling cathartics. I tried them well, many years ago, and speak from experience. Yellow fever is strongly neuropathic in its character, and the physician who thinks himself justifiable in looking to the pyrexial character of the fever alone, and draining the system of his patient, greatly mistakes his case. Take blood-letting for instance :* Some years ago *V. S. ad deliq. anim.* appeared to be the panacea, in the Charity Hospital, if sufficiently often repeated. I tried that panacea. The patient is robust, and the fever furious. I bled him according to the above flippant formula. Oh! what a change. The patient is in raptures; for the pain, the heat, the excitement, are gone, and the perspiration drips from the surface. In less, perhaps, than two hours the whole phenomena have returned, and the throbbing is as though a previously bound demon had been let loose in the heart and arteries. Let the prescription be repeated. Fresh gratitude, and a renewal of the symptoms, with a quick, irritable pulse. Here the Sangradian, if he doubt, will recede; but if he be enthusiastic, he will

* Dame Nature has the greatest abhorrence of hæmorrhage. She spares effusion of blood in all her vital actions, save only in the urgent and indispensable case of providing for the continuation of our species. *Ovulation* and *Parturition* are her only acts which result in bloodshed; but in these conditions she acts with the greatest economy, and holds fast to her own tourniquet. She seals up and stops all the leaks that she possibly can, and stands by the surgeon, tying a living cord by the side of his threaden ligature. She seals up all the blood openings in the walls of the largest abscesses, which she causes to point, thin out, and burst without loss of blood. Though mortification may throw off at once its "pound of flesh" in one slough, she, by separating the living from the dead parts, allows "no jot of blood." In this respect she is parsimonious to the last degree. One of the strongest characteristics of yellow fever is the tendency to hæmorrhage, and thus to make war on the habitudes of nature. The Sangradians first assault her with their active hæmorrhages of the cup, lancet and leech, and then turn her over to tender mercies of passive blood-letting at the hands of the pestilential bleeder. I do not deny the virtue of blood-letting as a remedial agent, but let the Sangradian ponder well when he is dabbling in nature's food, and throwing it by bowlfuls to the dogs.

bleed the third or fourth time. The chances are, that he will, in either case, discover, in due time, that he has, with the best intentions, established a triple alliance between the lancet, hæmorrhagy, and debility. In any case, he has put his patient in additional bodily danger.

In the advanced stages of the disease, the circumstances that govern the treatment, are very diversified. Some cases may be greatly benefited by direct stimulants; others may require cooling salines, or alkaline remedies. Some patients may require, for a time, to not only abstain from all medicine, but even from drinks. The state of the bowels demands attention, and should they not be in an open condition, an aperient should be administered. One of the best, and at the same time least used of these, is the *sulphate of soda*. It is one of the mildest, and least objectionable aperients, and should take the place of that most disgusting of medicines, castor oil.—Should black vomit occur, we must give nature all the glory, if the patient recover. Nevertheless, we should never turn our backs on black vomit patients. If we do not continue to attend and “cure” them, perhaps some good old woman may; as has happened. The *vis medicatrix* smiled on four of my patients in 1853, and gave me the credit of the cures. One of these cases is so extraordinary, that I will glance at it for a moment: Miss L., æt. 18, sanguine temperament, light complexion, flaxen hair; tendency to *embonpoint*, was attacked with extremely urgent yellow fever symptoms, on the afternoon of the 14th of August, 1853. She took, immediately, gr. ijss of opium with gr. xx of the sulphate of quinine. The excitement abated, the pain was relieved, and there was free perspiration. I saw her next morning at 10 o'clock, at which time she took gr. ijss of opium and gr. x quinine, to meet a slight increase of fever. The febrile symptoms disappeared at the end of the third day, and there was a calm on the fourth day, which might have deceived the uninitiated into the belief that all was doing well. But there were breakers ahead. The eye, which was almost bloodshot from the beginning, showed now coagulated blood beneath the conjunctiva. Blood began to ooze from the gums, causing the tongue to look as though it had been dabbled in blood, and there was hæmorrhage from the nose. These hæmorrhages, however, soon ceased. Blood at one time escaped from one of the eyes. The sclerotica became of a very dark greenish yellow. On the fifth day she threw up large quantities of the genuine matter of black vomit; some of

it heavy, flaky, and settling to the bottom of the vessel, and some half diffused through the fluid vomited. She took the following :

R—Pulv. Aromat.
 Sodæ Exsiccāt. $\frac{\bar{a}}{a}$ $\frac{\bar{a}}{a}$ 3 ss;
 Ferri. Pulv. (u. s. p.)
 Pulv. Kino compos. $\frac{\bar{a}}{a}$ $\frac{\bar{a}}{a}$ gr. xii;
 M.ft. pulv. no. vj.

One to be given in a little water after each effort at vomiting. The prescription appeared for a time to quiet the action of the stomach; but the black vomit soon re-appeared. I then ordered her as follows :

Argent. Nitrat. gr. vj.
 Micæ panis, q. s.
 M.ft. pil. no. ix.

One every three hours.

She took the whole with evident improvement, the morbid irritability of the mucous membrane of the stomach appearing to be greatly diminished. The medicine even appeared to have some effect in arresting the sanguineous oozing, doubtless taking place from that membrane. The patient's skin soon after put on a singular aspect. In some places it was of a deep yellow, in others greenish yellow, in others of an orange hue, mixed with violet, in some places a *black-and-blue* appearance, the whole putting on the appearance that results from severe contusions, or from a thrombus which has discolored the arm from awkward blood-letting. Elevated tumors appeared in various parts of the body, indicating plainly that there were numerous subcutaneous hæmorrhages, lodged in the form of coagula in the cellular membrane.

There was a low, irritative fever, delirium, and tossing from side to side. The patient continued pretty much in this state to the 14th day. The hæmagastic symptoms having ceased, she had taken a little broth, milk and rice, and had also occasionally taken a little milk-punch, as a stimulant. The darker colors of the body began to fade, leaving the whole body a kind of greenish yellow. Some of the tumors which were principally about the breast, neck, and sides of the chest, began to put on a dusky red color, others did not differ from the color of the surrounding skin. I opened one of the latter, and found it to be a simple coagulum of blood, and also one of the former, and found it to be the same thing, surrounded by a sanguineous pus. I gave them all time for a slight sup-

puration and then discharged their contents. The patient began to convalesce, and at the end of five weeks quit her room. She lost her hair, which was speedily re-produced. She is now in the most perfect health. I gave her during her convalescence, the impalpable powder of metallic iron twice a day, which produced the happiest effect on the blood.

A good deal has been said in diagnostics about the proper color of the yellow fever patient, and a particular shade of yellow has been set down as pathognomonic of the disease. This case goes to show: First, that the patient may have every possible hue in the disease, from jaundice to purpura hæmorrhagica; Secondly, that the discoloration, whether the disease be yellow fever or not, is of the nature of hæmorrhage, ecchymosis, or bruise—some kind of bloody endosmosis. In what manner soever the blood may be affected in yellow fever, there is, as we know full well, a leaky condition of the solids, and often they will not hold blood.

In severe cases of yellow fever there is a remarkable tendency to abrasion of the cuticle in the scrotum of males. A large bloody vesicle appears on the posterior surface, which soon carrying away the whole cuticle, leaves a large cherry red or black surface, which is very formidable; sometimes there is hæmorrhage. A still more formidable condition appears in the *sinus pudoris* of females. In a hæmorrhagic case you will sometimes be told by the female or her friends, that her courses have come on, though her *pudeur* may prevent her from complaining of suffering in the vulva. Inquiry discloses the latter fact, and on examination you find that the whole epithelium has been carried away as far as you can explore, the whole being of a dark cherry red, and there is an exudation of an offensive sanguinolent discharge. The vulva is tumid, and the labiæ everted by the intervening tumefaction. The denudation of the urethral *meatus*, renders it impossible for the patient to urinate, and the catheter must be used, which gives the patient acute pain. I saw a case of this kind in a child, seven years old, recently.—An Irish girl, aged eighteen, who died with black vomit* a few days ago, had this most formidable lesion. I saw several such cases last year; they were totally unconnected with any venereal affection. This hæmorrhagic condition of the vaginal mucous membrane may give us an idea of the terrible condition of the

*She passed about half a gallon of black vomit *per anum*, in bed, just as she expired.

gastric and intestinal mucous membranes during life, and which we are often unable fully to realise after death.

I have had only five cases of negroes, as yet, in the epidemic of 1854. I treated them without reference either to free-soilism or the ultraism of your world renowned contributor, Dr. Cartwright, who, whatever he may expect from the laity, cannot expect any medical man, be he fire-eater, unionist or abolitionist, to swallow his paradoxes with regard to negroes. My first case, a woman, was the only one of interest. She was the fat, stout, lusty slave of Counsellor P. The fever was furious, and there was considerable hæmorrhage at the nose. Here was a case for Dr. C.'s "hot sun," his "oil," his "strap," his "turnip greens," his "corn bread" and "fat bacon." I, however, took my "*hot and black knee-bender*," and treated her on the same principles I would have treated a golden-haired daughter of Japhet. The opiate acted in the most salutary manner. She showed no repugnance to "white folks' diet," but, on the contrary, during convalescence, her disposition to substitute the kitchen for the sick room amounted almost to a "*drapeto-mania*."

I have spoken of *apyrexial* and *walking cases* of yellow fever, in which the pestilence has lightly touched the patient. I have, however, said nothing of those cases which occasionally occur, in which there is little or no febrile excitement, owing to the excessive and overwhelming malignity of the attack. The patient takes his bed, and the uninitiated would suppose that little or nothing ails him, there being little if any febrile excitement. But the disease has overwhelmed him at its inception, and the organism is unable to react. I have at the present time, (Sept. 25,) in one house, in Jefferson City, three cases in one room which will serve for illustration in a remarkable manner. Eight days ago, the daughters of Frau M., Marianna and Wilhelmina, the former æt. 20, and the latter 18, all German inmigrants, were attacked with the pestilence. Wilhelmina was attacked with a simple debility and perspiration, and slight cerebral and articular pains, without febrile symptoms, from which she has not yet completely recovered. I left the case to nature, diet and good ale. During her attack she has menstruated naturally. Marianna was attacked with great depression, slow pulse, skin nearly natural, injected eye; made little or no complaint; started up in a querulous manner when spoken to. She has been free from febrile excitement from first to last. During the last three days there have been, occasionally, nasal hæmorrhage,

and bleeding from the sides of the tongue, and in the mean time, though her menstrual period had not yet arrived, she has had a copious sanguineous discharge from the vagina, and there is some epithelial abrasion in the external parts. To-day she is threatened with black vomit. In none such cases can either quinine or opium be administered. Frau M. took the disease three days ago, with the highest febrile excitement, which was promptly put an end to, and a favorable crisis was brought about by the administration of opium. In 1853, I saw a deadly *walking case* of yellow fever. The patient, a German servant girl, about eighteen years of age, had attended to her work for two or three days, all the time feeling very unwell. I was requested to see her, in order to determine whether it was necessary to send her to the hospital. She came to meet me, looking extremely ill—denied having had any fever. I ordered her to go and lie down. She died the same day with black vomit. How peculiar this pestilence! How different from all other *fevers!* Mr. P. sent for me two weeks ago, to prescribe for his wife, who was lying ill with the disease.—He was resting by the side of his wife on the bed. “I wish you to prescribe for my wife,” said he, “she has got the yellow fever.” I looked at him, and told him that he was as bad off as his wife. He gazed at me with an incredulous stare, and said: “Not at all, sir; I have only a slight cold. I require, and will take nothing.” There was only a slight excitement of the pulse, very little pain, heat scarcely increased, yet it was easily seen that the man was in the deadly coils of the serpent. His case proved completely intractable, and he threw up astonishing quantities of black vomit before the fatal termination, on the fourth day.

There are a large number of hæmagastic cases, in which the patient does not, and cannot, throw up the black vomit. There are generally retchings, straining, a thrusting forward, and acute pointing of the tongue; what is called, in the Hibernian dialect, a *dry boken*. Mrs. L., whose case was noticed in the September number of this Journal, was affected with these symptoms, in connection with nasal hæmorrhage. If there be a large and sudden accumulation of the black vomit in the stomach, it sometimes boils out of the patient's mouth, as he lays on his back, or it may be squirted across the room with considerable force. On the contrary, if there be a slow and partial exudation from the mucous membrane, all the conditions of black vomit may exist, without any ejection of that fluid from the

stomach. Hence, a much greater number of real hæmagastric cases recover, and a much greater number occur, than is generally believed.

There are certain severe cases that, having passed the ordinary crisis of the disease, fall into an anæmic, jaundiced, decayed, adynamic condition, in which there is a protracted tenacity to life, which is most unaccountable. There is a slight fever, which appears to be all that is left of life. I have, every epidemic year, seen samples of these breathing corpses. I have invariably here pursued the alcoholic treatment, and many times with the happiest results. I have had several remarkable cases in the course of my practice. I will glance at one I had in the epidemic 1853: I was called to see S., coffee-house keeper, a stout, athletic German, æt. 40. He had allowed the sixth day to pass without any medical treatment. He was of an intensely deep greenish yellow, all over his body; there was extravasation of blood on the sclerotica, which had a deeper color than the skin. He was retching and straining to throw up, and ever and anon he raised a small speck of the black vomit. I gave him eight grains of the nitrate of silver, in broken doses, at three hour intervals, in pills. He took, also, simultaneously the following:

℞—Confectionis Aromat., ʒj;
 Aq. Cinnamomi, ʒiv;
 Creasoti, gtt. iv;
 Liquoris opii sedativ, ʒss;
 Syrup. Acaciæ, ʒiiss;
 Miscè Optime.

Take a dessert spoonful after each spell of straining and retching. These symptoms were mitigated; otherwise his case was as bad as possible. There were debility, delirium, and subsultus. I temporised with him for several days, during which time he appeared to be neither better nor worse; I then began the alcoholic treatment. His urine being scant, and resembling spoiled porter in its appearance, I preferred gin as an alcoholic, owing to its diuretic properties. He took a table-spoonful five or six times a day, which was continued for several days, taking small and often repeated portions of tender animal food. He took, also, night and morning, a few grains of the impalpable powder of metallic iron. He improved daily, and left his room after about four weeks of sickness. I have generally treated cases of this kind with brandy. Alcoholics, as a luxury must be

condemned by every well regulated mind, but they can often be wielded to the greatest advantage in the practice of medicine.

The yellow fever presents some specialities with regard to children, of the greatest interest. The epidemic of 1853, has thrown further light on a fact of which I was long since convinced, namely, that the children born and stationary in New Orleans, all undergo what is called acclimation, and take the yellow fever, either at the breast, as they now are doing, and as they did in 1839, '41, '43, &c., when we had an epidemic every alternate year; or at all ages under five years, as they did in the epidemic of 1853, when five years had elapsed since the great epidemic of 1847. The term *acclimation*, as defined by Dunglison, is totally inapplicable to the process by which immunity is acquired against yellow fever. That process is not a thing of *climate*, but of mere locality. There is no difference between the climate of New Orleans and that of all the Southern parishes of the State; yet the city has been regularly smitten, while the parishes have been exempt from yellow fever. The creoles of these parishes have been born, have lived and grown gray there, and never were attacked till 1853. It is not our climate that is chargeable with the disease. The patient takes the disease but once, and his protection is good for all climates; and that protection the patient can only acquire by having once undergone the disease. The term *acclimation*, if thus construed, I do not object to. One word is as good as another, provided the proper idea be attached. Now it was apparent to all that during the epidemic of 1853, the native and fixed population of New Orleans, over five years of age, did not suffer from yellow fever, whilst those under that age were almost indiscriminately attacked. The children in New Orleans, under five years of age, were nearly all "unacclimated;" whilst old and young, who were born and resident in our parishes, out of this yellow fever locality, were last year in the same predicament. The yellow fever is now prevailing amongst children under one year old. My own child, Henry Summers, born last November, had the disease a few days ago, and is now "acclimated." My two other children, born here, became "acclimated" last year; both born since the epidemic of 1847. I am now accosted on this wise constantly, in visiting patients: "*I wish you would look at the baby, it don't seem well,*"—

the infant having nothing more nor less than the yellow fever. Infantile yellow fever is, happily, a light disease; in ordinary cases, it requires little, if any, medical treatment. Nature espouses the cause of the little patient, and the case generally goes on well. The body may be sponged with cold spirits, and an aperient administered if necessary. We must, however, be on our guard; if the febrile action be high, convulsions may come on, and black vomit may attack very young children. Last year I saw convulsions, black vomit, and death, in case of a native born child, of 18 months. I saw the same thing, with recovery, in 1841: A boy, six years old, born and resident here, was attacked with black vomit last season, and was a month recovering. The same principles of treatment should be applied to children as to grown persons. Common sense will teach caution in the use of quinine and opium in such cases, as they strongly affect children. In most of the cases nothing will be required. A simple refrigeration of the body with cold spirits, and wrapping the child afterwards in a blanket, will put an end to the febrile symptoms.

I cannot close this communication without noticing one characteristic of yellow fever which should never be lost sight of by either physician or patient. The visible, cognoscible symptoms of yellow fever are often speedily put an end to, either by nature, or through the agency of medicine. The patient is to all appearance perfectly liberated from the yellow fever agency, whatever it may be, or the patient may have, on the contrary, gone through a tedious and dangerous attack, and have attained this condition. Nevertheless, a *relapse* may take place, and the identical or still worse symptoms may afterwards attack the patient. Some have advanced the monstrous absurdity that it is not right to put an end to the symptoms too soon, for fear of relapse, and the same argument might be addressed to Nature when she "cures" the patient at the very onset. We shall not stop to discuss such inanities. We merely wish to notice the undoubted fact, that when the visible and cognoscible symptoms of yellow fever disappear, by what means soever this may be brought about, the yellow fever agency, whatever it may be, still remains with the patient in a latent, innocuous and inactive state in all cases, for a considerable length of time. I have known a mortal relapse to take place in five weeks after the subsiding of all the visible symptoms of a protracted first attack.

No physician who is at all acquainted with yellow fever will pretend that there has been an interregnum in the yellow fever agency, and that this is a separate and distinct attack. There is no second attack in yellow fever, according to my experience of eighteen years in this city. I would sooner expect a second attack of small pox. I could cite many instances to show that this latent condition may continue for weeks, and that the above morbid symptoms may again arise. That there is no separate, independent attack, is perfectly conclusive. If, then, the yellow fever agency is thus persistent, how solemnly are the physician and patient warned to guard against all causes that may tend to awake the sleeping destroyer in his lair!

But I draw this epistle to a close, already too extended.

Rousseau Street, Fourth District, New Orleans, Sept. 25, 1854.

Part Second.

EXCERPTA.

ART. I.—*Dr. Smith, of Warsaw, on the Hydrochlorate of Ammonia, and its Therapeutic Uses. Translated from the "Revue de Thérapeutique Medico-Chirurgicale."* By M. MORTON DOWLER, M. D.

The discovery and therapeutic application of the hydrochlorate of ammonia, muriate of ammonia, sal ammoniac, *hydrochloras ammoniacus, sal ammoniacum, ammoniacum muriaticum, chloretum ammonici*, may be traced back to a very remote period. It is said that its name, *sal ammoniacum*, is derived from that of the province of Ammonium, in Lybia, in which stood the temple of Jupiter Ammon. Blancard (*Lexicon Medicum Renovatum, editio tertia Hallæ, 1739*) expresses himself in these terms: *Ammoniacum sal nativum, Cyrenaicum, veterum a majoribus descriptum, sub arenis Lybicus concresebat. Ex notis bonitatis quas Plinius indicavit, nostro hodierno factitio similium apparet. Verum, nostralibus non nisi artificiale cognitum est. Quod nihil aliud est, quam sal factitium, compositum, volatile, sublimatum, ex urina et sale communi, vel sale gemmæ et similibus. Tale et circa ignivomos montes crustatur variis terræ locis. Vesuvi optimum censetur hodie. Eligitur candidum ac purum, vel nisi satis nitidum fuerit, purificari potest.—Præstantius habetur Antuerpianum et Venetianum. Alchimistæ in suis libris varia et partim monstrosa nomina, prodesignando hoc sale adhibent, quibus referendis supersedere liceat. Nominis ratio obscura. Forte ab arena, quia olim credebatur ex urina jumentorum in arenosis Lybiæ condensari, aut a Jove Ammone, ad cujus templum per arenosos illos tractus iterfaciendum est. Dicitur et sal mirabile et clavis metallorum quæ fluere facit. Signum ejus chemicis est asterismus.* At any rate, the primitive manufacture of this article was in Egypt, and hence it received the name of *Sal Ammonici Ægyptiacum*. In Germany, it was manufactured for the first time in 1759, at Braunschweig. At present, it is manufactured in various ways, as by the dry distillation of bones, and the combination of ammonia which they form with hydrochloric acid. The salt which is used for medicinal purposes should be purified by being dissolved in boiling water, and by repeated crystallizations. The hydrochlorate of ammonia crystallizes with great facility. Its crystals are prominent hexaëdral prisms, of a white color, and have a sharp, saline taste. It dissolves in three parts of cold water; boiling water dissolves still more. It is but little soluble in alcohol.

Action of the Hydrochlorate of Ammonia on the Animal Economy.

Locally, the salt acts as an irritant, but not to the same extent as the carbonate of ammonia. Internally administered in small and medium doses, its action resembles that of all the neutral salts, acting on the intestinal mucous membrane, and producing a cooling effect, analogous to that of cold water. It neither acts on the nerves nor vascular system, nor does it sensibly affect the pulse. Nevertheless, given in doses of eight grammes, (3 ii) and continued for several days, it brings about (as do all the ammoniacal salts) a sensible increase of the cutaneous transpiration, increased secretion of urine, and in a special manner a notable increase of the bronchial mucus. The pulse undergoes little or no change. But if the doses be still further increased, and if they be for a long time continued, the nervous system becomes affected. The head then suffers, and there is cephalalgia, vertigo, and in the mean time, the stomach and intestines secrete a greatly increased quantity of mucus. The digestion becomes simultaneously deranged, and there are vomitings. The patient acquires (al though rarely) a sickly color, the cutaneous transpiration, the urine, and the bronchial secretion strongly increase, while the animal heat undergoes no sensible change, so that sal ammoniac appears to act in general on digestion and nutrition in the same manner as the alkalis and the other salts.*

Given in large doses, it produces irritation of the *primæ viæ*, and violent vomiting, diarrhoea, diuresis, copious perspiration, and all the symptoms of inflammation of the stomach and intestines.

Gumpert † has observed, that as a result of the administration of sal ammoniac, given in quantities of from 15 to 24 grammes (3 ss to 3 vj) in 24 hours, in the treatment of strictures of the urethra, indurations of the liver, chronic cystitis, &c., (in which case it is necessary to give 250 grammes, (3 viii) at this average, in the space of about four weeks,) it produced a combination of symptoms which simulated the pituitous diathesis. The patients swallowed with a kind of repugnance; their eyes were heavy and acquired a peculiar lustre. The whole body had a sense of fatigue, lassitude, and depression; the tongue was white, the patients felt a continual biting sensation between the skin and the muscles. They coughed continually, owing to a sense of tickling in the throat, without, however, being able to expectorate much mucus. Their stomachs, though empty, loathed food. Ordinarily the perspiration is increased, and the sweat breaks forth every time the patient moves. The urine flows abundantly, has a strong ammoniacal smell, and sometimes becomes putrid, without losing, in consequence, its ordinary clearness. The stools contain a glairy and glassy mucus, though this is not, nor is diarrhoea constant attendants. This state does not continue long. The patients have an attack of fever, which ends in an abundant perspiration. After the paroxysm, which resembles that of an ordinary intermittent

* Wibmer (Beitraege z. Heilkund t. 1, 1849.)

† Schmidt's Encyclop., t. 1, p. 116.

fever, there is a complete remission, during which time the symptoms, before mentioned, successively disappear. There remains only for a long time, a loathing of sal ammoniac, followed by a notable improvement in all the morbid phenomena. When this remission does not take place the consequent paroxysms assume the septenary type. They manifest themselves so long as the system is sufficiently under the influence of sal ammoniac, and they cease or become more feeble, so soon as this state of saturation disappears. To produce this state, Gumbert found that he never had to administer more than from 250 to 300 grammes ($\bar{3}$ viii to $\bar{3}$ ix). In every case it appeared that the sal ammoniac given in large doses, passed into the mass of the blood, unchanged, and that it was excreted by the skin, and especially by the kidneys, without undergoing any alteration. *Thinefeld* poisoned rabbits with strong doses of sal ammoniac, and he found in their urine ammoniacal crystals.

Therapeutic Application.

The hydrochlorate of ammonia is one of the most heroic agents of the whole *Materia Medica*. The physician may give it without apprehension at the outset of disease in cases in which he has not yet been able to determine either the kind or character of the malady to be treated.* It lessens the irritability of the vascular system, augments and favors the secretion of the gastro-intestinal and bronchial mucous membrane, rouses the functions of the skin and kidneys, and it very often suffices alone successfully to combat the disease without disturbing in any manner the critical efforts of nature. In many cases it is preferable to mercury, to iodine, to antimonials, being one of the best alterative medicines of which the *Materia Medica* can boast.

The long continued action of this agent produces a serosity of the blood, and may even coagulate it. *Wibmer, Fischer, Oesscerlen, Roesch*, and others maintain, on their own experience, that this great solvent can determine the resolution of membranous and glandular tissues in hypertrophies, indurations, and scrofulous infiltrations into different organs and tissues; as in tubercles, schirri, induration of the

*"Le médecin peut le donner sans crainte dans tous les cas où il n' a pas encore saisi dans les premiers jours ni le genre ni le caractère de la maladie," which, being very literally translated, *mot-à-mot*, would read thus: "The physician may give it without fear in every case in which he has understood in the first days, neither the kind nor character of the disease." While we have given a very free translation of the author, in order to avoid broken English, we have maintained a rigid regard to his meaning. How our Polish physician, who appears to be a man of eminent attainments and extended experience, should have given the absurd license to prescribe sal ammoniac, contained in the above sentence, it is not easy to conceive. We should say positively, and *à priori* that no physician should give his patient sal ammoniac or any other such potent agent, "sans crainte," unless he has "discovered the kind and character of the disease." That is a regular *sine quâ non*.

So vast a tribute to the merits of sal ammoniac falls little short of that which was made to this agent in "Peregrine Pickle," by the erudite old doctor, who was so wonderfully carried away and captivated with the whole of antiquity. He resolved to get up a dinner to a large circle of distinguished friends, and to have the whole repast served up according to the manner of the ancients, to enable him to show off by contrast, the handy-work of one who was regularly *mageiricos*, with the foppery and superficiality of the modern gastronomers. Amongst his other *bicarreries* of dishes and condiments, he concluded to invoke the assistance of Jupiter Ammon to help his guests to a good digestion, and to this end, he seasoned his numerous viands with a notable sprinkling of sal ammoniac. The classic feast naturally terminated in a general stampede; and the novelist gives a graphic description of the effect of the condiment on the healthy organism in which its emetic properties is exhibited to the highest advantage. [Translator.]

prostate, of the spleen, liver, &c., in blenorrhagia of the bladder, (vesical catarrh,) induration of the mammæ, and even in hypertrophy of the pylorus and œsophagus. This resolvent virtue that we have ourselves observed, has led to its very general employment, especially in Germany and Poland, in place of the alkalis, the antimonials, iodine, and mercury. It is but little employed in France and England. In my practice, which extends through twenty years, I have been able fully to appreciate this salt, and the frequent use I have made of it, enables me at present to designate the affections in which it is really efficacious. All that I advance has been consigned to my journal, and rests in rigorous observations and experience.

Pituitous Fevers,—Mucous Fevers.*

The pituitous fever frequently appears in the localities where intermittent fevers prevail. This disease makes its appearance sporadically, during warm, humid summers, especially amongst the country people, whose diet consists of farinaceous food, milk, water, &c. The period of the first appearance of the fever is often difficult to ascertain, and it is not till after an attentive inquiry, that it can be made out, for, in general, the patient has not recourse to the physician, till the eighth or ninth day from the invasion of the disease. In no year have I had to treat so great a number of diseases of this sort, as in the years 1843, and in 1852,-'3; cases which I treated in connection with intermittent fever.

The following are the symptoms of this fever: After eight or ten days of initiatory symptoms, which exhibit themselves in loss of appetite, regurgitation, indigestion, orbital cephalalgia, a tongue charged with a whitish mucus, with a clammy taste, eructations, diarrhœa, a light chill every evening, traversing the back and thighs, followed by a sweat, which greatly enfeeble the patient. The pulse becomes gradually frequent; the teeth, gums, and tongue are covered with a thick mucus, and the fever has exacerbations and intermissions, which are so slight as to be scarcely discernible. The patient has a bloated and leuco-phlegmatic appearance, and the eyes and mouth are encircled with pale bluish rings. Towards morning there is vomiting or retching, coughing up of saliva and mucus, slight thirst, great depression, urine red and clouded, diarrhœa, nocturnal sweats, a miliary eruption, insomnia, slight delirium, showing that the nervous system has fallen into the disease. I have observed amongst many young and robust subjects, attacked by this disease, that it took on a sub-inflammatory character; that hepatitis, bilious fever, and especially passive pneumonia and acute pharyngitis, complicated the malady, and

* The Polcs appear to suffer severely from the *pituitous* diathesis; also the French people. Sue makes two of his characters, in the "Mysteries of Paris," fight a duel on the strength of it. M. Robert demands of the Duc de Luçenay if it be true that he has asserted that he, M. Robert, has phlegm. M. le Duc makes it a point of honor, to neither avow nor disavow that he has so asserted. M. Robert challenges the duke, and a shot is exchanged. The duke then comes forward, and frankly declares that it had never entered into his head, to either think or say, that M. Robert had phlegm. M. Robert's feelings overpowered him, at the manly avowel of the duke.

that it became transformed into a quotidian intermittent fever, or into a masked intermittent, with cephalic symptoms. Ordinarily the cure was slow; if the patients had long delayed resorting to medical aid, they had great difficulty in re-establishing their bodily and mental forces. I have observed, at the same time, that during its prevalence this year, colics, diarrhœas, and cardialgias frequently made their appearance.

The plan of treatment pursued, and which was successful with me in all of my cases of pituitous fevers, was as follows: 1st, to increase the fluidity of the mucus, and to thus facilitate its expulsion by the exhibition of sal ammoniac: 2d, to expel the mucosity by purgatives: 3d, to fortify the digestive organs, and subsequently attack the cause of the disease, by appropriate dietetic and pharmaceutical means: and 4th, to endeavor to bring about a favorable crisis.

A servant, aged 34 years, having been for eight days complaining of a clammy state of the mouth; cephalalgia; lassitude; depression, with whitish tongue; red urine, colic, thirst; pulse ninety, coughing continually, night sweats, chills, and flushes of heat. I gave him sal ammoniac in the quantity of eight grammes, (ʒ ii,) in two hundred grammes, (ʒ vi,) of tilia water, and thirty grammes (ʒ j) of oxymel, of which he took a table-spoonful every hour. Two days after, the patient insisted on taking an emetic, and it was given him in compliance with his request, but it produced only the ejection of a glairy fluid, and an operation by stool. On the fifth day, the above ammoniacal mixture was resumed, with the addition of 0,05 centigrammes ($\frac{3}{4}$ grain) of tart. antim. On the seventh day, the tongue was much coated; gums and teeth covered with thick mucus, pulse 87, thirst, depression, insomnia. (emetocathartic.) On the eighth day bilious vomitings, and eight glairy stools, tongue whitish,—sensible improvement; the same pituitous stools as on the seventh day, (laxative decoction, which produced ten mucobilious stools.) The tongue, on the thirteenth day, was clean on the edges; the mouth less clammy, pulse ninety, urine red and highly charged, perspiration abundant; rested well at night, bronchial expectoration abundant. I then prescribed a decoction of acorus aromaticus, with infusion of valerian, and compound tincture of cinchona; a strengthening regimen; and, at the end of twenty-four days of this treatment, the patient's health was entirely re-established.

All the other observations that I have made in pituitous fevers accord with the above case, which I have taken indifferently from amongst a large number of others, excepting that I have seen some cases in which it was necessary to repeat the sal ammoniac from two to four times before prescribing purgatives. The indications for the repeated employment of the salt are based on the state of the tongue, the mouth, the taste, and the continued cough. Here the sal ammoniac acts principally as a solvent, especially on the mucus membrane. Its effects on the abdominal viscera are very marked, and this effect is subsequently propagated to the lungs through the medium of the ganglionic nerves. In gastro-bilious and rheumatismo-catarrhal fevers, and even in inflam-

matory affections of special organs, we must not expect the sal ammoniac given *alone* to act as an evacuant; for in the dose in which I administer it, the salt never produces evacuations. It only regulates and excites the mucous secretions, and under these circumstances it is often preferable to calomel. Bennius, Mager, Rainmann, Sarcone, Sobernheim, Oesterlen and others extol highly its efficacy in the afore-cited fevers, and I can add that I have made extensive use of this medicine, which I believe to be extremely efficacious in gastric, and especially, in mucoso-bilious and rheumatismo-catarrhal affections.

Pituitous State of the Stomach and Intestines.

One of the morbid conditions in which sal ammoniac renders the most important service, is the mucous or pituitous state, (*status pituitosus, blenorrhœa ventriculi et intestinorum*) which is very frequently met with in practice, in an inveterate chronic form. This malady is often seen in aged phlegmatic subjects. The following is a case in point: Mr. Riehl, agriculturalist, aged forty-two years, presented the following symptoms: foul tongue, covered with a layer of thick mucus, augmentation of the mucous secretion in the mouth and in the throat, loss of appetite, nausea in the mornings, continual cough, bad, gluey taste in the mouth, dull pains in the epigastrium, a feeling of constriction in the belly, which exhibits swelling with meteorism, and is affected with slight colic pains, obstinate constipation, stools covered with thick transparent mucosity in great abundance, pulse regular and slow, (68), no thirst. This state had continued already many weeks, and the patient became greatly emaciated. Before I saw him he had had leeches applied over the pit of the stomach, and on the abdomen. As these symptoms plainly denoted an inveterate pituitous diathesis, I prescribed sal ammoniac in the following manner:

℞—Ammon. Hydrochlor.
Succ. Glycyrrh. \bar{a} \bar{a} \bar{z} ii
Aq. Fœniculi \bar{z} vj

A table-spoonful every hour. Give nutritious broth, with wine and water. This mixture repeated three times in six days. On the eighth day the following:

℞—Tart. Antim. gr. ii
Ipecac ʒ ss

Divided into four parts, and one to be taken every four hours. The medicine produced bilioso-mucous vomiting, and abundant pituitous stools. On the sixth day medicines discontinued, appetite returned; gave nourishing broth, and a little roast beef, with horse-radish, red wine or good beer. On the seventh day, the preceding sal ammoniac mixture; pulse continues at 70; no thirst. On the eighth and ninth days, a purgative of senna, rhubarb and phosphate of soda, followed in an hour after by a cup of strong coffee, and a strengthening regimen adopted. On the following day there was a gradual disappearance of all the before-

mentioned symptoms; appetite, sleep and spirits returned, and I then prescribed for the patient as follows :

℞—Tinct. Cinchonæ Compos. ʒ i
 Ext. Aloes Aquos. gr. iij
 Tinct. Humuli ʒ iiss
 Tinct. Rhei Aquos. ʒ ss

Three dessert-spoonfuls daily, strengthening regimen, meat, food slightly spiced, horse-radish cooked, good red wine or beer, exercise in the open air. The patient, after having undergone this treatment for four weeks was perfectly restored to health.

Generally this pituitous, or gastric diathesis, is not dangerous; but as it very often puts on a chronic character, it has for the greater part of the time an intermittent and periodic character. It breaks out according to the seasons, with much less intensity, and becomes extremely serious to the patient, in bringing about a dropsical condition.* In these cases, the sal ammoniac, by reason of its primary resolvent effect on the mucus, dissolves, liquifies, and evacuates, downwards the glairy contents, exalts the ganglionic system, and prepares the way to other medicines—at first purgatives, and then tonico-excitants—that this pituitous diathesis demands. In all these cases the strengthening regimen is indispensable to a complete cure, and to the prevention of relapse.

Intermittent Fevers.

As intermittent fevers are very often complicated with the mucous diathesis, especially during the spring season; the administration of the hydrochlorate of ammonia has been recommended by various practitioners, not as a febrifuge, but as a prelude to the administration of quinine. Before the discovery of sulphate of quinine, the sal ammoniac was mixed with Peruvian bark, and with opium, and administered an hour or half an hour before the ordinary time of the paroxysm. *Jacobi* and *Vogel* prescribed the hydrochlorate, sometimes in powder, and other times in mixture. Meanwhile, it is well known, that if the attacks of intermittent fever are frequent, or if the paroxysms are too rapidly arrested, the *primæ viæ* are generally in a state of disturbance, and that abdominal viscera are left in a state of inaction; but in administering the hydrochlorate at first, during three or four days, we obtain a more complete apyrexia, throw off the crudities from the digestive organs, and thereby render the sulphate of quinine more efficacious. We have lately treated a great number of spring intermittent fevers, tertian and double, complicated with gastric symptoms, in which there was not a free apyrexia, and we have never prescribed the sulphate of quinine in these cases, without preceding it by sal ammoniac.

*I have at present a little negress under treatment, strongly affected with ascitis and anasarca, resulting from a previous attack of scarlatina. She has now taken sal ammoniac for three days, in doses of two grains every four hours, dissolved in water. It has acted strongly on the skin, and especially on the kidneys, and there is a great diminution of the dropsical distension.

Pleuro-Pneumonia.

The employment of the hydrochlorate of ammonia in inflammation of the respiratory organs, has given a high reputation to the medicine, which is fully justified by the eminent service it has rendered in the hands of those by whom it has been administered. It is especially after the employment of antiphlogistics, that its well marked action on the bronchia and pulmonary parenchyma is observed.

It augments and facilitates, at first, the mucous secretion, renders respiration and expectoration more free, the cough less fatiguing, promotes diaphoresis and diuresis—crises by which the disease comes to a favorable termination. Even in the course of other diseases complicated with bronchitis or pneumonia, as in typhoid fevers, acute exanthemata, in inflammations of the liver, &c.; or, where the cough is dry, where the pain in the breast is light, where the expectoration is difficult, and even where there is diarrhœa, the use of this salt is not contra-indicated. In such cases we prefer giving the medicine in the decoction of senega.

Asthma and Bronchitis.

In humid and pituitous asthma, in chronic bronchitis, chiefly of old persons, the working bakers, millers, carpenters, &c.; when after an attack of the characteristic dyspnœa, the patient is seized with a dry cough, which later in the disease becomes humid, the sal ammoniac is of the greatest utility. A baker, Oct. 26, has an attack of this kind two or three times annually. At each attack his expectoration is mucous, gluey and even purulent, intermixed with striæ of blood. I always have succeeded in the space of from eight to ten hours, in these attacks, by administering sal ammoniac and sulphur; and afterwards administering bitter expectorants.

We ought to remark that this agent, administered for a long time, and especially to patients already feeble and aged, that may be attacked with asthma or chronic bronchitis, may become very injurious. For if on the one hand it favors the formation of bronchial mucus, and facilitates expectoration, experience on the other hand proves that its prolonged use is dangerous when the expectoration is more or less difficult, or even impossible.

Gum ammoniacum, camphor, ipecacuanha, oxymel of squill, and the antimonial preparations are indicated as first; and it is only when the mucus is tenacious, and the *râle* is mucous and abundant, that the employment of sal ammoniac is indispensable.

Phthisis Pulmonalis.

The ancients as *Lentin*, *Mar*; and more recently *Cless*, *Most*, *Doerfelt*, *Roesch*, etc., recommend sal ammoniac in pulmonary consumption. Meanwhile there is a diversity of opinion, not only in relation to its efficacy in this disease, but in relation to the proper period in the disease in which it should be administered. Thus *Kortum* and *Roesch* praise the medicine in the crude state of tubercle, whilst others prefer it when the object is to soften the tuberculous masses, and to favor their expectoration. The

latter view, the most natural, and which is confirmed by the most satisfactory observations accords with mine. It agrees with my experience extending through twenty years, in the midst of an industrial population, (manufacturers of cotton tissues,) where I had an opportunity of administering this salt, and of observing its effects on a great number of consumptives. It is true I have never seen any case of this kind cured by it, but its action was strongly marked in cases in which the object was to soften the tubercles, allay the fatiguing cough by the expectoration of the tuberculous mass and even to arrest the purulent secretion in the vomicæ. If we further reflect on the softening action of the sal ammoniac, of which we have already spoken at the beginning of this memoir, it will not appear astonishing that its effects extend themselves to the tuberculous masses, and favor their expulsion. It is only in cases in which the intercostal pains, have been removed by appropriate antiphlogistics, when the pulse has become less frequent, and the cough less dry, that the use of sal ammoniac is indicated. It is then that the dyspnoea disappears, the respiration becomes more free, and the patients obtain a respite for a greater or less time.

Laryngeal Phthisis.

In laryngeal consumption this salt enjoys some degree of reputation. *Neumann* recommends here sal ammoniac in powder with an equal part of powdered liquorice root. He even contends that this combination is sufficient to effect a cure, being given at the very beginning of the disease, and that by virtue of its stimulant action, it has the power of subduing this affection. His conclusion is in some measure true, for in the treatment of all the chronic phlegmasiæ, one of the objects to be obtained is to produce a new activity in the part affected. Thus in laryngeal phthisis, we ought to employ means which strongly excite the throat, and which by that means transmit the irritation to the larynx. Here the sal ammoniac, given in powder, is in a state to produce the desired result. It is right, notwithstanding, that I should acknowledge, that in many cases of laryngeal phthisis, I have been obliged to suspend the employment of the medicine, owing to the irritation, to the cough, and to the suffocation that its contact with the throat produced; the patients generally refusing to continue it, and it becomes necessary to resort to demulcents and lubricatives.

Croup and Pseudo-membranous Bronchitis.

Dr. Hirtz cites a case of inveterate croup, (*Gazette Médicale de Strasbourg*, 1853,) in which the administration of sal ammoniac in powder, succeeded better with him than any other means. I have, myself, treated the case of an infant two years old, affected with croup, in which this salt, after the application of leeches, and the administration of emetic, did not produce any effect. In the case of a young girl aged sixteen years, attacked with pseudo-membranous bronchitis, the sal ammoniac has succeeded better in my hands. This girl was of a robust sanguine constitution, and had been laboring for two days under a hoarse cough, with crowing inspiration, and accompanied with

aphonia. After having passed the night in a cold room, she remained an hour and a half at church, and returned to her house almost suffocated. The cough became more hoarse and low, the inspiration dry, and hot, and hissing; pain in the larynx and trachea, the patient was almost aphoniated, constant tightness and suffocation; pulse frequent, face lived-red. She had been bled before my arrival, I gave her gr. iiss of tart. antim., which vomited her, and the debris of tubular false membrane, was observed to be thrown up. After the application of leeches to the anterior region of the neck, I prescribed sal ammoniac in a mixture for six days. This mixture, by its solvent effect on the bronchial membrane, accompanied by a copious diaphoresis, succeeded in re-establishing the health of this young girl in the space of ten days. This is the only case of acute pseudo-membranous bronchitis, that I have observed in my practice of twenty years.

Angina.

The catarrhal angina, called pituitous, lymphatic, œdematous, false, watery, serous, mucous, is the affection in which sal ammoniac produces the surest results. If we reflect that the disease sets in with a sense of dryness and constriction in the throat, in consequence of the suppression of the buccal secretion, by pricking in the neck, and in the tonsils, by great dysphagia; that later in the disease the secretion of mucus becomes more abundant, and that on an attentive examination, there are discovered small white suppurating points in the interior of the isthmus, that a disagreeable odor is exhaled by the patient in ejecting an abundant stringy saliva, that in fine, the inflammation arrives at this point that the patient is affected to suffocation, when the tonsils are entering into suppuration, we may readily presume that the sal ammoniac is a most heroic agent in controlling this affection. The use of this salt is more efficient than either leeches or blood-letting, from its acting directly on the disease, and removing the dryness, producing an abundant secretion of the buccal mucus, and acting more effectually on the disease in either the resolution or suppuration of the inflamed tonsils. It is after a severe spell of cold weather, during the spring and autumn, that we meet with this kind of angina, and an asthenic and typhoid character has been erroneously attributed to it. The best means that can be used in these cases are cooling diaphoretics, and especially, the sal ammoniac. A butcher was attacked with a severe sense of chilliness, and the result was that catarrhal angina appeared with all its violence. The patient had the severest suffering in making the least attempt at deglutition, and he had ear-ache, enormously swelled tonsils, discharge of a liquid, stringy, limpid mucus, tongue coated, dyspnœa, cephalalgia, thirst, pulse 110. This state had existed for three days. I ordered the following mixture:

℞—Am. Hydrochlor.
 Succ. Glycyrrh. \bar{a} \bar{a} 3 iiss
 Aq. Tiliæ \bar{z} vj
 Tart. Antim. gr. 5-6
 Syrup. Simp. \bar{z} j

Every hour a table-spoonful. Under the use of this mixture there was a notable diminution of the difficulty of swallowing, the buccal mucus secretion was re-established, and on the fifth day one of the tonsillar abscesses discharged its matter, and on the next day the other. Astringent gargles soon completed the cure. I have treated a great number of quinsies that have occurred in my practice by the sal ammoniac alone. In case the above mixture should be very repugnant to the taste of the patient, the dose may be given in a cup of barley water, which will render the taste less disagreeable. In these cases the hydrochlorate of ammonia, as a light irritant, favors the resolution of the tonsillar inflammation, hastens by its contact the suppuration and opening of the abscesses, and counteracts the production of the chronic morbid condition that, as a result, may set itself up in these glands; furthermore, it facilitates the expulsion of the tenacious mucus that accumulates in the throat and mouth. *Loeffler* and *Freyer*, (*Materia Medica*, Warsaw, 1817, t. ii, p. 350,) *Hauser* and *Hornurg*, (*Hufeland Jour.* t. xliii, p. 2,) have corroborated the efficacy of the medicine in quinsy, and affirm, as my own experience proves, that it has never been found inefficient in catarrhal and œdematous anginas.

Diseases of the Bladder.

A. F. Fischer was the first who recommended sal ammoniac in induration and swelling of the prostate and bladder, vesical catarrh, in suppuration of the bladder. *Lately*, *Hufeland*, *Crammer*, *Blum*, *Kuntzmann*, *Most*, *Bultner* have verified by repeated observations that this salt undoubtedly cures affections of the vesical mucus membrane. I have had occasion to treat many cases of vesical catarrh, and I have now before me a case of an officer, æt. 70, to whom I prescribed this salt with great success. This subject was affected with gout, but had never labored under any symptoms which would have given rise to the presumption of disease in the bladder till within the preceding three weeks he perceived that his urine was deranged, and charged with mucosity, that the sediment which fell to the bottom of the vessel was tenacious, whitish and yellowish, inodorous, and often in a considerable quantity; that micturition was more frequent, attended with violent pains, which extended to the prostate. As the secretion of morbid mucosity increased, the micturition was proportionably slow, the urine escaped *guttatim*, and with great effort, and he very frequently having to arise from fifteen to twenty times in the night. The mucus became thicker, more stringy, resembling jelly. After having treated the irritation of the bladder by emollient remedies, to which I added opiates and extract of cicuta, I prescribed the following mixture:

℞ — Fol. Uvæ Urs.
 Radicis Ononis Spinosæ
 Fol. Diosm. Crenat. ā ā. ʒ ss
 Boil with Aq. Font. q. s.
 Strain, and reduce to ʒ ix; add
 Sal Ammon.
 Succ. Glycyrrh. ā ā. ʒ ii to ʒ ss

Take six or eight table-spoonfuls daily. The effect of this mixture on the mucous secretion of the bladder was most remarkable. Regularly as the patient took the medicine, the urine, at first highly colored, became clear, the sediment daily decreased, as also the uric acid, the mucus less thick, and the micturition less frequent and less painful. At the present time the urine is almost entirely natural, and the patient is in an excellent state of health. I am at present giving him oil of white pepper, with the tincture of cynorrhodon,* a prescription extolled by *Rademacher*. I have since obtained a like success in the case of two other old men, afflicted with chronic cystitis, and who have also undergone the sal ammoniac treatment. The rationale of this treatment in vesical catarrh will readily occur to all when we refer to what we have already said, touching its action on the mucous tissues of the organism. If it modifies the diseased suppurating surfaces, the gastro-intestinal and bronchial mucous membranes, we might naturally expect a still more remarkable effect on the mucous membrane of the bladder, for, given in large doses, and often repeated, it enters the mass of the blood, and from hence it is found anew in the urine, and in direct contact with the mucous membrane of the bladder.

I respectfully invite my esteemed *confrères* to make a trial of this mixture in the aforementioned cases, and they will be enabled to realize the progressive changes which take place in the mucoso-purulent discharge from the bladder. They cannot fail to be convinced of its efficacy in diseases of the urinary passages, which so often attack aged men. It is useful also in urinary gravel.

Its effects in various affections.

This salt is highly extolled in indurations of the liver, spleen, ovaries, stomach, and rectum. It is proper that I should say, that in these affections I have derived from it but little advantage. *Ellinger*, (*Schewitz Zeitschrift*, 1847, p. 202,) speaks of producing by this remedy, the absorption of purulent effusions into the lungs and brain. *Fischer* has cured a stricture of the anus, by the remedy, (*Casper's Wochenschrift*, 1834, No. 329.) *Rademacher*, the new empirical reformer in Germany, has, for the last twelve years, recommended it in *catarrhal dysentery*, and in *catarrhal inflammations* of the bladder and urethra. *Schlüssel* prescribes it in *leucorrhœa* and *gonorrhœa*, in doses of two grammes (thirty grains) every hour, dissolved in water, with bitter extracts. It has been extolled in *anasæra* and *ascitis*, resulting from suppressed perspiration; and also in ulcerations of the vagina and uterus. As it is incontestible that the suppurating surfaces have a great analogy to one another, it should not be considered astonishing that this agent should be of great utility in cases of ulcers and suppurating surfaces in these organs. *Rust*, *Fischer*, and *Van Oye*, praise it highly as a resolvent, in affections of the prostate, and especially in cases of induration. I have treated only one case of this affection, which appeared to be very greatly modified, by the administration of sal ammoniac, in large doses.

**Rosa canina*, dog rose.

External employment of Sal Ammoniac.

Externally we employ sal ammoniac as a cooling and soothing medium, a resolvent, a gentle excitant; a promoter of resorption. It is especially beneficial in contusions, infiltrations, glandular swellings; in hernia; fractures; in apoplexy; in light burns of the skin; in hydrocele; and warty excrescences. The cold which is developed during its solution in water, resembles that produced by the action of water on common salt. In all cases, *Hauser, Rust, Graefe,* and *Schmucker* obtained excellent results from fomentations with this salt, in the form of saturated solution, in rheumatic hydrarthrosis of the knee, and in hydrocele and chilblains. In angina, where there is a great accumulation of mucus in the throat, a weak solution of sal ammoniac is extremely useful. In mammary abscesses, and in engorgements, this agent, according to *Vogel,* and other authors, is of undoubted utility. Fifty grammes of this salt ($\frac{3}{4}$ iss.) should be dissolved in five hundred grammes ($\frac{3}{4}$ xvi) of water, with the addition of sixty grammes ($\frac{3}{4}$ ij) of alcohol; and with this a tepid fomentation should be made for the breast, through the medium of flannel. This salt is also used to recall suppressed perspiration to the feet. It further modifies the condition of gangrenous and malignant ulcers, when applied to their surface, in the form of powder; and, finally, it is given in the form of enemata, in asphyxia and syncope, as an excitant.

The best mode of internal administration is in the form of powder with *succ. Glycyrr.* in solution, or in pills. In administering it, we must avoid the simultaneous employment of the fixed alkalis, lime-water, acids, calomel, or alum.

FORMULÆ.

I.

R—Amm. Hydrochlor. $\frac{3}{4}$ ij—8 grammes.
 Potas. Nitratis, $\frac{3}{4}$ j—4 grammes.
 Aq. Frambœs. Destil., $\frac{3}{4}$ vi—200 grammes.
 Syrup. Corticis Aurant. $\frac{3}{4}$ vijss—30 grammes.
 M. ft. mist.

A tablespoonful to be taken every two hours, in the early stages of rheumatismo-catarrhal fever.—(Clarus.)

II.

R—Amm. Hydrochlor.
 Succ. Glycyrrh. $\frac{3}{4}$ a a. $\frac{3}{4}$ ij—8 grammes.
 Aq. Tiliæ. $\frac{3}{4}$ vj—200 grammes.
 Tart. Antim. gr. $\frac{3}{4}$ to gr. iss—0.05 to 0.10 centigrammes.
 M. ft. mist.

Every two hours a tablespoonful, in the pituitous diathesis, in the commencement of catarrhal fevers, in the second stages of pneumonia, in angina, and asthma.—(Smith, of Warsaw.)

III.

R—*Anti Emetic draught of Rivière*, ℥ ij—60 grammes.
 Aq. Fœniculi, ℥ v—150 grammes.
 Am. Hydrochlor. ℥ ij—8 grammes.
 Succ. Glycyrrh. ℥ iss—6 grammes.
 Tart. Antim., gr. $\frac{3}{4}$ —0.05 centigrammes.

M.

Take a tablespoonful every hour, in pulmonary consumption, to promote the expectoration of tuberculous masses.—(Smith, of Warsaw.)

IV.

R—Ammon. Hydrochlor.
 Succ. Glycyrrh, $\bar{a} \bar{a}$ gr. xv—1 gramme.
 Ext. Leon. Tarax. (*pissenlit, piss-a-bed*;) q. s.

Make into a bolus. Take a bolus every two hours, in cases of induration of the prostate, bladder and ovaries; strictures of the urethra, cesophagus, stomach and intestines, with good wine or beer, and nourishing regimen.—(Fischer.)

V.

R—Ammon. Hydrochlor. ℥ ij—8 grammes.
 Aq. Rutæ. (*eau de rue*), ℥ viij—250 grammes.
 Acet. Rutæ, (*vinaiigre de rue*), ℥ iv—120 grammes.
 Tinct. Arnica, ℥ ij—8 grammes.

For fomentations in contusions, hydrocele and extravasations.—(Clarus.)

VI.

R—Ammon. Hydrochlor. ℥ ij—8 grammes.
 Assofoetidæ.
 Aloës Socot. $\bar{a} \bar{a}$ ℥ i—4 grammes.
 Succ. Glycyrrh. q. s.

Make into pills of the weight of 0.10 centigrammes, (gr. iss.) Give from six to ten pills daily—in obstinate obstructions of the liver and spleen.—(Radius.)

ART. II—*Ovariotomy Performed with Success*.—Translated from Rev. de Théor: By M. MORTON DOWLER, M. D.

Professor Langenbeck has performed ovariotomy with success, in the case of a maiden, thirty-four years old, who was afflicted with a tumor of the ovary. The tumor had existed for five years, and had become so largely developed, that the abdomen appeared to be that of a woman at the full period of utero-gestation. The patient was operated on under the influence of chloroform. Professor Langenbeck began by making an incision in the parietes of the abdomen, on the *linea alba*, to the extent of two inches and a quarter; and proceeding from below upwards, and commencing a little above the symphysis pubis. The cyst laid

bare, and, fixed in the wound by means of a crochet, was punctured. There flowed out about twenty-eight litres, (19 pints,) of a limpid albuminous fluid. During the flow, the cyst was drawn outwards to its pedicle. The wound was then closed with pins, and the cyst excised on a level with the skin, and afterwards the suture was applied. In nine weeks the wound was cicatrized; but during this time the patient complained constantly of colic pains.

The extirpated tumor comprised, in addition to the ovary, other cysts, of the size of hazel nuts, and also the fallopian tube. The parietes of the cyst were thick, and presented, interiorly, numerous folds. The liquor was strongly albuminous, and exhibited, under the microscope, numerous scattered epithelial cells. This operation is the seventh of the kind performed by Professor Langenbeck. Of these seven, three died, and four recovered.—*Deutsche Klin.*

ART. III.—*Good Effects of Belladonna, in a Case of Spermatorrhœa.*—

Translated from Rev. de Thér: By M. MORTON DOWLER,
M. D.

A young man, of the purest morals, and of entire continence, came to consult M. Lepri, in consequence of nocturnal pollutions, which, re-appearing every night, and even many times in a single night, began to exhaust his strength, and to effect his moral and intellectual faculties. These pollutions had existed for some weeks, and had not been relieved by any medicine, nor by any of the hygienic means applied, such as reposing on a hard bed, awakening in the night, and the cold affusion, &c. Two years previously, this young man had an attack of miliary fever, which had left, as one of its effects, nocturnal incontinence of urine.

Some months had passed, in which the patient had been sometimes better, and sometimes worse, when, all at once, this vexatious infirmity disappeared.

After having applied a blister to each thigh, advised a tonic and corroborant treatment, and, also, treated the patient with camphor combined with laudanum, alum, &c., without the least success, M. Lapri came to the conclusion that the spermatorrhea, in this case, might have some connection with the previous incontinence of urine; and recalling to mind the good effects of belladonna, in the latter affection, he prescribed this remedy for the patient in the evening, on retiring to bed, and in the morning on rising. At the end of a few days, every trace of the malady disappeared.

ART. IV.—*Translated from Rev. de Thér. Academy of Sciences, sitting of May 29, 1854: On the efficacy of ice used in connection with compression, in reducing strangulated hernia, and in combatting consecutive peritonitis: By M. MORTON DOWLER, M. D.*

Mr. Baudens read a summary of a memoir bearing the above title. The treatment that we adopt, says he, with so much success in cases of traumatic lesions—the application of ice, with or without the addition of common salt—we have extended to the treatment of strangulated hernia. In sixteen cases of hernia complicated with strangulation, when all the ordinary means of reduction had failed, the whole number was successfully treated by the application of ice, conjoined with methodic and permanent local compression.

In order to reduce such herniæ, it is evident that first of all, the attention must be directed to the reduction of the excessive development of the tumor, occasioned, as it is well known, by the arrest of the capillary circulation, and by the production of sanguineous congestion. To effect this result ice is of all agents with which we are acquainted the most efficacious. We have two things to expect of it, which it never fails to confer, namely; first: the driving back of the fluids with which the hernia is engorged; and secondly, to put an end to the inflammation, which soon brings about gangrene, if measures are not interposed to prevent it. The effects of ice on a strangulated hernia, are, to drive back the fluids, to allay pain, to condense the intestinal flatus, to efface the hernial tumor, to remove the danger of inflammation, and especially the danger of peritonitis, so often fatal.

Should the ice produce only the latter result, and afford time to arrest the rapid march of danger, should it render precipitancy on the part of the surgeon unnecessary, and enable him without danger from temporising, to exhaust the milder resources before resorting to the knife, a most valuable agent is presented, and one which we are justified in placing the greatest reliance.

The repulsion of the fluids, and the condensation of the expanded flatus, superinduces in the herniaëd parts, a diminution of volume, which gives full power to the taxis, previously powerless.

Spontaneous reduction may take place under the influence of refrigerants alone. We can cite three examples of this. But when it does not thus take place, we must aid in bringing it about, by assisting the ice with a compressing bandage. This joint action has always, up to the present time, succeeded in our hands.

When the tumor is so painful that the least compression cannot be borne, we most always begin with the ice alone. The pain once assuaged, we can, if we judge proper, apply a *spica* bandage, which is to be covered with pounded ice. In order to give this bandage a continued contractile action, analogous to the fingers, we must provide an elastic band of caoutchouc. This special bandage we call the *spica-taxis*.

We know that the ordinary taxis can be but little applied when the

tumor is inflamed and very painful. We know that there is danger of bruises and even lacerations, however cautious we may be, for the intestine is much less resistant in a state of inflammation. The compression of the taxis, preceded and accompanied by ice, made by an elastic band, does not expose the patient to this danger. To efficiently assist, the pelvis ought to be elevated, in order to facilitate by their gravity the return of the protruded viscera.

It is under these circumstances, after having exhausted, in vain, the whole list of means recommended for the reduction of a hernia, that the surgeon has no other resource except the knife.

In sixteen analogous cases, being the whole number up to the present date, in which we have had occasion to resort to our treatment, we have been enabled to avoid celotomy; and all the patients have obtained prompt reduction of the hernia. These cases are detailed in the memoir presented, and of which we here present a summary.

We are thoroughly convinced that ice is destined to produce a complete revolution in the treatment of traumatic lesions. During twenty-four years, it has constantly responded to our efforts. If it has not yet triumphed, it is owing to the fact that nothing is so difficult to eradicate as prejudice, and especially error. We leave it to the Academy to determine whether or not we have put forth a well grounded and certain guide, and established the truth of the principles we have adopted.

ART. V.—*Animalcular Origin of Epidemics.*

Joseph Leidy, M. D., Professor of Anatomy in the University of Pennsylvania, says (Smith. Contr. Knowl.) that many important diseases have been supposed to originate from parasitic animals and vegetables.

The former are not the true entozoa, for these are too large, and may be detected by the naked eye; but they are considered to be animalculæ so small that they cannot be discovered even with the highest powers of the microscope. But, independent of the fact that the existence of such entities is a mere suspicion, none of the well-known animalculæ are poisonous. At various times I have purposely swallowed large draughts of water containing myriads of *Monas*, *Vibrio*, *Euglenia*, *Volvox*, *Leucophrys*, *Paramecium*, *Vorticelli*, &c., without ever having perceived any subsequent effect.

The production of certain diseases, however, through the agency of entophyta, is no longer a subject of doubt; as in the case of Muscardine in the silk-worm, the Mycoderm of Porrigo favosa in man, &c.; but that malarial and epidemic fevers have their origin in cryptogamic vegetables or spores, requires yet a single proof. If such were the case, minute vegetables and spores, conveyed through the air, and introduced

into the body in respiration, could be detected. The minutest of all known living beings is the *Vibrio lineola* of Müller, measuring only the 36,000 of an inch, and the smallest known vegetable spore is very much larger than this, whilst particles of inorganic matter can be distinguished the 200,000 of an inch in size.

I have frequently examined the rains and dews of localities in which intermittents were epidemic upon the Schuylkill and Susquehanna rivers, but without being able to detect animalculæ, spores, or even any solid particles whatever. I have examined the air itself for such bodies, by passing a current through clear water. This was done by means of a bottle, with two tubes passing through a cork stopper; one tube dipping into the water, the other reaching not quite to its surface. By sucking upon the latter tube, a current of air passed through the former, and was deprived in its course of any solid particles. Ordinarily, when the atmosphere was still, early in the morning, or in the evening, neither spores nor animalcules could be detected. When piles of decaying sticks or dry leaves were stirred up, or the dust was blown about by the wind, a host of most incongruous objects could be obtained from the air; none, however, which could be supposed capable of producing disease.

To assert, under these circumstances, that there are spores and animalculæ capable of giving rise to epidemics, but not discernible by any means at our command, is absurd; as it is only saying in other words that such spores and animalculæ are liquid and dissolved in air, or in a condition of chemical solution. That the air may be poisoned by matters incapable of detection by the chemist, is proved by the emanations from such plants as the *Rhus vernix*, *Hippomane mancinella*, &c.—*Va. Med. and Surg. Jour.*

ART. VI.—*Vermifuges.*

Inasmuch as but few practitioners can solely from their own experience deduce either the ætiology, pathology, or therapeutics of verminous *maladies*, it has been thought advisable to give a considerable portion of the present number of this Journal to helminthology, with the expectation that the data submitted to the reader's consideration, imperfect though they be, will not only prove interesting, but serve as incitants to further investigations into this neglected department of medical science.

Vermifuge properties of Pumpkin Seed.

The seed of the pumpkin (*Cucurbita Pepo*) a highly valued remedy of tapeworm, and one that has been the most fully tested, though not

discovered by Americans, seems to be altogether ignored by American compilers of Dispensatories and Formularies.

The pumpkin seed remedy is often mentioned as being a new one, and as having been first introduced into practice in America. It appears, however, that this article was first introduced to the notice of the medical world by Dr. Mongeney, of Bordeaux, about three years ago, at a meeting of the Medical Society of that city; he then and there declared that for thirty years he had used with great success for the expulsion of *tænia*, a paste of pumpkin seed (*la pâte de citrouille*;) 90 grammes of fresh seed mixed with twice as much honey—a dose which in seven hours without producing any unpleasant effect, dislodged the worm. Dr. M.'s confrères of that city, among whom were MM. Brunet, Sarramea, and others, had in a great many instances used this article with the most complete success—some of whom gave 45 grammes of the skinned seed (*semences dépouillées*) united with the same amount of sugar. One of the physicians, who had suffered extremely for two years, from an almost constant pain in the lumbar region, excessive debility, indigestion—symptoms which he regarded as due to a disease of his nervous system. He finally, having voided some flattened fragments which he thought might be portions of *tænia*, took by the advice of M. Sarramea, 30 grammes of pumpkin seed pounded with 10 grammes of sugar; he suffered all night from a violent fever and great agitation; by morning, twelve hours after having taken this dose, an injection simply of water brought away seven mètres, or about twenty-three feet of *tænia*.

As suggested by M. Rollet, measures were taken to convey to the Council of the Administration, a recommendation that this valuable remedy should be admitted into the pharmacopœia (*codex*.) (See *Jour. des Connais. Méd. Chir.*, June 1, 1852.)

Cure for the Tapeworm.

Procure sufficient seed of the pumpkin (those grown in the West Indies are the best) to make two ounces after removing the outside shell of the seed; put them into a mortar and add half a pint of water; pound them well up, and make a liquid orgeat of them, which strain through a cloth. Drink this mixture in the morning on a fasting stomach. If it does not operate in the course of an hour and a half, take one ounce of castor oil. Drink all the time as much fresh cool water as the stomach can bear or contain; that is, drench yourself with water. After taking the orgeat, if the stomach is well rubbed

with ether, and an injection of about sixty drops of it is taken, you will find it an assistant to the orgeat, but this may not be necessary. Should the first application of the remedy not answer, repeat it the next morning, and there is no doubt your complaint will be removed. The worm will leave the patient all at once, and probably entire. This can be ascertained by finding the small end or head of it, which tapers almost to a point.

The New York friend, from whom I received the recipe, of which the preceding is a copy, in March, 1848, remarks, in support of his opinion of the efficacy of this remedy, that Capt. — says he did not have to take the injection, although he took two separate doses of the seed, (the first not operating sufficiently,) which relieved him at once, and since which time has cured probably a dozen different persons afflicted with the tapeworm, who had been given over by the physicians. The worm from him was about thirty-four feet long, each link about one inch. He rubbed the stomach with ether after taking the orgeat. It may be advisable to use the forenamed remedy under the advice and with the assistance of a physician. I have only to add that the suffering lady in this city, for whose relief the writer's aid and influence was solicited by her husband, has been restored to perfect health, after years of prostration and efforts for relief; and in thankfulness for the interest I had manifested in the case, sent me a glass jar containing a large part, if not the whole of the worm that had been her tormentor for several years — *Boston Med. and Surg. Journal.*

F. W. Cragin, M. D., says: "On reading an article on pumpkin seeds, in a late number of the Boston Medical and Surgical Journal, I recommended it to an intimate friend, who had, two months before, discharged about four yards of that detested parasite, a tapeworm, and who was sure there was "more of the same sort left." He, in three days afterwards, showed me the bottle, since left at your office, containing what was formerly discharged, together with the tapering part of that which was removed (in all about four yards) by the remedy.

His statements, which may be implicitly relied on, are, that for want of West Indian or other pumpkin seeds, he took undried acorn or marrow squash seeds, and proceeded, *secundum artem*, following the orgeat, in about one hour and a half, with about six drachms of castor oil, taken in two spoonfuls of Holland gin. He drank very little water twice; drank and ate nothing else till noon, when the only effect of his faith and practice was manifested "in one liquid discharge containing the squirming worm; at one end about one-third of an inch broad, and tapering down to nothing."

If this remedy should continue to prove as efficacious as in this and former instances, it is to be hoped a specific has been found for one more of "the ills that flesh is heir to;" which remedy should never be lost sight of."—*ib.*

On the Treatment of Tapeworm by the Oil of Pumpkin Seeds: By the late Prof. HENRY S. PATTERSON, M. D.—In the Medical Examiner,

for October, 1852, I reported a case of radical cure of tænia by the use of an emulsion of pumpkin seeds, after the Ol. Terebinth. and even Kouso, had signally failed. Several other cases have been reported before and since mine, all going to establish the efficacy of this new remedy. Should it prove as generally successful in expelling the worm as the cases indicate, it will become a valuable accession to our means of treatment in a troublesome and often obstinate affection.

The seeds of the common pumpkin (*cucurbita pepo*,) consist of a leathery white envelop, inclosing an oily albumen of a slightly greenish tinge. They are inodorous, and have a sweetish, mucilaginous taste. Rubbed up with warm water or milk, and sweetened, they form a very pleasant emulsion; and this is the way in which they have generally been administered. They abound in fixed oil, which is readily yielded on expression, and appears to be the only constituent of any importance. Conceiving this oil to be the anthelmintic principle, I determined to use it in the first case of tænia I should encounter. A quantity was obtained, by cold expression, by our accomplished pharmacist, Mr. Frederick L. John, of Race street. From four lbs. of the seeds he procured $\frac{3}{4}$ xiv of oil; but, he has no doubt that, if the operation were conducted on a larger scale and more carefully, the yield would be from thirty to forty per cent. The oil is clear, transparent, of a light brownish green color, with a slight oily odor, and a perfectly bland taste, like that of the oil of sweet almonds. It has now been kept some ten months, in well stoppered bottles, and is perfectly sweet and bland.

No case of tænia has occurred in my own practice; but in May last I learned from Mr. John C. Lyons, an intelligent member of the medical class of Pennsylvania College, that a poor woman in his neighborhood (Kensington) labored under the disease, and had asked his advice. I requested him to use the pumpkin seed oil, which he did, with the happiest results. Causing her to fast rigidly for twenty-four hours, he gave her $\frac{3}{4}$ ss of the oil in the morning, and in about two hours $\frac{3}{4}$ ss more. This produced a slight disposition to looseness of the bowels. In two hours after the last dose, $\frac{3}{4}$ i of castor oil was given, and purged freely, bringing away a considerable portion of the worm. From that period until the present, (September) she has remained entirely free from any symptom of verminous irritation, and there can be no doubt that the worm was altogether destroyed.

Tænia is of so rare occurrence with us, that no individual practitioner sees enough of it to enable him alone fairly to test any medicine. I therefore beg leave to call the attention of my medical brethren to a remedy readily obtained, cheap and pleasant, and which I believe will be found quite efficient.

The same gentleman reported the following case:

Failure of Kouso—Successful Use of Pumpkin Seeds.—The subject of this case was for some time under my care, in consultation with my colleague, Dr. Darrach. I can aver that he was most tho-

roughly put through the entire routine of tapeworm remedies before he left Philadelphia. He tells his own story so well that I prefer to give the following extract from a letter announcing his restoration to health: "In the early part of January, 1836, I was rather suddenly attacked with what seemed to be an alarming diarrhœa, which continued for some weeks, resisting the usual remedies. My symptoms had been peculiar for some time previous to the attack. Indeed, I had all the prominent symptoms of tœnia as laid down in the books, viz: dizziness; occasional false vision; variable appetite; pain in the lumbar region; pain in the knee joint; swelling of the abdomen; hesitancy of speech; restlessness in time of sleep; unusual drowsiness during the day; variable strength, being sometimes quite strong, and then again quite feeble. Somewhere about the middle of February of the same year, I discharged at a morning stool, about nine yards of the tœnia. From that time onward, for six years, I was more or less under medical treatment continuously. I took large quantities of the spts. turpentine, (once or twice two ounces at the dose;) also, the male-fern, calomel, and jalap, and Jayne's vermifuge; and was several times under homœopathic treatment. I took also iodide of potassium, iodide of iron, decoction of pomegranate, and the 'kouso.' I discharged large quantities of the worm, but no head could be perceived. When the kouso failed, I began to despair of being cured at all, but my sister, Mrs. —, sent me in December last two numbers of the Boston Medical and Surgical Journal, containing two several accounts of the cure of tœnia by the use of pumpkin seeds. Having previously abstained from usual food for a day, on the 10th of January last, I took, at 8 o'clock in the morning, two ounces of the kernels of pumpkin seeds pulverized with two tablespoonfuls of white sugar, and commingled with a half pint of boiling water, making a very pleasant drink for a fasting man. I kept my bed, drinking frequently of cold water, and at 9½ o'clock, I took an ounce of castor oil. At 10½, I drank a cup of hot black tea, and, in about two minutes discharged about eight yards of the tapeworm, *with the head*. O, how I wept for joy that I was again a free man, after a servitude of six sad years to this awful complaint. Since then I feel like a new being in a new world. My life had often been a weary burden, and yet I grew fleshy and looked healthful. For months in succession I had discharged the worm daily in pieces of six to eighteen inches, and also in gourd seed form. I suppose that without any over-estimate, I discharged, during the six years of my affliction, about *four hundred yards!* The remedy is very simple. Were I a practising physician, I would never administer turpentine for tapeworm; I sometimes fear that I have experienced irretrievable harm to my kidneys by using it. There is virtue in pumpkin seeds, doctor, even if it be a *Yankee notion.*"

In the Northwestern Medical Journal, for May, 1853, Dr. J. McCreary Sudduth, gives the following case:

B. H., male, aged 28 years—occupation farmer, volunteered in 1847 to go to Mexico. In good health when he left home (Ky). Sickened while on the gulf, Nov. '47. Landed and went as far as the City of Mexico; though unfit for duty, and on the sick list during his entire stay in Mexico. Discharged and returned to Kentucky early in the summer of 1848, quite weak and much emaciated. In August of 1848 began to pass by stool small portions of what proved afterwards to be joints of tapeworm. Shortly after his return to Kentucky, he applied to a number of physicians for advice; receiving no benefit from any article recommended, except those used by one doctor whose prescription brought away eight feet of tape worm. Patient thinks the articles used by him were cal. and Dr. Jayne's vermifuge. Shortly after passing this piece of worm, finding himself not relieved, and knowing now what it was that troubled him, he went to Louisville and consulted a number of physicians of that place, offering one hundred dollars to any one who would rid him of his troublesome companion. He however received no benefit from any articles used by physicians of that place. Soon after leaving Louisville, he came to Illinois, still poor and in bad health. He consulted with all the doctors that came in his way, using all articles by them recommended, as well as all the patent medicines that he saw extolled for the removal of tapeworm, (and their name is legion) in newspapers, almanacs and receipt books, &c.; however all failed. When he consulted Dr. Smick and myself, he presented the appearance and symptoms as follows: Was lean in flesh, and ænemic, judging from the appearance of *the surface* and color of his lips. Abdomen somewhat distended, troubled much and especially at night by a moving, creeping sensation in the abdomen. Variable appetite, at times voracious, at others none at all, passing daily by stool a number of joints of the worm. We advised him to eat the paste of pumpkin seed and honey, ℥ iij, one ounce at a time, with an interval of two hours. Six hours after taking the last portion he passed twenty-two feet of the worm, though in three pieces, the longest of which was eighteen feet, and bearing the head of the parasite; showing the superiority of paste of pumpkin seed and honey over all known articles for the removal of this troublesome, if not destructive parasite. All symptoms of worms have disappeared.

In the American Journal of the Medical Sciences for July, 1854, D. Leasure, M. D., of Newcastle, Pa., says: "Mary——, aged twenty-eight, unmarried, has been delicate all her life, and for fifteen years subject to severe cramping pains of the abdomen, accompanied sometimes, by obstinate vomiting. About ten years since, she noticed that she passed portions of tapeworm, of lengths varying from a single joint up to many feet, and if the statements of the patient and her mother are to be relied on, sometimes half filling an ordinary chamber mug. Her mother had also, at an early period of her life, been a victim to a tapeworm, which had been expelled by a secret *vegetable remedy*, probably *male fern*, given to her by a worm doctor.

My attention was called to Mary's case some time in last February, while in attendance on her sister, for another disease; but from causes not necessary to mention, I did not prescribe till last week. I had intended to use the male fern or kouso, or both; but not having access to either of them in a fresh state, I determined to wait till they could be procured from Philadelphia. While thus waiting, I noticed in one of the journals a report of a case of tænia expelled by the use of emulsion of pumpkin seeds. Curiosity, more than the expectation of success, prompted me to give it a trial. I directed a pint of the bruised seeds to be infused in three pints of soft boiling water, and left over night, the whole to be taken during the next day, the patient fasting in the meantime.

On the morning of the 9th of May, the patient commenced its use, and in the afternoon experienced the most violent cramps and pains in the bowels for several hours; and on the morning of the 10th, she passed eleven feet of the parasite, including the head, as proved by observation under the microscope. The animal was entirely dead when voided from the bowel, and is a most beautiful specimen of a perfect tænia.

Kouso.

The three following cases, treated by Dr. Budd, who determined to give kouso a trial, having often found ineffectual the remedies in common use, as turpentine, pomegranate bark, &c., which create disgust in the patient, as well as, in some cases, severe constitutional symptoms. This plant, as long ago as the beginning of the year 1847, had been brought under the notice of the Academy of Medicine, at Paris, who appointed a committee to inquire as to its properties. It was in consequence tried, under their direction, in several hospitals, and the result of their experience was such, that they were induced to return a very favorable report. The Academy of Sciences were equally satisfied of its efficacy.

The parts of the plants used are the flowers, which, being reduced to a fine powder, are macerated in lukewarm water for fifteen minutes. The powder and solution are taken either in one, two, or three doses, quickly following each other. It is recommended that lemon-juice should be taken freely before and after the kouso. The patient must be prepared by low diet for a day previously, and the medicine taken on an empty stomach, before breakfast. The clear infusion has the color, and a somewhat similar taste, to a very weak senna tea. It rarely causes any annoyance or uneasiness, except slight nausea, and this but seldom.

The first woman to whom the kouso was given had generally enjoyed good health. From her account it is probable that she had not been troubled with tapeworm previous to her coming to London, a year and a half ago. When the first symptoms of it came on, twelve months since, she took oil of turpentine and castor oil, under the use of which a large portion of tænia passed. She had, at first, a ravenous appetite, which passed away, leaving a constant feeling of flatulency in the stomach. Languor, general debility, incapacity for work, and nausea, were her chief symptoms. During the four months preceding her admission, she was

constantly taking various remedies, such as turpentine, &c., under the direction of her medical attendant; but with none of them has the worm passed. When admitted into the hospital she was ordered half a drachm of jalap and low diet, and subsequently other purges, but without bringing away any joints of the worm for four days, when the kouso was administered on an empty stomach, which in the course of the day brought away a large worm. Its head could not be detected, but the narrow portions which seemed to have been joined to it came away. During the same and following day, there was considerable diuresis, but afterwards the urine became scanty. The motions were loose and dark. Her general state improved, and she left the hospital apparently cured.

The next patient was also a woman, aged about 44, who had apparently got the worm at Fort Beaufort, in the Cape of Good Hope, at which place she resided for some time. She began to pass joints in the year following that on which she went to that place. Worms are very common among the natives, who are in the habit of taking infusion of pomegranate bark, turpentine, and also, a scraped root called "Cacay." Of all the remedies which she has used, the pomegranate was the most effectual, which has not, however, cured her, as she continues to pass joints. Her symptoms are gnawing pain, and constant feeling of sinking in the epigastrium, pain in limbs, general lassitude, dimness of sight, loss of appetite, short, dry cough, and a sensation as of the movement of the tænia. She took kouso as the other patient; it was followed by slight nausea for a quarter of an hour. Its taste, she says, is very much like pomegranate. A tapeworm of very large size was passed four hours and a half afterwards, and subsequently, some isolated joints. The head could not be found; but there is no doubt that it came away, on account of the narrowness of some of the pieces. During the same and two following days, numerous joints, apparently long dead, and partially decayed, were passed.

The other was a delicate, anæmic looking woman, who had had the usual symptoms of tapeworm for some time, but did not to her knowledge pass any until three weeks, and again one week ago. Is a native of Norfolk, but has latterly resided in Soho, which neighborhood is supplied with water by the New River Company. She had the kouso exhibited in the same way, followed by a dose of carbonate and sulphate of magnesiã. A portion of worm passed with every motion. Both these patients have left the hospital, improved in health, and apparently free from any symptoms of tænia.[—Ranking's Abstract.

To the Editor of the Lancet.

Sir:—I have much pleasure in forwarding for the Lancet the particulars of a successful trial of the kouso. Mr. B., residing in Cheapside, a delicate young looking man, had been troubled with tænia for some years, and had taken the usual remedy, turpentine, with partial success, having at times seen parts of the worm only. I obtained a bottle of kouso from my druggist, which my patient took on Sunday morning, the 15th; after waiting two hours, with the aid of seidlitz powder, the monster was expelled, *tête et col* complete, measuring twenty-one feet.

I need not add that my patient was highly delighted at the good effects of the koussou, and has presented me with the largest specimen of a tape worm I have ever seen.

I am, Sir, your obedient servant,

THOMAS SMITH.

In former numbers of the *Lancet*, (March 16, 1850, and April 20, 1850,) cases were noticed in which the koussou was found very efficacious for procuring the expulsion of the *tænia solium*. This plant is now acknowledged to be so useful in tapeworm, that it seems almost unnecessary to adduce new cases; we shall, however, just sketch a few of those which were lately benefited by the koussou, as they present various features of interest.

The first case, as taken from Mr. Jordan's notes, runs as follows:—Rebecca R., aged 22, is a native of Wapping; she went to Devonport when seven years of age, but only stayed there about a fortnight; with this exception she has constantly lived in town, generally at Wapping, but about eighteen months ago she spent a year at Peckham. Patient's sister, who has been dead nine years, also suffered from tapeworm, which remained upon her to the time of her death. Patient likewise knows of a neighbor of hers in Wapping, close to her own home, who suffers from the *tænia*. This latter person and the above-mentioned sister are the only people she knows to be thus affected. The water is supplied by the New River Company to the whole neighborhood.

Patient was quite healthy until about two years ago, since which time she has had great pain in the side and stomach; her appetite was good, but she used to feel sick on first getting up; she had, however, no idea that she harbored a tapeworm until a week before Christmas, when she first passed joints of it, and from that period, such joints have been passed almost every day.

Twice since she first noticed the joints she has passed long pieces of the worm, once after opening medicine, the other time without any such agency. She has never taken any turpentine nor any other remedy expressly for the worm.

Patient was admitted under the care of Dr. Budd, and took the koussou at half-past nine in the morning, the day after her admission; and, after taking a dose of castor oil in the middle of the day, the worm was passed with a motion at a quarter to five in the afternoon. This entozoon was nearly three yards in length, and the narrow segments approaching to the head were attached to it, though not the head itself. The medicine gave patient a slight feeling of sickness, which soon went off again. Her appetite was bad on the day she took the koussou, and she felt weak. With the exception of the tapeworm patient seems to have generally had good health; she has only a slight cough. Her mother and sister died of phthisis, but patient's appearance is remarkably florid and healthy. The day after admission, this woman left the hospital in good condition, without passing any more of the worm.

The second case was admitted under the care of Dr. Todd. The subject is a young woman, native of Scotland, *four months advanced in*

pregnancy. She complained to Mr. Steele, the house-physician, that she was in the habit of passing long, *round* worms, but when she brought the joint which she had lately evacuated, they were found to be pieces of the *tænia solium*. When the nature of the worm was ascertained, the patient was admitted into the house and took the kousso in the morning; at seven in the evening, she went home, and a quarter of an hour after she had reached her residence, she passed five yards of the worm.

The third case was sent to Dr. Todd from the country. The patient is a middle aged woman, residing at Bow, who took the kousso at three o'clock in the afternoon, and left the house immediately afterwards, promising to bring the worm as soon as she should evacuate it. The next morning she brought a tapeworm measuring about six yards in length.

The fourth case, who was admitted under the care of Dr. Budd, is that of a man, about forty-six. His health has, in general, been pretty good; last winter, however, he was attacked by cholera, and treated in King's College Hospital. Whilst laboring under this disease patient did not pass any joints of the tapeworm, though, previous to his being visited by the epidemic, he had now and then evacuated portions of the *tænia*. When convalescent, he took some oil of turpentine, and by the agency of this medicine he voided a few joints. From that period he continued passing joints, and was admitted under the care of Dr. Budd, May 3, 1850. Patient took the kousso in the morning, and had two doses of house medicine in the course of the day. At six o'clock in the evening, he passed a tapeworm of a very great length, since it measured nearly ten yards. The next day he voided a piece six inches long, which came evidently from very near the head. It is to be regretted, as we stated before, that this medicine is so expensive; still, when it is considered how rapidly and effectually it promotes the evacuation of the *tænia*, the 17s. 6d. can hardly be looked upon as a high price; the more so, as in hospital practice, the patients need stay in the house but a short time. It will be extremely interesting to keep an eye upon these patients, in order to ascertain whether the benefit is of a lasting or temporary kind.

The late Dr. Frydenger, of New Orleans, in this journal, about two years ago, gave a very interesting case illustrative of the efficacy of kousso in destroying tapeworm, from which it appears that a young gentleman was treated in both Europe and America by the entire routine of vermifuges, and particularly, by the terebinthinate preparations, with so much energy that his general health had become impaired, and his life endangered. It may not be improper to recall the attention of the reader to the following paragraphs of Dr. F.'s paper, and the more so, because kousso has been treated with neglect in the South:

"I have treated him for the last three years, at intervals, with varied

results, but invariably gave him some relief for a time; at times he would discharge from a few separate joints to several hundred, at others, portions of the worm measuring in length from three or four inches to seventy or eighty feet; and I may here state, that since I commenced treating him, he has discharged over a *thousand feet of worm*.

In February last it annoyed him very much, and I determined to try kousso on him; accordingly, half an ounce was administered in water at bedtime, followed in the morning by a seidlitz powder. In a few hours his bowels were moved, but no signs of worm or the kousso could be discovered in the evacuations. During the following night the kousso came away, bringing with it a large quantity of the worm, broken and torn and mangled to such a degree that it was some time before it could be recognized in the mass of matter. Such portions as selected from the mass, of sufficient size and form, worthy of preservation, can be seen at my office, with specimens of the entire worm from the same person."

The late Professor H. S. Patteson gave Miss W., aged twenty-two, long a sufferer from tapeworm, "six drachms of kousso at once, the patient having fasted from the previous day. It excited some nausea but no vomiting. It was followed in a few hours by a dose of castor oil, which brought away a tapeworm several yards in length, but which unfortunately, was not preserved for more minute examination. There can be no doubt, however, that the entire worm was expelled, as the patient rapidly convalesced, and has been in the enjoyment of uninterrupted health since that period."—[Med. Exam.]

Prickley Ash Bark.

The late Prof. Henry S. Patterson relates the following case:

Successful use of Xanthoxylon fraxineum.—For the following curious case I am indebted to my friend Dr. Thomas J. Turner, of Port Richmond. J. R. æt. 41 years, is a workman in a chemical laboratory. In Dec., 1847, whilst a private in the British army in Ireland, he first perceived that he was afflicted with tapeworm. He states that he passed fifteen to twenty joints at almost every stool for a time, and on several occasions as much as thirty feet at once. The surgeon of his regiment treated him with *Ol. Terebinth.* ℥j every other day. He also took tin-powder, male fern root, and "every other article he ever heard of." He finally abandoned the hope of a radical cure. The symptom most prominent was a sense of gnawing and beating at the epigastrium in the morning. He was obliged to eat before rising, as he otherwise became faint and "had all sorts of queer feelings." His appetite was insatiable. While at Port Richmond in the autumn of last year, he was attacked with a tertian intermittent, for which he was recommended to take an infusion of prickly ash bark in brandy—a popular domestic remedy. He digested an ounce of the bark in a pint of brandy, and drank the whole during the apyrexia. The result was a most copious diaphoresis, as usual,

and also some purgation, bringing away the entire worm. He has remained perfectly well since."—*Md. Exm.*

Tannic Acid.

At a meeting of the fellows and licentiates of the College of Physicians in Ireland, Prof. Osborne mentioned that he had been lately led to use tannic acid for the destruction of tapeworm by several considerations, and especially from its action on gelatine, as well as on albumen. Having ascertained in the usual way the presence both of gelatine and albumen in these parasites, it is to be inferred from analogy that the former is in their integuments, and if so, that in tannic acid we have the desideratum of an agent acting chemically on the worm, but not irritating the stomach or bowels of the patient, and, moreover, capable of being retained long enough to produce its full effect. He stated that in the case of two patients whom he had under this treatment, the appearance of the ejecta indicated that the worms had suffered from a chemical irritant, being in some instances curled and contracted, and in others friable and broken down, when expelled by a purgative.—*Dublin Quarterly Journal*, Nov. 1853, p. 457.

Quicksilver and Zinc as a Vermifuge.

The following case, reported by the late Professor Caldwell, of Kentucky, authenticated by his own direct testimony and by collateral evidence, is reproduced, not for its therapeutic value, but for its physiological, pathological, and chemical suggestions. The Professor says that "in the treatment of tænia, an amalgam of zinc and quicksilver is a valuable remedy, but that in the following case the dose was too large:"

"In the year 1818, J. P., of Louisville, was attacked severely by the tapeworm. After the employment of sundry remedies, by which portions of the worm were, from time to time expelled, he was advised to the use of an amalgam, composed of equal parts of zinc and quicksilver. Of this he was directed to swallow, every hour, a dose of an ounce weight, until he had taken *twenty-four doses*. He commenced, intrepidly, the formidable process, and proceeded in it regularly, until he had swallowed sixteen ounces of the amalgam, in sixteen hours. By this time, an uncomfortable and increasing weight (he called it very correctly, "a dragging down") in his bowels, admonished him to stop. He did so; and, on the second day afterwards, took an active purgative, by the aid of which he discharged a much larger portion of the worm than he had ever done before. But, of the amalgam, he discharged very little. By means of repeated cathartics, taken subsequently, at periods of time not very remote from each other, he conceived, at length, that the worm was entirely removed; and that he was now well, except that almost the whole of the amalgam was still in his bowels. Feeling no great inconvenience from this, he made a voyage to New Orleans. Perceiving, during his absence, that he still

retained some remains of the worm, he swallowed, on his return to Louisville, in four successive hours, four ounces more of the same amalgam, having now taken, in all, *twenty ounces*.

From the worm affection he was soon afterwards relieved; but not from that produced by the remedy. A large portion of the metallic mass still remained in his bowels, and began to excite in them considerable uneasiness.

In the summer of 1820, I saw him for the first time. His health was much shattered, his person emaciated, his countenance sallow, and his whole appearance strikingly hypochondriacal. The amalgam had been in his bowels nearly two years. When he related to me his case, I considered the story a morbid fiction, and belived his intellect somewhat deranged. But on further examination I discovered my mistake. Near to the umbilicus was a hard, round, floating lump, of considerable size, which, by the requisite management, I could grasp in my hand, and which appeared to weigh about a pound. The patient himself was confident it weighed more, for he asserted, that, of the twenty ounces of amalgam swallowed, he had never discharged more than two.

The account he gave me of the effects of this movable ball on him was, that if in walking he made a false step, or in any other way jarred himself, it gave him great uneasiness; and that it entirely disqualified him for riding on horseback. When he attempted to exercise in that way, it created, by its irritation, a constant and painful *erectio penis*. He could not, without inconvenience, bear even the motion of the easiest carriage. He was exceedingly anxious for the removal of the foreign body, and urged its extraction by a surgical operation, if he could not be relieved from it in any other way. From this resolution he was dissuaded for the present, and advised, to bear with his misfortune as long as it should be tolerable, reserving its removal by a surgical operation for the last extremity. In the mean time, a suitable diet and regimen were prescribed, with the use of occasional laxatives or purgatives, as circumstances might require, with a view to guard as much as possible against the bad effects of intestinal irritation.

In the autumn of 1823, the patient died of the malignant fever, which prevailed that season in Louisville. The metallic mass had now been in his bowels nearly five years. On opening his abdomen, the quantity found amounted to about eight or ten ounces. My belief had been that it was in one of the cells of the colon, where nature had formed a cyst around it to retain it, and keep it from moving along the intestine, analogous to the capsule which she throws around a leaden bullet, when buried in muscular or cellular substance. But I was mistaken."

The facts of this case, so far as they had transpired anterior to 1821, were published in the Philadelphia Medical and Physical Journal several years before the patient died,

The original prescription is thus :

℞.—Hydrargyri, } \bar{a} \bar{a}
 Stanni, } $\frac{3}{3}$ xii.
 F.—Amalgama.

Action of Vermifuges.

Dr. Budd, of Kingsbury College Hospital, 1850, says : “ Tapeworms are very tenacious of life. They are seldom voided entire without the aid of medicines that act especially upon them. Single joints often come away, and pieces two or three feet long are often then voided, but it very seldom happens, unless after medicine, that a portion comprising the head is thus passad. This portion remains and grows again, so that a person is often troubled with the parasite for years. One of our patients, Sarah Wheeler, had been so plagued for sixteen years, during which, she told us that she had seldom gone a week without passing joints of the worm. She had taken turpentine, and the bark of the pomegranate root, which brought away large pieces of the worm, but the head and the portion near it remained, and the worm grew again. The worm was expelled after the kouso in different portions, and she has since had none of the symptoms which she attributed to it.

Another case that shows more strikingly still the tenacity of life of the tapeworm, is that of Samuel Payne. He first passed joints of the worm seven years ago. In September last, he was brought into the hospital with cholera, and on the day of his admission passed a portion of the worm two yards in length. He remained in the hospital three weeks on account of the cholera. Some time after this he again passed joints of the worm, and to get rid of it came to the hospital as an out patient. Turpentine and castor oil were given him, and brought away a long piece of the worm. The ailments which he attributed to the worm were much relieved for a time. They then became again more severe; joints of the worm were again passed, and in January last he came for the second time to the hospital to be rid of the worm. Turpentine and castor oil were given him as before. A long piece of the worm was again expelled, but the creature was not destroyed; so that here the worm had existed seven years, had kept its place during the terrible commotion and flooding of the intestines in cholera, and had escaped destruction by two doses of turpentine. Payne took the kouso on the third of May, and the next morning voided the worm, which was ten yards long.

In all the cases in which the kouso had been given in the hospital, the head of the worm, or the taper portion near the head, has been found, so that there is reason to suppose that the creature has been entirely destroyed.

Dr. Budd, as shown in these extracts, seems to regard the discharge of portions of the tapeworm as proving the tenacity of life in this animal, as if the dropping away of the fully developed articulations might

be expected to cause the death of the entire animal, whereas, it appears probable that the detachment and discharge of these articulations are consistent with the normal condition of this parasite, and may even be proof of its high state of vital development.

Dr. Küchenmeister, of Zittau, has examined the various vermifuges by immersing the living entestinal worms of fowls, cats and dogs, in albumen at a temperature exceeding 77° Fahr., and adding the anthelmintics in the form of infusion or of powder. In some cases, a mixture of warm milk and water was substituted for the albumen. The experiments were not continued for more than from forty to forty-eight hours, if the worm had not been killed before the expiration of that time. Dr. Küchenmeister made use of electricity as the most delicate re-agent for proving the occurrence of the death of the worms. In the first place, electricity cannot be considered as a vermifuge. The author subjected a female *Heterakis vesicularis*, taken from a partridge that had been killed, to the action of a rotatory apparatus, which was kept up with longer or shorter intervals during an entire day. The animal was not destroyed by the experiment. He next tried the remedies employed for the removal of tæniæ, and first tested kouso in the following manner:—A living *Tenia crassicolis*, procured from a cat, was placed at four o'clock in a mixture of albumen and dolichos pruriens. The worm appeared to be perfectly well in this mixture, and at two o'clock on the following afternoon exhibited the most vigorous movements. The tænia was now transferred to a vessel containing a mixture of infusion of kouso and some of the infused as well as some of the fresh powder with albumen. The temperature of the mixture was 30° R. (99.5° F.) On its introduction, the worm quickly extended itself; after some time it was found to be dead, its color having changed to a dirty reddish yellow. Two *Tenia serrata* were placed at about half-past one in the afternoon in a mixture of albumen and kouso; at two o'clock they were dying, and at three completely dead. Two *Tenia serrata* from the same dog were brought in contact with kouso and milk at half-past one in the afternoon, and at two o'clock were dead. Two *Tenia serrata* were placed at half-past one in the afternoon in albumen, mixed with decoction of pomegranate root and with some of the powdered root: they died in three hours. Two others were placed in milk mixed with the decoction only: they died in three and a half hours. A *Tenia crassicolis* was put into a mixture of albumen with ethereal extract of male fern: it died gradually in three hours and three-quarters. A number of *Tenia cucumerina* were placed in a mixture of albumen and oil of turpentine: they were dead in an hour and a quarter.

A number of the same were put into a mixture of albumen and castor oil; they appeared lively at first, but were dead in seven hours. Similar worms were put into a salad, composed of pieces of unwa-

tered herring, boiled potatoes, large pieces of onion and garlic, albumen, vinegar, and a large quantity of oil. They died in eight hours. Lastly, the author tested the vermifuge powers of the brown oxide of copper; fifteen grains were administered in the course of four days to a strong cat. When the body was opened, the entire intestinal canal was found to be full of fluid, yellow, flaky fæces; the intestine was softened, and denuded of epithelium, especially at the termination of the ileum, where the adjoining Peyer's glands were much swollen, particularly in two situations, one of which was an inch and a half long by one-third of an inch broad; the other was nearly circular, and its diameter one-third of an inch. The cat had been purged. The tæniæ and ascarides it contained were lively. It would hence appear that this substance is both inefficacious as a vermifuge and dangerous to the system. The following table contains the results of the above experiments:

In milk boiled with kouso tæniæ died in half an hour.	
In a mixture of oil of turpentine and albumen, in	1 to 1½ hour.
In decoction of kouso with albumen.....	1½ to 3 hours.
In decoction of pomegranate root with milk....	3 to 3½ “
In decoction of pomegranate root with albumen,	3 “
In ethereal extract of male fern with albumen,...	3½ to 4 “
In castor oil with albumen,.....	8 “
In salmagundi with garlic and onions,.....	8 “

Kouso would therefore seem to be the most efficacious remedy against tæniæ. When pomegranate bark and male fern root fail, their failure may be owing to the habit of administering a laxative in from four to six hours after the exhibition of the vermifuge, by which the latter may be carried beyond the worm. With regard to pomegranate root, it must be observed, that in large doses it occasions diarrhœa. The same remark applies to castor oil. The author also alludes to cold water, strawberries, dolichos pruriens, and filings of tin. When tæniæ are placed in water containing ice, they are instantly benumbed, and if allowed to remain in it, they will always be found at the end of ten hours to be quite dead. Strawberries may be useful as a mild remedy in cases of tapeworm; if large quantities of them be taken on an empty stomach, entire portions of the worm will often be passed. Dolichos pruriens, with which the author tried many experiments, appears to possess no power of destroying worms. The author has also minutely studied the medicines recommended for the removal of round worms. In albumen, these worms behave as the tæniæ; in water, at about 77° F., they live for some days, but swell, stiffen, become longer, thicker, and more sluggish; they lose their power of suction, and their motions become slow and only partial—they resemble leeches which have gorged themselves. In general, however, the males and young neutrals resist the effects of water longer than the mature, impregnated, egg-bearing females, which become quite rigid and inflexible, and swell considerably. Milk and whey affect the

worms like water. The following are the medicines, the effects of which were tested :

1. Camphor. An ascaris lived from eighteen to twenty hours in albumen into which some camphor had been introduced.
2. A mixture of oil of turpentine and albumen killed some ascarides which were placed in it from two and a half to six hours.
3. Ascarides lived forty hours in albumen and wormseed, whether the latter was employed in the form of powder or infusion.
4. Some ascarides were placed in albumen mixed with santonine; they did not die in it, nor did they die in a watery infusion of santonine. When santonine was dissolved in oil, especially in castor oil, and mixed with albumen and ascarides, the latter died in ten minutes. An injection of santonine and castor oil was thrown up the rectum of a cat, and produced numerous motions containing dead worms; and on killing the cat, the entire of the lower portion of the intestinal canal was ascertained to be free from worms, while four were found near the stomach quite rigid and extended, and retaining but little life. A *Tœnia crassicolis*, however, was found in the intestines, and appeared to be quite uninjured and very lively.
5. A mixture of albumen and aniseed, with a strong infusion of the latter, killed the worms in about twenty-four hours.
6. Parsley, mixed with albumen, killed ascarides very slowly.
7. Flour of mustard and albumen destroyed them in about four hours.
8. In rue the worms lived upwards of twenty-four hours.
9. The same was the case with millefoil. In contact with tansy, valerian and camomile, great numbers of them lived for twenty-four hours. With onions and garlic they perished in from ten to fifteen hours. A decoction of cloves, with or without albumen, killed them in twelve hours. In an infusion of ginger, with or without albumen, they lived about twenty-four hours. Petroleum, mixed with albumen, killed them in less than six hours, as did also oil of cajeput and albumen.

A series of vermifuges, taken from the class of balsamics, was tried in like manner, namely, assafoetida, ammoniacum, balsam of Peru, extract of juniper, and Venice turpentine. In all these the worms lived more than twenty-four hours. Of the class of empyreumatics (brenzlichen stoffe) the following were tried:—Oleum chaberti, [a mixture of four parts of oil of turpentine, and one of the animal oil of Dippel,] oil of amber, castor oil, tar water, creasote, wood-vinegar, and wood-soot. In these, for the most part, the worms lived from twenty-four to forty-eight hours; except the wood-vinegar, in which they lived rather more than twelve; and creasote, in which they died within two hours. Of bitters, the author tried aloes, gamboge, ox-gall, wormwood, myrrh, gentian, quassia, hops, bitter orange, and acorus calamus; in all these the ascarides lived from twenty-four to forty hours. Of astringents, pure tannic acid, pomegranate root, kousoo, extract of walnuts, cinchona bark and quina, elm bark, willow bark, the flowers and stalks of meadow sweet, oak bark, dragon's blood, catechu and kino. In these the worms died in from twenty-four to thirty hours, with but two exceptions, namely, tincture of galls and pomegranate root, both of which killed them in the

space of eleven hours. Of saline preparations, sulphate of soda, chloride of sodium, and the roe of the herring, were tried. In the first the worms died in from fifteen to eighteen hours; in the second, in from two to six; and in the roe of the herring, in four hours. The following metallic poisons were experimented on:—Arsenic, calomel, corrosive sublimate, and the salts of tin, of lead, and of copper. Corrosive sublimate alone destroyed the worms in so short a time as two hours; all the other metallic salts required a much longer period. From these experiments it would appear that santonine, mixed with oil, is the most powerful vermifuge, then chloride of sodium, the roe of the herring, garlic, onions, &c. The author advises that santonine should be given as a vermifuge; mixed with oil, in the proportion of from two to five grains to an ounce of castor oil. This solution should be given in the doses of a tea spoonful until the effect is produced. As auxiliary treatment, chloride of sodium, herring-brine, mustard, onions and garlic, may be employed.—*Virginia Medical and Surgical Journal.*

Part Third.

MEDICAL INTELLIGENCE.

ART. I.—*Sketches of the Epidemic Yellow Fever of 1854.*

The following sketch of the yellow fever of 1854, founded in part upon published letters, telegraphic dispatches, documentary publications, editorial remarks in the public journals, and the writer's personal experiences and observations, is, of course, imperfect. The whole evidence has not as yet been elicited. The events themselves, yet in progress, have not reached their *dénouement*—the drama has not yet terminated—the curtain has not yet fallen.

Unfortunately, the medical journals which have as yet been received in New Orleans, either ignore the existing epidemic altogether, or barely allude to it, giving no details.

The meagre tableau now offered has been undertaken with reluctance yet with a view to meet the supposed wishes of the readers of this Journal; and although this sketch may fall short of their expectations, it has required no little labor: Take the will for the deed.

It must not be forgotten that delay instead of bringing authentic records of a desolating epidemic but too often obliterates from the memory the unwelcome impressions made during its constringing march—impressions which like writings in the sand, the next wave destroys forever. Agonies, corpses, shrouds, coffins and funeral marches from hour to hour for weeks and months in succession, like a terrible

tragedy tend to depress the mind so that re-action is natural. The past is soon forgotten, or replaced with far other concerns.

Man soon turns away from the repulsive realities of death to the attractive illusions of life, and pursues wealth, power, fame, or pleasure which either elude his grasp or disappoint his expectation, or at most can be enjoyed only for a short period until "death, great proprietor of all who treads out empires and quenches the stars," shall beckon him hence.

Happily humanity was not made to mourn, but wrapping itself up in sublime selfishness pursues its illusions less grieved to hear that the expedition to the Crimea, or all China, has fallen in battle, than that its own little finger must be amputated on the morrow. New Orleans is not addicted to unavailing sorrows; its revulsion from an epidemic catastrophe to commercial activity and cheerfulness is as rapid as it is salutary. "To enjoy is to obey."

To the rigid Æsculapian bent on statistical tables, therapeutic formulas, and post mortem examinations, these remarks may appear as the veriest of platitudes. But let him bear in mind as an extenuating circumstance that the *New Orleans Medical Journal* has some readers—thanks to the liberality of the public, who do not practice venesections nor administer castor oil, or other drugs (with which this number is replete) but follow other avocations.

The writer's experience (which however, may not have been sufficient to characterise the epidemic completely as a whole) justifies him in believing that, in no former epidemic for eighteen years has yellow fever yielded more readily to timely medication. Had he hitherto been a sceptic in the efficacy of medicine in the treatment of this disease, the present epidemic would have afforded grounds for faith in this behalf. Of faith however, real or affected, there is no lack in the medical world, be the mode of treatment what it may. It is proper, however, to state that, with few exceptions, he received early calls, that is, within from three to twelve hours from the attack. The violence of the disease, that is, the great pain, heat, and so forth, generally subsided on the second day. Hæmorrhages from the tongue, gums, nose, bowels, vagina, and scrotum, usually so portentous, by no means boded a fatal result in 1854; and, although he has had but one recovery from black vomit, some practitioners have probably witnessed several such cases. The comparative frequency of recovery after black vomit in the late epidemics of 1853-'4, is the most salient point of symptomatology in which they differ from those of former years. It is remarkable that black vomit is not only becoming less fatal in, but less characteristic of yellow fever, owing to its late extension to some other maladies. The writer may be permitted to mention a single case which he treated, and which occurred during the present epidemic, namely, that of Mr. Heiner's child, born in New Orleans, aged fifteen months, which was under treatment, at intervals for about two months for cholera infantum. During the last day of the child's disease, a febrile state supervened followed by black vomit and death.

Among the auxiliary causes or conditions favoring the development, aggravating the symptoms, and enhancing the dangers of yellow fever, may be mentioned severe labor, exposure to the sun, damp, filthy, unventilated and crowded lodgings, late hours, a bad diet, and intemperance; many ignorant of the danger or reckless of the consequences of this malady, neglect medical aid until death impends and medication becomes unavailing.

With regard to the various modes of treatment and their comparative successes, nothing will now be said. No compliment that one can bestow upon himself—no self-advertising formula yet invented can equal the statistical. He who cures ninety-nine in the hundred must, of course, be a better doctor than one who cures but forty-nine. Figures cannot lie. One who is his own statistician, and is addicted to mental reservation, *suppressio veri, ex parte* evidence, selfish biases, and to the practice of reporting an incredibly low ratio of mortality, should not forget that quacks have in all ages used this same kind of argument, namely, figures whereby to establish their superior skill and the infallibility of their panaceas. In this kind of arithmetic the most competent and conscientious physician may not mingle, without more or less suspicion, being no match for the quack whose lies are broad, yet deep and full to overflowing.

Weather in New Orleans, in 1854.

The weather is a most convenient, if not an edifying topic in conversation, and tends to non-committalism. In this point of view it is rarely referred to by the grave expounder of the causation of yellow fever. Whether this topic, valuable enough to the agriculturist, should not be placed on the conversational platform so far as the causation of epidemics is concerned, or whether it should remain, as heretofore, the inexhaustible staple for dogmatism, sophistry, pedantry, platitudes and book making, is questionable, seeing its failure to account for yellow fever. For example, of all the supposed causes of this disease, temperature, would seem the most conclusive and important, but in reality it cannot be identified as the cause of a single epidemic; that is, no special maximum, minimum, or mean heat has been ascertained as yet even as an invariable antecedent or forerunner of this malady; nay, a probable prognostication* cannot rest upon such a sandy foundation. In neither the temperate nor tropical climates of the Eastern and Western hemispheres do similar temperatures produce similar maladies.

During the epidemic of 1853, the yellow fever declined in New Orleans before cold weather; in some interior towns it prevailed in the face of heavy frosts with undiminished force, for weeks.

It is not intended to give elaborate tables of the daily mortality, weather, rain, thermometer, barometer, and so forth, which indeed the limits of this Journal will not admit at present; nor is it likely that

*Æsculapians, who have prophesied the advent of epidemics have been ~~often~~ wrong than right, which is surprising where the chances seem equal.

such enumerations replete with decimal fractions will afford much light in the pathology and treatment of the yellow fever.

The year 1854, as to wetness and dryness may be thus summed up: But little rain fell until near the close of February; the early and latter days of March brought forth several rains; the 14th April inundated the city; May committed no excesses; June and July afforded from five to six timely showers; August and the early part of September, though very arid, were not without light showers. As the equinox drew near heavy rains with winds prevailed for several days:

“The wind, 'tis true
Was somewhat high, but that was nothing new,
No more than usual equinoxes blew”

The equinoctial storms along the littoral of the maritime States of the South, were scarcely ever paralleled in violence, duration, and destructiveness. Before, during, and after this war of the elements, the epidemic raged unchanged; at least it did not recede, but followed its usual course.

Texas, Georgia, and South Carolina, which suffered most from this hurricane, suffered also most from the epidemic. In Savannah and Charleston the storm preceded the equinox two weeks, and in Galveston as many days. In New Orleans it was by no means violent, but it introduced the musquitoes. During the summer these insects had not been annoying.

Writers on yellow fever bent on discovering its cause, as every one should be, lay great stress on stagnant air, though this latter is rare in the present yellow fever zone. This disease like the storm “goeth whither it listeth.” The great epidemic of 1837, gradually traveled from the lower to the upper confines of New Orleans in about six weeks, during which, that is, on the 6th and 7th of October, the memorable storm which dashed the waves of the lake to the centre of the city, prevailed, without having exerted any marked influence upon the pace of the epidemic, then declining below Canal street. On the 12th of October frost was reported; but during the whole of that month the fever prevailed in the upper part of the Second Municipality and in all Lafayette. Although the storm stranded vessels in the cypress forest, sunk boats, made Rampart street navigable, demolished Milneburg, and killed or drowned people (one of whom the writer saw covered and crushed beneath the ruins of her house) it proved an utter failure as an extinguisher of the epidemic, thereby bringing into contempt many of the wise men of New Orleans, medical and non-medical who had declared that the epidemic was done for. Returning from this digression to 1854, let it be observed that October set in rainy, but soon cleared up pleasantly, being as yet, only ten days old: compared with other epidemic seasons, 1854 was wetter than 1837, dryer than 1839, and fell short of the timely rains usual in most epidemics in this city. The year 1854 in regard to cold and heat appears from Lillie & Co.'s Meteorological Journal, which has been directly consulted, on this wise: January, coldest $32\frac{1}{2}^{\circ}$, hottest 79° ; Febru-

ary, coldest 37°, hottest 80°; March, coldest 41°, hottest 83°; April, coldest 47°, hottest 83°; May, coldest 63°, hottest 91°; June, coldest 71°, hottest 95 $\frac{3}{4}$ °; July, coldest 77°, hottest 94°; August, coldest 79°, hottest 95°; September, coldest 75°, hottest 94°; the first ten days of October, coldest 69°, hottest 84°.

Filth in New Orleans.

The question of the filth of New Orleans, in 1854, has given rise to contradictory statements—one affirming that the city was never more filthy—the other that it was never more cleanly. Those who believe that yellow fever is caused by filth would find their theory damaged by admitting the prevalence of a great epidemic in the absence of the cause aforesaid, and as usual, they charged the city authorities—not with being accessories, but principals in the causation of the epidemic. As the Lord liveth they should be indicted by the Grand Jury of the Parish of Orleans, if they have done this great evil to the people.

On the other hand, those who steadfastly believe that yellow fever is a foreigner, always imported from some unknown territory (*terra incognita*) in which it is born and bred, and from which it feloniously emigrates to New Orleans, and that as there is no fundamental law in the constitution by which it can be naturalized and become a native of these States, there can be but little danger to their theory by the filthiness of the city in this behalf. The general opinion, however, by the majority who have no theories or selfish ends to support, probably is, that the city in 1854, has been less filthy than usual, notwithstanding the interregnum in the sessions of the common council, while the epidemic has been severer than in any former year with the exception of the last, and perhaps, that of 1847. The council is as guiltless of the epidemic as it is of the equinoctial storm.

If the Æsculapians of the importation school can be believed “yellow jack” has no country, is a native of no locality, is generated nowhere, is a *filius nullius*, the son of nobody, since the importationists of every place unanimously, and most indignantly disavow his paternity as Lear did that of Cordelia :

“By the sacred radiance of the sun;
By all the operations of the orbs,
Here I disclaim all my paternal care,
Propinquity and property of blood,
And as a stranger, hold thee forever.”

No *Ignis Fatuus* of the shaking prairies, black crocodilian swamps of Louisiana will claim propinquity with the aforesaid pestilent yellow jack. Until the importationists find out the place of his nativity he might as well retire to the Hartz Mountain and join the witches’ chorus—

“The limits of the sphere of dreams,
The bounds of true and false are past;
Lead us on thou wandering gleam—
Lead us onward far and fast.”

Mortality.

His Honor Mayor Lewis, on the 3d of Oct., 1854, in his Message to the Common Council of New Orleans, says:

“At the date of your last meeting the health of the city was in a most favorable condition; the mortuary calendar, for a city like ours, was very light; but I am sorry to inform you that, shortly after your adjournment, the yellow fever appeared in our midst, assuming, as I am informed, a similar type to that of last year; the weekly reports of deaths from yellow fever within the last ten weeks have ranged from 12 to 341 per week; the total deaths reported by the different sextons of cemeteries, of all diseases, during that time, up to the 1st October inst., were 3,513, of which 1,805 were of yellow fever.”

On the 8th of October the official report for the week, gave this result: total 358, yellow fever 207; total for the period commencing with that mentioned by the Mayor, to the 8th of October, 3,871, of which 2,012 is the number of deaths from yellow fever alone—of the 2,012 who died from yellow fever, it is presumed, that, as usual, nearly all were whites aged above fifteen years. If the deaths from the epidemic during the season yet remaining shall reach a total of 2,400, this will more than double the whole mortality of the whites aged fifteen years and upward in New Orleans, Lafayette, and their environs from the 1st of January to the 18th of October, in 1845, as given in Mr. T. Stringer's compilation, from which it appears that 1,183 died from all causes in the nine months and eighteen days of that non-epidemic year.

The epidemics of 1837, 1839, 1841, 1842 and 1843 averaged 1,100 deaths; the epidemic of 1841 gave the highest number (1,800) and that of 1842, the lowest. The mortality from yellow fever, in 1847, which was the highest which up to that time had ever occurred, (2,600) and the mortality of 1853, afford the only two examples that surpass the mortality of the year 1854, although these estimates are liable to the objection that they do not regard the numerical ratio of the deaths to the fluctuations of the population.

Towards the close of June sunstroke prevailed epidemically, so to speak. The yellow fever scarcely could be called epidemic until after the middle of August; public opinion hardly recognized it as such until September.

The first official notice of the Howard Association appeared on the 22d of September offering relief to the indigent yellow fever patients of New Orleans; subsequently the Association extended its benevolent operations both as it regards pecuniary and personal aid to the cities of Galveston, Savannah, Charleston and Augusta.

The following data represent a more detailed view of the weekly mortality of New Orleans, in 1854; from the 25th of June to the 8th of October; the first figures denote the total deaths for each week—the second, in parentheses, the number of deaths, if any, from yellow fever for the corresponding week: 329 (0); 191 (0); 129 (0); 159 (0);

122 (0); 212 (29); 207 (43); 258 (118); 380 (186); 393 (185); 484 (284); 530 (340); 504 (341); 423 (269); 358 (207); total deaths in fifteen weeks 4,629—total deaths from yellow fever 2,012.* The great mortality of the first week in this enumeration ending July 2d, was occasioned by sunstroke; 83 deaths were reported from that cause, and 22 from apoplexy; but it is probable that 100 may be attributed to the former, which suddenly appeared, and as suddenly subsided, as did the usual mortality, so that "heat and health" were regarded as synonymous for a time.

Those who affect statistics and sanitary wisdom—who disparage the city authorities and the salubrity of New Orleans, and undertake to prove that the English or some other people are the healthiest and the longest lived in the world on account of their supposed sanitary improvements, know little of what has yet been done in the premises, notwithstanding their fallacious figures, decimal fractions, and vital statistics. The military element in France, and the commercial in the United States, have led to some reliable statistical results. But the British, have failed fundamentally in every census, unless, perhaps, that of 1851. Their plan has been excellent, its execution faulty. The hasty comparisons in favor of the health of British towns and against New Orleans, are insufficient, for this plain reason, namely, that a sufficient time has not yet elapsed, even if the boasted improvement had as yet been actually made, to afford reliable deductions—as for instance, the average duration of life and so forth, inasmuch as the sanitary questions concerning the public health of towns in England were not broached until the year 1839, even by petition to Lord John Russell, from the Poor Law Commissioners. The next year the House Lords petitioned Her Majesty's Government on this behalf. In 1840, a Committee of Investigation was appointed by the House of Commons. In 1842, the Poor Law Commissioners reported. In 1843, the Government Commissioners were appointed; their first report was presented in 1844; their second, in 1845. The National Cyclopædia, of 1851, of London, says that "the large towns have already begun to make improvements. Of fifty towns visited by direction of the Commissioners, only eight were found to be in a tolerable state as to drainage and cleansing; and as to the supply of water, the reports were still more unfavorable." These reports tend to show that the improvements, with all the means of the nation, appear to enhance the salubrity of towns; but one or more generations must elapse before such a question as the average duration of life can even be compared with that of the preceding generation. Moreover the British Empire has not been recently, nor is it now healthful in a remarkable degree.

Her Britanic Majesty, with her immense treasure, her willing Parliament, has not improved her good city of London in a ratio comparable to the government of the much abused city of New Orleans; which

* Since the above enumeration, the mortality of the week ending October 15th has been announced—total 272 (134); giving a grand total of 4,901; including yellow fever which latter gives a mortality of 2,146.

latter has within a few years reclaimed more than half of the city from the water fowl and the alligator. These sanitary measures as the filling of lots, the paving of streets, drainage and so forth, are set at naught, because they accord with mother wit, and are not based on astrology or something too subtle for common sense.

The researches of the vital statisticians of modern times, tend to prove that the physical comforts promote health, longevity, and the increase of population, while on the other hand, extreme poverty, unwholesome food, insufficient clothing, bad lodging, imperfect ventilation, filth, overcrowding, excessive labor, and exposure to the inclemency of the weather, have a contrary tendency. If, however, facts are to guide the sanitarian, he will be compelled to accept, what few would expect, the negro population of the Southern States of the North American Republic, as the most perfect types of increase of population, health, and long life. No nation, no class how favored soever it may be, from the dwellers in the deepest mines and cellars to the most gorgeous thrones—no town how clean soever it may be, can, as yet equal the negro slaves in these fundamental criteria, as all reliable vital enumerations, including six censuses of the United States (1790—1850) will show. The difficulty of accounting for this postulate cannot invalidate it in the least.

Another inexplicable fact, opposed to the sanitary theory of crowding, the cholera of 1854, has developed, namely, that this epidemic prevails in an inverse ratio to density of population.

It appears from *Revue de Thérapeutique*,* of September 15th, 1854, that cholera which has prevailed in France for nearly a year, has been most fatal where the population has been the least dense. It is believed that the United States will furnish a similar result.

Among the large cities, New York has suffered most, but had the mortality from cholera in that city been in a ratio similar to some small inland towns, villages, and thinly populated settlements, more than 100,000 would have perished during the last summer. Had New Orleans been affected with cholera as severely as a few isolated plantations in the South have been, perhaps 50,000 would have died in this city. Even Marseilles and Naples have not suffered in an equal ratio to many small towns and villages in Europe and America. But it is time to return to the yellow fever: early in the season yellow fever appeared, though to a very limited extent in Vera Cruz and Havana. Before the close of summer, it passed the Tropic of Cancer, in an epidemic form, to New Orleans, Galveston, and Key West. It soon doubled the cape of Florida, and having concentrated its greatest force upon Savannah, reached Charleston subsequently; with few exceptions it spared the intervening towns. Lastly, it visited several inland cities on the Savannah, Alabama, and

* This Journal says that the "cholera of 1854, presents this characteristic, namely, that the proportional intensity of the scourge, is in inverse ratio to the population; the villages being more severely dealt with than the cities; the hamlets, than the villages; and certain farms more still than the latter. In La Meurthe, on the banks of the torrents, the disease prevails more strongly than in the vicinity of the large streams. In the Vosges it is found, also, that the smallest places are the most frequently attacked. Mirécourt has lost 250 persons out of 5,000, that is to say one twentieth; Dompaire 180 in 1,800, or one tenth; Bouz6 100 in 500, or one-fifth; and to crown all, a village of the environs of Saint Dié has lost 49 in 100; or one-half."

Mississippi rivers, where it produced great consternation, but little mortality.

A few remarks concerning some of the places visited by yellow fever, will close this sketch.

Key West.

Key West, was the first portion of the Republic invaded by yellow fever in the epidemic of 1854. A correspondent of the Charleston Courier, in a letter dated at Key West, September 8, 1854, affirms that the town had then been exempt from yellow fever for six weeks, though, at the military barracks it still raged terribly, having attacked almost every soldier and destroyed one-third of the entire force.

This island, the gift of the tiny coral insect, so unique in its medical topography and interesting to the ætiologist, is, as all know the most Southern portion of the United States, the town of Key West being situated 24° 32' North latitude. The whole island constituting Monroe county, Florida, has independently of the troops, nearly 3,000 souls, chiefly wreckers, and scarcely affords an area of 200 acres.

Its general elevation above the level of the sea, six or seven feet, no where rising to twenty feet, almost the whole surface during hurricanes being washed by the waves of the sea. An outlier of the Peninsula of Florida, forty miles distant from the Continent, a mass of coral having but a slight stratum of soil, washed by the ever rolling Gulf stream, fanned by the sea breezes, having a delightful temperature, never very hot nor cold, distant from Morasses, Cypress swamps, crowded filthy cities, alluvial deltas, and malaria, and yet this gem of the sea, with its handful of earth, its tropical flowers and shrubs, its clean sands, its variegated shells, its coral strand laved by the waves of the sea, has been visited by yellow fever notwithstanding the general salubrity of the island. Upon the theory of contagion, this coralline ought to be the centre of imported yellow fever from Havana, New Orleans, Vera Cruz, and towns lying on the Gulf of Mexico. By what telluric, astral, or Neptunian sub-marine entity its late visitation or its general exemption from yellow fever may be owing, has not yet transpired. The Æsculapian oracles are either silent, or *Philippise*, to use the Demosthenian idea as to the satisfactory responses which Philip of Macedon, got from the Pythia at Delphi.*

The annual mean temperature of Key West, the highest in the Union, is for nine years 77.1°, about ten and a half degrees above that of New Orleans (67.5° for eighteen years—Smith. Rep. 1853); yet in many northern cities the heat for a portion of the season is greater than in the

* A few years ago the Academy of Sciences, at Paris, appointed a commission to ascertain and report on the prevailing diseases and meteorology of the seasons in Paris. This report drawn up by the illustrious Arago and the distinguished Dr. Double, based on ten preceding years, quarter by quarter, concludes that, no connection can be traced between the characteristics of the seasons (*caractères des saisons dominantes*) and the prevalence of epidemics; though the weather serves to guide the practitioner in the treatment of the sick, (*Comptes Rendus*, T. IX. 515.) The Academy, however, appear not to understand the word *Burcombe*, which, like "the invention of sleep covers one over like a garment."

former. The arid rock of Gibraltar, and the almost submerged island of Key West, though subject to yellow fever, are devoid of its assumed causes in a most remarkable degree.

The first case of yellow fever which appeared in Charleston, in 1854, occurred on the 11th of May, according to the official report, in the person of a passenger directly from Key West; the second case two months later, arrived from the same port.

Savannah.

Savannah, 32° 04' 56" North latitude, contained in 1852, a total population of 18,801; of which the colored amounted to 6,122 (free 624—slaves 5,478) or about one-third—whites 12,679; by the United States census of 1850, it appears that the total population is but 15,312.

This city on the Savannah river, eighteen miles from the ocean, was founded about ten years after the foundation of New Orleans.

The white population of Savannah during the existing epidemic has been estimated variously at from 3,000 to 5,000, consisting chiefly of the unacclimated; according to some published estimates the unacclimated reached eighty in the hundred of the whites.

The mortality from the epidemic in this city is already, perhaps, unparalleled in the history of yellow fever in the United States. The week ending August 29th, gave a total mortality of 92—yellow fever 60; the week ending September 5th, total 123—yellow fever 74; the four weeks ending September 12th, total 492—yellow fever 305; for five weeks ending on the 19th September, total 681—yellow fever 436. Now taking this last figure of the total mortality as a divisor, and the total white population then remaining in the city at 5,000, it will be found that nearly one in seven perished in five weeks. The mortality before and after that period was great; a ratio of mortality which in little more than half a year would have left none alive.

The mortality among the medical faculty of Savannah has been appalling. While the epidemic was still progressing, the deaths of Drs. Welles, Wildman, Harris, Schley, Ellis, Gordon, and it is said four others, were announced. Estimating the physicians of Savannah as being in the ratio of one to every thousand souls, and reckoning the population at about 15,000 as given in the last United States census about two-thirds of the physicians must have died before the decline of the epidemic.*

EDITOR.

NEW ORLEANS, October 10, 1854.

Art. II.—*Letter on Yellow Fever, (continued from page 380:)* By M. MORTON DOWLER, M. D.

[To the Editor of the N. O. Medical and Surgical Journal.]

In my communication addressed to you, dated ten days since, I spoke of the epidemic in relation to the front or river section of the Fourth

* Owing to a mistake of the printer, discovered too late to be remedied, it was found that the space in this number of the Journal was less by eight pages than the editor expected, though the subscribers will receive the full amount of matter. Hence, several short communications intended for this number, as well as several editorial notices of the yellow fever at Galveston, Charleston, &c., were crowded out. The editor regrets the mistake, not on his own account, but because the space allotted to the friends of the Journal had been unintentionally pre-occupied.

District; (formerly Lafayette) and detailed a portion of what I had seen and known. At that time I had seen but two cases in the rear of Magazine street. During the last ten days, however, there has been no portion of the Fourth District, nor of Jefferson City, in which I have not been called to yellow fever patients, and I may say the same thing of all that portion of the First District above Race street, and about the Melpomene Canal, and the Gormley Canal and Basin. The epidemic is, I believe, quite generally diffused throughout the whole city, there being no section of it in which the disease has not been seen. It has never been surpassed here in malignity; never has there here been seen a greater proportion of hæmorrhagic and black vomit cases.

The utmost attention has been paid in this district to the enforcement of the ordinances in relation to cleaning the streets, and the removal of nuisances. In no season for the last eighteen years has there been a more satisfactory state of things in this respect. I believe the street commissioner has effected this season what no Board of Health ever did or ever could do. Notwithstanding the denunciations which as usual when epidemics prevail have been fulminated against the Common Council, I hope that neither they, nor any future city government will ever again organize another of those useless debating societies called "*Board of Health.*" They never have nor ever can meet the approbation of the people of New Orleans. But even on the supposition that a Board could do all this, never was, nor ever will any Board here be furnished with the pecuniary means to effect any measures of importance. The subjects regularly entertained by such Boards, as for instance, filling of lots, draining of swamps, paving of streets, cleansing of privies, and the general purification of the city, to say nothing of the millions to be expended in "confidence," and quarantine, all implying an unlimited access to the public purse with independent functions, are totally out of the proper jurisdiction of any power save the direct legislative and responsible authority alone. Never yet has any measure emanating from any Board of Health in this city, warned us of an approaching epidemic, prevented its extension, shortened its duration, or given the least idea of its origin. The Health Committees of the Common Council duly empowered, co-operating with the Street Commissioner, constitute all that is necessary in any emergency. The Common Council should never throw off its proper legitimate authority, and delegate the same to any subordinate, secondary, and irresponsible Board. The fact that Boards of Health, do very well in Berlin, London, or Paris, affords no argument in favor of their applicability here. In the countries in which these cities are situated every thing is done to render government as complicated as possible, and to clothe every department of a complicated system with ample powers and pecuniary resources, and there is little or no responsibility to the people. Here the very reverse obtains. Government here is simplified to the utmost, and everything in relation to the public interest is always botched completely, if not done under the direction of the immediate authorities themselves. We have borrowed many valuable things from European institutions, but

Boards of Health are not amongst the valuables. On this subject I may have something to say in a future number. I may merely remark that our streets and city generally have never been in so good condition; that we have had the best possible Board of Health, namely, an efficient body of laborers led on by an able and efficient street commissioner; and that had such "Board of Health" been placed at the head of affairs during the epidemic of 1853, both the money and reputation of our city would have been greatly husbanded. As it is this season, the gutter sends up no intelligence in relation to the origin of the epidemic, and the ship hold maintains a profound silence.

A word in relation to meteorology. It is a science which has shed but a feeble and unsatisfactory light on the ætiology of epidemics generally, proving the same proposition to be true and false at one and the same time. In relation to that of yellow fever it has shown itself to be absolutely worthless. It shows the following truisms, and nothing more; and I call on any one who can, to make anything further out of it. When the barometer shows either a dense, a lightly, or extremely rarified atmosphere; when the hygrometer shows either a gentle moisture, a recking wetness, or an intense desiccation; when the thermometer indicates either a mild, airy, pleasant temperature, a breathless, heated atmosphere, or one modified by a protracted chilly breeze; when we behold either a deep, cloudless, sunny sky, or a horizon overcast with impenetrable clouds, blotting out the sun; in all or any of these conditions, all we can make out or infer is, *that the yellow fever may or may not appear as an epidemic; or if it has already thus made its appearance, that it may or may not become more or less general or malignant; or that it may or not disappear!* The meteorologist points out to you and classifies the clouds. He shows you his *cirri*, his *cumuli*, his *strati*. He points out the *cirro-cumuli*, the *cirro-strati*, the *cumulo-strati*, and the *cumulo-cirro-strati*, or *nimbi*. He gives you a learned disquisition "about the weather," and yet all he can tell you at last is that in relation to yellow fever "fair is foul and foul is fair." He can neither warn you of an approaching epidemic nor tell you what atmospherical conditions will put an end to it; nor what conditions will render it more or less malignant. I would respectfully call on the meteorologist if he knows any thing more about the ætiology of yellow fever than his own coachman, to come forward and publish it to the world. Nothing has yet appeared that every drayman does not know to be without foundation.

The yellow fever agency cannot be transported from one place to another through the medium of *fomites* nor can it be retained in our city in such manner. On examining the mortuary records of 1853, it will be seen that the epidemic was virtually at an end about the middle of September; for during the week beginning at that date the daily average mortality did not exceed eighteen per day. Fresh immigrants from Europe, and especially Germans, poured into the city. The weather was burning hot, and frost distant, and it is palpable to every one that the disease did not cease for want of subjects. Of yellow fever subjects there were thousands left. But the disease did rapidly disappear here, while it was ravaging other places. If the morbid agency then can possibly nes-

tle itself into, and attach itself to goods, merchandize, houses, &c., it follows that every house in New Orleans was at that time but a receptacle of poisoned articles. The yellow fever and death had touched every thing. But none of the newly arrived suffered from eating, drinking, wearing, or touching any article in New Orleans. Still less is the likelihood that any person did or could suffer from any article sent to a distant city after perhaps a three months voyage. If the newly arrived immigrant can sleep with impunity on a cotton bed which has passed through the pestilence, there can be no possible danger to the people on the shores of the Mediterranean or elsewhere from a free reception of New Orleans merchandize. If the propagation of the disease by *fomites* or trade be true, it is incumbent on every conscientious dealer to fumigate and disinfect every article in his store, and never to presume to send any article into the country from New Orleans without the use of ozone, chlorine, or some other disinfecting agent. No such article should be received without the certificate of the *fumigator general* of the city. If there be any possibility of infecting our trans-Atlantic brethren, how much greater the danger of domestic infection! The idea of transporting and carrying the yellow fever agency from New Orleans, is a thing in which every person practically displays his disbelief. All experience shows its utter and entire falsity. If the disease can be thus transported, then the sum total of all the goods in the city of New Orleans is, on this sixth day of October, 1854, but a mass of yellow fever *fomites*, for there is not at present in our corporate limits a house or lot in which an unacclimated person would not take the yellow fever. What becomes of the conscience of a sincere contagionist and importationist when he sends away his goods into the country by the ton without fumigation, or disinfection, and thus sends poison to millions of his countrymen? Our commercial interests and the cause of truth require that every person whether physician or layman should with one accord come forward and give the lie to so monstrous a doctrine. Yet there are those who believe that the yellow fever can be corked up, and the vial of wrath poured on a distant city!

The peculiar morbid phenomena to which we give the name of yellow fever, are the effects of an unknown noxious agency. It is to the assemblage of the effects manifesting themselves on the organism to which we attach this name; and the person laboring under these effects has received an injury from an unknown agency. The injuries he has received he cannot communicate either directly or indirectly to another. A vulnerable bystander incurs no additional risk from being in contact with the injured party. The latter has been assaulted and his injuries show themselves by the resulting pain, heat, thirst, delirium, hæmorrhage, black vomit, &c.; but the injuries he has sustained, the effects which have been produced, are no more communicable to the looker on than are the wounds, contusions, and injuries received in an affray. The same agency which produced the yellow fever injury in one person may and does produce it in another, but one person cannot communicate his symptoms and sufferings to another. Hence we see the unacclimated husband and wife occupying the same bed, one of them escaping entirely, and the other being smitten. Or husband, wife and children may all be attacked

at nearly or quite the same moment of time without all or any of them having been in contact with the injured, proving clearly thereby, 1st that the injury was not communicated to them by other sufferers; and 2d that they were suffering independently of the bodily condition of each other, or indeed the unacclimated husband while in perfect health, nursing his sick neighbor with impunity, may be summoned home in haste to find his wife and children who may never have approached a yellow fever patient, all attacked at the same moment showing that those who are least exposed to contact with the sufferer are often the first to suffer. Mrs. L. died on the 12th of June last, with the disease, none of the rest of the family suffered at the time, nor did the unacclimated neighbors. I did not see another yellow fever case for about five weeks, when I was called to attend on two ladies—one in the clean, retired, aristocratic portion of the Fourth District, and the other in a fine pleasant house in the front of the same, both attacked with yellow fever. Even the filthy and destitute immigrants had hardly yet begun to suffer from the disease when these two patients were attacked, strangers to filth and to yellow fever contact.

New Orleans stands on the latest of geological formations. The interminable and irreclaimable swamp is seen in the distance at every point of the compass, and in many districts the sun rises and sets, to the eye, in the waves of a sea of gigantic grass rooted beneath the water, yet there is nothing here to explain the causes of yellow fever. Cities resting on secondary rocks and high tertiary formations, are visited annually by the yellow fever in its most malignant form. No one has ever brought forward a single satisfactory proof that our swamps have any thing to do with the production of yellow fever, nor is there any reason to believe that if the swamps of lower Louisiana were at once obliterated, and New Orleans rendered as high and dry as Gibraltar, she would be otherwise than as Gibraltar herself or Havana, still liable to the yellow fever. New Orleans stands on an alluvion but in regard to this pestilence she is none the worse off for that. Drainage is good, filling up is good, reclaiming the swamps is good, but no one need ever expect that all this will have any influence on the appearance of yellow fever. If as some think the yellow fever is generated in the swamps and wafted into the city by the winds, then it is plain that the cause can never be removed by any thing we can do in and around New Orleans. Nothing short of draining, filling up, or reclaiming the whole of lower Louisiana—requiring all the gold of California to effect the object, would be of any avail. The marshes of Louisiana are literally made up of decayed vegetable and animal matter by which they are gradually elevated. The sudden draining and drying up of millions of acres such as these may be considered *anceps remedium*. One thing is certain, we have a population residing on live oak platforms called *chenières* wholly surrounded by those marshes and our city people deem it a privilege to reside amongst them in the summer season *for health!* In such places yellow fever was wholly unknown till the disastrous season of 1853.

The filth theory of yellow fever has been totally annihilated by the events of the past season and the present epidemic. In May and June we

had rain and open sluices admitting the water of the Mississippi into the swamp. June 1854 was not unlike June 1853 in meteorological character. A few straggling cases of yellow fever appeared in June and July last, but so few that the disease was as usual ignored. In the latter month never was there felt in this city a more heated and becalmed atmosphere. Though we have by no means, so filthy a city as New York, it is quite impossible for the most vigilant authorities to entirely rid our city of filth. Every privy, gutter, basin, canal, unfilled lot, square, excavation, or cemetery sends up its exhalation. The swamp and the common exhibit the remains of poisoned dogs, sun-struck horses, dairymen's cows, and bullocks and sheep that have proven themselves too debilitated for the meat market. The evaporation that took place in July was very active. The cypress platform in the rear of the city which had been flooded with rain and river water was as it were boiled to dryness. The Gormley excavation after presenting its festering waters for a long time, now lay *anhydrous* to the sun. Every impurity was in a state of ebullition. Yellow fever, however, had been but barely seen. The mortuary records of the city never read more favorably save only in relation to the effects of the sun, man and beast having expired from insolation in every direction. Filth, it is true, was very active as it ever is, whether yellow fever prevails or not.

The cause of yellow fever is, we know, in an altogether essential degree, subjective or internal. There is, however, an external, or objective cause or agency which is absolutely essential also to the production of the disease. The objective agency, so far from being a thing which is movable and floating in the air, is no doubt an agency as fixed and immovable as the very streets themselves. It yields no obedience to the winds; and it has this season stood as firm as our alluvion before the tempest. Like the cholera it counternarches the whirlwind and the storm. On the first of October this agency was assailed by a hurricane of wind, and a cataract of rain, and the stern disease smote the people in this District during the two succeeding days with increased violence. I saw more new cases during those two days than I have seen in any other two during the present epidemic. So far as the atmosphere is concerned, I have not the least doubt that it may and does exist in its almost purity, alike in the absence or presence of a yellow fever epidemic. Whoever discovers the objective or external cause of yellow fever, must look beyond the crude dealings in gases, animalculæ, cryptogamia, quarantine, filth, and meteorology which are now being exhibited before the world. The mystery lies deeper than all this rubbish. It may or may not be in the power of man to grasp the secret, but if ever that is done new laws must be investigated and new problems solved. A higher scientific era will have been inaugurated.

The ætiology of epidemics and especially of yellow fever and cholera, is a subject in which the public have always taken the deepest interest. The mass of the people ever enthusiastic and hopeful, have generally been rather inclined to receive every new theory with more or less favor, and a like spirit has animated the legislative mind. A singular spirit has in the meantime pervaded medical writings on the subject. There are com-

paratively few of the profession who do not believe that little or nothing is known in the premises, and even the people at large are beginning to ridicule the entire array of "causes" which have so long been paraded before them. Nevertheless, when we consult the greater part of the treatises on the practice of medicine, and the special books, papers, and pamphlets touching on the ætiology of yellow fever, we find a most notable display of causes. The writers cautiously make out these by giving a little "meteorology," a little "medical topography," a little "geology," a little "putrefaction and exhalation," together with a very circumspect reference to the organic, microzoic, cryptogamic, electric, calorific, gaseous, and filth theories, and so forth. A very specious appearance of real knowledge is thereby kept up, and an undoubted display of impartiality. All that the medical reader can make out is, that nothing can be known; and all the non-professional one can discover is that he cannot understand the scientific method of finding out how it happens that nothing is known. It does not comport with the dignity of authorship or professorship to say, "I don't know." It must be said gradually, and with the appearance of knowing everything. Speaking for myself, I may say that I have been a student and practitioner for twenty years; have read and reflected much on this subject, and I have never yet obtained any explanation of the causes of either one or other of these diseases, to which the least importance should be attached—any theory worthy of belief or of legislative attention.

V

ART. III.—*Discovery of Viviparous Fish in Louisiana.*

"What have we here? A fish—a strange fish."—*The Tempest.*

In the month of October, 1854, through the politeness of J. C. B. Harvey, M. D., of Tchoupitoulas street, I received a small osseous fish, caught in the New Orleans canal, which connects the city with Lake Pontchartrain. This fish had been placed in a basket containing crabs, one of which wounded it slightly in the abdomen near the cloaca, thereby exposing several foetal fish enveloped in a delicate membrane. The parent fish, which had been rudely thrust into a narrow mouthed phial of spirits, retains after immersion for two weeks, the original *rigor mortis*, and the same remark applies to the fetuses, though they have been soaked in water; some of them have been forcibly straightened. On the 17th of October, in the presence of, and assisted by Drs. J. Hale and M. M. Dowler, I enlarged the wound and proceeded to dissect a somewhat globular mass of fetuses bounded by the intestines before, and separated from them by an indescribably thin, diaphanous membrane; this mass was further bounded above by the spine and ribs, below and behind by the posterior inferior abdominal walls, bulging backward of the anal orifice and fin. The exterior envelope of this oblong globe consisted of a very thin, pelucid, extremely delicate and apparently laminated and

flocculent membrane, like the amnion of the human embryo in the early state; it did not form a simple sack, but consisted of many duplications like the arachnoidal reflections among the sinuosities and convolutions of the human brain, sending its prolongations as the hyaloid membrane does, through the vitreous mass of the eye.

This uterine membrane (ovisac it may not be termed) contained twenty-two fishes. It is probable that the inner surface of the uterine membrane sent forth a still more delicate membrane which enveloped each fish after the manner that the peritoneum envelops the abdominal viscera; but the parent fish, and still more its inclosed organs, were too minute to admit of full demonstration during a necessarily hurried examination; moreover, the wish not to mutilate the parent fish very much prevented a fuller dissection of the foetal mass *in situ*.

Each foetal fish was doubled laterally, sometimes to the right, sometimes to the left, into the globular form, the caudal fin which is inclined to the lancet shape, though blunter, overlapped one eye and one side of the mouth; each fish *in situ*, and even after forcible extraction from its bed was infolded in a sack; some were drawn out united by pedicles to a common stem, somewhat like an umbilical cord.

These foetal fishes presented a perfect example of close packing. A perceptible force was required to dislodge them from their beds. The concavity left by their extraction appeared to be lined with a smooth, black, peritoneal membrane.

The intestines which were very minute were crowded forward by the rounded mass of fetuses which occupied the greater portion of the abdominal cavity. No ova were discovered.

The maternal fish not being much mutilated, is reserved for a more detailed technical description, which my leisure and the limits of this Journal will not admit of at present.

Without attempting fully to describe even the dermal skeleton, I may observe that this tiny fish is a most symmetrical one. Its minuteness may be imagined when I state that after the removal of the inclosed fetuses it weighed only seven grains, though not disembowelled. Thorough desiccation would probably reduce its weight about half or more. The fish exposed for two hours in the shade on a damp day, was but slightly desiccated. It was weighed by Mr. Macpherson, apothecary, in my presence; but fearing a mistake I had it weighed a second time, with the same result. If each foetus should weigh but one grain, the aggregate would be more than three times greater than that of the mother!

Measurements in inches: Length including the caudal fin 2 inches; greatest circumference $1\frac{3}{4}$; width vertically $\frac{1}{2}$; length of thoracic fin $\frac{1}{4}$; the caudal fin does not expand from its base or proximal end, but terminates ovals, its length $\frac{1}{2}$; the anal but little expanded $\frac{1}{4}$; the ventral is too minute for convenient measurement, being almost invisible without a lens; the dorsal which is single, has but a slight vertical width, arising from a base $\frac{1}{4}$ of an inch, nearly opposite, though a little forward of the anal.

The teeth are advanced, nearly ranging with the lips, being very numerous, close and small, though scarcely discernible without a magnifying

glass. Lips thin, the under one slightly projecting; angles of the mouth not depressed; eyes medium size; head flattened at the frontal bone; operculum much expanded. The branchiæ largely developed in three great arches, densely fringed with thick tufts, the outer and inner rows inclining to the central, having also, one, perhaps more rows behind, which are shorter.

The predominant hue of this fish is a tawny or fawn color; the opercula silvery; head metallic gray; muzzle blackish, slightly projecting.

There are six rows of rather quadrangular black spots, more particularly marked in the posterior half of the body, averaging twenty-five spots for each row. These black spots, resting on a tawny ground, leaving intervals something larger than themselves, give a picturesque appearance, forming stripes of alternating hues, the three upper of which slightly curve corresponding to the arching back; but each becomes straighter, the fourth and fifth being nearly straight; the sixth or lower row follows the abdominal curve, and disappears at the anal fin; the other five rows gradually converge without coalescing at the origin of the caudal fin. At the origin of this fin the spots are displaced out of line. By this arrangement the six rows of alternating black and tawny leave in the longitudinal direction six other continuous tawny stripes, all of which except the two interrupted ones, are lost at the anal fin, and converge without mingling in the tail, all being about equal in length. The colors fade somewhat into a greyish yellow around the thoracic fins, which are nearly central between the dorsum and abdomen, being on a level with the eyes, and about one line from the opercula.

There are six or seven rows of scales. The spinous rays of the fins are about twenty-five caudal, twelve anal, fifteen dorsal, ten thoracic.

These fœtuses are half an inch long, all alike, exactly resembling the maternal form and proportion, with the following slight exceptions, namely: their bodies are more slender and compressed laterally; their heads are comparatively larger, and their eyes more prominent; their colors are less variegated, and paler; a still greater difference appears about the middle of the abdomen, where there is attached to each fœtus a whitish, faintly yellowish, placental-like irregularly formed mass of considerable size, having a broad base, being apparently implanted in or blended with the abdominal integument, possessing considerable strength, and constituting what may be termed the umbilical prominence; perhaps, it may turn out upon further examination that this mass may be not placental, but an adherent mesenteric mass of convoluted membrane.

These fœtal fishes were probably sufficiently developed at the time of the parent's death to live independent of the mother.

It appears from the proceedings of the Academy of Natural Sciences of Philadelphia, for 1854, that Dr. Gibbons, of the Academy of Natural Sciences of San Francisco, "claims priority of description of viviparous fish," in behalf of the gold-shimmering waters of California, and consequently, that State takes precedence over Louisiana. Agassiz, whose sounding (fishing) line has passed the living waters to the most ancient palæozoic rocks, says, in regard to the California viviparous fishes, that "a country which furnishes such novelties in our days, bids fair to enrich science with many other unexpected facts."

B. DOWLER, M. D.

ADVERTISEMENTS.

MEDICAL COLLEGE OF THE STATE OF SOUTH CAROLINA.

The Annual Course of Lectures in this Institution will commence on the first Monday in November, and terminate on the first Saturday in March, upon the following branches:

- Anatomy, by J. E. HOLBROOK, M. D.
- Surgery, by E. GEDDINGS, M. D.
- Institutes and Practice, by S. HENRY DICKSON, M. D.
- Physiology, by JAMES MOULTRIE, M. D.
- Materia Medica, by HENRY R. FROST, M. D.
- Obstetrics, by THOS. G. PRISLEAU, M. D.
- Chemistry, by C. U. SHEPARD, M. D.
- Demonstrator of Anatomy, F. T. MILES, M. D.
- Prosecutor to the Professor of Surgery, J. F. M. GEDDINGS, M. D.
- Clinical Instructor, D. J. CAIN, M. D., Physician to the Marine

Hospital and Clinical Instructor, lectures twice a week on the Diseases of that Institution.

H. W. DESAUSSURE, M. D., Physician to the Hospital of the Alms-House, at which lectures are delivered twice a week on Diseases, the diagnosis discriminated, and the student indoctrinated in their treatment.

Demonstrative Instruction in Medicine and Surgery, at the College Hospital, by the Professors of the Medical College.

The Anatomical Rooms will be opened in October and dissections conducted daily.

The Faculty of the College take pleasure in calling the attention of the friends of the Institute to its present prosperous condition—the class of the past year exceeding any former years.

They have been enabled by the liberality of the Legislature, at its last session, to make such alterations in extending and improving the college buildings as will promote materially the comfort of those in attendance on the Lectures.

The Anatomical Theatre has been enlarged and completely renovated, and such changes made as will secure free ventilation with a pleasant arrangement of the seats. They confidently believe that it will not suffer in comparison with any like structure in the United States; the edifice with its appurtenances being as commodious and attractive as any such establishment in our country. They have been enabled also to make considerable additions to the museum.

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HENRY R. FROST, M. D., Dean.

WORKS OF THE SYDENHAM SOCIETY, (London.)

The undersigned having received the appointment of "*Local Secretary*" of this Society, for the Southern States, would respectfully invite attention to the valuable *Standard Works* which it publishes each year, and furnishes to subscribers, at extremely low rates. The annual subscription is only *Five Dollars*, for which, usually *three*. but sometimes *four*, valuable volumes of the best Medical Works, are furnished. The works of several of the last years are still to be had.

For further information, apply to the undersigned, who will receive subscriptions, and have the works delivered with the utmost despatch. The attention of Medical Colleges is particularly invited to the rare opportunity here presented, of supplying their libraries with *Standard Works*, both ancient and modern.

E. D. FENNER, M. D., Local Sec'y, Sydenham Society,

December 16, 1851.

No. 5 Carondelet street,

THE PRACTICE OF MEDICINE. WHAT IS IT?

The regular Practitioner of Medicine has almost insurmountable difficulties to contend with, in the fact, that his prescriptions are necessarily little better than experiments, more particularly the Physician of the South and West from the fact that the purity and strength of Medicines vary so very materially.

It is well known that Laudanum is usually made from the most inferior and unsaleable pieces of Opium, which seldom if ever contain half the proper quantity of morphine.

Rhubarb is powdered from roots varying in price, from 20 cents to \$1 00 per pound; the lower prices, of course, more or less decayed and worm-eaten; and thus, being unsaleable, are powdered and colored to sell 'cheap.'

Instead of Jalap, large quantities of the "Spurious Jalap," and also of a variety known as "Jalap Tops," are sold at about one-quarter the price of the true Jalap.

For Peruvian Bark, at least a thousand pounds of the worthless, inert Carthagena and Maracaibo Bark, are ground and sold for every pound of the true Peruvian Calasaya Bark.

Now, *if* these are facts—and they certainly *are* well-known facts, and very serious facts too—how is it possible to prescribe with any certainty? Is prescribing with such Medicines anything else than experimenting.

And that they are facts, is abundantly proven by the Report of the Custom House Inspector of Drugs and Medicines, and also by the Report of the Special Commissioner to the Secretary of the Treasury, on Adulterated and Spurious Drugs; which Reports, shocking and humiliating as they are, do not show a tithe of the facts in regard to the wholesale adulteration of Medicines.

The Report of the U. S. Examiner says:

"Such sir, are the fruits, thus far, at this port, of the wise and eminently beneficial sanitary measures, so heartily approved of by every friend of humanity; that measure, too, which met from its inception, the open, determined and unremitting hostility of a God-forsaken portion of our trading community. From the moment the question was first agitated, and during the progress of the bill through Congress, intense excitement and ill-feeling was manifested among certain importers and speculators who had long made the murderous traffic not only a source of profit but of wealth, and no means were left untried by them calculated to defeat the measure.

"Most persons, we admit, can judge very correctly, by sight, of the quality of most articles of food and clothing; but where is the man who can, by simply looking at the almost countless number of medicinal preparations, chemical and otherwise, say whether they are pure or adulterated? or by looking at the various preparations of morphine, say whether they do or do not contain five, ten, or twenty per cent. of *amygdaline*? or can detect by sight, *corrosive sublimate*, *prepared chalk*, *gypsum*, and other impurities in calomel? or can by sight say whether blue-pill mass contains its full equivalent of mercury, or only one-fourth or less of the requisite quantity? or can say whether hydriodate or iodide of potash is pure, or is adulterated by the admixture of *sal acetosella*, *sup. tartrate* and *sulphate of potash*? or can in the same way detect *salicine*, *mannite*, *sulphate of barytes*, and *oxide of zinc* in sulphate of quinine? or can say whether Croton oil is, or is not, adulterated by the admixture of inert *fats* or whether it is, not, in fact, an entirely *fictional article*? or by looking at the powdered cinchona bark, say whether it is genuine powder of that species which affords the largest quantity of quinine and some cinchonine, or whether it contains thirty or fifty per cent. of the powdered *Maracaibo* or *Carthagena* bark, which affords but a trace of either of these important alkaloids, and is consequently worse than worthless for medicinal purposes; or whether it is not, in fact, composed entirely of the latter worthless variety? or can say, by looking at powdered rhubarb, whether it is of that prime quality which

affords from sixty to seventy per cent. of soluble matter, and some twelve per cent. of pure resin, or whether it is an article produced from the decayed and worthless root, (the color and smell having been heightened by artificial means,) which affords not to exceed fifteen per cent. of soluble matter and no resin at all?

"The several barks before alluded to, although differing in physical appearance, are those generally known in the trade as the red and yellow Maracaibo and Cartagena barks; and as they resemble the true officinal bark in color, they have long been used in a powdered state for the purpose of adulterating those barks, or sold to the unsuspecting as the genuine article. This fact shows very clearly why it has long been almost impossible to find on sale in the country, or even in our minor drug and apothecary establishments in town, one pound of the red or yellow cinchona bark, of the requisite strength and purity; or, in other words, that will afford, on analysis, a per centage of alkaloids corresponding with that produced by the genuine barks. Some samples that have been obtained afforded neither quinine nor cinchonine in any perceptible quantity! others less than one-fourth part of the alkaloids found in the true and pure barks; and, so upward, according to the extent of the adulteration. From the quality of samples that have been forwarded to me from a distance, I am satisfied that the country is filled with such base mixtures and worthless trash.

"The question now very naturally and properly comes up, will prime crude drugs, after having been powdered and prepared, be found on sale in town and country in as pure a condition as when imported; or, in other words, be found free from adulteration? I fear not, unless a strict watch is kept over the operations of the unprincipled portion of those among us whose mission it is 'to buy, sell, and get gain,' honestly if they can; if not, get it.

"It has heretofore been too frequently found that drugs become astonishingly reduced in strength and purity during their transition state from root, bark, gum, &c., to powder. Primo fresh drugs are no doubt (as well as worthless) sent to the drug-mill; but somehow or other, 'by falling into bad company,' I suppose, they are apt, during their stay, to lose their virtue; and as a matter of course are returned to their owner, and sent out into the market, with a character decidedly tarnished—an article fair to look upon, but whose touch is death. Badinage apart—the business of drug-grinding or powdering requires a searching and thorough reform.

"I have already alluded to the mysteries and trickery of the laboratory when in skillful but dishonest hands; but be assured, sir, its conjurations and diablerie if I may so express myself, in the preparation of adulterated chemical medicinal compounds, hardly exceed in ingenuity, deception, and iniquity, the frauds committed under the roof of the drug-mill.

"I have in my possession the voluntary confession of a drug-grinder, who has retired after amassing a fortune in the business; but I will not swell this report by entering at this time into an extended detail.

"This is a very important subject; and one, too, which the profession throughout the country, as well as the medical staff of the army and navy, whether on duty at a distance or at home in hospital practice, should lose no time in investigating; for how is it possible for the physician to do justice either to his patient or himself, however judicious and correct his prescriptions may be, as long as there is so much uncertainty as to the strength and purity of the curative agents he may recommend? I cannot but believe that many, very many valuable lives have been lost, owing to this lamentable condition of things."

Ought not the whole Profession to feel that their reputation, their success, and the lives of their patients should rest on a surer foundation than "guess work" or experiments?

Are not Pure Medicines far safer for the reputation of a Physician, and far more economical, taking success in view, than the cheap Medicines, which are entirely unreliable, even when they have any virtue?

THEREFORE, your earnest attention is requested to a branch of business intimately connected with success in the treatment of disease.

It is well known among dealers, and yet not generally known by the profession and the public, that pure and genuine medicines, particularly pure powdered drugs, from the first quality of gums and roots are scarcely procurable in this

country, and therefore physicians often prescribe medicines to meet certain indications in the disease of the patient, without obtaining the desired and expected beneficial result. To enumerate the articles of adulterated medicines that are daily sold in market would be to name almost the entire list of the *materia medica*. From the finer and more important chemicals and pharmaceutical preparations, such as Morphine, Quinine, Hydriodate Potass, Calomel, Blue Pill, &c. &c., down to the most common, and those of daily use, such as Cream Tartar, Rhubarb, Ipecac, &c., the adulterations are so adroitly made, that (without analyzation) even the closest inspection will fail to detect them. Quinine is often found largely adulterated (in some instances more than half) with manuite and other substances. Blue Mass and Calomel have been found much more than half adulteration. A gentleman at one time connected with an extensive manufacturing establishment, informed us, that just before he left England, the factory turned out more than four thousand pounds of Blue Pill, containing Barytes, instead of Mercury; and it was all designed for the American market.

Knowing this matter to be worthy the first and earnest consideration of the practitioner, we would respectfully ask attention to the accompanying

CIRCULAR.

We wish to call particular attention to our Extra Powders, which are pulverized from selected roots and gums of the very best quality; and when necessary, every piece is broken and examined under our own immediate supervision, and consequently possesses a purity hitherto unknown in this country, and a uniformity of action upon which the physician may rely with perfect confidence.

Our powdered Ipecac, extra, also will be found much superior to the usual article of commerce, being made from the true Brazilian Ipecacuanha, and consisting solely of the active outer coating of the root, carefully separated from the ligneous parts, and from all other inert matters. We pulverize only the true Mexican Jalap. In pulverizing Colocynth, extra, we retain only the active pulp of the apple, rejecting the seeds, which latter constitute the principal part of the weight of the fruit and are nearly inert. Powdered Rhubarb, extra, we prepare from the best East India Rhubarb, which is culled over with great care, every root being broken to detect any unsoundness. The Powdered Resin Guaiac, extra, is the pure Resin collected in tiers entirely from the dross and dirt usual to the ordinary Guaiac of commerce.

The Scammony also is powdered from an article differing in appearance and very much superior to what is usually sold for Aleppo Scammony. Blue Pill, bearing our label, will always contain one-third part of mercury, and our Hydrosublimed Calomel will be found to be of superior and regular quality.

Many of the roots from which the Extra Powders are prepared, are sifted and washed, and so many extraneous roots &c. are culled out, that the loss is often from one-quarter to one-third of the original weight, making, consequently, a very different article from the ordinary powders of commerce.

PARTICULAR NOTICE.

Having repeatedly heard that it is asserted that the superior quality of our "Extra Powders," and the unusual care in preparing them is all pretence, and that no one would take so much trouble and time, we merely state that we have on hand to show to any one doubting the facts, the stems culled from cubebs, the seeds taken from the pulp of the Colocynth, the woody, inert parts of the Ipecac, the extraneous roots culled from Pink, Senega, *Serpentaria* &c., and various other tangible proofs of the difference between our "Extra Powders" and the ordinary powders of the trade.

Although many Druggists denounce the Extra Medicines as all humbug, yet they have imitated our style of bottles and put in them the ordinary inferior powder of trade.

Powders can be imitated so easily by coloring, those wishing the pure would do well to compare them; such for instance as Rhubarb, Gum Arabic.

Care is taken to have these "Extra Medicines" not only pure, but of the best quality procurable.

When required, any of these articles can be obtained of us in their original state as some may desire a superior article to use unpowdered.

The life of the patient as well as the success and reputation of the physician and apothecary, depend so much upon the prompt action of the medicines used in sickness, that we feel every confidence in any effort to furnish them with pure and superior drugs will be fully appreciated.

To preserve the preparations from being injured by the air and moisture, they are generally put up in bottles and jars containing one pound each; and also in five and ten pound canisters. They should be kept as much as possible from the light.

It will be observed that the prices of these superior articles are necessarily higher than those of the ordinary kind; and physicians and merchants at a distance, when they wish this quality sent them, are particularly requested to write for the 'Extra Medicines' of Philip Schiffelin, Haines & Co., Chemists and Druggists, New York.

EXTRA POWDERS.

Aromatic Powder, U S P	Pulv. Blood Root	Pulv. Lobelia
Pulv. African Pepper	Bitter Root	“ Seeds
Alum	Black Root	Mandrake
Allspice	Borax	Marsh Rosemary
Aloes, Cape	Buchu	Mace
“ Socot	Caraway Seeds	Mezereon
Assafœtida	Cantharides	Nux Vomica
Antum Ref. Blk. Sulph	Banella Alba	Nitrate Potass, pure
Agustura Bark	Cardamon (Seeds)	Nutmegs
Ariseeds	Cicuta	Nutgalls
Elecampane	Cranesbill	Pleurisy Root
Digitalis	Cloves	Rhubarb, Turkey
Ergot	Cinnamon, ordinary	“ East India
“ 1 oz. phials	“ Ceylon	Rhatany Root
Extract Colocynth C'd	Cohosh, Black	Sarsaparilla, Para
“ Licorice, Calab	Columbo	Sage
Fœnugrek Seed	Colchicum Root	Summer Savory
Gentian Root	“ Seed	Sweet Majoram
Ginger, Jamaica	Colocynth Pulp	Sabina
Golden Seal	Cubebs	Senna, Alex.
Opium, Turkey	Guaiac Resin f. Tears	Seneka Root
Orange Peel	Gum Arabic, Turkey	Scammony, Alp. No. 1
Orric Root	“ Gamboge	“ Virgin
Pepper, Cayenne, A.	“ Mastich	“ “
Pepper, Black	“ Myrrh	“ “ 1 oz.v.
Peruvian Bark, Loxa	“ Tragacanth	Snakeroot, Virginia
“ “ Red	Hellebore, White	Squills
“ “ Calasaya	Hyoscyamus	Sulphate Copper
Prickly Ash Bark	Ipecac	“ Iron
Pink Root	Ipecac and Opium	“ Potass
Bayberry Bark	Jalap	Uva Ursi
Belladonna	Kino, True	Valerian, English

BOTTLES AND CANS AT COST,

When put up in Quarter and Half-Pound Bottles additional ten cents per pound.

As many of the Gums, &c., are of unusual purity, for instance Guaiac, Aloes, Assafœtida, &c. they are very liable to run together and become solid. Even the ordinary common Gums of commerce are so apt to run together that Drug-Ginders usually grind with them some woody substance, such as Licorice root, Gentian root, &c.

Some roots that are rich in resin, such as Rhubarb, Jalap, &c., are also apt to agglomerate. The Extra Powders being perfectly pure and free from all foreign substances, are therefore more liable to become solid than the common qualities, but where they do so, we will replace them with others that are freshly powdered.

PURE CHEMICALS PREPARED AT OUR LABORATORY.

Ammonia, Aqua	Mercury, Bin Iodide	Spirits Nitri Dulc
“ Liquor Fort	“ “ “ crystals	Syrup Iodide Iron
Ammoniated Alcohol	“ “ Protoiodide	Sulphuret Potassa
Argent Nitras	Morphine Sulphate	Vallets Ferruginous Mass
“ Crystals	“ Acetate	Zinci Acetas
Lunar Caustic, Nos. 1, 2, 3	Granvilles' Lotion	Zinci Sulphas
Ferri Carb, precipitated	Gallic Acid	Zinci Chloride
“ “ Sulphas, pure	Oil Capaiva	Chloride Soda, Labarraque
“ “ “ Exsicato	Oil Cubebs	Confection Rosas
Iodide Arsenic	Precipitated Chalk	Confection Senna
“ Lead	Prussic Acid	Blue Pill Mass
“ “ Crystals	Potasse Nitras, pure	Ung. Mercurial
“ Iron	Strychnine	

Many of these chemicals differ decidedly in appearance from the chemicals of commerce as well as in their valuable properties, and bear externally the style of our manufacture, being in crystals and having the peculiar crystalline characteristic of each article; while those usually sold are in the powdered state, in which form it is difficult to judge of purity.

The Chrystals of the Iodines of Lead and Mercury, and the Sulphate of Morphine more particularly, are much esteemed by all who have tried them, and our Soluble Precipitate Carb. Iron, Nitrate of Silver, Extra Blue Pill Mass, hold the first rank among choice chemicals.

Our Hydro Alcoholic Extracts are prepared by steam process, in a patent Vacuum Apparatus, and at a low temperature of nearly an hundred degrees below the boiling point, so that the valuable properties of the plant are preserved uninjured, and at the same time a consistence, color and taste are obtained, which are sufficient evidence of their superiority. Among them are

Extract of Belladonna	Extract of Digitalis	Extract of Quassia
Butternut	Gentian, opt.	Rhubarb
Buchu	“ Ordinary	Sarsaparilla Simplex
Bloodroot	Hyoscyamus	“ Compound
Boneset	Hops	Para Sars. Alcoholic
Conium	Jalap	Hon. Sars. Alcoholic
Chamomile	Lupuline	Stramonium
Colocynth, Ordinary	Mandrake	“ Seeds
Colocynth, Opt.	Nux Vomica	English Valerian
Dandelion, Alcoholic	Opium	Dutch “
“ Inspissated	Pinkroot	

Observe that it is a well established fact that many plants which in their native conditions are possessed of very active medicinal properties, lose by cultivation their peculiar characteristics and become nearly inert. Some vegetable which belong to poisonous families of plants, by cultivation, are made innocuous, and are freely eaten as food, as the potatoe, parsnip, cellery, &c., &c.

To insure to our extracts, such as Hyoscyamus, Belladonna, Conium, &c., all the active therapeutical effects which they should possess, we take pains to procure the herbs, from which they are prepared, from places where they are indigenous to the soil, viz: from Germany, France, England, India, &c., &c., and they are consequently much superior to extracts made from the cultivated plants of American growth.

The difference is very apparant between these extracts and those of any other make, not only in appearance but also far more in flavor and in the peculiar aroma of the herbs; so much so, that even without the labels they are easily distinguishable by the taste and smell. These are facts of so much importance, that they should be remembered by all who have any occasion to use medicinal vegetable extracts.

These Extracts together with our pure Chemicals and Extra Powders have received the unqualified approbation of various Medical Associations, and of Phy-

sicians and Apothecaries, and have elicited many complimentary letters on their good qualities. The following token of approbation is from the Ohio Medical Convention:

“Resolved, That the thanks of the Medical Professors are due to the house of Philip Schieffelin & Co., of New York, for their efforts to furnish the community with pure Drugs; and we recommend their Extra Medicines to the confidence of Dealers and Practitioners.”

And also from the following eminent practitioners :

PHILIP SCHIEFFELIN, ESQ. :

Dear Sir—It is with pleasure I add my commendation to that of other physicians as regards the superior quality of the Extracts and Chemicals, prepared and sold by your house. The extraordinary care and assiduity shown by you in obtaining and putting up Drugs free from adulterations, and Chemicals prepared perfectly pure, deserve the highest commendation, not only from the physicians, but also from the public, whose safety is eminently concerned in the employment of articles of the *Materia Medica*, free from inert or injurious additions. Although the public generally has been warned by the publications of “Inspectors of Drugs,” and by the medical press, that such adulterations have been made by dishonest vendors and speculators, still the imposition continues to be exercised, and, in many cases, almost without reserve. The Cod Liver Oil, manufactured for your house, I find far superior to any other, being, I believe, perfectly pure, and yielding to the tests the absence of the oils generally employed in the adulteration of this really valuable article. My patients also find it far less disagreeable, and more readily digestible than the kinds I have formerly prescribed.

Wishing you the success you so fully deserve. I remain yours, very truly,

H. P. DEWEES.

SEAMANS' RETREAT HOSPITAL, STATEN ISLAND, June 21st, 1850.

Messrs. P. SCHIEFFELIN & Co. :

Gentlemen—Having used your Drugs and Chemicals in this Institution, as well as in private practice, for the last twelve years, it affords me much pleasure to bear testimony as to their quality and purity; for without these, no physician, be he ever so skilful, can calculate the result to his patients.

Trusting that the public, as well as the profession, will appreciate your endeavors to furnish them with pure articles in medicine. I remain, yours respectfully,

JAS. R. BOARDMAN, M. D.,
Resident Physician and Surgeon.

Their superior efficacy, in all prescriptions, will be at once apparent to every one who reflects upon the difficulty oftentimes experienced in the administration of the common Drugs of commerce, and the *loss of life* and of reputation, consequent upon the use of inert remedies.

COD LIVER OIL.—The great and increasing demand for Cod Liver Oil, and the difficulty of procuring the oil in its pure state, and such as we can guarantee to our customers, have induced us to send our agent to the fisheries for the purpose of having the best article that can be offered in the market. This article will also bear our label when put up in bottles, and be warranted pure, when ordered in bulk.

TO DRUGGISTS.—In addition to the Extra Medicines, we also keep a large and well assorted stock of the ordinary Drugs and Medicines of commerce, carefully selected, and the best that can be procured. Our Essential Oil, and other Liquids, we obtain from the most reliable sources, and are submitted to every known test for impurities; and we avoid purchasing any kind of Drugs in the powdered state.

Our arrangements and facilities are such, that we can offer inducements to dealers, which must influence all, who, not only like to have a fair equivalent for their money, but at the same time to have goods that are what they purport to be, and such as will bear the strictest examination and analysis.

PURE CHLOROFORM.—Much of the Chloroform of commerce being very impure, and its use having in some cases been attended with unpleasant consequences, we have been repeatedly urged to make some at our Laboratory, of a quality superior to that generally for sale in this market. We would, therefore, inform the Medical Profession that we have prepared an article, the purity of which can be implicitly relied on.

NITRATE OF SILVER can also be obtained from us perfectly *pure*, either in crystals or crystals, manufactured at our Laboratory.

MORPHINE.—Our Morphine having acquired a reputation superior to any

other, those who have occasion to use the article will be satisfied of its excellence by giving it a trial.

☞ We also prepare the SYRUP OF IODIDE OF IRON U. S. P., now so highly esteemed as a remedy in Scrofulous Complaints, and also Dupasquier's Syr. Iod. Iron, which is a much milder preparation, and better adapted for Ladies and Children. These articles [which it is the greatest consequence to physicians to have of reliable quality,] are, with our other preparations, offered to the notice of those desiring pure Drugs and Chemicals.

☞ N. B —Letters directed to "Schieffelin & Co.," intended for us, have frequently gone to other houses, there being several firms of that name; therefore please be careful to write our names in full.

PHILIP SCHIEFFELIN, HAINES & CO.,

September, 1853-ly.

107 Water Street, New York.

ORTHOPÆDIC INSTITUTION,

FOR THE CURE OF DEFORMITIES,

Nos. 457, 459 and 461 Pacific Street, South Brooklyn,

Afford to in-door patients, afflicted exclusively with bodily deformities, domestic accommodation, Orthopædic Apparatus, and a superior and attentive medical treatment hardly procurable at a private home.

Office hours, before 11 o'clock, A. M., for private patients only. The poor attended gratuitously on Tuesdays and Fridays, between 3 and 6 o'clock P. M., when clinical instruction will be given to medical students, who may favor the institution with their presence.

The profession at large is most respectfully invited to honor the institution with their attention and visits. Physicians of the Institution:

L. BAUER, M. D, Physician and Surgeon, (Berlin,) formerly Surgeon of the Royal Orthopædic Institution in Manchester, Great Britain; Member of the Royal College of Surgeons, of England; Fellow of the Medical Society of London, etc.

R. BARTHELMESS, M. D, Physician and Surgeon, (Wurzburg, Bavaria,) formerly Assistant Physician of the Lying-In Hospital of the University of Wurzburg, and of the City Hospital, Nuremburg, etc.

UTERINE COMPLAINTS, AND DISEASES OF FEMALES.

From fifteen years experience specially devoted to the study and treatment of Uterine Diseases in all their various forms, and complaints peculiar to Females, the undersigned has been induced to establish an institution for the purpose of combining every possible advantage and means requisite for successful treatment and cure of this intricate class of disorders. Patients from abroad can be accommodated with board, and every facility desired, to suit their various cases.

Lying-in patients also received, and offered the best of professional treatment and nursing.

If medical gentlemen at a distance should recommend patients, they will confer a favor by sending a statement of the disease and treatment.

A. STONE, M. D.

Brooklyn, New York State, No. 343 Gold street.

UNIVERSITY OF LOUISIANA.

MEDICAL FACULTY:

JAMES JONES, M. D.,
Professor of the Theory and Practice of Medicine.

WARREN STONE, M. D.,
Professor of Surgery.

J. L. RIDDELL, M. D.,
Professor of Chemistry.

A. H. CENAS, M. D.,
*Professor of Obstetrics, and of the Diseases of
Women and Children.*

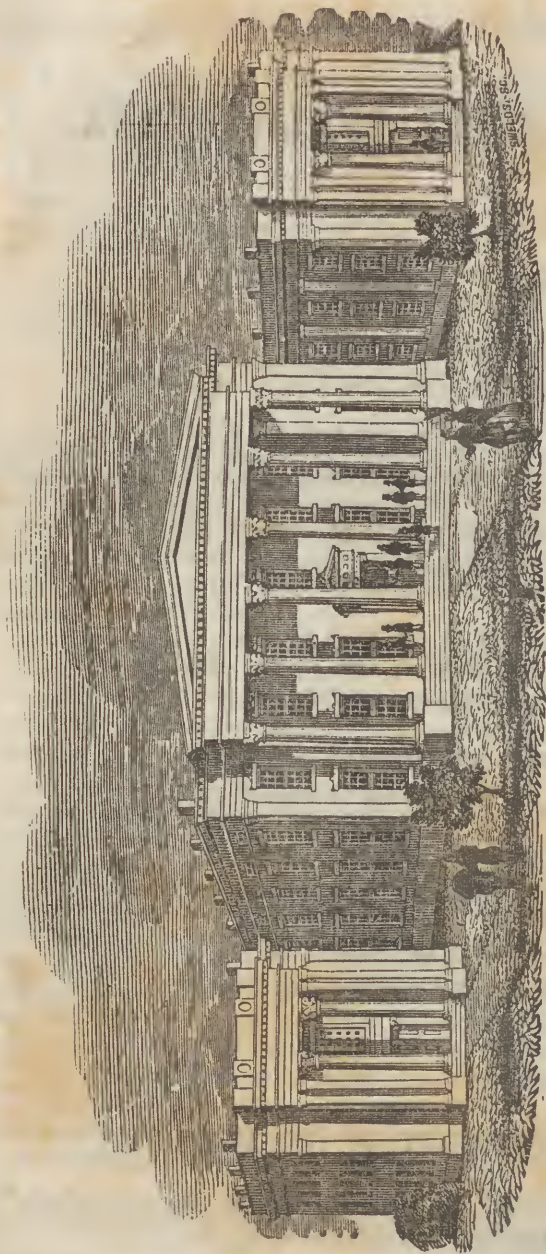
A. J. WEDDERBURN, M. D.,
Professor of Anatomy.

GUSTAVUS A. NOTT, M. D.,
Professor of Materia Medica and Therapeutics.

THOMAS HUNT, M. D.,
Professor of Physiology and Pathology.

DEMONSTRATORS OF ANATOMY:

CORNELIUS C. BEARD, M. D.,
SAMUEL P. CHOPPIN, M. D.



UNIVERSITY OF LOUISIANA.

The centre building is occupied by the Medical Department—its dimensions are one hundred feet front, by one hundred and four feet in depth—it contains three large Lecture Rooms, each of which will accommodate more than six hundred persons, a large Museum, Dissecting Rooms, &c., &c., &c.

UNIVERSITY OF LOUISIANA.

MEDICAL DEPARTMENT.

The regular Annual Course of Lectures in this Department will commence on Monday the 13th of November, 1854, and terminate in March, 1855.

In 1853, '54 there were one hundred and ninety-one Matriculants, and fifty-one Graduates in the Department.

The Students of the Class, during the last session, were from Louisiana, Mississippi, Alabama, Texas, Arkansas, Tennessee, Kentucky, Missouri, Virginia, North Carolina, Georgia, Florida, and France.

The Faculty announce to the Public the continued prosperity of the Medical College of Louisiana.

More than nineteen hundred names are on the Register of Matriculants, and many of the Graduates have acquired honorable distinction as Surgeons and Physicians.

The College is endowed by the State, and its usefulness has been extended by judicious appropriations of the Legislature.

MUSEUM.

The Museum of Anatomy is very extensive. The preparations are chiefly works of scientific artists in England, France, and Italy.

In Human and Comparative Anatomy, there is—

1st.—A collection of Muscular preparations, from the Academy of Anatomy, at Florence.

They represent more than three hundred and fifty separate dissections.

2d.—Thibert's Tableaux of the Microscopic Anatomy of the Tissues.

3d.—Auzou's Cabinet of Human and Comparative Anatomy.

4th.—A Cabinet of Human Bones, and a collection in Comparative Osteology.

5th.—Preparations in Wax, (exhibiting exquisite skill in art, and knowledge in science,) representing the Anatomy of the Viscera, of the Nervous and Vascular Systems, and of every Organ in the Human Body.

The Pathological Department is enriched by models from England and France, which represent Diseases of the Eye, and Diseases of the Skin; and, by a great number of specimens of Urinary and Biliary Calculi, obtained from Dupuytren's Museum, at Paris; from the College of Surgeons, London; and from amongst ourselves.

The models of the Diseases of the Skin were made by Mr. Towne, of Guy's Hospital, London. They are perfect, and each disease can be studied with their aid almost as well as in nature. No other Museum contains these models except Guy's Hospital.

Besides the collections, brought from Europe, the Museum contains Anatomical preparations made by Members of the Faculty and others presented by their scientific brethren.

The Pathological collection of Bones is excellent.

The other specimens of Pathological Anatomy are highly interesting. Every year this Department is rendered more useful by additions derived from the enlightened friends of the Institution.

CLINICAL INSTRUCTION.

The following extract is taken from a Report of the Board of Administrators of the Charity Hospital :

"This institution, the largest Hospital for the reception and treatment of patients in America, presents an admirable field for the study of disease, and is a valuable acquisition to our Medical College, the Professors in which are allowed every facility they desire during the course of lectures. We have ten resident students in the Hospital, who are furnished board and lodging, and are required to execute the orders of the Visiting Physicians and Surgeons. We venture to assert, that those of them who remain their full period of two years, go forth with a better stock of practical knowledge and experience than any young physicians in the country."

The number of Medical cases, treated during the last year in the Hospital, was about 11,259 ; the Surgical patients numbered 2,000, and the Obstetrical cases, and those of special Diseases of Women and Children, numbered 500.

The College affords opportunities to the students unsurpassed by any other institution in the world. The Act which established the University of Louisiana, gave the Professors of the Medical Department the use of the Charity Hospital as a school of practical instruction : it is during the session of the school, therefore, in charge of the Professors.

There are about 800 cases usually in the wards of the Charity Hospital. The Professors visit every morning between 8 and 10 o'clock, the Medical, Surgical, and Obstetrical wards. Hence, a wide field is open for the practical study of diseases and their scientific treatment.

Members of the Classes of the Medical Department have gratuitous and free admission to the wards of the Hospital ; they also are permitted to attend post mortem examinations.

Attendance on cases of labor in the Obstetrical wards is provided by the Professor of Obstetrics, from among the candidates for Graduation.

The Professor of Surgery performs Operations and Dressings in the presence of the students, in the amphitheatre of the Hospital ; and here, too, lectures are delivered on Wednesday and Saturday of every week, on Clinical Medicine and Surgery, and on special Pathological Anatomy.

LECTURES.

- 1.—The Professor of Anatomy, lectures on five days of every week during the session.
- 2.—The Professor of Physiology and Pathology, lectures on five days of every week.
- 3.—The Professor of the Theory and Practice of Medicine, lectures on six days of every week.
- 4.—The Professor of Surgery, lectures on six days of every week.
- 5.—The Professor of Obstetrics, lectures on four days of every week.
- 6.—The Professor of Materia Medica and Therapeutics, lectures on four days of every week.
- 7.—The Professor of Chemistry, lectures on four days of every week.

The Professors have every thing deemed necessary to aid them in teaching the various branches of Medical Science, viz : 1.—Chemical and Philosophical Apparatus, of modern style. 2.—Specimens of Materia Medica and Chemical products. 3.—Surgical Instruments, from Charrière, a complete set. 4.—Paintings, plates, models, drawings, books, and special apparatus.

The Faculty supply, almost gratuitously, subjects for dissection. The supply exceeds the demand of the Class.

The Medical College of Louisiana affords to the student means and opportunities of acquiring information in regard to the causes, nature, and treatment of the local diseases peculiar to our climate, which he cannot obtain in any Northern, Southern, or Western College in our country.

CANDIDATES FOR THE DEGREE OF DOCTOR OF MEDICINE.


- 1.—The Candidate must be twenty-one years of age, of moral character, and must have studied medicine three years.
- 2.—He must have attended two full Courses of Lectures—the last of which must have been in this Institution.
- 3.—He must write a Thesis on a Medical subject, and present it to the Dean one month before the close of the session.
- 4.—He must be examined by the Faculty.

The rooms for Practical Anatomy will be open from the third Monday in October to the 1st of April.

The Dissecting Rooms will be open during the whole day; and from 7 P. M. to 10 P. M. it is the duty of the Demonstrators to be constantly in attendance, for the purpose of instructing the students dissecting.

TERMS:

FOR THE TICKET OF EACH PROFESSOR.....	\$15 00
FOR THE TICKET OF PRACTICAL ANATOMY.....	10 00
MATRICULATION	5 00
DIPLOMA	30 00

 FEES FOR TICKETS REQUIRED IN ADVANCE.

Boarding for Students is as cheap in New Orleans as in any other large city of the Union. Graduates of all respectable schools will be admitted to the course without charge except for Matriculation.

Students who desire further information, will address themselves to the Dean.

THOMAS HUNT, M. D., DEAN.

NEW ORLEANS, July 18, 1854.

TEXT BOOKS:

WILSON'S ANATOMY,	FERGUSON'S OR DRUITT'S SURGERY,
CARPENTER'S PHYSIOLOGY,	FOWNES' CHEMISTRY,
WATSON'S PRACTICE OF PHYSIC,	CHURCHILL'S MIDWIFERY.
UNITED STATES DISPENSATORY,	

[COPY.]
YEARLY REPORT OF DISEASES FOR 1853.

DISEASES.	Admissions		Discharges.	Deaths.	DISEASES.	Admissions		Discharges.	Deaths.
	Color.					White.	Black.		
	White.	Black.							
Abscess.....	101		113		Am't brought forward	1,400	23	1,230	214
Do. Urinary.....	1			1	Cirrhosis of Liver.....	7			6
Do. of Liver.....	4	1	1	7	Cynanche Parotidea.....	4		2	2
Albuminuria.....	3		3	2	Cataract.....	3			
Anguioia.....					Debility.....	71		81	7
Amourisio.....	2		1		Delirium Tremens.....	162		114	52
Abortion.....	5		7	1	Dyspepsia.....	22		25	3
Arachnitis.....			1		Dysmenorrhœa.....	9		9	
Amputation of leg.....			5	1	Dentition.....	1		1	
Do. of thigh.....			2	1	Dropsy.....	43		22	18
Do. of arm.....			2		Diarrhœa.....	656	11	520	117
Do. of fingers.....			4		Dysentery.....	250		103	110
Anthrithis.....	7		14		Dislocation of humerus..	9		11	
Anus, excoriation of.....	1		1		Do. of hip.....	1		2	
Apoplexy.....	9		2	7	Do. of cervical	1			1
Anæmia.....	30		24	4	vertebræ.....				
Ascites.....	27	1	18	14	Dislocation of radius....	2		1	
Amenorrhœa.....	21		19	1	Do. of cartilage	4		4	
Amputation of toe.....	1		1		of knæ.....				
Anchylosis.....	2		3		Deafness.....	4		3	
Anasarca.....	13		13	2	Enteritis.....	25		30	3
Age, "old".....	3	2		3	Eczema.....	21		26	
Asthma.....	1		1		Erysipclas.....	52	1	53	5
Aneurism Popliteal.....	1		1		Exposure, effects of.....	2		1	1
Anthrax.....	6		5		Enlargement of spleen..	2		1	
Adenitis.....	3		2		Echthyma.....	3			2
Ascarides.....	1			1	Enteralgia.....	1			
Aphtha.....	1		1		Encephalitis.....	1			2
Artropathie.....	2		3		Epydidimitis.....	1			
Bronchitis.....	211	4	219	12	Epistaxis.....	4		2	
Burn.....	32	3	27	6	Empysema of lungs.....	4		2	1
Bite of a dog.....	1		1		Epilepsy.....	18		13	6
Blepharitis.....	30		13		Ectropium.....			2	
Blenorrhagia.....	92		112		Endo-carditis.....	1			
Blindness.....	1				Exostosis.....	1		1	
Catarrh.....	43	2	41	3	Erythemia.....	2		5	
Constipation.....	76		77		Elephanthiasis Græceum	1		1	
Congestion of brain.....	13		7	9	Do. Arabica.....	1			
Concussion of brain.....	8	1	1	1	Exhaustion nervous.....	3			2
Conjunctivitis.....	45		50		Enlargement of uterus..	1			
Do. Granular.....	13		7		Eruption vesicular.....	1		1	
Cephalagia.....	7	1	5	4	Ecchymosis of the eye..	1		1	
Contusion.....	194	1	197		Fever, intermitten.....	2,631	4	2,462	6
Colica Pictonum.....	49		48	2	Do. remittent.....	372	1	393	12
Colic.....	51		47		Do. typhus.....	213		182	59
Cornea, opacity of.....	3		3		Do. typhoid.....	162	5	155	57
Croup.....	2		2		Do. bilious.....	57	2	67	2
Cough, whooping.....	2		2		Do. congestive.....	23		5	18
Cystitis.....	7		5		Do. continued.....	78		78	3
Choroiditis.....	2		2		Do. ephemerical.....	73	2	66	
Congestion of Retina.....	3		2		Do. bilious remittent	10		4	2
Corobritis.....	4			1	Do. pernicious inter't	30		7	20
Cornea Ulcerated.....	8		9		Do. scarlet.....	8		6	2
Carics.....	10		10		Do. puerperal.....				2
Chlorosis.....	17		20		Do. inflammatory.....	3		2	
Congestive Chills.....	2		1		Do. typhoid remitt'nt	5			6
Cholera Morbus.....	13		7	4	Do. hectic.....	1			2
Do. Asiatica.....	187	7	44	115	Do. yellow.....	3,212	7	1,427	1890
Do. Infantum.....	2		2	2	Fracture of clavicle.....	22		19	
Coxalgia.....	2		6		Do. of humerus.....	11		6	1
Carbuncle.....	13		10		Do. of scull.....	5		1	3
Chorea.....	1		1		Do. of leg.....	24	1	27	1
Convulsions.....	3		2	1	Do. of thigh.....	15		16	2
Do. Puerperal.....				1	Do. of fore arm.....	13	1	7	
Cancer of liver.....	1			1	Do. of metatarses..	1		1	
Do. of neck.....	1			1	Do. of ribs.....	12	1	8	1
Do. of breast.....	2		1	1	Do. of maxillary	6		6	1
Do. of womb.....	1			1	bone.....				
Do. of stomach.....	5			5	Fracture of scapula.....	1		1	
Do. of heart.....			1		Do. of fingers.....	1		1	
Carried forward....	1,400	23	1,230	214	Carried forward.....	9,784	59	7,214	2,642

[COPY.]
YEARLY REPORT OF DISEASES FOR 1853.—CONTINUED.

DISEASES.	Admissions				DISEASES.	Admissions			
	Color.		Discharges.	Deaths.		Color.		Discharges.	Deaths.
	White.	Black.				White.	Black.		
Am't brought forward	9 784	59	7 214	2642	Am't brought forward	10.325	64	7.672	2694
Fracture of sternum	1		1		Keratitis	8		7	
Do. of glenoid cavity of scapula	1				Do. ulcerated	2		1	
Fracture of patella	3		1		Laryngitis	8		9	
Do. of malleoles	1		1		Lumbago	7		8	
Do. of wrist			2		Leucorrhœa	11		10	
Furunculi	8		10		Luxation of Vertebrae			1	
Fissures of lips	1		2		Lipoma	1		1	
Fistula perinaeum	1		4		Lupus	1		1	
Do. in uno	13		13		Laceration of Urethra	1			
Do. verigo vaginalis	5	1	2		Labor Prematured	1		1	
Do. recto vaginalis	1		2		Marasmus	11		2	13
Gangrena		1	1		Mania-a-potu	14		11	2
Do. of lungs	4		4		Meningitis	7	1	2	6
Do. hospital	2		1		Mania puerperal	2			1
Gastro hepatitis	1		1		Menstruation Vicarious	1		1	
Gastralgia	17		18		Measles	29	6	32	1
Gastritis	91		85	4	Menorrhagia	5		5	
Gastro enteritis	9		4	4	Metritis	3		4	
Gout	1		1		Metro Peritonitis	2		1	
Gastric disorder	16		0		Mamma, supuration of	1			
Gun boil	1		1		Morbus Periduculosus	4		4	
Gastro-duo-dentitis	2		1		Myelitis			1	
Hygroma	1		1		Nothing	299		299	
Hepatitis	35		30	6	Neuralgia	25		25	
Hysteritis	2		3		Nostalgia	2		1	
Hydrothorax	3		1	1	Necrosis	4		3	
Hæmoptysis	7		4	2	Nephritis	1		2	
Heart, disease of	33		11	12	Ophthalmia	7		5	
Hæmatemesis	1		2		Do. granular	2		1	
Hæmaturæa	1				Do. strumous	4		1	
Heart, palpitation nerv's	6		5		Do. catarrhal	33		57	
Hydrocele	5		5		Do. purulent	11		10	
Herpes-zoster	1		1		Otitis	4		2	
Hernia inguinal	9		5		Orchitis Tuberculosis	2		2	
Do. serotal	6		11		Oedema of lungs	1			1
Hysteria	7		5		Do. of legs	9		10	
Hystericalgia			1		Opacity of Cornea	4		11	
Hæmorrhoides	20		22		Onyxis	1		1	
Hæmorrhage passive	2				Orchitis	33	1	31	
Hypertrophy of liver	2				Ostitis	3		3	
Hemiplegia	2	1	2		Onanism	2			
Hæmorrhage purpura	5		5	1	Paraplegia	1		3	
Hypertrophy of spleen	2		5		Pertussis	4		5	
Hæmorrhage of scrotum	1			1	Phthisis Pulmonatis	318	5	165	199
Hemicrania	3		4		Pregnancy	209		83	
Hydro-pneumo-thorax				1	Parturition	3		181	2
Hypochondriasis	2				Pneumonia	17	2	49	25
Influenza	11	1	9		Do. typhoid	4	1		3
Itch	18		24		Pleuro Pneumonia	16	1	8	9
Injury of brain	1		1		Pleuritis	62	3	58	4
Do. of spine	2		3	1	Pleurodinia	11		10	
Do. of spinal marrow	2		2		Paralysis	31		19	4
Irritation of spine	4		1		Paraplegia	1			
Iritis	16		12		Pneumo Thorax	2			1
Iritis syphilitic	4		2		Pnnus	3		2	
Do vascular keratitis	1		1		Ptyalismus	19		15	2
Ischuria vesicalis	1		1		Paronychia	27	1	24	
Insanity	20		14		Prurigo	8		5	
Infection Purulent				1	Phimosis	1		3	
Intemperance	88		80	7	Pericarditis	3	1	4	1
Indigestion	7	1	6		Peritonitis	10		6	5
Impetigo	1		2		Paraphimosis	3		3	
Irido Choroiditis	1				Phlegmon of eye	1		1	
Do. keratitis	1				Phlebitis	3		3	
Induratum of Mamma			1		Psoriasis	3		1	
Incontinence of Urine	1		1		Pustules Malignant	1		1	
Intoxication of Chloroform				1	Pemphigus	1		1	
Jaundice	27		25	1	Do. of face	1		1	
Carried forward	10.325	64	7 672	2694	Parotitis	7		8	
					Carried forward	11.661	86	8.898	2973

[copy.]
YEARLY REPORT OF DISEASES FOR 1853.—CONCLUDED.

DISEASES.	Admissions		Discharges.	Deaths.	DISEASES.	Admissions		Discharges.	Deaths.
	Color.					White.	Black.		
	White.	Black.							
Am't brought forward	11,661	86	8,898	2973	Am't brought forward	12,813	96	9,866	3003
Periostitis	4				Torticollis	1		1	
Pharyngitis	1				Tonsillitis	29		32	
Prostatitis	1		1		Tonsils, enlargement of	1		1	
Prolapsus of Rectum	1				Tumour Benignant	6		14	
Do. of Uteri	11		10		Tubes mesenterica	2			3
Do. of Anus	1		1		Temulentia	5		5	
Retention of Urine	7		6		Tinea Capitis	5		5	
Rupia	2		1		Tetanus idiopathic	3		2	1
Do. syphilitic	4		3		Do. traumatic	7	1	2	3
Retroversio Uteri	1		1		Tremor mercurial	1		1	
Rheumatism	469	5	455	2	Taenia solium	1		1	
Syphilis primitive	380	2	361	4	Tyroiditis	1			
Do. secondary	55	1	17	3	Ulcer	362	2	390	
Do. tertiary	18		8	3	Uncertain	120	3	87	43
Sprain	43	1	38		Ulcer Phagedenic	4		2	
Scald	5	1	7	1	Urticaria	1		1	
Scrofula	23		17	1	Ulceration of the ear	1		1	
Sun Stroke	20		8	12	Do. of bowels	2			2
Stricture of Oesophagus	1				Incised wound	99		97	1
Do. of Urethra	17		18	1	Lacerated wound	49	2	55	
Suppuration of parotid gland			1		Penetrating wound	15		12	5
Spina Ventosa			1		Contused wound	70		72	
Scorbutus	70		1	1	Gunshot wound	14		9	1
Scleræma	1				Punctured wound	7		5	2
Swelling of clavicle gland	1				Poisoned wound	3	1	2	
Sarcoma Medullary	1				Worms	1		1	
Sciatica	7		10		Varicella	1			
Sclerotitis	1		1		Varices	7	1	8	
Suppression of Urine	1				Variola	5		5	
Splenitis	1				Variolid	8		7	
Softening of the Brain	1				Venigitis	11		9	
Staphyloma	3		2	1	Vertigo	3		2	
Carried forward	12,813	96	9,866	3003	Total	13,653	106	10,733	3164

REPORT FROM THE OBSTETRICAL WARD, SHOWING THE NUMBER OF BIRTHS WHICH OCCURRED IN THAT DEPARTMENT:

Males..... 61 || Still Born..... 24
 Females..... 74 ||
 Total..... 159

(Signed)

H. VANDERLINDEN, CLERK.



THE
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No. 4.

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JANUARY 1, 1855.

BOOKS AND PAMPHLETS RECEIVED.

- I.—*On the Nature, Signs, and Treatment of Childbed Fevers*; in a series of letters, addressed to the students of his class: by CHARLES D. MEIGS, M. D., Professor of Midwifery, and the Diseases of Women, in Jefferson Medical College, Philadelphia, &c., &c. Philadelphia:

- Blanchard & Lea, 1854, pp. 362. From Mr. T. L. White, Bookseller, 105 Canal street.
- II.—*Human Physiology*: designed for Colleges and the higher classes in Schools, and for general reading: by WORTHINGTON HOOKER, M. D., Professor of the Theory and Practice of Medicine, in Yale College, author of "Physician and Patient." Illustrated by nearly 200 engravings. New York: Farmer, Brace & Co., 1854, pp. 389.
- III.—*Résumé de Recherches Cliniques sur la Fièvre Continue la Dysenterie, la Pleurésie Chronique et sur les variations du ton dans les sous fournis par la Percussion et par l'Auscultation.* Par AUSTIN FLINT, M. D., Professeur de Médecine Théorique et Pratique à l'Université de Louisville, Etat de Kentucky, États-Unis d'Amérique. Paris: Hector Bossange et fils, J. B. Baillière, 1854, pp. 112.
- IV.—*Insanity in Italy*: by JOHN M. GALT, M. D., Superintendent and Physician of the Eastern Asylum of Virginia. 1854, pp. 19.
- V.—*Report of the Select Committee of the Senate of the United States on the Sickness and Mortality on board Emigrant ships, August 2d, 1854, Washington, pp. 147.* From Hon. John Slidell, of the U. S. Senate.
- VI.—*The Transactions of the American Medical Association*, Vol. VII, 1854. New York: Charles B. Norton, pp. 668.
- VII.—*A Manual of Microscopical Anatomy of the Human Body.* By ALBERT KOLLIKER, Professor of Anatomy and Physiology, in Wurtemberg. Translated from the German, by GEORGE BUSK, F. R. S., and THOMAS HUXLEY, F. R. S; edited, with notes and additions, by J. DA COSTA, M. D., Member of the Academy of Natural Sciences, Philadelphia: with three hundred and thirteen wood-cuts, in one volume, octavo, pp. 802. Philadelphia: Lippincott, Grambo & Co., 1854. From Mr. T. L. White, Bookseller, 105 Canal street.
- VIII.—*Report on the Epidemics of Louisiana, Mississippi, Arkansas and Texas, in the year 1853.* By E. D. FENNER, M. D., New Orleans, La. New York: Baker Godwin & Co., 1854, pp. 133, 8vo. From the author.
- IX.—*A Dictionary of Medical Terminology, Dental Surgery, and the collateral Sciences.* By CHAPIN A. HARRIS, M. D., D. D. S., Professor of the Principles of Dental Surgery in the Baltimore College, Member of the American Medical Association, Member of the Medico-Chirurgical Faculty of Maryland, author of Principles and Practice of Dental Surgery, etc., etc. Second Edition, carefully revised and enlarged. Philadelphia: Lindsay and Blackiston, 1855, pp. 800, 8vo. From T. L. White, Bookseller, 105 Canal street.
- X.—*A Manual of Pathological Anatomy*: By C. HANFIELD JONES, M. B., F. R. S., Fellow of the Royal College Physicians, Assistant Physician to, and Lecturer on, Physiology, at St. Mary's Hospital; and EDWARD H. SIEVEKING, M. D., Fellow Royal College Physicians, Assistant Physician to, and Lecturer on, Materia Medica, at St. Mary's Hospital. First American Edition, revised, 397 illustrations. Philadelphia: Blanchard & Lea, 1854, pp. 735, 8vo. From Mr. J. C. Morgan, Bookseller, Exchange Place.

- XI.—*Nature in Disease*, illustrated in various discourses and essays; to which are added miscellaneous writings, chiefly on medical subjects. By JACOB BIGELOW, M. D., Physician and Lecturer on Clinical Medicine in the Massachusetts General Hospital, Professor of Materia Medica in Harvard University, President of the American Academy of Sciences, and late President of the Massachusetts Medical Society. Boston: Ticknor & Fields, 1854, pp. 391, 12mo. From Mr. T. L. White, Bookseller, 105 Canal street.
- XII.—*Elements of Human Anatomy*: By T. G. RICHARDSON, M. D., of Louisville, Kentucky. With over 400 illustrations, pp. 734, octavo. Philadelphia: Lippincott, Grambo & Co., 1854. From the author.
- XIII.—*A Systematic Treatise on the Principal Diseases of the Interior Valley of North America*. By DANIEL DRAKE, M. D.; edited by Professors S. H. SMITH and F. G. SMITH. Second Series. Philadelphia: Lippincott, Grambo & Co., 1854, pp. 985, octavo. From J. B. Steele, Bookseller, 60 Camp street.
- XIV.—*Transactions of the College of Physicians of Philadelphia, from August 2, 1854, to October 4, 1854, inclusive*. Pp. 28.
- XV.—*Recollections of Europe in 1854*; an Introductory Lecture to the Class of Institutes of Medicine, &c., in the Jefferson Medical College, delivered October 9, 1854: by Professor ROBLEY DUNGLISON, M. D. Pp. 28.
- XVI.—*On the Blind, and Institutions for the Blind, in Europe*; a Letter to the President of the Board of Managers of the Pennsylvania Institution for the Instruction of the Blind. By ROBLEY DUNGLISON, M. D., Chairman of the Committee of Instruction. Philadelphia: 1854, pp. 24.
- XVII.—*Institutions for the Insane, in Prussia, Austria, and Germany*: By PLINY EARLE, M. D., one of the Physicians to the Lunatic Asylum of the city of New York, &c. New York: S. S. & Wm. Wood, 1854, pp. 229, with addenda, 8vo. From Mr. T. L. White, Bookseller, 105 Canal street.
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- Lea, 1854, pp. 88, 8vo. From Mr. T. L. White, Bookseller, 105 Canal street.
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- XXII.—*On the Construction, Organization, and General Arrangements of Hospitals for the Insane*: By Thomas S. KIRKBRIDE, M. D., Physician to the Pennsylvania Hospital for the Insane. Philadelphia: Lindsay & Blakiston, 1854, pp. 80, 8vo. From the author, through Mr. T. L. White, Bookseller, 105 Canal street.
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- XXV.—*Transactions of the Illinois State Medical Society*, for the year 1854. Chicago: J. F. Ballantyne, 1854, pp. 112, with plates.
- XXVI.—*Microscopic Observations Pertaining to Yellow Fever*. [100 copies only printed for private distribution.] By J. L. RIDDELL, M. D., Professor of Chemistry in the Medical Department of the University of Louisiana. Four lithographs, 4 pages of letter-press. From the author.
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- XXVIII.—*Catalogue of Medical Books for sale* by S. S. & Wm. Wood, 261 Pearl street. New York: 1845. From T. L. White, Bookseller, 105 Canal street.
- XXIX.—*Lexicon Poëticum Antiquæ Linguae Septentrionalis*. Conscriptit Sveinbjörn Egilsson. EDIT. Societas Regia Antiquariorum Septentrionalium. Fasc. 1. Hafniæ. Typis J. D. Qvist. MDCCCLIV. Small 4to. From Prof. Chas. C. Rafn, Counsellor of State, Secretary Royal Society of Northern Antiquaries, Copenhagen.
- XXX.—*The Glasgow Medical Journal*: October, 1854. Quarterly.
- XXXI.—*A Lecture*, Introductory to the course of Surgical Instruction in the Kentucky School of Medicine, 1854-55. By JOSHUA B. FLINT, M. D., Professor of Surgery. Louisville: 1854, pp. 31.
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- XXXIII.—*A Practical Treatise on Foreign Bodies in the Air-Passages.* By S. D. GROSS, M. D., Professor of Surgery in the University of Louisville, Member of the American Philosophical Society, Author of Elements of Pathological Anatomy, A Treatise on the Diseases of the Urinary Organs, &c., &c., &c. With illustrations. Philadelphia: Blanchard & Lea, 1854, pp. 468, 8vo. From Mr. J. B. Steel, Bookseller, 60 Camp street.
- XXXIV.—*The Unity of Mankind.* An Introductory Lecture, delivered before the Class of the Medical Department of the St. Louis University. By M. L. LINTON, M. D., Professor of the Theory and Practice of Medicine. Published by the Class. Session '54-55. St. Louis, Mo.: pp. 26.
- XXXV.—*Observations on Yellow Fever, and its Relations to Quarantine,* and other Hygienic Measures. By S. L. GRIER, M. D. New Orleans: printed at the office of "The Creole," 94 Camp street, 1854, pp. 41.
- XXXVI.—*History and Observations on Asiatic Cholera, in Brooklyn,* N. Y., in 1854. By JOSEPH C. HUTCHISON, M. D., late Physician to the Brooklyn Cholera Hospital, Member of the New York Pathological Society, etc. New York: 1855, pp. 24. From the author.

COMMUNICATIONS.

A communication received from Dr. Fenner, of Memphis, Tenn.; also from Dr. Gorrie, of Apalachicola.

LITHOGRAPHS.

Two Splendid Lithographs, 22 by 14 inches, executed by M. LION; representing the appearance before and after Dr. PICTON'S operation upon the slave Nelson. From Dr. Picton.

HONORS TO DR. BEUGNOT, OF NEW ORLEANS.

M. Beugnot, Médecin Français à la Nouvelle-Orléans, a été nommé chevalier de la Légion d'Honneur. Le Docteur Beugnot a été professeur agrégé à la Faculté de Médecine de Paris, et s'est distingué par un bon traité de chimie et d'excellentes publications sur l'art vétérinaire.—[*Gaz. Hebdom. de Méd.*]

STATE MEDICAL SOCIETY.

The FIFTH ANNUAL SESSION of this Society will be held at the Medical College, in the city of New Orleans, on the First Monday in February, at noon. A full meeting of the members is desired.

Those members of the Profession throughout the State who have not yet joined, but are desirous of doing so, are invited to be present, or their applications may be handed in prior to that date, so as to be acted on as early as possible.

JANUARY, 1855.

D. MACGIBBON, M. D.,
Recording Secretary.

EDITORIAL NOTICE.

The undersigned is in no respect to blame for the delay which has occurred in the publication of this number of the Journal. The undersigned begs the subscribers to the New Orleans Medical and Surgical Journal, to take notice, that his connection with this Journal expires, by contract, with the issue of the March Number, 1855. As a great proportion of the business letters have been and continue to be addressed personally to the undersigned, it will be necessary to discontinue this practice after the 1st of next March.

The undersigned, as EDITOR and PROPRIETOR, will issue the first number of THE NEW ORLEANS QUARTERLY JOURNAL OF MEDICINE, in May, 1855, at five dollars per year, payable upon the reception of the first number. Each number will contain 216 pages, octavo.

This journal will be devoted to Original Communications from the Profession, to Analytical and Critical Reviews, Medical Intelligence, the current questions of the day in Surgery, Pathology, Therapeutics, Chemistry, Pharmacy, Obstetrics, Anatomy—normal, pathological and comparative; Physiology, Medical Jurisprudence, the Hygiene of Cities, ships, plantations, &c.

The undersigned has the wish and the will to make this journal a highly practical one.

Should the patronage of the Journal justify a serial issue of original treatises, a portion of the work will be set apart for this purpose though not originally intended for the Medical Journals. These treatises and monographs comprehend general Pathological Anatomy; Pathology, Therapeutics, and the special Pathological Anatomy of fevers and the principal diseases of the South; experimental Physiology, Climate, History of Epidemics; all of which will be copyrighted arranged, paged and indexed, so that they may be bound into volumes separately from the Journal matter. These materials have been laboriously accumulated during nearly a quarter of a century, occupying twenty-two volumes MSS., filling about 10,000 pages relating chiefly to practical and experimental researches.

ADDRESS BENNET DOWLER, M. D.,

No. 80 St. Charles street,

Residence Delord st., between Camp and Magazine sts.

Jan. 1855.

THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL

FOR JANUARY, 1855.

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Part First:  
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ORIGINAL COMMUNICATIONS. ✓

ART. 1.—ELEPHANTIASIS OF THE SCROTUM AND BUTTOCK.—
OPERATION.—DEATH OF THE PATIENT, ON THE TWELFTH
DAY.—AUTOPSY.

BY N. BOZEMAN, M. D., OF MONTGOMERY, ALA.

I have been induced to furnish a report of the following case, thinking that, if in no other respect, it might be interesting in a statistical point of view. But few cases of the kind have been recorded in this country, and but one, I believe, as having occurred in Southern practice. That case was reported in this Journal,* by Dr. Picton, of New Orleans.

Patient, a colored boy, aged twenty years, well grown, stout and healthy looking, entered my Infirmary, March 21st, laboring under the above disease. He stated that, when twelve years old, he received a kick in the perinæum. Inflammation soon followed the injury, and finally resulted in a discharge of pus and urine. Through the fistula thus formed, a portion of the urine continued to flow for a year; it was then discovered that the scrotum was enlarging, and soon afterwards the

*Vol. II. p. 316.

buttock. These parts were occasionally the seat of severe pain, and at such times great difficulty attended the passage of urine. As the disease progressed, these paroxysms became more intense, and after each one there would be a perceptible augmentation in the size and weight of the morbid growth. Three years prior to admission, the urine ceased entirely to flow *per via naturale*, but found an outlet through numerous fistulous openings.

Upon examination, I found the thighs widely separated by the enormous growth. It occupied the left buttock and the whole of the perineal region, and extended several inches below the knees. In its greatest circumference it measured forty-one inches, and from the anus to the most depending point, nineteen. I may as well observe here, that just before operating—about two months after admission—these dimensions were greater, having increased respectively from forty-one to forty-four, and from nineteen to twenty-two inches. This increase took place after one of the paroxysms alluded to, and as there was but the one, I observed very closely the result. Dr. Titley remarks,* that this *per saltem* growth, as it were, is one of the peculiar features of the disease; and in this instance his opinion was certainly very beautifully illustrated. As no allusion is made to it by the authors I have examined, I will state that there existed, to a very remarkable extent, a vermicular motion of the tumor similar to that seen in the healthy scrotum. It could be produced at pleasure, by pressure or the application of cold water. If caused, as doubtless it was, by contraction of the dartos, it certainly retained, in its pathological changes, that property in a very eminent degree; and from this circumstance, I think, is an interesting fact.

A more correct idea, perhaps, of the shape, size and attachment of the tumor, may be obtained from the accompanying sketches. In the front view, (figure 1,) may be seen the prepuce, not only thickened, but enormously elongated, measuring nine inches.

*Curling, on the Testis, p. 533.

FIGURE 1.



A

In the posterior view, (fig. 2,) the main body of the tumor, as well as its greatest length, are presented.

FIGURE 2.



A deep fissure, separating the tumor into a gluteal and a scrotal portion, is here seen. Near the anus, in this fissure, are five or six fistulous openings, from which the greater part of the urine dribbled away. At the most depending point, and in front of the tumor, are two or three others,

through which passed not only urine, but occasionally a muco-purulent looking fluid. The whole surface is seen to be more or less uneven, but especially so towards the lower part, where the furrows are deeper and more numerous. Here the tumor is also more firm and inelastic than elsewhere.

After considering the case in all its bearings, I scarcely knew what course to take. The experience of writers upon the subject bore me out in the belief that little or nothing could be accomplished by any mode of medication. The age and health of the patient seemed, at first view, to warrant the use of the knife; still, the extensive attachment of the tumor to the body, and especially the existence of stricture of the urethra, and perineal fistulae, all taken together, rendered the propriety of even this procedure very questionable. Placed in this dilemma, with such a rare complication of disease before me, I was induced to consult several of my medical friends in regard to an operation, all of whom, after examining the case, concurred in the belief that it afforded the only prospect of relief to the patient.

Having decided, then, upon this course, the question arose as to the mode of proceeding before attacking the tumor. A very important step, I considered, towards success, was to remove the stricture and if possible cure the perineal fistulae; though this could not be attempted by proper means until the glans penis was exposed by excision of the prepuce. This I did a few days afterwards, but it was two weeks before the parts were well enough to commence the use of instruments. A silver catheter was now employed for the first time, to explore the urethra. Scarcely had it entered above three inches, however, before a seemingly insurmountable obstacle was encountered. A stricture of uncommon hardness and resistance was found to exist. Many were my efforts to overcome it. In addition to catheters and sounds, gutta percha and gum elastic bougies of all shapes and sizes, were pressed into use, but the result was no less discouraging.

The patient had now to be abandoned to his wretched condition, or submit to a more formidable operation than the one originally proposed, and one, too, which greatly lessened his chances of recovery. The testicles, and, in all probability, the penis, had to be sacrificed. The former, in view of not being able to obtain a sufficiency of integuments to cover and protect them from the urine, which was likely to be uncontrolled;

and the latter, from a belief that the organ itself might be involved in the disease, or that the stricture would still prove rebellious, in either of which events I deemed its removal highly necessary. Having gone this far with the case, and knowing that an operation, even if unsuccessful, could not shorten the patient's life much, while it gave him the only chance of living, relieved of his sufferings, I could not now shrink from the responsibility of performing it.

On the 25th of May, everything being favorable, I prepared to operate, at 11 o'clock, A. M. Assistants: Drs. Hill, Hall, Jackson, Pollard, Shackelford, Williams, Geo. Rives, Jr., and Dudley.

The hour having arrived, the patient was placed with his knees on a box a foot high, and his breast resting on a bed. Being then chloroformed by Dr. Jackson, I made my first incision. This extended from a point just behind and to the left of the anus, across the buttock and down upon the corresponding side of the tumor. It was twelve inches in length. My second incision extended from the beginning of the preceding one, along the verge of the anus, and down upon the opposite side of the tumor. This was also twelve inches in length. By these incisions the whole of the gluteal, and a portion of the scrotal tumor were embraced. These parts were now detached by rapid dissection, and this stage of the operation thus completed. Three or four large branches of the inferior hemorrhoidal and superficial perineal arteries were cut, but by the application of ligatures very little blood was lost. The patient was next placed upon a table, on his back. To bring the penis into view now, a small catlin was introduced within the prepuce, and carried along its dorsum to the symphysis pubis, and then brought out at one sweep. The cut was eight inches in length, and laid bare the organ in almost its entire extent. The upper extremity of this incision was now connected with the termination of the first one upon the left side of the tumor, by a curvilinear sweep of ten inches. At this state of the operation, the patient swooned, but upon cold water being dashed in the face and a little brandy given, he soon revived. The integuments preserved for a covering having been dissected back, only a few strokes of the knife were required now to bring into view the left spermatic cord, the cremaster muscle of which I found very much hypertrophied. Not wishing to be detained here, I threw a temporary ligature around the cord, and then cut it off about three inches from the external abdominal ring, with the view of ty-

ing the spermatic artery separately at the conclusion of the operation. Another curvilinear incision, twelve inches in length, was next made, to connect the beginning of the preceding one with the termination of the second, upon the right side of the tumor. This being done, the spermatic cord was soon brought into view temporarily, tied, and cut off, similarly to the one on the opposite side.

I now attempted to disengage the penis entirely. By careful dissection this was effected to a point three or four inches from its extremity where several small calculi were encountered, both in and out of the urethra. Here the corpus spongiosum was involved in the morbid growth, and the urethra entirely obliterated. Notwithstanding this condition of things, I continued to separate, as well as I could, the organ from its morbid attachments, intending to examine the parts more thoroughly after the tumor was removed. By rapid dissection, the latter was soon accomplished. The penis was now found more extensively diseased than I had at first supposed. The urethra was obliterated. I did not know to what extent; and without either this or testicles, I considered the organ a useless appendage, even should we be able to preserve both it and the life of the patient. The propriety of its removal was at once concurred in by all the physicians present. It was an object now to get rid of all the disease. The corpora cavernosa were accordingly cut close to the pubis, and the corpus spongiosum through the beginning of the bulb. The dorsal arteries and the arteries of the bulb being secured, I endeavored then to find the urethra, intending, if this could not be done, to make an artificial passage. Fortunately, I succeeded in the former, and without any difficulty passed a female catheter into the bladder.

Nothing now remained but to remove the ligature upon each cord, and to tie the spermatic arteries, as I had contemplated. There being no hemorrhage, I proceeded to dress the wound. The integuments from the sides were brought together in the median line and secured by interrupted sutures, taking care to leave a small slit for the catheter, which was to be confined in the bladder. The whole of the wound was thus covered, excepting a small portion of the buttock, which, of course, had to be left to the granulating process. Compresses were next applied, and the whole secured by a figure-eight bandage. This being done, the patient was removed to his bed, and a large anodyne given. His pulse was 128, but not very feeble. During the whole of the operation, which lasted

one hour and a quarter, not exceeding twenty ounces of blood were lost and I may say most of this occurred in making the posterior incisions.

The tumor weighed forty pounds. Near its centre both testicles were found embedded. They were considerably atrophied, but otherwise normal.

Nine o'clock, P. M.—Patient comfortable, excepting a slight hiccough; pulse 88, and of good volume; passes urine at pleasure, through the ca heter. Ordered sulph. morph. gr. ss.

Twenty-sixth, 7 o'clock, A. M.—Did not rest well through the night; slight hiccough still; pulse 130; considerable thirst, nausea and vomiting. Prescribed tinct. veratri, gtt. ij; sulph. morph. gr. 1-5th, every two hours.

Nine o'clock, P. M.—Feels better; pulse 104; not so much hiccough; less thirst; no vomiting; skin moist, &c. Veratrum discontinued. Ordered subm. hydarg. gr. xij; sulph. morph. gr. ss.

Twenty-seventh.—Comfortable, but has no appetite yet; pulse 100. Prescription of yesterday resumed.

Twenty-eighth, 6 o'clock, A. M.—Took but five doses of the veratrum yesterday; slept well last night, and feels very much refreshed from it; pulse 84; no action of the bowels yet. Gave oli. ricini ℥i ss.

Six o'clock, P. M.—Not so well; oil has not operated. Ordered an enema.

Twenty-ninth.—Bowels been moved twice; feels better since; has some appetite; took a little chicken water.

Thirtieth.—Slight abdominal soreness; occasional hiccough; otherwise feels very well; pulse 84; skin cool; appetite improving, &c. Dressings removed; parts look rather badly; inclined to slough, towards the anus; no disposition to heal anywhere; discharge thin and offensive; catheter removed, cleansed, and replaced; two ligatures came away; Ordered a large poultice to be applied; allowed a more generous diet.

Thirty-first.—Rested only tolerably last night; lower part of the abdomen tense and slightly painful under pressure; pulse 108, and rather more feeble; bound, and discharge about the same; all the ligatures, excepting those on the spermatic arteries, came away. Poultice renewed, and made to cover the abdomen. Also, prescribed musk and carb. ammonia. Beef soup for diet.

June 1st.—Rather better; does not complain of so much abdominal tenderness; pulse 100, still, however, small and feeble. Catheter removed out of the bladder several hours to-day; had perfect control over the urine. Ordered warm-water dressings, instead of poultices. Prescription of yesterday continued, with the addition of brandy. Diet the same.

June 2d.—Looks badly; pulse 108, small and feeble; skin cold and clammy; hiccough; little or no appetite; coated tongue; sordes, &c. Soup, brandy, musk and ammonia, given more freely.

June 3d.—Had a free action of the bowels last night; feels better than at any time since the operation; countenance improved; pulse still 108, but of more volume; skin warm and pleasant; tongue inclined to clean off; good appetite, and relishes food. Prescription of yesterday continued, with the addition of tinct. cinchona.

June 4th.—Rested well through the night; wound suppurating freely, and has a more healthy appearance; abdomen softer, &c. Treatment continued.

June 5th, 6 o'clock, A. M.—Patient imprudently got up last night to have his mattress turned over; did not rest well afterwards; looks exhausted; pulse 120, small and compressible; pain in the left iliac region; occasional rigors. Ordered stimulants to be given freely. At 5 o'clock, P. M., he expired.

Autopsy, twelve hours after death.—The exposed surface of the wound bore rather an unhealthy aspect. The integuments brought together in the median line, had united only partially. The ligatures on the spermatic arteries were not detached. Extending from the wound upwards, as far as the umbilicus, the sub-cutaneous cellular tissue was infiltrated with a thin purulent fluid. Upon opening the abdomen, slight peritonitis was found to exist in the left iliac region, having been lighted up there by the extension of inflammation along the spermatic cord. The deposit of lymph here was scanty, and seemed to be quite recent. All the viscera of this cavity were normal, excepting the bladder. Its walls were slightly hypertrophied.



ART. II.—CRITICAL RETROSPECTION OF THE CASES OF SCROTAL ELEPHANTIASIS, AS TREATED BY DRs. PICTON, BOZEMAN, AND SOME OTHERS.

BY BENNET DOWLER, M. D.

Do not American lawyers, divines, authors, artists, and statesmen, display more independence of thought and self-reliance in action than the great majority of that class of medical men in America, who undertake to edit, annotate and remodel foreign books? A bias in favor of foreigners, and against their own countrymen, is predominant, until the latter have been endorsed by "an European reputation"—an endorsement which is often little more than a mere accident. Distance, prejudice, antagonistic institutions, unacquaintance with American periodicals and books, difference of language, and, in many cases, the ignorance and self-sufficiency of foreigners—all contribute to render this coveted "European reputation" equivocal, uncertain, and little worthy of a laudable ambition. The chief cause, however, of the low appreciation of American authors abroad, is found in the discreditable conduct of that numerous class of editors and annotators of European works, who, placing their own names in the title pages of pirated books, often diluting the latter with unimportant additions, or deforming them by mutilations, studiously ignore the researches of their own meritorious countrymen, thereby virtually justifying the contempt of foreigners for American genius, originality and independence.

There is not, perhaps, a city in Christendom containing less than two hundred thousand souls, in which greater, more numerous and successful surgical operations have been performed than in New Orleans. And, if Stone, Mercier, Wedderburn, and some others, who pursue the "noiseless tenor of their way," have not "an European reputation," nor an American one either, as being in the very front rank of the great and successful operators, it is not for want of merit. The written, and the unwritten surgery of this city, has been not only brilliant in its execution, but successful in the highest degree. That distinguished surgeon, Dr. Mercier, in a discourse delivered before the Physico-Medical Society, early in December of the present year, attributed the remarkable success of surgical operations in New Orleans, partly to the climate, which he regards

as equal to that of Egypt, and far better than any Northern climate in this particular.

These reflections have grown out of a perusal of Dr. Bozeman's paper as above, and of a re-perusal of Dr. Picton's case of Elephantiasis of the scrotum, alluded to by Dr. Bozeman, and also in the sequel. It is not so much the mere success of Dr. Picton's brilliant operation, though in this point of view it is unsurpassed, seeing that the negro he operated on still enjoys health, and has, since the operation, become the father of two children,—it is not because the tumor weighed *fifty-three* pounds; but it is the lucid synthetical and analytical judgment he had formed,—it is the logic, and not the knife, that wins appreciative commendation, and encircles the head with a bright halo.

A recapitulation of Dr. Picton's pathological views, without reproducing the entire history of the case, will, doubtless, be acceptable to the readers of this Journal, many of the subscribers to which, ten years ago, when the case was published, together with three of the four honored names, who were then Editors—Carpenter, Harrison, and Hester—have descended to the tomb, “by strangers honored” and by neighbors mourned. Many new subscribers have never seen the number of the Journal, from which the following extract is taken, not as “a vain repetition,” but as something that it would be discreditable not to read more than once.

To the large number of medical gentlemen who attended Dr. Picton's operation, nearly all of whom considered the case to be a hopeless one, Dr. P. spoke thus:

“In compliance with my invitation, the medical gentlemen whose names are below,* assembled at the Orleans Infirmary, at eleven o'clock, A. M. The preparatory arrangements were completed at twenty minutes after eleven.

“I gave a succinct account of the nature and duration of the disease, and observed that three considerations would be involved in the performance of the operation: *First*, the practicability of success, with the preservation of the life of the individual. *Secondly*, an extirpation of some portion, or the whole of the genital organs, if diseased. *Thirdly*, the formation of an artificial scrotum, and the best mode of effecting the union of parts in the event of the healthy condition of those organs.

“With reference to the first consideration, it was evident that the venous and lymphatic circulation would be found mainly to contribute to the volume and, perhaps, nutrition of the tumor; and that a division of such vessels, would withdraw from its own particular and abnormal circula-

* “Drs. Davidson, Labatut, Landreaux, S one, Hunt, Puissan, assistants in the operation. Drs. McFarlane, Thompson, McNeil, Kennedy, Gaillardet, Thomas, Carey, Harris, Meux, Grny, Wharton, Heermant, Harral, Lambert, St. Martin.

tion, rather than from the system at large. It is true, that previous to the operation, I had discovered two large arteries, one on each side of the neck of the tumor, yet I believed they would be sufficiently under control. Consequently, there could be no apprehension from an excessive loss of fluids, with proper assistance.

“As the second involved considerations wholly dependent upon the first, nevertheless, they would determine the *utility* of the operation, although its *practicability* had been ascertained.

“The preservation of the penis and testicles was regarded by very few of the medical gentlemen, who had examined this case from time to time, as at all attainable. Some proposed entire abscission, after the mode of Dr. Jacobs, of the West Indies; others, the use of the ligature; another supposed that a removal of a portion of the mass, repeated at appropriate intervals of time, would succeed better.

“From the length of time the disease had existed, the complete envelopment of the genital organs, and the traction of the same consentaneously with its growth, it was very reasonable to conclude that their structure and functions had materially changed; that the compression necessarily exercised by such a compact tissue, would nearly destroy the body of the penis, leaving only the urethra, and obliterate the spermatic cords, and, thereby, destroy the testicles.

“Since the genital organs were entirely concealed by the encroachments of the tumor, it was impossible to ascertain their positive condition, and in seeking an approximation, even, to their normal state, would be very unsatisfactory, by a reliance alone upon the declarations of the patient; when nature had interposed an almost insuperable obstacle to the exercise of two of the most essential qualities of the surgeon—delicacy of touch and acuteness of vision. The only means I possessed, therefore, was an exploration of the glans, through the protruded prepuce, which is seen near the centre of the mass, about four inches from the orifice. This mode was often resorted to, by the introduction of the finger, and I determined that the penis, at least, was essentially and integrally sound, or so far so, as to warrant the trial to preserve it. My opinion was based on the following grounds: the uniform elasticity and sensibility of the tissue forming the glans, titillation inducing an effort to erection, and an augmentation of its volume, thereby indicating that its erectile power had been retained.

“The patient stated that he was subject to pain, and almost always to uneasiness, in the posterior and middle portions of the tumor, and near each groin, in the direction of the inguinal canals. Upon exercising pressure over this track, he experienced much of that peculiar sensibility which belongs to the testicles and spermatic cords. He was also sensible of partial erections, which occasionally produced the sensation of violent distension.

“I therefore believed, that the external genital organs had retained, in a great degree, their appropriate functions. The great traction exerted upon the cords contributed much to his pains, and he, therefore, very naturally resorted to a broad suspensory bandage, which was passed be-

neath the tumor, and crossed over the shoulders behind the head; and upon a recurrence of more than usual pain, he elevated the mass, and obtained relief.

“These circumstances justified me in attempting the preservation of those organs; or, at least, to bring them under my inspection during the operation.

“With regard to the third proposition, it was highly important to select such a portion of the tegumentary tissue, in forming the artificial scrotum, as would least incur the hazard of a return of the disease, and to make due allowance for the retraction of that tissue, in order to accommodate properly the testicles and elongated cords. That portion was found less diseased from the perineal space down the posterior surface of the mass, about six inches, and covering a superficies of perhaps twenty-five or thirty square inches. The tumor presented here, a less lobulated appearance; the skin was thin and soft, and exhibited the lustre peculiar to the black. I therefore selected this portion, and determined to use uninterrupted sutures.”

So reasoned Picton, of New Orleans, who had not the happiness of being born in Paris, nor even in London. Velpeau glories in Delpech's case, “as the most remarkable yet known.” The tumor weighed sixty pounds. He removed it, saving the organs of generation; but Authier, the patient operated on, “was attacked, after the lapse of some months, with internal inflammation, which proved fatal.” (*Oper. Surg.*, 2d Ed.) M. Velpeau mentions an operation for this disease, by Mr. Key, and one by M. Clot, of Egypt; in both cases, the organs of generation were sacrificed.

If the writer's recollection of M. Velpeau's last edition be not inaccurate, his researches on scrotal elephantiasis are most unsatisfactory, amounting to little more than the statistics of the weight of a few specimens, some of which were reported as reaching one hundred and sixty pounds—more wonderful than instructive; while the Editor of his work, it is believed, has claimed nothing for America.

Let the reader turn to the systems of surgery by the most celebrated English authors, all published, edited, annotated, and augmented, in this country, since Dr. Picton's ignored operation, and what will he find? The late Mr. Liston, Professor of Surgery, performed an operation of this kind, which has been often cited, by which his patient lost the organs of generation. The tumor weighed twenty-three pounds. This great master of surgery justifies himself by saying, “I was more anxious to save the patient's life, than concerned about preserving his organs of generation.

In trying to save these, it was more than probable that I might have lost the man, from hemorrhage and protracted pain." (Prac. Surg. 232.) "Lame and impotent conclusion!" Impotent in more senses than one—since he made an eunuch—but not "for the Kingdom of Heaven's sake"—having determined beforehand upon an unjustifiable and horrid mutilation, in a case, apparently, more favorable for a successful operation than that of Dr. Picton.

Mr. W. Ferguson, a distinguished professor of surgery, in London, says of Mr. Liston's operation, "that it was the largest case of the kind successfully treated in this country," (Great Britain.) Professor F. adds, "when these growths are large—say forty, sixty, or one hundred pounds—as the immediate safety of the patient is implicated, I imagine that the surgeon does *wrong in attempting to save them* by any protracted dissections." (Surg. 575, 3d Am. Ed., 1848.) Wholly gratuitous!

Mr. John Erichsen, Professor of Surgery, and Mr. Liston's worthy successor in the University of London, in his able System of Surgery, (1854,) repeats Liston's case, adding, that "tumors of this kind necessarily require extirpation; and in performing such operations, there are two points that demand special attention. The first is the preservation of the penis and testicles, which will usually be found buried towards the upper part of the mass, and which may be done *if the tumor is of small size*; and the second is, to endeavor to prevent the hæmorrhage." (875.) One unwarrantable hypothesis, and one truism! Had the American annotators of these works given an account of Dr. Picton's reasonings, and of the steps of his operation, in the case cited, these authors would have been saved from the unintentional inhumanity of laying down rules which require a horrible mutilation, and which can never apply anterior to an actual exploration of the concealed organs, by means of the knife, during the operation. Dr. Picton informed the writer, during a recent conversation, that, in order to guard against the danger of hæmorrhage, he did not wait to tie the divided vessels until after the operation, but secured them instantly, by means of Amussat's forceps, of which he had provided a sufficient number for the emergency.

This operation, performed on the 3d of October, 1837, was followed by a cure in two months. The slave Nelson, the subject operated on, still lives, as Dr. Picton says, in good health, near New Orleans, in the parish of St. Charles. The tumor was deposited in the Anatomical Cabinet of

the University of Pennsylvania, where it remains. Dr. Picton says, that the testicles are movable, that is, not adherent to the scrotum, which he formed from the skin! Those who know Dr. Picton will not question his veracity.

In the surgical history of scrotal elephantiasis, a more remarkable case than Dr. Bozeman's, as given in the preceding article, can scarcely be found, combining, as it does, the scrotum, buttock, numerous urinary fistulæ, obliteration of the urethra, the formation of calculous concretions, the degeneration of the genitalia, having originated traumatically, and progressed paroxysmally to a colossal size, yet essentially conforming to the spontaneous types. Although the patient died in about two weeks after the operation, partly from imprudence, and partly from an unhealthy, non-adhesive, non-granulating, inflammatory condition, often supervening upon simple wounds of less magnitude, in which, from occult and inexplicable causes, the reparative process is defective or perverted,—added to which, in this case, there was an accidental peritonitis. When all these circumstances are considered, the operation must be considered, in a surgical aspect, as virtually successful; the shock, the hæmorrhage, and the other immediate effects of the operation, had all been surmounted. The fatal result was not, then, an immediate result, but a remote contingency,—not inherent, but accidental,—a possible, not a certain eventuality, which human foresight could divine.

Elephantic surgery owes an ovation to the names of MM. Delpech and Larrey, and Drs. Picton and Bozeman,

Surgeons within and bordering on the tropics, where elephantiasis abounds, must, above all others, take a deep interest in the cure of this most disgusting and gigantic monstrosity of pathology.

Dr. Cowling, late of Rio de Janeiro, now of New Orleans, informs the writer, that scrotal elephantiasis is very prevalent in Brazil, among the negroes, where he has seen it as common as in the East Indies among the natives.

It is a matter of regret that Drs. Picton and Bozeman have not examined, by careful dissection, and by the microscope, the anatomical characteristics of these morbid specimens; and the more so, because little has been done to elucidate the pathological histology of scrotal elephantiasis.

Dr. Bozeman's post-mortem examination of the subject of his operation is highly interesting, particularly in a negative point of view, as it

shows that the viscera may be normal, not being cotemporaneously involved in or connected with the external malady—which is always a circumstance highly favorable in a formidable operation, and in this a fundamental desideratum. The peritoneal inflammation which was found, dated posterior to the removal of the tumor, having been recent and accidental, like hospital gangrene, traumatic erysipelas, and other unforeseen eventualities.

ART. III.—DYSENTERY AMONG NEGROES.

BY H. V. WOOTEN, M. D., LOWNDESBORO', ALABAMA.

In the September number of the Journal, I find an article under this heading, from the pen of Dr. Samuel A. Cartwright, which, I think, calls for some notice. Dr. C. is one of our most eminent medical writers and practitioners, whose writings are extensively read amongst the members of our profession, (notwithstanding his surmises to the contrary,) and by many persons out of it. Publications on subjects of the first importance to the public, and emanating from high and influential sources, should, surely, be as clear of error as possible; and it is because the one referred to is based upon erroneous premises, and therefore made up of mis-applied arguments and doctrines, that I propose to call attention to it. This I should not feel specially called upon to do, but for the circumstance that most of the facts particularly spoken of by Dr. C., came under my own observation.

The first point to which I would direct attention, is the ground which Dr. Cartwright assumes, as the basis of his article, that the epidemic dysentery of South Alabama *is confined to, or particularly fatal amongst, the negro population.* This I know to be erroneous; and Dr. Cartwright might easily have found it so, by inquiring of the physicians of the different parts of the State where the disease prevailed, or by reading the numerous articles which they have published on the subject. But I suppose he rests his conclusion upon the remark of his hon. Alabama correspondent, that “the populous planting counties of South Alabama have been chiefly afflicted;” and, again, that the disease “has recently appeared on many of the large plantations,” &c. All this is, no doubt, in accordance with the information of his correspondent on the subject; but, if it is fully

true, it does not prove that the disease is more prevalent or fatal amongst negroes than white people, in proportion to numbers, where it prevails.

On two of the three places of its prevalence, named by his correspondent, I was the regular medical adviser. He says that Mr. Robinson "lost twenty-five negroes by this disease." Mr. R. had on his plantation about two hundred negroes. In January, 1852, pneumonia and dysentery commenced a joint prevalence amongst them. From these diseases, some eight or ten had died, when measles appeared among them, and all under forty years old were subject to it. The three diseases, and particularly measles and dysentery, continued their ravages until nearly all were attacked, many with two, and not a few with all three of the diseases. One negro died of phrenitis, one of malignant chill, and one in labor; and from all these diseases Mr. R. lost, during the winter and ensuing spring, about the number mentioned; but I do not believe that one-third of them died from dysentery as a determining influence. It has been reported in one direction, that Mr. R. lost twenty-five negroes by dysentery; at another place, that he lost them by measles; and again, by pneumonia—showing the reliability of such hearsay reports. One of Mr. Robinson's sons staid at the plantation a portion of the time, and escaped; another staid at the landing in connection with the plantation, and was attacked with the dysentery, gravely at first, but he recovered—the only white persons who staid on the place. The disease reached Mr. R.'s residence, where several, white and black, were attacked, and one of his sons and a negro boy died.

At Mrs. Bonnell's, it was more fatal. When it commenced, there were on the place four white persons and, I suppose, about twenty negroes. It was the residence, and not the negro quarter, that was visited by the disease. One-half of these white persons died: Mrs. B., and her overseer, as mentioned. Another overseer came, took the disease, and died; making three, out of five white persons, who perished. Dr. C.'s correspondent says, that from fifteen to twenty negroes died of the disease; but it seems to me that this must be a mistake. I visited the place occasionally during the whole time, and my co-partner attended regularly; and I am sure that I never heard of more than half the smallest number—and I heard a great deal about the disease, and the number destroyed. Besides this place, Mrs. B. had two large quarters of negroes, and it may be that the number mentioned may have died amongst them all. I have no idea that there was any disposition to over-rate the mortality, and I certainly have

none to under-rate it, but freely admit that all, white and black, who were attacked with the *special form*, or combination of disease in question, died, except one. About which I shall say more hereafter.

During the same season, the disease fell upon another family in the neighborhood. Here there were eight white persons, and nineteen negroes. Four of the white, and five of the blacks—one-half the white persons, and but a fraction over one-fourth of the negroes—died.

The same type of the disease visited the village of Hayneville, four miles from Mrs. Bonnell's, during this season. Here, it swept off a large number of persons, and the large proportion of adult white persons, and especially of ladies, which appeared in the obituary record, and was a common subject of remark, while the comparative exemption of the negroes was equally so.

During all this time there was a *general* prevalence of dysenteric diseases throughout the community. And there were many scattering cases of the true typhoid type. Of these, I think, there were more fatal cases among whites than negroes, though a far larger proportion of these cases recovered than of those which occurred in the families spoken of, where it prevailed so heavily.

Thus far, I have spoken of matters coming within my own knowledge, but I have gathered reliable information concerning this fatal scourge from other sections. It prevailed for several previous summers in the lower part of the same county, and in those adjoining; and I have been particular in getting correct information concerning its character. I am assured that its fatality is by no means confined to negroes, but that it was terribly fatal amongst white persons. Dr. Bythewood, a very intelligent physician, related instances of its ravages in white families, far surpassing anything that I have heard of it among negroes.

In many portions of the South, this has been for several years the most dreaded disease. On this account, I have made a special business of gathering all reliable information concerning it in my power. Early in the present year, I published, in nearly all the medical journals of the South, a circular, calling for information upon this particular subject. It has prevailed in many parts of Alabama, Georgia, Mississippi and Tennessee, with fearful fatality. I have been informed of many white families that have been more than half taken off by it. Some have seen far more fatality from it among whites than negroes; others, who have wit-

nessed its prevalence chiefly on large plantations, have seen most fatality amongst negroes. But all agree, that it attacks both races with fearful frequency and force, as they come within its scope.

I have heard of some physicians who had encountered a great number of cases, and lost none; but, on investigation, it has turned out, invariably, the endemic form of dysentery that they have treated, and nothing more. Several years ago, an epidemic dysentery visited my neighborhood, and, in many cases, was very violent and obstinate. My co-partner and myself treated over two hundred cases, and lost only two white infants, and one adult negro. But this was as different from the adynamic form, or complication of the complaint, as common remittent fever is from the most malignant yellow fever.

Dr. Cartwright's Alabama correspondent is a most excellent and intelligent gentleman, and one on whose observation and judgment in medical matters I would as soon rely as upon those of any other non-medical man. But Dr. C. knows very well the great difficulty which we always meet in in getting full and satisfactory information concerning an intricate and complicated disease, from non-medical observers, however intelligent they may be, even by the most searching inquiries. But the doctor's correspondent did not see the cases about which he writes, and must have received his account of them from others, and they not the medical attendants. But, I take no exception to Judge B.'s account of the disease, so far as it goes. If he had used the word *generally*, for *invariably*, it would have done very well for a superficial view of a large number of cases, and is as good a description of the disease as could be given in *eleven lines*.* Any one must admit, however, that it is not such an account of such a disease, as physicians generally want, to found a confident and practical opinion upon. And it is not a little singular, that Dr. Cartwright should take so meagre an account of so terrible a disease as the basis of such a lecture on the medical practice and teaching of the South, and that he should direct his opinions and criticisms so pointedly to the very men who have had so much experience in the matter in question, and from whom he does not appear to have heard a word.

I have seen a number of physicians who have seen the disease in its full character, and all agree, that it is impossible to impart a just idea of

*His account of the disease, though he did not see it, is, no doubt, as correct and full as Dr. Cartwright or myself could give, in the same space, of an intricate and complicated case in law.

it to one who has not seen it in all its stages. The accession of the disease was, in a large number of cases, quite different from the description given by Dr. C.'s correspondent, as "invariable." In fact, so far as the symptoms of dysentery were concerned, it came on in almost every conceivable manner. In several cases, a diarrhoea first attacked the patient, and "bloody flux" supervened. Again, in many cases, the patient appeared to be taken with a genuine typhoid fever, and after several days the bloody stools would appear.

I have not undertaken to discuss the nature of the disease in question, but simply to give a correct statement of the facts involved. A great number of inquiries have been made of me by medical men, concerning its history and pathological nature, but I have never felt prepared to give either, in full. The best and most comprehensive short description which I have been able to give of the peculiar form of the disease, is about this: A low, grave form of typhoid fever, with frequent discharges of sero-sanguinous fluid from the bowels, sometimes attended with pain at stool, more or less severe, but not during the intervals. In short, a grave typhoid fever, with its diarrhoeal discharges changed from a "cider" to a sanguinolent character. The patient generally expresses himself as feeling better; and the mental torpor—which Dr. C. characterizes as a "*cholera of the mind*," (would not *constipation* be a more appropriate term?)—was precisely the same that we so often meet with in grave cases of typhoid fever, and in both white and black persons.

As to the local cause or influences which may have originated the disease, nothing satisfactory can be discovered. On Mr. R.'s place, the cabins were, literally, in the woods—some old, and some new—and not a brush unnecessarily cut away. Mrs. B.'s was an old, settled place, but the houses were not old, and were very good frame buildings. On the other place, the houses were of logs, new, in a beautiful natural grove, and upon a high, dry, and, commonly, a very healthy hill.

In Hayneville, it occurred without any regard to differences in local affairs, that could be observed, or to the manner of living.

The account of the treatment of the disease in South Alabama, upon which Dr. Cartwright bases his philosophy, and against which he directs his batteries, is all comprised in two lines, thus: "The treatment, with some occasional variations, has consisted in exhibitions of blue mass and opium, and sometimes astringent injections." Now, no account of the

treatment could well be more erroneous than this, which I can account for only from the fact, that the correspondent did not witness the treatment himself, and was, in some way, singularly mis-informed. The blue mass and opium is a remedy in very common use in the neighborhood for the ordinary forms of diarrhoea and dysentery which prevail, and especially amongst the negroes on plantations. Many of the planters keep the pills already made up, for the purpose, and find them very successful. In the typhoid dysentery in question, the blue mass and opium were used by the physicians, separate or in combination, to meet special indications, in many cases; but in a great many cases their use was not called for, and in none were they relied upon and continued as a means of cure. So that their use constituted the variation, and not the treatment.

A full account of the treatment of the disease, during the season, in that single county, would fill a volume as large as one of the "text-books," about which Dr. C. has so much to say, and would comprise about every medicine, combination of medicines, or agencies, which experience, judgment or science could suggest. Stimulants increased the local irritation of the alimentary canal, as evinced in dryness of the tongue, thirst, a sense of internal burning, &c.; purgatives would not do, as Dr. C. says; sedatives, or depressants, direct or indirect, were pernicious; nourishment, in any form, was almost uniformly hurtful. In fact, it may be said that every agency known, or that we could invent, which promised any favorable influence, external or internal, was resorted to. The "one dose cure," and the sweating process, or diluting and washing out, were remembered; and even the "vinegar and salt" was not overlooked. Several cases were treated with scarcely any medicine at all. Of course, I do not intend to say that all these things were used in any one case; but simply, that their power over the disease was tested during its prevalence. I had a number of cases on my own plantation, and lost none, but there was only one of a grave typhoid character. My conclusion, drawn mainly from observation on these cases, where I could have their entire management under my own direction, was, that it was best to treat symptoms as they arise in the case, and sustain the patient as well as possible through the attack. The dysentery is a small portion of the disease, and treatment directed to it alone is of little avail. Some of the most rapidly fatal cases were the least troubled with dysenteric flux.

I cannot understand how it is, that, while Dr. Cartwright places such

high confidence in the description of the disease, and its treatment, by his correspondent, as to base his whole communication upon it, he should treat his opinion of the physicians with such contempt. The correspondent says, "that particular section of the county was not destitute of experienced and skillful physicians." Dr. C. says, "so long as we hear of frightful mortality among negroes from dysentery, cholera, &c., &c., in defiance of the best directed medical skill—meaning, thereby, that skill that would treat an owl like an eagle," &c. Going to show, that the mortality is owing to a want of skill in discriminating between the wants of the white and black races, in the treatment of disease. Wonder if Dr. Cartwright has never known these diseases, and other epidemics, to be *somewhat* fatal among *white* persons, in defiance of medical skill? and, if he knows any good reason why *negroes* should be wholly exempt? I believe that all classes of animals are subjected occasionally to these scourges.

Dr. C. speaks continually of the treatment of the disease in question, as being drawn from the "text-books" of Watson, Bell and Stokes, &c. Now, I can assure him, that the text-books aforesaid had nothing whatever to do with it, in the particular section referred to. For myself, I could not tell him the treatment laid down in either of the works spoken of, for dysentery. And, moreover, although I have practised on dysentery every year since these books have been published, and have had them at hand, I do not think that I have ever made a practical reference to them, not considering them as proper guides, in my latitude, for the treatment of dysentery. I am at this time away from home, having no books at hand, and do not know what would, or what should, be called a text-book treatment of dysentery.

I speak confidently, and of what I know, when I say, that the physicians of South Alabama, of whom Dr. Cartwright so contemptuously speaks, as men incapable of understanding things which they continually see for themselves, are as well instructed, talented, and skillful a set of men as can be found in any section of our country. If Dr. C. will read the proceedings of the Alabama State Medical Association, for the last four years, he will not only find much said about the disease in question, but he will find that there are, in "that particular section," a goodly number who are capable of distinguishing between an eagle and an owl, as well as the proper treatment of the different classes of diseases and patients

with which they meet. I can assure him, moreover, that the practitioners who attended the places of which we have spoken, read, regularly, all the Southern medical journals, and that when they wish to refer to anything that Dr. Cartwright has published in the New Orleans Journal, they do not have to "rummage among dusty old magazines and papers" to find it, but have it all "done up" in neatly bound volumes, containing everything ever printed in it.

I suppose it is the blue mass and opium treatment, about which Dr. C. says so much, as that of the European, text-book, and white man's treatment; and, as I knew of but one case which was treated by it throughout, I will give a brief account of it. It is the case previously spoken of as the one that recovered on Mrs. Bonnell's place. While sick myself, I was urgently requested to visit the overseers mentioned, and who had then been sick some two weeks or more. While there, I was requested by a gentleman who was on the place, and one of the executors of the estate, to see a negro man, who was down with the flux. I found him several days advanced in the disease, of the same type of those cases which had proved so fatal. His case was not so grave as many of those which had died, had been at a similar stage, but more so than some of them, and he was fully impressed with the belief that he was bound to die, and objected to taking medicine. The gentleman in charge was very intelligent, and had seen much disease and its treatment, the blue mass and opium being his favorite medicines in dysentery. He informed me that he had given the patient a pill composed of opium, 1 gr., and blue mass, 3 grs., every three or four hours, for two days. When the discharges would cease for a time, the pills were withheld. While the dysentery was moderate, a mixture of nitric acid, camphor and laudanum was administered occasionally. But whenever the stools became frequent, the pills were resumed. I declined undertaking the case, on account of my own condition. The overseer was removed the next day, and, there being no more professional attendance on the place, the gentlemen referred to continued the treatment, as he afterwards informed me, throughout the case, and the patient recovered. This was the real blue mass and opium treatment, with the variations. And the patient was not a "black white man;" but a stout, well set, medium sized and aged *negro*, as black as the blackest "knee bender" of Hebrew history could have been, or as the seed of Ham could have made him.

Now, it is no part of my purpose to deny the difference between the natures, morally and physically, of the white and negro races. I believe that they differ physiologically, pathologically, and therapeutically; but as to the particulars and extent of these differences, I think our knowledge is very imperfect.

I was "born and raised" in a community where three-fourths of the population were negroes, and I have practised medicine diligently sixteen years where five-sixths are negroes. The peculiarities of these people, and the treatment especially adapted to them, have ever been leading points of study with me; and no one will call me a careless observer. I have, certainly, no desire to controvert Dr. Cartwright's opinions or published doctrines upon the subject; but my object simply is, to correct an important error of fact.

As to Dr. C.'s views of the treatment of this complaint, among negroes, I think about all his suggestions were resorted to, except the breaking up and running away to the woods. I believe that removal from the place of its prevalence, to almost any other, is one of the best means of preventing the spread of the disease, but I have not seen it do much good for the *sick*.

As to the treatment of convalescence in negroes, I will remark, that on no place were I practised were there any hospital regulations. I have generally resorted to good, wholesome and substantial nourishment so soon as the removal of disease would allow, and have not neglected nourishing the system even before. We did not lose a single case after the cessation or removal of disease and the appearance of convalescence. But, as the negroes generally mixed promiscuously, giving each other whatever they called for, we frequently had much trouble on account of imprudent eating, both before and during convalescence. I well remember several cases, in which I was given much trouble by the patient returning too soon to the "fat pork, corn bread and greens" diet. And I have had many *fever* convalescents dragged in from the hot sun, (where they had laid themselves after a full meal of the sort,) in a profound stupor, with stertorous breathing, &c., and relieved them only by relieving their stomachs of their burthen. Experience has taught me to guard against extremes in both directions. Not to starve too much, on the one hand, or feed too heavily, on the other.

White Sulphur Springs, Tenn., Sept., 15th, 1854.

ART. IV.—EXTIRPATION OF TUMORS FROM THE NECK.

BY B. BROOKS, M. D., WHEELOCK, TEXAS.

In the month of April, 1854, D. Alex. Neilson, M. D., and myself were requested to see a negro man, Caleb, aged about forty years, apparently of good constitution, the property of Mrs. A., who had a large tumor growing on the left side of his neck. It was first discovered about twelve months previously, immediately in front and about midway of the sternomastoid muscle; was, at that time, easily movable, and quite elastic under pressure. Since then it had grown rapidly. We found it extending from the trachea, in front, to the posterior part of the neck, and from the clavicle below, as high up as the mastoid process of the temporal bone, above. It presented prominences and depressions upon its external surface; moderately firm under pressure; somewhat movable, though apparently attached by its under surface. The rapidity of its growth, and the pressure it was beginning to make upon the trachea, thereby impeding respiration, induced us to believe that it should be extirpated, without delay. Accordingly, on the morning of May 6th, I proceeded to operate for its removal, as follows, assisted by my friends, Dr. Neilson and J. L. Hill, and in presence of a number of spectators:

Made two elliptical incisions, about eight inches long, from the upper part of the tumor, down to the middle third of the clavicle; removed the integuments contained within the ellipsis. Then a transverse incision, of about the same length as the above, from the posterior part of the tumor to the trachea, in front, passing a little below the inferior maxillary bone. Dissected back the integuments, so as to expose to view the sternomastoid muscle, beneath and on either side of which the tumor was situated. Attachments of tumor to the muscle, dissected up, without dividing the muscle, then dissected down in front until the under side of the tumor was reached, where it was found to be strongly attached by dense cellular tissue. Commenced, then, dissecting from its posterior part, continuing downwards and forwards until it was removed. After its removal I discovered a number of smaller ones, varying in size from a pea to a large sized almond, surrounding the parts from which it had been removed. Some of them had been pressed down under the clavicle by the

large tumor, though they were easily removed, having no firm attachments, as did the first one. Upon removing the large tumor the subclavian artery was exposed, and in removing some of the smaller ones from their situation alongside of the trachea, the carotid artery was brought into view.

The patient, during the operation, which lasted about an hour, was kept under the influence of chloroform. During the operation, but two small arteries were divided that required the ligature. About a pint and a half of blood was lost. After blood had ceased oozing from the wound, the integuments were brought together and secured in their places by a few interrupted sutures and adhesive plaster.

Upon examining the large tumor, after its removal, it was found to be lobulated, some of the lobes being adipose, while others were steatomatous, the lobes all being held together in their centre by condensed cellular tissue. This mass, two or three hours after its removal, weighed two pounds. Some of the smaller tumors were adipose, while others were steatomatous. They weighed, eighteen in number, half a pound. During the afternoon, patient rested well and felt comfortable. At night, had to get up to evacuate his bowels, and the nurse that was left with him being asleep, he made the effort alone, but being too feeble to get along unaided, he fell against a chair.

On the day after the operation, 7th, patient appeared cheerful, and expressed himself well satisfied at the result of the operation. Did not dress the wound to-day; pulse 80; skin of natural temperature. Drank some gruel.

April 8th.—Assisted by Dr. Neilson, who attended the case regularly with me, dressed the wound this morning. Patient feels well; pulse as on yesterday; skin of natural temperature; bowels acted upon without medicines; wound presents a healthy appearance; incision beneath the lower jaw, two and a half inches long, healing by first intention.

9th.—This morning found patient complaining of pain below the right axilla. Upon inquiring the cause, ascertained that it had proceeded from the fall above mentioned, and upon examining the parts, found a swelling nearly as large as a man's fist. Wound healthy, and healing finely; pulse 85; skin rather dry, and a little inclined to be hot; no action on his bowels since yesterday morning. Gave dose of castor oil; ordered poultice applied to the swelling. Appetite moderately good.

10th.—Patient about as on yesterday. Wound doing well. On the 11th and 12th patient was getting along well.

13th.—Wound still healthy and healing; one of the ligatures came away this morning; incision below the under jaw entirely healed; pulse 85. In an hour or two after the wound was dressed, was called in haste to see patient. Found him with a decided *ague* upon him, which lasted about an hour. Then followed reaction; hot and dry skin; pulse 130; considerable thirst, &c. The fever lasted about three hours, and was succeeded by profuse perspiration. Had two operations on his bowels to-day.

14th.—Wound presents a healthy appearance this morning, notwithstanding the excitement of yesterday; pulse 90, and not quite so full as heretofore; skin moist. Gave quiniæ sulph. grs. x, and ordered it repeated in two hours. Chill came on in advance of its appearance on yesterday; was followed by high fever, and then profuse perspiration. Ordered the quinine to be given early to-morrow morning, increasing the quantity to thirty grains, at two doses.

15th.—Patient growing weaker; pulse 100, and feeble; complains of some stiffness about the jaws; skin warmer than natural; bowels acted upon twice since yesterday morning; appetite not so good as heretofore; wound still looks healthy; patient has slight cough, and complains of some pain in the chest. Auscultation detected incipient inflammation in each lung. Applied blisters over them; gave a dose of syrup scillæ comp. every three hours. Beef tea as diet. Afternoon: Patient worse; pulse 130, and feeble; no chill to-day; growing quite feeble; mind considerably disturbed, though answers rationally when spoken to; jaws more rigid; skin hot, though moist; cough about the same as this morning.

16th.—Patient still sinking; comatose; extremities growing cold; jaws firmly locked; involuntary discharge of urine and feces. Gave brandy and other stimulants through the day, though without any good effect. He died at 11 o'clock, on the night of the 16th.

Post mortem, on the 17th, twelve hours after death. Body very rigid; wound more than half healed. Upon opening the thorax, the lungs were found to be in a high state of inflammation, increased towards their posterior portion, where the left one was found to be firmly adhered to the pleura, the result of an old inflammation. Pericardium contained two ounces of fluid of a reddish cast; substance of the heart softened to such

an extent that the finger was made to penetrate it with but very little difficulty; left ventricle of natural size, while the right one was enlarged to more than double its ordinary capacity. Upon re-opening the wound, we found the carotid artery, with its accompanying veins and nerves, in a healthy condition; as was, also, the thyroid gland, liver, and stomach.

The heart and lungs were preserved in a wet preparation. On the 15th of November, five months after the operation, Dr. Neilson and myself made a more minute examination of the cavities of the heart, when, to our surprise, we found, in the right auricle, and attached to it, a substance resembling in all respects the smaller steatomatous tumors, having an appendage two inches long, extending into the superior vena cava. This substance weighed full two drachms.



ART. V.—VISIT TO THE BROOKLYN, (NEW YORK,) ORTHOPÆDIC INSTITUTION.

BY J. C. NOTT, M. D., MOBILE, ALABAMA.

In calling the attention of your readers to this Institution, of Drs. Bauer and Barthelmess, I feel that I am not only doing an act of justice to two gentlemen of high professional merit, but good service to the cause of humanity. It is conceded on all hands that establishments of this kind, when well conducted, are of immense utility; and, with shame be it spoken, so far as I can learn, the one recently commenced at Brooklyn is the only one in the United States.

Few surgeons, even of our large cities, possess knowledge enough of the treatment of deformities and diseases of joints, to "know their own ignorance;" and at the South, very few are capable of treating a simple case of club-foot, to say nothing of diseases of the hip joint, of the spine, and other affections of this class. The reasons for this ignorance are obvious. The surgical works in common use give us little information respecting them; they require a vast deal of time and detail in their treatment; they cannot be managed without complicated and varied mechanical apparatus. The cases in the hands of the general practitioners are few and far between; and in our medical colleges their management, pathology, &c., are scarcely alluded to. In Europe, the treatment of

deformities is a speciality, and so it must be in this country, before competent knowledge can be attained.

Let us instance the very common case of club-foot, which every practitioner, in town and country, has frequently presented to him. We all remember how, but a few years ago, the press was teeming with books and essays on this subject, and how certain writers, to make capital for themselves, were deluding the inexperienced by extravagant statements of speedy cures. It was taught, that by cutting a tendon or two, and using the shoe of Dr. A, B or C, not only this deformity, in young children, but in adults of any age, could be overcome in a few weeks, and the fortunate subjects brought into a perfectly normal state. But the delusion is dying away, and those general practitioners who, like myself, have tried the experiment, must candidly confess that their success has been by no means in proportion to the trouble, expense and annoyance which they have gone through. These deformities are infinite in their kind and complications, requiring apparatus of different sizes and construction, and incessant attention for weeks and months. For my own part, I have, for several years, abandoned the treatment of club-foot, as a thing utterly incompatible with my other and absorbing professional duties.

The foregoing remarks apply with equal force to affections of the hip joint and spine, as well as many other deformities arising from diseases of muscles, joints, bones and cartilages.

Drs. Bauer and Barthelmess have the highest professional education which Europe can afford—have had long practical experience in orthopædic institutions in Germany and England, and their merit is fully vouched for by leading medical gentlemen in the city of New York. I was struck, in my first visit, not only by their full mastery of the mechanical part of their treatment, but by many of their physiological, pathological and curative views. My space will permit me to allude particularly but to a single point of great practical interest, and one which will be entirely new to most of your readers, viz: their treatment of *suppurating joints*.

It has long been an axiom in surgery, that joints cannot be opened without serious danger to life! But, while these gentlemen fully admit the principle, as applied to a *healthy* joint, they assert, that in a physiological and pathological point of view, there is nothing in common between a *healthy* and a *suppurating joint*. If we plunge a bistoury, for example,

into a healthy hip joint, every one knows that there is danger of high inflammation of the synovial membrane; other tissues become involved, constitutional disturbance comes on, and death may follow. On the other hand, suppose we have to deal with a chronic disease of the hip joint, which has already passed through the various stages of inflammation—has suppurated and ulcerated—not only the sensitive synovial membrane, but the cartilages, ligaments and bones are broken down in structure, and even air, (so much dreaded,) has entered, through a spontaneous opening, into the carious joint—what course, let us ask, in such a case would common sense and sound principles dictate? We have, in fact, no longer a *joint* to deal with; the tissues which characterize such cavities are either changed or destroyed, and in their place we have an abscess, enclosing the ends and debris of two carious bones. Is it not proper, at once, to make free openings and to remove the offending mass of pus, dead cartilages, bones, &c., then cleanse the cavity thoroughly, and thus give nature her only chance to repair the injury, by healthy granulations? Is not the proposition so plain as to strike the understanding of any one who will reflect upon it for a moment? This is no longer an hypothesis. These gentlemen have reduced this important principle to practice, and demonstrated, fully, its soundness.

I might say much about the rules of diagnosis,—the position in which the limb should be placed,—the mechanical apparatus to be used, but space does not permit. I must not, however, pass one remark, viz: the great value of chloroform in these cases. Not only may joints, under its influence, be freely opened and cleansed without pain, but all the motions necessary for previous examination and diagnosis. A patient, under the full influence of chloroform, may have the limb freely rotated, the joint opened and cleansed of the detritus, and the position suitably arranged, without suffering.

However great the difficulties in the treatment of club-foot and hip disease may be to the general practitioner, these almost sink into insignificance compared with spinal diseases. It was in this department I was most forcibly struck with the necessity of orthopædic institutions. Leaving aside the all important constitutional treatment, these cases have to be managed, first, by taking accurate plaster casts of the back of the deformed subject, which is to be placed before the mechanic who is to make the required apparatus. A metallic frame-work is then to be fashioned to fit it,

so as to support the parts perfectly in their proper position, and to allow the patient to be carried about, exercised in the open air, &c.

This establishment is located in a fashionable and most healthy part of the city of Brooklyn. It consists of three adjoining, large brick buildings, connected in the rear by a veranda. The rooms are spacious, airy, and lighted with gas. There are well furnished parlors, newspapers, books, &c. In the garden there is a gymnasium erected, for exercise. The physicians are always in attendance. Mechanics reside in the house, to meet all exigencies in their line; and nothing is wanting for success. Letters addressed to Box No. 147, Brooklyn postoffice, New York, will meet with prompt attention.

In a sketch like this, I can only direct the attention of readers to this Institution. I have no doubt many will be glad to learn that such a refuge exists from this terrible class of maladies.

I will here express the hope, that Dr. Bauer (whose personal acquaintance only, I formed) will, ere long, favor the profession with a work *in extenso* on the treatment of deformities. For, though difficult cases, to be well treated, should go to him, yet, many patients cannot afford to go so far, and, with a proper guide, the general practitioner might do much good in the simpler forms of these deformities.

ART. VI.—THICKENING OF MEMBRANA TYMPANUM, CAUSING COPHOSIS.

BY DR. JAMES COWLING.

Mr. B., aged 44 years, planter, applied to me on February 15th, 1854, on account of deafness in his left ear. His statement was, that, about four years ago, he, without thinking about being deaf, was in the habit, during conversation, of turning his head partly round, so as to have the right ear nearest the speaker. At first, this was but little noticed; in less than a year, however, it was so remarkable as to call forth observation from his friends, that he was certainly becoming deaf. Which so far proved to be correct, that he could not hear with the left ear, except very loud noises. Cannot recollect ever having had pain in the ear, and, to use his own expression, was never sick in his life. On examining the ear with the speculum, I

found an unnatural dryness in the membrana tympani, instead of presenting its usual appearance of concavity, together with a whiteness; no cerumen. On introducing a probe, passing it to the bottom of the meatus, it did not cause the slightest uneasiness. From the examination, I concluded that the cophosis depended upon a morbid secretion, or a thickened state of the cuticle. The eustachian tube was not examined, the patient not being willing to submit. The indications were, however, plain as to what treatment should be adopted, as no benefit could be expected unless those abnormal deposits could be removed.

For this purpose, after surrounding the parts with almond oil, I introduced, with a camel's hair pencil, a thick layer of ungt. oxyd. hgd., depositing it on the apex of the convexity.

February 17th.—After syringing the ear for sometime, examined it, but could see little difference, except that the membrane did not appear quite so white. Introduced the ungt. as before.

Feb. 19th.—After syringing freely, found the ear in much the same state as before. Continued ungt.

Feb. 21st.—On examining the ear to-day, and not finding any improvement, I substituted the following: R. cupri. sulph. gr. xxx, aqua dist. ʒ ss, M., apply with camel's hair pencil.

Feb. 23d.—After syringing with warm water, the membrane appeared roughened, or, I should rather say, the deposit on the membrane. Repeat. This treatment was continued for three weeks. The lotion was applied every second day; syringing the same. At the end of which time, small flaky exfoliations began to be detached, without, however, any improvement in hearing.

April 15th.—On examining the ear to-day, found the convexity much less. A large quantity of thin flakes having been removed, the parts feeling very tender, discontinued treatment for a few days.

April 29th.—All tenderness being removed, I directed R. argt. nit., gr. xx, aqua ʒ ss, M., of this about half was thrown into the ear, the surrounding parts having been previously protected by almond oil. This had the effect of blackening, to some extent, the epidermis of the meatus. Syringe with warm water daily.

May 2d.—A good effect has been produced by the argt. nit., the exfoliations separating more rapidly. Repeated the syringing, and

threw in the other portion of the lotion. Complained of some pain after the injection.

May 5th.—R. argt. nit., grs. xx, aqua dist., ℥ ss, M. Of this, half was thrown into the ear; to be syringed with warm water every day.

May 8th.—After syringing with warm water to-day for sometime, the patient complained of a good deal of pain and unpleasant noise. Upon examining the water carefully that had been used in the injections, I found a rather thick, flaky exfoliation, sufficiently large to cover the tympan, the separation of which from the membrane, no doubt, caused the pain and sound. There had, however, up to this time, been no cerumen secreted. Considering the main obstacle now removed, and that by applying stimulants to the now more sensitive tympanum, and thereby assisting nature to bring the parts to their normal state, and thus enable them to perform their functions in the economy, I gave R. ungt. hyd, biniod. After being melted by a gentle heat, it was applied by means of a camel's hair pencil to the membrane and surrounding parts. This was repeated every second day. At the end of fifteen days the cerumen was natural and all traces of eophosis had entirely disappeared.

The above case I do not think void of interest, as cases of a similar nature are frequent causes of partial and entire deafness. Such cases frequently admit of relief and cure, thereby conferring an inestimable benefit on those who have been deprived of this blessing.

In this case there are two things remarkable: First, the covering of the tympanum; secondly, the absence of all symptoms, to account for the great thickening of the membrane—as the patient could not recollect ever having had pain in the ear; was not subject to head aches or colds, In fact, his general state of health was that of a robust, muscular man. There can be but little doubt that this eophosis was caused by one of these low chronic inflammations, but of such a mild character as to escape the notice of the patient for a length of time—in fact, altogether, as all that he wished my advice for was the deafness, which he appeared to have been made aware of by his friends.

This deafness of one ear is by no means uncommon. I know several gentlemen in this city who hear imperfectly with one ear, and who

will always in conversation bring the head round in such a manner that the normal ear shall be nearest the speaker.

The convexity of the tympanum, in the above case, was caused by the great accumulation of a peculiar, white, scaly matter, adhering to the membrane, such accumulation being sufficient to cause this organ to be incapable of its natural functions. In several instances, I have met with this kind of scaly matter adhering to the membrana tympanum, with this difference, that they had been most frequently accompanied with cerumen, and, not unfrequently, by a thin, whey-like discharge. Mr. Earle, and others, who have paid much attention to this branch of surgery, mention it as not uncommon, and as being frequently amenable to treatment. In the present case, ephosis had existed about four years; and after four months' treatment, all deafness disappeared.



ART. VII.—USE OF CHLOROFORM IN A CASE OF PUERPERAL CONVULSIONS.

BY DR. J. R. DOWLER, OF BEARDSTOWN, ILLINOIS.

September 15th, 1854, Mrs. Hageman, aged eighteen years, after a natural and rather easy labor, under the care of Dr. Sprague, was delivered of her first child. Between two and three hours after delivery, she was seized with convulsions, which continued to recur every twenty or thirty minutes, attended with strong muscular contractions—eyes rolled up; clenched jaws; (tongue having been bitten in first fit, was afterwards guarded by substances interposed between the teeth;) face bloated; breathing sterterous; frothy saliva issuing from the mouth, a gush of it being expelled at the commencement of each fit; unconscious in intervals of paroxysms. Such were the facts of the case, when seen by me, about nine hours from the commencement of the convulsions. Dr. Turpin and Ehrhardt had preceded me in the consultation. The treatment adopted had been bleeding, cold to the head, sinapisms to legs followed by the administration of opium, and other anti-spasmodics—all to no effect as to any controlling influence in the case. The effect of a further loss of blood was agreed to be tried between myself and Dr. S., (the other doctors being ab-

sent, at the time). The old orifice was re-opened, and near a pint of blood allowed to flow, when the faltering beat of the pulse gave us warning to desist from any further draft upon the sinking powers of life. After a short delay, for deliberation, the paroxysms had increased in frequency and deadly force. The symptoms seemed to indicate impending dissolution, without the interposition of some powerful agency to arrest the march of disease. Recollecting to have seen some recent notices of the successful use of Chloroform in the treatment of puerperal convulsions, I was led to propose the use of that agent in the case. Meeting the approval of Dr. S., it was applied, just as she was coming out of a severe fit. Under its influence, the muscular contractions and twitchings calmed down; breathing became less sterterous and labored; all painful symptoms soon disappearing. She was kept under the influence of the anæsthetic from the time of its first application, five o'clock. P. M., until midnight, at which time its use was discontinued—Dr. Ehrhardt having remained, from eight o'clock, P. M., to keep up the impression of the remedy. The natural sensibilities had returned at the end of some three hours. There had been no return of the convulsions from the time of the application of the Chloroform. As the sensibilities returned, there were some muscular twitchings and nervous jactitations, which were relieved by a few partial applications of the remedy, during the forenoon of the succeeding day. There seemed to be nervous depression and some confusion of the intellect for about two days, attended with some feverishness, which passed off by the use of a cathartic and light diaphoretics.

I propose to offer no theoretical views in reference to the pathology of the disease in question, nor of the *modus operandi* of the *remedy*, that seemed so plainly to arrest the rapid march, in this case, towards an apparently fatal issue. The facts, however, would seem to warrant the hope that future experience may prove Chloroform to be a reliable agent to disarm this malady of its deadly power.

ART. VIII.—PERFORATION OF THE DUODENUM AND APPENDIX
CÆCI.

BY J. B. POWELL, M. D., OF LOUISIANA

Permit me to impose on the patience of the reader with the following account of a very interesting case which recently came under my practice. The subject of this case was a negro man, aged thirty years, the property Dr. B. Ballard, of Caldwell parish. He has been sick all the spring and summer with light fever and chills. During that time, has had repeated doses of calomel and tart. emetic, which generally had the desired effect. In conjunction with this treatment, has had very liberal doses of quinine. But did not seem to derive any benefit from this treatment, only for a short duration.

Before premising the history of this case, I will state, that I learned from Mr. R., the overseer, that the boy would work a few days, and then come back with fever; would very often tell the negroes on the plantation, that something was growing or giving way on his insides. He was suddenly taken sick, at his work, about midday, September 14th, 1854, with a most exerceiating pain in the bowels, accompanied with vomiting, which consisted of food he had taken for his breakfast that morning. Feeling a great solicitude on my part for the recovery of my patient, as my friend, the doctor, was absent from home, I, immediately on my arrival, which was late in the evening, made inquiry of Mr. R., the overseer, and also of the woman who had charge of the sick, what he had eaten for his breakfast that morning? and was told that he drank copiously that morning of sour and bitter milk, from the cows that had been feeding on the bitter weed—the botanical name of which I am not acquainted with, but believe it comes under the order of *helenium tenifolium*. It is indiginous to our Southern States, growing abundantly on the commons of Ouachita and Red River, and also around the towns of Monroe and Alexandria. At 6 o'clock, p. m., I found him vomiting, and occasionally making effort to defecate matter tinctured with bile, attended with very great distress about the right iliac region, which continued through the night. Seeing that it was a violent case of bilious colic, connected with

more or less acute peritonitis, caused, as I thought, from the copious draught of bitter milk taken on an empty stomach that morning, and seeing that he was constantly vomiting or retching, I immediately gave him an emetic dose of ipecac, which had the happy purpose of discharging an enormous quantity of sour fluids, like green tea infused in a solution of indigo. I must say, in my three years' practice of physic, that my olfactories have never come in contact with any secretion, either from the living or dead body, that was so offensive—so much so, that I could scarcely keep within the cabin. I then applied six cupping glasses over the region of the abdomen, and they drew admirably in a few moments. Gave him twenty grains of calomel combined with two grains of opium, and then left for home, having ordered the nurse to give him a dose of oil and twenty drops of oil of turpentine the next morning.

Called the next morning early, September 15th, and found that he had but one operation through the night, and that was very ichorous. Gave him an injection, consisting of warm water, half pint, turpentine, in the quantity of half a fluid ounce, with a small portion of castile soap. In a half hour, had a copious discharge, consisting of bile, mixed with hardened scybalæ, which was very fetid. Applied a large mustard plaster over the abdomen, and ordered opiates to be given through the night.

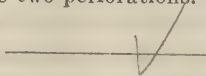
September 10th.—Found his symptoms increased; his pulse 140; his strength much exhausted; his eyes sunk in their sockets, appearing fixed; his mouth was dry and parched; tongue stiff and faltering, and his voice low and hoarse. He seemed very listless, and was under the impression that he must still evacuate a load of noxious matter, which, he said, caused, in his abdomen, a disagreeable and uneasy sense of fullness, although he had already vomited freely. His extremities were now getting cold, and clammy sweats appeared over his face and arms. Gave stimulants freely through the day and night. But, in defiance of quinine and diffusible stimulants, he expired on the night of the 10th, at 3 o'clock, preserving his mental faculties to the last moment. The duration of his suffering, from first to last, was only fifty hours.

Autopsy, five hours after death. Surfaces of the body very pallid, and all the abdominal muscles in a perfect state of rigidity. Abdo-

minal parietes protruded over the course of the colon, and especially about the duodenum and cæci.

In cutting through the abdominal parietes, I found the peritoneal covering and its contents to be in the highest state of inflammation. Large patches of pus were found between the intestinal convolutions, the great omentum being in a highly distended and partially adherent condition. The colon and cæcum were distended with gas and dark blood. In examining the liver, I found it completely indurated and bound down by old adhesions to the diaphragm above, and on its concave surface, to the transverse colon. Such being the condition of the abdominal organs that I was completely astounded at such a great amount of mischief arising in so short a time, when, upon inspecting the duodenum, the cause appeared at once. A perforation, about the size of a dime, one inch below its pyloric extremity, with serrated edges, and somewhat reddened, was observed. Also found eight large intestinal worms impacted just below this aperture. The lungs were healthy, and also the heart. In the right ventricle there was a large clot of dark blood. Upon close examination, I found the appendix cæci greatly enlarged, so as to admit the introduction of my index finger. The internal coats were perfectly soft. The appendix was adherent to the internal iliac artery. I then laid open the artery, and, just below the commencement, a small round opening came to my view, communicating with the appendix, through which clots of blood regurgitated. There was a small deposit of lymph just around the aperture. Such being the nature of this boy's affection, after a careful dissection by myself. His hasty dissolution was caused by a direct communication between the two canals.

There are several physiological points of interest in this case: The mental faculties being preserved to the last moment; 2d, the peculiar character of the two perforations.



ART. IX.—OBSERVATIONS UPON DISEASES OF THE UTERUS.

BY THOMAS E. MASSEY, M. D., OF MOBILE.

The character and extent of the influence exerted by the observations and conclusions of physicians of established reputation, are not easily defined or estimated. Their published views in reference to

subjects in course of investigation, form stand-points, from which the reflections and inquiries of numberless minds take their departure; and according as their foundations are reliable and secure, are their tendencies happy or deplorable, to the profession and humanity. Hence, while the increase of a physician's reputation augments his obligations to exercise caution and deliberation in the enunciation of his opinions, it becomes the more important that their merits be faithfully examined.

Such thoughts have been suggested after the perusal of a communication under the above title, in the November number of the New Orleans "Medical News and Hospital Gazette," by Dr. Warren Stone. Should an apology be deemed appropriate for the notice it is proposed to take of this communication, and especially as it may be judged to be rather elaborate in reference to an essay of hasty and un-studied composition, its authoritative constructions and the spirit of our introductory remarks, are offered.

The design of Dr. Stone's article is, doubtless, a laudable one—a caution, or protest, against the misuse or abuse of perhaps the most valuable contribution to modern pathology and therapeutics. Any degree of failure in the execution of such a purpose is a fair subject of regret. It is a theme worthy of laborious and powerful effort, but which demands a mastership of all the knowledge and observation of others, tempered by a faithful personal experience. A careful scrutiny of the elements of abuse or misconception, and a discreet exposition of their capacity for good or evil, are at variance with superficial generalizations and unsoundly declared conclusions.

The second paragraph of Dr. Stone's communication begins thus: "The conclusion I came to long ago, was, that either this organ (the uterus) is not governed by the same physiological and pathological laws as the other organs of the human system, or the writers on the subject have been blinded by their zeal and carried away by theory. And further observations incline me to the opinion, that men have been mistaken! * * * * Such minds can scarcely be content with plain matter-of-fact pursuits, but rather fix upon something that admits of speculation, and something more wonderful than reality."

To assert an entirely satisfactory comprehension of our author's position here, would hardly be safe, scarcely plausible. The attempt

must be made, however, to state the impression conveyed—all responsibility as to its identity with the author's intentions, being expressly disavowed. Dr. Stone *concluded*, "long ago," either that the uterus *was not* just like every other organ, as he had always supposed; or, that *it was*, and "writers had been carried away by theory" in giving it any particular attention. And, after having so *concluded*, "further observations incline him to the opinion" that he was right! This, then, must be understood to be a polite form of avowal of the opinion that the uterus *has* the same physiological and pathological laws as every other organ in the economy! And the natural inference must be, that Dr. Stone is "inclined to the opinion," that the expositions of Dr. Bennet, and other late uterine pathologists, are all balderdash! But, upon reading seven or eight pages further, suspicions arise as to the legitimacy of this inference; for the essayist gets to dipping incontinently and quite extensively into granulations, engorgements, nitrate of silver, and all that sort of thing!—in the manner, it is true, of one who deems them of slight importance, but had always been familiar with them—who is of little faith, but had been at considerable trouble to draw pathological distinctions, and select remedial applications, in addition to those of the delvers "after things more wonderful than the reality!"

But, passing from these recondite developments, it is worthy of remark, that modern uterine pathology introduces us to its study, where we have long been taught to begin that of other organs—inflammation. This inflammation it exposes to our *sense* of sight and touch. This scarcely evinces a less "matter-of-fact pursuit," or partakes more of "speculation," than the older pathology, in its floundering among but signs of disease! A rehearsal of the remedies for functional derangements of the generative apparatus, which were treated as the essence of morbid action, is unnecessary for the illustration of the relative preponderance of conjecture. The pages of any treatise on the "diseases of women," written previous to the last ten years, will be satisfactory on this point. Recent writers may have been, to some extent, "carried away by their zeal;" but, with less hesitation may it be asserted, that recent *practitioners* have, with avidity, caught at what they trusted was an easy solution of a multitude of difficulties. Each class will right itself after awhile. Men are prone to extremes; and progress in medicine, as well as in other things, wonderfully attests

the value of this trait of human nature. One extreme begets its opposite, and, in the attrition, truth is sometimes eliminated. Uterine pathology will probably lose nothing by expressions of adverse opinions.

In reference to the question of the government of the uterus "by the same physiological and pathological laws as the other organs" of our bodies, the profession is judged to be pretty unanimously arrayed in the negative. Our information induces the faith, that the elements of our organization assume different and specific combinations in the structure of our various organs, that each may be adapted to the functions assigned it. The functions of organs being distinctive, and their anatomy of course necessarily so, the creed of specific modifications of pathological laws, is easy and natural. Simple inflammation produces different symptoms, pursues a different course, and demands modifications of management, according to the tissue implicated; and, further, its laws and treatment vary according to the uses of the organs invaded. Will the modified character of inflammation, which we must study, for success in treatment, as it appears and is treated in the eye, the lungs, the liver, and the skin, suggest itself to our readers? The uterus is more distinctly appropriated and confined, in our apprehension, to one specific end than perhaps any other organ of the human body, and characterized by greater complexity in its adaptation. Instead of enumerating here its peculiarities of structure, position, and functions, let them for a moment be recalled, and wonder will cease that its pathological state should assume expressions requiring special study.

But, a word more. Not only has the uterus a physiology and pathology of its own, but he must have but limited experience who is not often struck by grave differences in the uterine structure and laws of individuals, necessitating, in disease, as many and important modifications of treatment as individual cases of any disorder known to the human economy. It may, perhaps, more truly be said of the uterus than of any other organ that, in disease, it requires comparison with itself, and not with another uterus; and the diversity of symptoms are only to be explained by the habits of health. Identical uterine symptoms seldom proceed from identical lesions. An amount of disease originating intense suffering in one woman, in another may produce no consciousness of its existence. The very shape, size, and relative pro-

portions of the uterus vary in different subjects, and all degrees of skill and experience cannot equally discriminate a healthy from a diseased uterus. The uterine neck varies, in health, in length, breadth, shape, position and consistence, and, oftentimes, in the same person at different examinations; and what is a diseased *function* of one, is normal and constant in another. A profuse menstruation to one, is natural to another. Menstruation is limited to two or three days, with some women; that of others extends to six or eight days—the quantity discharged being as disproportioned as the duration. Many women have borne large families, without having ever menstruated. That sensibility of the uterine cervix varies, independently of amount or character of disease, is evident from the different effects of the simple touch, or contact of remedial agents. From these and many other considerations, the writer's faith is strong, that the uterus is not exactly like any other organ!

Dr. Stone says: "The diseases of the uterus, to my view and experience, are *simple*; and, if the natural efforts are encouraged, are strongly disposed to recover!" Here, quite an extensive train of interrogations and reflections suggest themselves. *Are* the diseases of the uterus simple? If so, in what consists their simplicity? Their well defined and constant symptoms—their limited number—their invariable, identical relationship to specific, appreciable lesion, and exactly defined amenability to simple treatment? Are they simple in their origin, progress, in their relations, complications and consequences? Or does their simplicity consist in diagnosis and prognosis—in the facility, speed and certainty of cure? Are they disposed, of themselves, to resume a healthy condition? And, if so disposed—not in complete independence, but only by our encouraging natural efforts—is the manner in which we should tender our assistance well defined, simple, and readily applied? All these little preliminaries, and several others, must be adjusted, before the probable attainment of any very general assent to the doctrine of simplicity of uterine diseases. In truth, they exact but little familiarity with their manifestations to convince, that precisely similar symptoms may be present both with and without appreciable physical alterations; that identical local affections seldom produce identical symptoms; that what is either a functional or organic derangement in one instance, is a perfectly normal condition in another;

that appreciable morbid alterations of the uterus, and its cervix, are sometimes amenable to constitutional treatment—and at times to that mode alone, while in other cases they are totally rebellious to all save topical medication; and that remedies, for local application, must be greatly varied and judiciously chosen for various lesions and their stages, and for the general condition of the patient. These items are not very promising advocates of the doctrine of simplicity! Interrogating the most simple form of uterine disorder concerning which we are consulted—a morbid cervix uteri, accused of all the ills complained of—departure of all ailings, consequent upon restoration of the cervix to health, by means of the most simple local treatment—and we are presented with a case, than which the afflictions of humanity offer none more disagreeable to the practitioner; none more of a tax upon his energy, industry and patience! And when, to all these suggestions, is added one alluded to by Dr. Stone—that some sagacity or experience is requisite, to discriminate, habitually, a healthy from a diseased uterus—and that a fold of the vagina occasionally suffers—our admiration of the simplicity of uterine diseases is ineffable!

Are diseases of the uterus disposed, of themselves, to recover? It is a very interesting faith with many, that the evidences of disease are but indications of nature's efforts towards a cure, which she often makes desperate attempts to accomplish, and requires not the interference, but only the assistance of our art. With no wish to disturb this pretty conceit, doubts obtrude themselves as to such happy ascriptions to uterine lesions. It is very true, that the majority of females have, during the active part of uterine life, some more or less prolonged and severe derangements of that system; but how is it given us to decide either as to the final or approximate cause of such troubles? On the other hand, the cases of uterine disease presented for special treatment have been of previous considerable duration, from one year to twenty years; and every kind of available testimony generally establishes, a progressive increase in extent, complexity, and severity, instead of improvement! True, this is admitted to be frail evidence of benignant nature's first intention! and it is a matter for regret that more or other testimony is not attainable, especially in the direction of the faith of Dr. Stone; for, as nature mostly does her work very well, and as our best assistance is generally most wisely tendered by letting her alone,

we should have but little distress in controlling uterine diseases! Nature has most excellent laws, doubtless, and they may be most carefully indexed; yet, the distinctions between her own salutary efforts, and indications of the destructive processes, might be more distinctly disclosed to the cultivators of our art without disadvantage.

In the course of the article under review, it is asserted, that "in the examination to ascertain the state of the parts, the sense of touch is far better than the sense of sight." This is unquestionable; the touch is more extensively available than the sight; but no necessity exists for imposing all the responsibility upon one sense, when a second can also be pressed into service. The one is often desirable for the correction of the errors of the other; and sight is essential to local medication. The speculum is valuable in diagnosis—indispensable in treatment; but, for the welfare of some unfortunate patients, had better never been invented. Some, doubtless, "to appear wise and scientific," (for different people have different ways of creating impressions,) "make a display with the speculum;" but these are equally "charlatans" in most other respects, and as such are generally so well known and "abominated" by the rest of the profession, that they are illegitimate subjects of argument.

After long experience, Dr. Stone "became satisfied, that where there is no general disturbance of the system, all symptoms of disease of the uterus are confined to the organ itself." This may be true in a very limited sense; and regret is experienced that more precision of expression is not employed. If there is no "disturbance of system," preceding and causing, accompanying or consequent upon, the uterine disorder, it is, at least, *probable* that all the symptoms *are* confined to the organ itself! But, if preëxisting ill health shall have produced uterine disorder, the reaction from the latter will exasperate the former; there will be new symptoms, indicating the new source of irritation. Again, some functional derangements originate in organic change. What brought this last about, it boots it nothing here to inquire. These derangements have, time out of mind, been known to produce various depravations of general health; and are now known to be remediable by treatment directed to the cervix uteri. Chlorosis and amenorrhœa have been associated in the medical mind for some years! Dysmenorrhœa and menorrhagia, (signs mostly of local disease,) are suspected

of producing symptoms not "confined to the organ itself!" Leucorrhœa, (in the misapprehension of an effect for a cause,) has been accused of some quite grave, distant "disturbances!" While hysteria and neuralgia, of as violent and extensive character as ever witnessed, have retired before skillfully conducted assaults upon the os uteri! Judging by the mode of avowal of a want of confidence in *uterine sympathy* with distant organs, the suspicion is aroused that, perhaps, no friendship is entertained for the *word*. The remark occurs, that "the occasional nausea and irritation of the stomach, in connection with disturbance of the uterus, proves nothing, except that constant and persistent irritation in any part of the system may disorder the functions of the stomach." Precisely! An injured *toe* may occasion vomiting. But, probably the toe is not so often afflicted, or, if so, does not so constantly produce this specific effect—or for other grave reasons, too tedious to explore; but we certainly much more frequently detect derangement of the stomach in company with a diseased uterus, than with a distressed toe. It is added, that "it no more proves *sympathy*, than the disturbance of the stomach and bowels of the child during teething, proves sympathy between these organs and the gums." Not a whit more!—probably just about as much. Some other *word* might, probably, arrange this little matter. It is, however, unquestionable, that whatever diseases may be simulated, or however aggravated may be distant symptoms, when the uterus is implicated there are always ample evidences of its complicity clustering about its locality. And in all indications of uterine implication, in the sufferings of a patient, enlightened pathology demands as thorough examination of its intrinsic condition as its nature and position admits, for the purpose both of invoking every aid to a comprehension of the mysteries of disease, and the assistance of direct remedies when available.

As to the treatment of uterine diseases, as far as Dr. Stone's views are shadowed forth on this head, our notions are less conflicting: Specialities of treatment, in their restricted sense, find no advocate in either the communication under review, or its reviewer. Experience and sound pathology alike sanction a judicious selection from our copious pharmacopœia, of special classes of remedies and even particular drugs, for the eradication of disease, accordingly to the peculiarities of the organs, functions, and tissues involved, and of the entire organi-

zation. The uterus claims a compliment similar to that rendered to every other leading organ of the body—some special adaptation of remedies to its peculiarities of structure, position, and uses. Opium, mercury, and antimony, though invaluable, as a general rule, in the treatment of inflammations, allow of no exclusiveness or invariable application, circumstances often forbidding the resort to either, or strongly urging a selection. The nitrate of silver is no more a specific for diseases of the uterus, to be used in all cases, and to the exclusion of other remedies, than antimony, in pneumonia. And although Dr. Henry Bennet is as deserving of immortality, and is as accurate and complete in his development of diseases of the uterus, as Lænnec, in reference to those of the lungs—and as such, it is abhorrent to our every sense of justice and propriety to have his name flippantly passed by or not alluded to by neophytes in uterine pathology, the whole direction of whose late ideas is owing to his labors—yet, it is not a matter of doubt, that he will be the most successful uterine practitioner, who, after thorough examination and decisive diagnosis, turns for assistance to “William’s Principles of Pathology,” instead of to “Bennet on the Uterus.” A specialist is honorable, infamous or contemptible, according to his instincts, purposes or brains. And specialism, in reference to the uterus, is valuable for diagnosis, and of little worth for treatment.

There are other positions of Dr. Stone, about which expressions of assent or dissent would be agreeable, but the designed length of this article is already exceeded, and the writer’s own views and observations will probably form the subject of a future publication. To all advanced in reference to the frequent constitutional origin of uterine diseases, and constitutional indications of treatment,—to the “dangers of those engaged in specialities losing sight of general pathology and therapeutics,”—the advantages, in certain cases, of mercury, of quinine, &c.,—the condemnation of leeches and pessaries,—the writer of this is a cordial subscriber. The writer’s design has been, with the highest respect for Prof. Stone, to enter a more or less earnest protest against, in his opinion, dangerous teachings for those with whom the authority of Dr. Stone is preëminent.

The successful management of disease exacts an exalted appreciation of its importance, and the application of the physicians’ most

enlarged and vigilant abilities. Any instruction tending to promote superficial attention, or diminish the estimate of the requirements of our art, derogates from the character of our profession, and is of demoralizing influence upon its members. The Southern representatives of the healing art, should, especially, receive every stimulant to a thorough investigation of the nature of disease and its peculiarities. Thoughts which pander to our indolence or restrict our efforts should be studiously withheld, and our difficulties, preferably magnified, that the standard of excellence may be elevated. Not only the effects of climate, in directing the modifications of disease, solicit our best capabilities, but Southern pride, duty and interest appeal to the physicians, as well as to the merchants, planters, and politicians of the South. Our invalids throng the Northern resorts, and lavish their thousands in search of health and diversion. Every town of ten thousand inhabitants throughout our Southern tier of States, can, probably, now number one or more of its afflicted females under treatment at New York. And not one in twenty returns cured! With the lights of modern pathology more generally diffused, to direct the many hands and minds yet required to illustrate the existing mysteries of uterine disorder, and with zeal enlisted in proportion to the importance and complexity of the uterine organization and functions, the difficulties and duration of treatment will diminish, and our family circles be less frequently desolated, by the flight of its invalids for Northern succor; and by our curing them more comfortably and rapidly at home, an efficient and honorable guaranty be secured of one department of "Southern Rights."

Mobile, December 5th, 1854.

ART. X—HISTORICAL RETROSPECTION OF THE FUNDAMENTAL PRINCIPLES AND POLITY OF THE AMERICAN MEDICAL ASSOCIATION

BY BENNET DOWLER, M. D.

The American Medical Association, instituted in 1847, sent forth its first volume of Transactions in 1848, and its seventh during the year 1854. The typography, the illustrations, and many of the papers of the series, seven volumes, have merited and received high com-

mendation; nevertheless, like many other valuable works, they do not pay, as the treasury reports show but too plainly.

Instead of analyzing the seventh volume, mentioned below,* a slight historical retrospection of its primordial principles—its internal, rather than its external history and published Transactions—will be briefly indicated, fearlessly, without treachery, without malevolence, without courting martyrdom from its members and affiliated adherents, yet without a time-serving expediency, which imposes silence or commands acclaim.

The true point of departure for a retrospective history of the Association is found in the first volume of its *Proceedings*. The Association, when viewed apart from its internal primordial characteristics, becomes, or will become, an ex-animate body,—or, at best, an unwieldy and ill-constructed medical society, onerous to its members, who, after traveling thousands of miles, hold a few hasty sessions, under circumstances the most unfavorable for calm deliberation and scientific research, and, after a grand festival, retrace their steps to the remotest confines of the Republic.

The history of this Association is not only highly interesting in itself, but it displays a zeal, disinterestedness, and self-abnegation—a sacrifice of time, money, home, and comfort, for which it will be difficult to account, upon the principles which ordinarily govern mankind in its great movements and voluntary transactions, swayed to and fro, as it is, by passion and interest, wealth and power.

It is true, that membership in this Association is honorable, but, in this point of view, it is scarcely an object of ambition; nor is it, strictly speaking, a learned society, any more than the Congress of the United States. Its transactions, in its scientific matters, are seldom or never read, discussed, or critically examined in its sittings, much less are they sanctioned by the Association, as expressing its official acts or opinions; nay, it has, by a solemn vote, at the annual session in 1851, at Charleston, disavowed even an implied approval of the official report of its own committees, thereby exonerating itself from all responsibilities in this behalf. It has almost invariably referred all scientific papers and reports, without having been read, to the Publication Com-

* The Transactions of the American Medical Association, instituted 1847, Vol. vii, New York: Charles B. Norton, 1854. Pp. 668, 8vo.

mittee. It has occasionally, through committees, offered prizes, which have elicited several able papers, that have been disposed of in a like anomalous manner. Scientific papers, discourses and debates have been incidental, rather than primary objects and official acts.

What, then, is the leading purpose, which impels hundreds of physicians, scattered over more than one score parallels of latitude and a still greater longitudinal expansion, to encounter "perils by flood and field," in order to hold an annual medical congress, whose sessions continue less than a week—involving, in many cases, a loss of time of from two to four weeks, and an expense of \$100 to \$200? It is easy to comprehend the motives which bring together national assemblies, for political, legislative, and commercial objects and material interests, not to name "the cohesive power of public plunder." A higher and holier motive appears to have prompted the originators of the American Medical Association, namely: the cause of humanity and education, as it is identified with the healing art. But, at the same time, it must not be forgotten, in the transactions of mankind, how rarely it happens that a simple, isolated principle, devoid of all selfishness and antagonism, causes a great movement. Many, and even antagonistic motives and principles may concur and coalesce in producing a common result. Is it not so in the American Medical Association? Are not its avowed principles accompanied with a noiseless yet mighty under-current of an antagonistic character, not the less certain and potent because it is seldom seen? Of which more will be said hereafter.

The original founders of the Association saw, or might have seen, with regret, that the Professors of the medical colleges were, in some instances, incompetent, (which, however, they scarcely whispered); that these Professors were either directly or virtually self-appointed and self-perpetuated; that they had a direct interest in lowering the standard of medical education, by making the way easy for a multitude of candidates for the doctorate, which might have a pernicious influence so far as money can influence human action; that, having received the money of the candidate, and having the sole power of examining, and judging of his qualifications, they must labor under a bias; that a rigid examination, and a high standard of preliminary and professional education, would repel, and a low one attract students—a high one bring poverty to Professors, a low one wealth—a high one send forth candidates stigmatized by rejection and hostile to the insti-

tution, a low one a multitude of graduates, friends and partizans. The Association saw the country overrun with these incompetent physicians, holding diplomas, unworthily enjoying all the honors and advantages incident to the same, to the detriment of society and the degradation of the medical profession at large.

The late distinguished Professor Chapman, who probably examined more candidates, signed more diplomas, and made more money thereby than any man in America, and who was the first President of the Association which met at Baltimore, May 2, 1848, opened the proceedings by giving utterance to the great, but, as events have since proved, utopian expectations, saying :

“ This assemblage presents a spectacle of moral grandeur delightful to contemplate. Few of the kind have I ever witnessed more imposing in its aspect, and certainly none inspired by purer motives, or having views of a wider range of beneficence. The profession to which we belong, once venerated on account of its antiquity,—its various and profound science—its elegant literature—its polite accomplishments—its virtues,—*has become corrupt, and degenerate, to the forfeiture of its social position, and with it, of the homage it formerly received spontaneously and universally.* Do not suppose that I comprise the whole profession in this reprobation. There are numerous members of it who still retain the qualities by which it was formerly distinguished. It may, indeed, be affirmed, that never in its history has it exhibited so many claims to respect as at this very moment. With the present century the spirit of philosophy began to be infused into it, creative of real and substantial improvements in its theories and modes of practice, raising it from a low and conjectural art, to a place among the legitimate sciences, by which great good is already attained, and further benefit of inestimable value promised to suffering humanity. Nor have its disciples among us lingered behind in the career of reform and general advancement. Yet the preceding averment of the deterioration of the profession in some of its features, cannot be denied. The truth of it, indeed, is everywhere recognized and proclaimed. Complaints, at first heard only in the murmurs of discontent, are now so loud, distinct, and potential, as not to be disregarded or admit of further postponement. The commission which accredits you to this Association sufficiently attests the tone of professional sentiment on the subject. Does it not declare the fact, that the profession is envired by difficulties and dangers, arising mainly from the too ready admixture into it of individuals unworthy of the association, either by intellectual culture, or moral discipline, by whom it is abased? and are you not imperatively instructed to purify its taints and abuses, and restore it to its former elevation and dignity?

Encouragement is also afforded to the hopes entertained of the success of our enterprise, by the firm, though moderate and dispassionate

temper characteristic of the whole of our proceedings. We have been maddened by no extravagances of enthusiasm; no delirious hallucinations of imaginary perfectibility do we pursue. We are betrayed by no false lights,—and seek, only as an attainable good, in soberness of thought, a reform in medicine, enacted by a proper regard to its future glory and usefulness. From slumbers too long indulged, the profession has at length awoken, and shaking the poppies from its brow, is recalled to a sense of what is due to itself, and the obligations it owes to preserve its fair heritage, to be transmitted to posterity unsullied, and without detriment or loss. Excited by this generous impulse, it comes forward in the majesty of its might to vindicate its rights, and redress its wrongs. To no other tribunals does it deign to appeal for these purposes. No mean petition of grievances, or supplicatory memorials for relief, or more immediate addresses to popular feeling to engage its favor, shall sully our proceedings. We have, in a spirit becoming our just pride, trusted, and will, I hope, continue to trust, our cause exclusively to the clear heads, the warm hearts, and strong arms of the host enlisted in its service. We do not want, nor will condescend to accept of any extraneous assistance. Confiding in our own resources, we shall through them maintain the struggle till conducted to victory and triumph.”

Dr. Wellford read a report from the Committee on Medical Education, which, among other resolutions, embodied the following:

“*Resolved*, That this Association recommend to the Faculty of each Medical School to conduct the final examination of candidates for the diploma, in the presence of some official person or persons properly qualified to recognize the attainments of the candidate, but who have no pecuniary interest in the institution, or in the number of its pupils.”

Dr. Joel Hopkins offered the following resolutions, which were referred to the Committee on Medical Education:

“In order to promote the high purposes of this Association, and give practical efficiency to its recommendations, it is hereby

“*Resolved*, That a College or Bureau of Examiners be instituted to consist of the President and officers, *ex-officio*, and seven of the members, to be chosen annually, and that they be invested with power to confer diplomas on such persons as may apply for the same, provided they sustain satisfactory examinations in all those departments of Literature and Science, which may be deemed by the said Board necessary to belong to an accomplished physician.

“*Resolved*, That those diplomas may be of two grades: *First*, that of *Bachelor of Medicine*, to be conferred on those who may be found to possess the lowest standard of qualifications contemplated by this Association as essential; and, *second*, that of *Doctor of Medicine*, on those who may have previously obtained the degree of A. M. from some accredited College, or who can sustain an examination that would entitle them thereto.

“*Resolved*, That the fee to be required therefor shall not be more than sufficient to defray the cost of procuring and preparing the same.”

In the second volume of the Transactions, (of nearly 1,000 pages,) the Association reiterated its resolutions on Medical Education, “requiring due preliminary education prior to graduation,”—denouncing “private examinations for medical degrees,”—recommending “Boards of Examiners in each State, to examine candidates for license to practise,” and whether “they are familiar with the elementary branches of general knowledge,” &c., &c.

In the same volume, (page 42,) is the following resolution, which passed but to sleep—“perchance to dream:”

“Dr. Evans offered the following preamble and resolution, which were *adopted*:

“Whereas, merit should be the test by which one individual is preferred to another; and, whereas, the places of profit and honor in our profession should be open to the competition of all, in order that the best selections may be made, therefore

“*Resolved*, That Trustees and others exercising the office of appointing Professors in Medical Schools, be requested to adopt the system of *concours*, or public trials, among the means resorted to for calling out the talent of the profession, and ascertaining the qualifications of applicants.”

The special committee, of 1849, on the prolongation of the courses of medical lectures to six months, repeated the fundamental doctrines which called the Association into existence. This committee (Samuel Jackson, John L. Atlee, and Alfred Stillé) made the following statement:

“The medical profession was deeply impressed with the belief, that it had gradually become lowered in its standing. It no longer occupied the high position in public confidence that was once accorded to it. Everywhere it met with successful competition from empirics and pretenders; while absurd, fallacious, and dangerous doctrines were countenanced, not by the ignorant and vulgar alone, but by the educated and intelligent. It was not difficult to trace this abasement of the profession to its true cause. It had ceased to be a highly educated class. In its ranks were found those not only devoid of all pretensions to general science, but many who were absolutely illiterate. He must have occupied a low station, indeed, who could not produce the evidence of a diploma. The parchment, refused in one quarter, could be procured from another. To the imperfect and restricted courses of the schools, and the low standard for medical graduation, were attributed the superficiality and the degradation of medicine. Dissatisfaction

pervaded the profession, and distrust of the profession spread throughout society. The barriers that had separated the cultivated physician from the rude pretender and empiric, were broken down. Before the community, they stood on the same level, bore the same title, and presented the same outward attributes of professional knowledge and skill.

“Such was the feeling on this subject, possessed by the profession throughout the country, that no sooner was the tocsin struck, in a distant and obscure village, by an earnest heart and hand, than it was responded to throughout the land. A wide-spread movement of the profession took place. From that movement this Association had its origin; it stands, in some measure, pledged to accomplish a reform in the medical education of the country. Should it pass by, or fail to render effective, this leading idea of the profession, it will have neglected a chief object of its institution, and will disappoint the just expectations of its founders.”

These views have been reiterated, from year to year, in the meetings of the Association, in different forms, in varied expressions, but with diminished energy—if energy can by possibility inhere in “stale, flat and unprofitable” words,—stereotyped resolutions, which no one expects to be carried into effect, and which, if about to be effectuated, would be voted down by the many who seem to acquiesce (in the reforms which have been *agitated*) so long as the Association amuses itself with wordy potentialities,—abstractions which have no tangible eventualities,—theories that have no practical results.

The last attempt to galvanize the ghastly corpse of reform has just been made by Professor Cabell, of the University of Virginia, in his report, as Chairman, on Medical Education, for 1854:

“The evil in question originated, in a great measure, in the active competition of rival schools; some have contended that it can only be remedied by diminishing the number of these institutions. If this were so, we should be obliged to abandon all hope of relief, and submit to a perpetuity of disgrace; for those institutions now possess chartered rights, which it cannot be expected that they will relinquish. Nor is it probable that more good than harm would ensue from a diminution of the number of the schools. If students could obtain admission into the ranks of the profession *only* after giving such satisfactory proof of attainments and mental discipline as would test the thoroughness and the judicious character of the instruction they had received, the rivalry between the separate schools would consist in efforts to elevate the standard of medical education by enlarging the curriculum with the progressive advancement of the medical sciences, and by perfecting the means of illustration and exact demonstration. *The only available*

remedy, then, in the opinion of the Committee, is the establishment of boards of examination, distinct from the faculties of the schools. It does not suffice to have a committee of the State Society present at an examination conducted by the Faculty of the school. The desideratum is to have a uniform standard throughout the Union, or throughout the limits of each of those States in which such a board may be appointed."

In the origin and progress of the Association, there was, and there always will be, a fundamental principle—not the less potent because concealed—a mental reservation, which expediency sanctioned,—a secret, which policy dictated, unwritten law, (*lex non scripta*,) which every professor and every friend of the existing organization of the medical colleges carried in his own bosom—namely: that the Association is, was, and ever will be, virtually, in the hands of its most friendly enemies; friends nominally—enemies from the implied revolutionary nature of the reform movement,—enemies who, as yet, have no interest or wish to declare themselves such,—enemies whose "masterly inactivity" proves their sagacity,—enemies who must be enemies so long as self-interest, ambition, power, fame, and wealth, can influence human conduct. In a word, a reform in education must reach the teachers. Nor can seven years of professed diplomatic silence in their behalf, nor seven years' declamation against the ignorant students, fortified by an annual battery of resolutions, change the nature of the case, nor "elevate the standard of medical education." "The outs" will have to stay out, and the "ins" will stay in, as long as possible. Circumstances of expediency, rather than inclination, cause the latter to become the allies of the former; so that the whole column of students will be enfiladed once a year with a *fusillade* of the most deadly resolutions, by the allies.

Moreover, the acquiescing—yet, inly opposing cohort of reformers, the right wing of the Association, that is, the actual coalition, (not intending Professors,) may think, and conscientiously too, that the high behests of medical science and the well-being of society can be more completely effectuated by the existing system, than by the proposed reforms, which, to them, may appear altogether utopian. So long as the Association is, and continues to be, a powerless infant in their arms, silence is expedient,—particularly as it regards the two fundamental points, namely: the mode of appointing Professors, and the granting of degrees.

It is true, that Dr. Chapman, when his cloudless sun was setting, after a long and gorgeous day, advocated "rotation in office,"* at the first meeting of the Association. It is true, that Dr. Evans was so eccentric as to propose that Professors should be appointed, as in France, by *concours*, which had always succeeded in obtaining the most essential point, that is, able teachers. But, would not a proposition to all the crowned heads of Europe, requiring abdication, and an open *concours* to all the world for the honors of the throne, be quite as acceptable to kings, as Dr. Evans' proposition, to existing medical potentates and dynasties? It is true, that Dr. Hopkins proposed, and Professor Cabell has just been guilty of the "damnable iteration," (as Falstaff hath said,) that an independent and disinterested national board of medical examiners, separate from the colleges, should be established, for the granting of degrees; whereby it is self evident that the dignity, power and prerogatives of Professors would be completely merged and lost in that of private instructors, whose sole duties would consist in preparing their pupils to become candidates for degrees before a dreaded tribunal, beyond the control of the Professor, and the tuition fees of the pupil.

Let the most radical reformer establish a college of his own, (in Louisiana, by the general incorporation law, any number of men not less than six can do so,†) or let him receive an appointment from the Professors of an existing college, or from a board of trustees, who sometimes exercise a nominal, scarcely ever an actual control,—let him take a professorial chair, surrounded by hundreds of admiring students, and, perhaps, he may venture to think, and sincerely believe, that appointment by the rigid *concour* system of the French is not necessary; that tuition fees and diploma fees can never bias *his* judg-

*In his speech to the Association, Professor Chapman says: "Rotation in office, I am persuaded, is the vital principle of every institution in this country"—a principle very satisfactory to non-official persons, but which is never intended to have a *personal* application to an individual who is once irrevocably installed in a lucrative professorship, such as Dr. Chapman enjoyed for nearly half a century. In science, a pernicious principle it is. Pure science never was, is not now, never will be, popular.

†By an Act to amend an Act providing for the Organization of Corporations in Louisiana, passed April 30th, 1853, "Any number of persons, exceeding six, who may be desirous of forming themselves into a corporation or body politic, for any religious, scientific, literary or charitable purpose, and to acquire and enjoy the rights, privileges and powers of a body corporate and politic, in law," may, on specifying the object of the corporation, pass an act of incorporation before the Parish Recorder, provided the District Attorney "shall be of opinion that the purposes and objects of the said corporation are legal:" and the said corporation may alter and amend the original act of incorporation in like manner, provided nothing therein "be contrary to law." The corporators can compel the District Attorney to give a "certificate touching the legality of the same." Colleges, therefore, might be established by any number of persons not less than six, whether the parties be learned, or unlearned—whether Steam-Doctors, Hydropathists, Eclectics, Homœopathists, Mesmerists, Rappers, or Mormons, *ad infinitum*.

ment, in granting degrees; that his examinations will be as rigid and as conscientious as possible, without *destroying the prosperity* of the institution, and that the greater the number of students, the stronger the evidence in favor of the ability of the Professors,—the incidental emoluments being only a tangible proof to the same effect.

Were the existing Professorships all vacated, and placed in the hands of the reformers, what assurance would the public have that these vacancies would be filled by abler and better men? Would the new men prolong the medical sessions to six or eight months, in opposition to the wishes of their patrons, the students, who think three or four months quite long enough? Would they reject all graduates not possessing a thorough elementary and medical education, knowing that such requirements would bring pecuniary ruin upon such a college, and cause pupils to flock to one better adapted to their wishes?

That sincere reformers and dissatisfied aspirants will annually reaffirm the resolutions on education, or, what is the same thing, repeat the throwing of the tub to the whale, is as probable as it is unavailing. In fact, as already stated, the very first step towards reform—the qualification and appointment of Professors—has been politely ignored, nay, culpably evaded, and is no longer a topic for a whisper, or even a resolution; the latter being reserved solely for the deficiencies of the pupil. A reform which begins with learners, instead of teachers, needs reformation. This method must be reversed; the latter will secure the former.

The French system has been tested under all the revolutions of which that country has been the theatre. Dynasties have changed, thrones have been crushed and reconstructed, republicanism has raged, and imperialism has triumphed; but the medical schools of Paris have always presented an able staff of Professors, appointed after a thorough trial or *concour*, with a salary from the government, the Professors not being dependent on the number of students, nor on the corruption incidental to tuition and diploma fees. To this complexion reform must come at last, in order to be effectual. This desirable result might be accomplished by a central national board or medical senate, by combining the existing Naval and Army Medical Bureaus with a civil delegation of medical Professors, nominated by the Ameri-

can Medical Association, or by the medical societies, or by the medical colleges, who might meet like the Supreme Court of the United States, at Washington, for the examination of Professors and candidates for appointment in the navy, army, and marine hospitals; or, boards might be created in the different States. Adequate salaries should be assigned to all these officers, as being no less useful to society than those now compensated out of the public treasury for educational purposes. Such a board ought to be a national one, and its rules should be the same for all the States. Talents of the highest order could always be commanded. Such a board is a desideratum for the General Government, in its sanitary legislation in the navy, army, and commercial marine.

This medical senate or board would be competent to exercise a general but limited legislative jurisdiction, to be defined by Congress, and, if necessary, by an amendment to the Constitution, not inconsistent with the rights of the individual States; while its moral and advisory action would necessarily be great, paramount and final, as to medical education,—the nature, extent, and duration of collegiate courses: types of which already exist in ecclesiastical polity and other voluntary associations, which rival the statute book in a practical point of view.

The medical appointments now injudiciously made by the Executive of the United States, as the surgeons of the marine hospitals, inspectors of drugs, &c., as above indicated, would be made by and with the advice and consent of this efficient body, and not from political motives solely, to the detriment of humanity, science, and the public good.

One of the most admirable features of the French government presented by the Department of the Minister of the Interior, is that relating to *les hôpitaux, les hospices et les services de bienfaisance*—all of which imply an amount of medical knowledge, for a proper, wise and economical administration of the same, not be found among civilians.

If a thorough reform were effectuated in the appointment of teachers, and in assigning to them a suitable compensation, not dependent on the number of students and diploma fees, no motives to a bias in favor of conferring degrees on the undeserving could exist, and a

reform among candidates would follow as a matter of course. Medical legislation might stop at this point with safety.

The outcry against the graduates in medicine, for their destitution of elementary and professional education, might with perfect justice be at present applied to some of the Professors themselves. The former represent the latter.

The education question is, truly, a two-edged sword, equally dangerous to Professors, as now created, and to candidates for degrees, as now graduated. What would be said of a medical student who should confer on himself the degree of M. D.? or, what is nearly the same, get a degree from a nominal board of trustees, wholly incompetent to judge of his qualifications? Some Professors appoint themselves directly, others indirectly, under the mask of a board of non-professional trustees, who exercise no control but a nominal one; and even this is expressly prohibited by law in some of the States, so that a vacancy cannot be filled until a nomination is first made by the actual Professors of the school! There is, then, a parallelism but too evident, between the present defective method of making a Professor, and the graduating of a student of medicine.

Although the medical Professors of the United States are, in many instances, equal to those of any country, yet, as a body, every candid observer must admit that they are preëminently chargeable with charlatanic advertising—which is not a whit less discreditable than that of the nostrum monger in the newspapers—as their annual announcements, setting forth the unrivalled advantages of their colleges and professional teachers, fully attest. But the most stupendous of all errors in teaching, is that common to both Professors and pupils, namely: the boasting of the number of students in attendance; inasmuch as this is, a fundamental evil even in a common school, where the instruction is verbal, and not demonstrative, as all sound medical teaching must be. Of five or six hundred students in one class, not half of the number can see a surgical, chemical, or anatomical experiment! Other things being equal, the smaller the class, the better must be the opportunity of seeing, hearing, and learning. The propagation of this error has been found profitable, proving, as has been assumed, the superiority of institutions having the largest class! That this numerical argument may be satisfactory outside of the pro-

fession, and may even reconcile students to forego the advantages of demonstrative teachings, addressed to their senses of touch, hearing, and sight, is as true as it is deplorable. Auscultation is demonstrative, and so is the science of obstetrics to a considerable extent, by means of apparatus, and by attendance during actual delivery. *Materia Medica* has its specimens, pharmacy its preparations, chemistry its experiments, anatomy its dissections, disease its sensuous and material phenomena—all of which must be witnessed, in the concrete, not in the abstract,—not simply read of, and lectured upon, without demonstration; and the greater the crowd, the smaller the chances of acquiring experimental knowledge of all these and other positive sciences, and practical illustrations of a thorough medical education.

That a great number of the best informed parties believe, and sometimes give utterance through the medical journals to their belief, that the college catalogues fraudulently and falsely claim the benefit of this most pernicious numerical argument, is a truth altogether incontestable.

At the moment of writing this page, the *Nashville Journal of Medicine*, for December, came to hand; the Editor of which, Professor Bowling, and the Assistant Editor, Prof. Eve, (one of the ablest surgeons of the age,) say: "The trickery and rascality of the matriculating books of medical colleges has become a by-word. Few physicians have any sort of faith in them. * * * It was then not a little provoking to hear, every now and then, after the publication of each catalogue, that such a one had said it was 'a false record, and that he did not believe it.' * * * The adage, 'to lie like a college catalogue,' however true in its general application, is emphatically false when applied to ours, which is freer of 'dead heads,' &c. Many of the catalogues have twenty per cent. of M. D.s."

In one essential particular, the medical schools of foreign countries, particularly in France, are inferior to those of the United States, namely, *in number*—the schools being too few (only three) for the number of students in attendance. In the United States the number is little short of forty. An eminent physician, a native of New Orleans, who, some years ago, graduated in Paris, recently informed the writer that, desirous as he was, during his pupilage in that city, to attend Prof. Cruveilhier's lectures on anatomy, he found it impossible to

see or hear, in a class of fifteen hundred, unless, as rarely happened, he chanced to get a seat near the lecturer; whereupon he had to engage a private teacher, though nominally an attendant in the University. In some of the medical colleges of the United States, the classes, habitually, are by half too large to learn anything worth learning at the public lectures—unless mere readings by the Professor be called education. The Committee on Medical Education, (Drs. Stevens, Twitchell, Wellford, Naudain, Arnold, and Bush,) at the second meeting of the American Medical Association, gave, in their report, the following opinion concerning this mode of tuition: "In the United States alone is continued an obsolete system of teaching demonstrative science by description,—of teaching the manipulations of surgery, and the art of recognizing and healing diseases without exhibiting the practice of either, and of explaining the movements and changes of living bodies to those who are ignorant of the laws which govern inert matter."—Trans. i, 236.

If, as some contend, the number of medical colleges be too great in this country, it is because they are situated in unsuitable places, that is, places where there are no large hospitals in which medical, obstetrical, and surgical practice is witnessed, and where the supply of dead bodies, for practical, surgical, and pathological anatomy, is deficient.

Whether the Committee on Medical Education, for 1853, worshipped the rising sun, does not appear, though they "still harp" upon the students of medicine, and assert that the "disparity" between the schools in the United States and those of foreign countries is not in the teachers. The "disparity," they say, "has no reference to our public teachers." The king can do no wrong. The eighth resolution of the Committee is in the following significant words: "That measures be adopted to prevent the multiplication of medical schools." It is evident that a "multiplication of schools" would diminish the emoluments of those already established. The Sorbonne declared that the king had a right to all the money. Thus the reform degrades the medical profession in the United States below that of every other country. It endorses the Professors as equal to the best,—opposes new ones, as invaders

"Of the right divine, to govern wrong."

In conclusion, the writer begs leave to say, that, whatever errors of opinion or of reasoning he may have fallen into,—whatever censure he may incur for giving expression to his honest convictions, he is unconscious of having been biased by any selfish or unjustifiable purpose, in the views he has taken of the polity and tendencies of, perhaps, the greatest medical association that has yet appeared, in the history of medicine. Honored by this Association as its Chairman of the Committee on the Medical Sciences, at its meeting in Cincinnati, in 1850, the writer's connection with it is among the most pleasant *souvenirs* of his professional life. As a social and professional *reunion* of kindred spirits and great minds, its memories afford perennial delight. It has given impetus to the progress of medical polity and science; it exercises moral suasion, rather than that of authority; it has brought together a bright constellation of intellect, cemented the bonds of friendship among good men and true, and, should it fail to effectuate its original and grand finality—that is, a thorough reform in medical education—it will leave a luminous track of light in the moral firmament of the Æsculapian heavens, throughout the expansions of the Republic.

ART. XI.—LETTER ON YELLOW FEVER.

BY M. MORTON DOWLER, M. D., OF NEW ORLEANS.

To the Editor of the New Orleans Medical and Surgical Journal:

In the November number of the Journal, I detailed some of my experience in the late, and other yellow fever epidemics, and gave some of my views in relation to the ætiology, pathology, and therapeutics of the disease, and in relation to the sanitary measures which have at different times been entertained in our city. I propose, on the present occasion, to offer a few remarks in continuation of these subjects. Everything of late tends to invest the subject of yellow fever with extraordinary interest. A three-fold purpose presents itself to the mind of every Physician in this city, to write and publish all he knows, does, and believes, in relation to the disease, however discordant and opposite may be the deductions which may result, namely: First, he owes it to the medical profession at large;

who are immediately and specially interested, and who naturally expect us of the city of New Orleans to thoroughly investigate and probe the subject to the bottom; secondly, he owes it to the people, who are earnestly looking either to some result touching the ætiology and prevention of the disease, that will conduce to their safety and warn them of danger, or for an honest confession of ignorance in relation to the premises; and, thirdly, to our State and municipal legislatures, that are anxiously waiting for a single ray of light, to justify them in expending the public money in putting into operation such general laws and local ordinances as they can have any assurance will in any way contribute to warding off the pestilence.

No one will pretend to doubt that the yellow fever is, and has been, the stone of Sisyphus to New Orleans; that, were it not for this disease our city, which even now contains real estate to the amount of, at least, \$100,000,000, and which has no natural rival in the South or South-West, instead of numbering 120,000, would have now numbered at least 300,000 souls, with railroad communication with the whole North and West; that it has rendered our city a place of sojourn, rather than an abiding and beloved home, driving a vast proportion of our people, and our people's money, from our midst, at least one-third of the year; that it has injured our commerce, struck terror to the capitalists, depressed our credit and the value of our possessions. New Orleans the village, and New Orleans the city,—New Orleans in 1796, and New Orleans in 1854, gives us, to a greater or less extent, the same showing. No one, who has attended to the facts of the case, can venture to doubt that, for the last fifty-eight years, New Orleans has never, for twelve months at any one time, been entirely free from either the epidemic or sporadic appearance of yellow fever. To crown all, we suffer considerably by the hands of theorists. According to the quarantinists, who pertinaciously press their doctrines on the authorities, we are the unluckiest people on the face of the globe, having, every year for more than half a century, imported no less an evil than the yellow fever; and the nervous contagionists find themselves under the painful necessity of giving notice to all the world, in consequence of the existence amongst us of a contagious pestilence, to beware of our ships and our commerce, lest other cities fall into the misfortune by which we are surrounded.

Moreover, the events of 1853-4 have combined to give a painful interest to the subject of yellow fever, in which others than the people of our city are concerned. Whatever the external or objective cause of yellow fever may be, it was hitherto thought by our people, and by the profession, to be an agent specially incident to a dense and urban population. This generalization, at least, it was thought would hold good. It need scarcely be remarked, that this notion is now no longer relied on. It is no difficult matter to show, that the ratio of mortality in the two seasons adverted to, has been highest where all the conditions incident to a dense population were entirely wanting; that the mortality has been in a higher ratio on plantations than in towns, and greater in villages than in cities; that the doctrines of numerous urban theorists should be, especially, discredited by the legislative mind; and that the yellow fever is no longer a mere thing of New Orleans, and other large communities, they being rather comparative *points of safety*, the disease having become a rural scourge; and that its appearance in the Northern portion of the Republic, both in town and country, may be considered an occurrence not at all out of the bounds of probability.

All these, and numerous other circumstances, which need not be recounted, present this pestilence as a most important and as a leading medical subject, deeply interesting not only to this community, but to the people and profession throughout the whole Republic. It is a subject on which comparatively little has been published by New Orleans physicians, who so well know the disease, and who are so eminently distinguished for practical knowledge and good sense. How many of our most distinguished medical men have practiced in, and observed the disease during a long life, and descended to the tomb, without bequeathing to the profession a single record of their accumulated observation and experience!

Under circumstances such as these, the yellow fever should be the grand and engrossing medical topic of the day; and every physician amongst us, capable of profiting by observation and experience, should be considered as failing in duty, who fails to contribute to our recorded stock of knowledge in relation to this disease. The pages of the medical journals are his appropriate medium of communication. A new era is opening upon us. Medical journals are beginning to enjoy a larger sphere of usefulness. The people are beginning to subscribe for them, and avail

themselves of the contents; and legislative bodies eagerly look to them for information. And all this, not for the irrational purpose of learning the healing art, but with a view to the avoiding of a numerous class of causes by which life, health, property and prosperity are constantly imperilled. Whoever honestly and faithfully endeavors to direct the people in the premises, cannot fail to be read and duly appreciated, and to receive, not only the gratitude of the public, but the thanks and approbation of the profession, whatever differences of opinion may be thereby elicited.

In relation to the ætiology of yellow fever, in former numbers of this journal, I have more than suggested that we have made absolutely no progress; that the disease owes its existence to conditions which have never been determined; that no human effort has ever succeeded in artificially producing an agent capable of producing the phenomena of yellow fever; that no fixed or given association of natural or artificial conditions has ever been discovered, in which the yellow fever necessarily must or must not prevail; but that, on the contrary, the disease shows an entire indifference to all the objective conditions which theorists have prescribed for its appearance or non-appearance; and that its presence or absence, so far as actual discovery runs, occurs under similar circumstances,—the true dissimilarity of circumstances being the thing to be discovered, as the objective agency by which the disease is produced.

The conclusions may be considered, *pro tanto*, as presenting a picture little flattering to our science, and as doing but little homage to the authority of ponderous volumes which have been written; but it is right that the noble science of medicine should stand forth, without disguise, not only in all her strength, but in all her weakness. Candor, sincerity and humility will subserve her true honor and glory far better than bold hypotheses and pretentious dogmatism. Amidst all the controversies and conflicts of opinion with which she is beset, the number of her well established and fixed principles, facts and admitted truths, exceeds that of any other of the more conspicuous departments of human knowledge. The law gives us, on the same point, its five hundred contradictory decisions. "Guilty, or not guilty?" is determined in the midst of legal uncertainties, which neither judge, counsel nor jury can escape; and the whole law has been pronounced "a glorious uncertainty." Political institutions, which are as radically diversified as the physical surface of the

earth, give rise to international and intestine divisions of political sentiment to which there is no end; and never has, nor ever will, the grand question be settled, as to which form of government is the best. Theology, which points us to an eternity of existence and interests, is one great embodied controversy, leading our wise and glorious ancestors, in view of the awful past, to adopt a Constitution which guaranties the agreement to disagree. Meanwhile our religious dissensions have become multiplied and politico-religious, and the great fundamental question, as to whether it is true godliness to write out, adopt, and follow whatever creed we think proper, or to bow to such creed as may be written out, adopted, and followed by a supreme bishop, is still a subject of violent discussion. How many have yearned for the fellowship of the "true church," and have never been able to pronounce the *eureka!* Nevertheless, what should we be in a land without law and judges? Without political institutions we should be below the beaver and bee; and what miserable burrowing moles should we be without the assurance which theology alone can bestow? That society could at all exist without religious worship, remains to be proven. If the true God is excluded, a strange god will take his place. Devotion must have place, whether it be to the Creator of the world, or whether it appear in the mean and contemptible form of Fetichism itself.

And thus it is with the healing art. It has never been ignored by any nation, whether civilized or barbarous. In no age of the world has it not enjoyed the confidence of the people, and been deemed a thing quite indispensable. It has ever existed and must ever exist; and were it even abandoned as a science, it must still continue to flourish and exist as a mystery and as a superstition! * Even the Turk, who is a man of one book, a book which is to him, at once, law, theology, politics, and physic, a book declaring expressly, that "the fate of every man is bound about his neck;" that "the term of life is fixed;" and that "no one can anticipate or protract it for a single instant," still cannot divest himself of boundless and superstitious therapeutic faith. The faith of the people, throughout Christendom, never has, nor ever can be shaken as to the efficacy of medicine in the cure of disease; and as to the absolute necessity of competent and scientific practitioners; and if owing to the great

* The great Aristophanes of France, undertook through the medium of his unrivalled powers of ridicule to put down the physician. The scoffer could almost as soon put down the physician of souls. Medical men have enjoyed Molière's jokes better than he did himself.

number of unsettled questions with which our science must ever abound we often hear applied the trite proverb, that "doctors disagree," surely all the legal, political and clerical "doctors," who may happen to be within hearing distance, may each appropriate to himself more than an equal dividend of the gentle satire. All that is human, is more or less unsettled; "all discord, harmony, not understood."

But legitimate medical science does, and should give her assent to doctrines with an extremely jealous, rigid and discriminating spirit. In her sphere she does not nor should she affect the manner of the law, with its fictions, traditions and discrepancies. She favors not, nor should she favor the habitudes of politics, in which truth runs the chances of the ballot box, or the sovereign will and good pleasure of the despot. She ascends not the heights of theology; and walks by sight and not by faith in solving the mysteries of the mortal, tangible and material organism. She repudiates all vagaries, unsupported theories and pretensions dogmatism; and seeks only the removal of avoidable evils. Whilst, for instance, she directs the sanitary legislator with a humane and hopeful spirit, to the awful evils of pauperism, drunkenness, and prostitution, on the causes and prevention of yellow fever her lips are sealed. She speaks that she knows, and that only.

These reflections we have thought not inappropriate; first in behalf of our profession, that have incurred no little blame for not knowing every thing, and particularly for not informing the people as to what agency produces the yellow fever, and the appropriate means of getting rid of that agency; and secondly, in behalf of our individual selves, who in the name of medical science, do totally call in question, and deny the authority of voluminous theoretical writers, who profess to have fathomed the depths of the whole mystery in question.

Touching the views we have put forth heretofore in this journal, on the reputed causes of yellow fever, it may be urged that having no theory of our own to purpose tending to throw any light on the aetiology of the disease, we might as well pass over the subject in silence. Such method of disposing of the subject, however, is inadmissible. It so happens that at this time it is a matter of overwhelming importance that the question as to whether we know something or nothing of the causes of the disease; or whether it be within the power of man to do something or do nothing to ward off the pestilence, should be thoroughly canvassed.

It is a question involving the deepest interests known to our city. If we are to have the yellow fever legislation which is earnestly sought for, we should obtain with it every earthly assurance that it will bring some compensating benefit as an indemnity for the disastrous results it will bring our city. We should have something more for our money than false security, useless public work, Quixotic expenditures, ruinous commercial restrictions, and oppressive taxation. He who calls on our authorities to weigh well this matter, and to require more light, in advance of the legislation which has been proposed, can by no means be considered as agitating a merely negative or barren subject.

All that is known of the ætiology of yellow fever, lies very little deeper than mere truisms. The disease owes its existence to two special conditions, the one relating to the individual, and the other to an agency by which he is surrounded. These may be called respectively the subjective and objective causes of the disease. The existence of the subjective cause, constitutes the difference between what is unmeaningly called the *unacclimated* and the *acclimated*, and which we shall call the *unprotected* and the *protected*. The objective agency could never be demonstrated to exist at any time, in the absence of the subjective cause. During the existence of the yellow fever objective condition, nothing is found to exist inconsistent with perfect salubrity so far as concerns the protected. It clearly appears that there exists no general noxious agency. The protected population in the disastrous season of 1853, as well as generally in all epidemic yellow fever seasons, have exhibited a remarkable favorable state of health. Such state of things could not possibly exist, if the reputed causes of yellow fever actually do produce the disease.

In the present state of our knowledge, so far as relates to the subjective cause of yellow fever, it does not admit of either removal or of mitigation. We have no means of rendering an unprotected person insusceptible to the disease; nor can we subject him to any process that shall determine and secure him in advance, a milder attack of the disease. Individuals awaiting an attack of yellow fever, surrounded by all the resources known to our art, can have no assurance as to whether their cases shall be so mild or to yield to the therapeutics of nature, or so deadly as to defy the power of medicine; and yet it has been gravely asserted that the disease may be modified and mitigated, or annulled by the *process of inoculation*.

The subjective cause of our yellow fever epidemics, is kept up alone by the immense ingress of the unprotected. Were this ingress to cease, yellow fever epidemics would either wholly cease, or become but few and far between. Under these circumstances the disease would be confined, almost exclusively, to infancy and youth; and be attended with but a trifling mortality, as the process of protection at these periods of life, is attended with comparatively little danger. Indeed, under these circumstances, should the objective agency be governed by the same laws which has hitherto governed its appearance in this city, no adult subjects would probably ever be attacked with the disease.

Yellow fever cannot at all be confounded with the malarious and poludal fevers, whether intermittent, remittent, or continued. It has one radical resemblance to the contagious exanthematas in the fact of its being distinct, specific, and *sui generis* in its character as a pestilence; and on the other hand it exhibits a grand and radical dissimilarity in its having no subjective power of propagation, as in the case of small pox, measles, &c.

The objective and essential origin of the latter class of diseases, has eluded all attempts at discovery, and still greater difficulties, if possible, surround the objective causes of yellow fever. Its non-contagious character, is an exceptional feature, and superadded difficulty. The malarious fevers may occur in one and the same subject, year after year, no protection being acquired. They have so little resemblance to the disease in question, that to confound any of them with yellow fever, is to entertain an error too gross for serious refutation. Indeed the only disease with which any attempt has been made to identify it, is *bilious remittent fever, a high grade of bilious remittent!* In the mean time it is well known that a great proportion of the cases of the disease is milder than the mildest form of bilious remittents; and that some of them are quite *apyrexial*; and that the yellow color, which generally more or less accompanies the disease, is not bilious in its character, but hæmorrhagic, and is very rarely seen in bilious remittents. But it is unnecessary to argue such a point as this, which presents no open question. The distinctive character of the disease from all malarious fevers, being indisputable, its objective agency, distinctive and independent of that of all these fevers, must either be admitted, or else indeed, we must be made acquainted with

physical laws under which the same set of causes are capable of producing wholly different effects.

The change which the organism undergoes in acquiring immunity against yellow fever, is one of the most profound, inscrutable, and inappreciable of mysteries; but is not a whit more mysterious than is the nature of the objective agency to which that immunity owes its existence. Both the effect, and the cause which produces that effect, remain to be discovered. As much is known of the one as the other. That a concurrence of terrene filth, with certain meteorological conditions, should possess the power of infecting an individual with a given disease, and never after have that power over the same person, is at war with all that is known of filth and meteorology, under any possible conditions; that heat, moisture, animal and vegetable putrefaction and exhalation, should assume any such condition as to be capable of poisoning but once, is a mere figment of the imagination; that cryptogamic plants, microscopic animalculæ, and non-ozonic air should generate this pestilence, is wholly unsupported by a solitary discovery, and will not bear a minute's investigation. Such groundless assumptions constitute no real part of medical science, and should not only be discredited by all discreet legislators, but should be blotted out of the records of medicine.

These reputed causes are just about as likely to manufacture and keep in existence, the unknown objective agency of the personally contagious and protecting disease, small pox, as that of the non-contagious and protecting disease, yellow fever. Both of the objective agencies are unknown; both produce a disease specific, and *sui generis*; the one has certain known modes of propagation; the other has none whatever. Typhus fever, though a highly contagious disease, originates under certain determinate conditions of filth, confined air, bad diet, &c., &c., circumstances to which the objective cause of yellow fever is wholly indifferent; but the poison of typhus is as non-protective as the poison of arsenic. Typhus is emphatically a filth and starvation fever, spreading only by gross negligence, and the causes which produce it are scarcely ever sufficiently active to originate its objective agency in New Orleans. The same person may take the disease an indefinite number of times, and it is propagated only by close contact. How different the protecting disease which has invaded the whole South-west! Subjective contact add

nothing to its power, and isolation and non-intercourse are equally unavailing. The cosmopolite habits of the mysterious cholera, may, for aught we know, be assumed by the yellow fever, in place of its local predilections—a result foreshadowed by the events of 1853-4.

The cholera was never spread in any city nor excluded from it by any human agency otherwise than passive, neither was the yellow fever, and as relates to the origin of the latter disease we can draw no other conclusion than that its objective cause is the sublime of mysteries, and that its reported causes embody the sublime of absurdities.

To present the array of proofs which we are prepared to adduce in support of the views we have taken of the ætiology of yellow fever, would exceed the limits prescribed for this communication; and we refer the matter to the future. It may perhaps be considered by some, that our view upon this subject exhibits a gloomy picture as to the future of our city. To this we reply, that the adherents of the reported causes of yellow fever, envelop our future in far deeper gloom. It can be shown that all their hygienic and sanitary measures, even were it possible to carry them into effect, would prove utterly useless and impotent in the premises; no such measures ever having removed the objective cause of yellow fever from any city. The agency that originated the disease, in times past, in our Northern cities is unknown; neither did the disease disappear in these cities in obedience to any human effort. It appeared and disappeared clothed in the panoply of mystery, and all we can learn, is that it came without any appreciable and tangible cause, and in like manner disappeared. It was born on the theatre of its ravages as all yellow fever epidemics are, and there lived and there died. Did the limits of this communication permit, we could thoroughly set aside all the specious explanations which have been brought forward by the sanitarians, to account for the disappearance of yellow fever from cities that formerly suffered and now no longer suffer from this pestilence. Suffice it to say, that its appearance and disappearance in those places are as far as is known ultimate facts, which have never been referred to any reasonable or appreciable cause.

And are there no deductions to be drawn from all these premises tending to unveil the probable future of New Orleans under a more hopeful aspect than can be exhibited in the visionary sanitarian projects which have been proposed for the banishment of this disease? What is the

prospect before us, in view of all that is warranted by analogy and experience? Surely ample proofs crowd upon us that

"Troubles will not last forever;
Darkest hours will pass away"

What is the fact with regard to New Orleans? Why without any appreciable or tangible cause whatever; without the presence of any known condition to which the objective cause can be attributed, our city has for the last fifty-eight years known the yellow fever, and we have the very strongest analogies to show, that under precisely similar inexplicable and mysterious circumstances the disease will in due time, disappear here, as it has done elsewhere. There is nothing within the cognizance of medical science to weaken this radical and standing probability; but on the contrary, we have an exemplification in the fact, that the cities of New York, Boston, Philadelphia, suffered its ravages little less than one hundred years before it was known in this city. Analogy and experience would almost warrant the conclusion, that this appearance and disappearance is one of the fixed habitudes of the disease. The views, therefore, which we have proposed, touching the ætiology of yellow fever, so far from casting a shadow over the future of New Orleans, holds forth to her the suggestions of consolation and hope. The term *acclimation*, in the sense in which it is applied to the process by which immunity is acquired against the disease is not only quite nonsensical, but it is altogether an unnecessarily injurious appellation, as it is a fact, that the "climate" of Boston for instance, engendered the disease more than a hundred years before it was engendered by the "climate" of this city; and if the process of thus acquiring immunity be proven to be a thing of climate, altogether too much is thereby proven; as the whole would go to show that two different climates might exist almost within sight of each other; the climatural difference being referable to the presence or absence of the objective cause of yellow fever; and if the disease be truly and really a thing of climate properly speaking, a still more desperate case is made out against the yellow fever sanitarians, as they clearly, never can rectify any climate on the face of the globe. All hope and consolation would be at an end.

We have said, that the cessation of all further ingress of unprotected subjects, would tend to putting an end to the disease, as to the adult population, under the existing laws which govern the objective agency

of the disease. Such cessation is, of course, fancy's sketch, and is never expected, in any contingency, to happen; but it is illustrative of the naked possibility of banishing the disease as an epidemic. But there are other practical methods by which a hopeful future may be deduced for New Orleans, growing out of the principles involved in this barren possibility. It is a pestilence which, under the laws which now govern it, in this city, cannot continue to co-exist, as an adult disease, with a fixed, permanent, resident, and non-migratory population, however overgrown and enormous that population may become; and New Orleans, with 600,000 of such population, would enjoy all the security she could with any less number. It is not at all improbable that the day may arrive—however great or small may be the previous sacrifice of life by the disease in the meantime—when, instead of receiving the surplus population of other communities, the current may be changed, and swarms of our native population shall go forth to swell the population of other communities, to be succeeded by none but the native born. Immigration, at present, exhibits the world of humanity out of balance; is, for the most part, a forced and unnatural phenomenon, the promptings of stern necessity, and cannot be, in its present form, a permanent procedure. These circumstances, under the existing habitudes of the pestilence in this city, will fully warrant us in the anticipation, that the exemption to which we have alluded, may be fully realized in the future of New Orleans.

But this is not all. There are other circumstances which may be brought forward, going to show that events may occur which may greatly modify the prevalence of yellow fever here, if not altogether banish it from our midst. Let it be supposed that the motives which give rise to European immigration, should cease to exist, which, at no distant day, may actually occur. Here an awful source of mortality is at once at an end; for, the disease in this city has been emphatically a German and Irish disease. In the meantime, no one who has duly considered the events of 1853-4, can avoid revolving in his mind the serious apprehension, that the disease will, at no remote period, spread through town and country, throughout the United States. Every condition for its both rural and urban prevalence was fulfilled and did occur in 1853, as is proven by the extensive diffusion of the fever and difference and disparity of latitude and longitude, we have already suggested, afford no

argument against the worst realization on this subject. Suppose the well grounded apprehension should be actually realized, and that the disease should invade town and country throughout the Republic; then, in direct proportion to its prevalence, would be found the number of persons who might emigrate here and take up their residence with impunity. The widely diffused town and country epidemic of 1853, has already secured ample immunity to an enormous mass of the people residing in six States of the Union; the whole of whom, should they now take up their residence in New Orleans, would enjoy the most perfect protection. It is, therefore, evident, that a wide and general diffusion of the pestilence—which has been partially realized—would radically tend, by a removal of the subjective cause, to the removal of the disease from New Orleans; and though we devoutly pray God to avert all advantages from us which are obtainable only at so fearful a cost to our well beloved fellow-citizens of the United States, yet such may be His sovereign will and purpose.

Thus, then, so far from picturing the future of New Orleans with unmitigated gloom in regard to yellow fever, ours is the doctrine of hope and consolation. True, we promise the people nothing in the premises from the wretched and unfeasible expedients which have been proposed in this city, and called sanitary, and which are but “a sounding brass and tinkling cymbal”—nothing but disappointed hopes, false security, and useless taxation; but, on the other hand, we hold that the spontaneous cessation of the disease, its mitigation and non-appearance as an epidemic, and even its extinction in the future of New Orleans may be fairly entertained, not only from analogical reasoning, but from independent deductions.

In the March number of your Journal, I shall offer you some further remarks on the subject of yellow fever.

FOURTH DISTRICT, NEW ORLEANS, Dec. 17th, 1854.

ART. XII.—ORATION DELIVERED BEFORE THE PHYSICO-MEDICAL SOCIETY OF NEW ORLEANS, LA., AT THEIR ANNIVERSARY MEETING, HELD DECEMBER, 1854.

BY A. MERCIER, M. D. P.,

PRINCIPAL SURGEON OF THE CIRCUS STREET HOSPITAL.

[Published at the request of the Society.]

Gentlemen :—It is a grand design of union, of progress and of humanity which consecrates, by a public ceremony, the anniversary of the foundation of the Physico-Medical Society of New Orleans. In this solemnity, one of its members is charged with the duty of developing his ideas upon some general or special point of our science, at the same time that it is permitted him to testify his regrets at the premature removal of some of our confrères and of throwing some flowers upon the tomb of those whom death has unpityingly taken away.

Last year, when the Society did me the honor to select me for the orator of to-day, I had hoped that I should be spared the performance of this sad and mournful task. My wishes have been but half fulfilled. It is true that this Society has not to deplore the loss of any of its active members, although they were to be found faithful at their post during the ravages of the cruel epidemic which has once again visited our city. But do I not faithfully express the sentiments of this entire Society, when I give utterance to the profound regret felt for the premature loss of the three noble and unfortunate medical students who, so to speak, died upon the field of battle, their arms in their hands?

Young, Jarreau and Mickie were born, the first in Arkansas, the second in Louisiana, and the last in the State of Mississippi. Prompted by a desire for knowledge, they came to New Orleans, where, at a brilliant concour, held in April last, they showed themselves worthy of being admitted resident students of the Charity Hospital. There they gave themselves up to the study of the different branches of medicine, with zeal and assiduity, and showed for their art a love so passionate, that it could be already foreseen that, one day, these three students would make good physicians and useful citizens. Under these circumstances, the yellow fever made its appearance in this city, in July last. Young, Jarreau and Mickie, far from fearing the approach of this new and unknown ene-

my, exerted that ardor in seeking and finding it out which others too often exhibit in flying from it. They, with praiseworthy energy, visited each new case of yellow fever which was admitted to the Hospital; they examined it, studied it, compared it with the other cases which they had already seen, and let no occasion escape for studying the disease in all its forms, in all its details, in all its symptoms, in all its horror, in all its dangers, thus giving the proof that the courage of the warrior who, in the excitement of the combat, confronts a glorious death is totally different, and deserving far less praise, than that civic courage which prompts an exposure to danger from the sentiment of duty only, and to a death without glory, of which the imagination doubles the horror.

In spite of the advice of their friends, who pressed them to greater precautions and less exposures, in spite of the repeated supplications of their anxious relatives, who conjured them to come and seek security and happiness in their midst, and to enjoy, in their words, the benefits of life and the too fleeting pleasures of youth, these three intrepid young men remained unmoved, and continued to march upon the perilous route for which they had enlisted. Their turn arrived; the scourge struck them. They were enabled to oppose but a feeble resistance, debilitated and exhausted as they were by incessant labor and by a service prolonged through the day and often extended to the night.

In spite of the devoted and assiduous cares of the accomplished physicians who had charge of them, in spite of the fraternal attentions with which their fellow students of the Hospital surrounded them, a few days sufficed for the scourge to accomplish its work of destruction. Young, Jarreau and Mickie were fated to an early tomb. The religious respect and the mournful silence of those who accompanied them to their last resting place, were witnesses of the esteem and affection which they had known how to inspire in all those who were thrown in connection with them. Let us hope that the profound and painful wound inflicted upon the hearts of their inconsolable families may be, if not entirely cicatrized, at least soothed and assuaged by the thought that these three much regretted young men, like three soldiers of the faith, have known how, at the call of duty, to stand firm at their post and there receive their death-blow.

Having paid this slight tribute of regrets and admiration that these three valliant and unfortunate sons of the South have so justly merited

by their fidelity to their duty, and their courage in encountering death, permit me, gentlemen, to present you with some general considerations upon the progressive state of surgery since the commencement of this century; upon the part which American surgery has taken in this movement of progress and amelioration; upon the causes which prevent the physicians of the United States in marching *pari passu* with the physicians on the Old Continent; in fine upon the means to be employed in removing the obstacles which others have created, and which we, ourselves, create to the successful and ascending march of our science.

Of the great and important operations of surgery which latterly have placed this portion of the art of healing upon the same level with the other branches of natural sciences, some were neglected, were hardly known and had not yet found a place in the classical works published in the beginning of this century. In this number it is necessary to comprise Rhinoplasty, Cheiloplasty, Blepharoplasty, Otoplasty, Bronchoplasty, Cauterization of the Urethra, Amputation of the Womb, Extirpation of the Ovary and of the Anus, and a certain member of Resections.

It may be said that the possibility of performing these operations and the happy results which have followed their practice, did not justify the neglect into which they had fallen, and had really rendered indispensable a review of the whole of operative medicine.

The others appertain to the surgeons of our day. They bear testimony to a spirit of progress and of investigation so ardent; their practice has been followed by results so happy, so fruitful and so numerous, that every one, in his joy and admiration, asks himself if modern surgery has not arrived at its columns of Hercules, or indeed if it is not of infinite perfectibility. In this category of operations, it is necessary to include the ligature of the Common Iliac and of the Brachio-cephalic; the complete extirpation of the Parotid gland; the Tenotomy, sub-cutaneous properly called; the Tenotomy of muscle with or without incision of the subjacent parts; the disarticulation of the inferior maxillary bone without lesion of the facial nerve; the treatment of strictures of the urethra by means of forced dilatation, according to the method of Mayor, of Lausanne, Switzerland; of graduated dilatation by Wakley's instruments, or of excentric incision from within outwards recommended, put in practice and generalized by Stafford; autoplasty *par glissement*, recently introduced by Jobert de Lamballe; in fine the application of immovable apparatus in the treatment of fractures adopted by Dr. Seutin.

Those operations which have for their object the reparation of accidental mutilations, or of frightful deformities from loss of substance, are certainly among the most brilliant conquests of surgery

If it is difficult to admit as exact the observative of DeHorn, who cites the example of a hand which was held on only by the tendon of the index and which Jung succeeded in completely re-uniting; if it would be absurd to admit that Esculapius re-united the head of a decapitated woman; or not to consider as a pleasantry the story which Rabelais has given us of Epistemon who had had his head cut off and which Panurge re-joined exactly for him, vein to vein, nerve to nerve, vertebra to vertebra, &c., the which Epistimon aforesaid was skillfully cured, except that his voice remained a little hoarse, and that he had a dry cough of which he could not be relieved, except by hard drinking; we ought at least to accept as certain the observations of Percy and Hoffmann of one arm which, adhering only by the vessels and a portion of the biceps, the humerus and integuments being completely severed, was however preserved and perfectly cured. We, physicians, of New Orleans, ought to accept these extraordinary cases with less incredulity than our confrères of colder climates.

Each day, so to say, we are witnesses of frightful disorders for which the surgeons of Europe would not hesitate, one instant, to resort to amputation, and which, however, treated by the wise gifts of autoplasty are cured, and a complete restoration of the limb to its primitive functions follows.

It is, in the application of autoplasty to the cure of deformities, either acquired or congenital of the roof of the palate, of the face and of the neck, in its artistic part, so to speak, that this surgical conquest demonstrates itself in all its richness and in all its utility. Do we not see in our saloons young ladies with an elegant port, with their head gracefully poised upon their shoulders, who had a congenital wry neck and for whom such an infirmity would have been a constant cause of grief and disappointment, had not the sub-cutaneous tenotomy come to their succor. How many persons do we encounter in the streets who, had they not received the benefit of a surgical operation, would have presented to you, instead of a white linear cicatrix under the lobe of the nose, a frightful hiatus disclosing deformed gums, irregular and badly planted teeth and a palate fearfully divided in its entire extent.

American surgery marches in the front rank in company with European surgery in this department of our science. It is my profound conviction that autoplasty, better studied among us and applied upon a wider scale, will considerably diminish the number of operations which we to-day deem indispensable. The nature of our climate invites us to new attempts and already one of our old colleagues, Dr. Dupierris, has invented an instrument for suture in palatoplasty as ingenious as that which our fellow-citizen, Dr. Fahnestock, has invented for the resection of the tonsils. Already this mode of treatment has been successfully resorted to by some of our surgeons and a great many more persons would be seen in our community with a wry neck, a crippled arm, twisted fingers or a cork leg, were it not that they had had the good sense to make an appeal to the skill and learning of the Nestor of the surgeons of our country Professor Warren Stone.

Among the new acquisitions made by the surgeons of our day, I have mentioned the extirpation of the womb. This operation can only be considered as useful in cases of prolapsus of the uterus. That the fallen womb has been several times removed from the body without inducing the death of the woman, no one will doubt. Langenbeck practised this operation with success, in 1813. The observations of Fœdéré, of Récamier, of Marjolin, of Delpèch and of Bellini, are reported with a care and details which do not permit the least hesitation. If the high reputation of these distinguished surgeons was not a certain gage of the authenticity of their observations, can the slightest doubt be entertained in regard to Dr. Galot who forwarded the anatomo-pathological specimen to the Medical Academy of Paris, or to Dr. Marschall, of Strasbourg, who upon the death of a woman who had survived this operation for ten years, had an opportunity to prove upon the corpse the absence of the organ of gestation ?

It is not the same with the extirpation of the uterus without displacement. Twenty-three extirpations of this kind, authentic, indisputable, practised in Europe and all followed by death ; two other attempts made some years ago in this country and followed by a result not less fatal, are they not of a nature to cause this deadly operation to be abolished forever from practice ?

While reflecting upon attempts so unfortunate, it is gratifying to find at the same time that there have been some new and valuable acquisitions

in modern surgery; I wish to refer to the extirpation of tumours of the ovary and to some Resections. It is in addition most satisfactory to me to be able to state that the first was attempted and happily performed by an American surgeon and that the second has been pushed to its extreme limits by one of our colleagues, a member of this society.

It was only in 1825, that Dr. Lizars called attention to the idea that Morand had advocated, that it was possible to extirpate the ovary. But already Drs. McDowell, Nathan and Alban Smith had long before practised this operation in the United States, and had seen their attempts, in some cases, crowned with complete success. In fact it was in 1809, that Dr. McDowell removed from Mrs. Crawford an ovarian tumor, which weighed fifteen pounds. Thirty-five days after the performance of the operation, the patient had perfectly recovered. He has repeated the same operation four times since. One of those operated upon, was not able to withstand the results of the extirpation; the second remained uncured; with a third, the two ovaries were diseased and were removed with full success; in fine in 1816, McDowell happily removed from a negro-woman an ovarian tumor, weighing six pounds. Later in 1821, four years before this operation had excited attention in Europe, the pure and simple extirpation of a diseased ovary, practised by Dr. Smith, in the United States, was followed by no untoward accident.

If the physicians of our country are not able to claim priority in Resections, they do not the less deserve the credit of having practiced them in all their difficulties upon the bones which lay the deepest. In 1843, the two inferior thirds of the sternum and the extremity of the two corresponding ribs were resected in the surgical wards of the Charity Hospital of New Orleans. The point of the heart immediately under the external fold of the pericardium could be touched with the finger, and the impulses of the heart could be counted by the eye. The patient perfectly recovered, he was still living in 1851, at which period his surgeon lost sight of him.*

In 1852, the most extensive resection with which I am acquainted, that of the radius and the cubitus all entire, was practiced with complete success upon the arm, and the honor of this operation belongs to a member of this Society, Professor Wedderburn.

We have just stated the active part that the American surgeons have

*This operation was performed by Dr. Mercier.—Ed.

taken in the revival which has taken place, since the beginning of this century, in those operations the possibility of which had been foreseen by the physicians of the last century, but which they had let fall in oblivion. But Surgery, far from having said its last word, saw a new era of progress and amelioration disclose itself. New methods were about to be invented and put in practice, the study better understood of the pathological anatomy of some organic lesions was about to induce methods of radical treatment for some affections, against which so far, only palliatives had been opposed; new operations which had always been considered impracticable, on account of the manual difficulties of the operation, or as useless on account of the vital importance of the organs to be submitted to the action of the bistoury—such operations were to be tried and performed with a success for which timidity dared not to hope. Let us examine if American surgeons have marched in the front rank with their *confrères* of the Old Continent, and let us try to determine whether a part of the glory which so far has been bestowed only upon the latter, does not appertain to them.

The operation which most contributed to bring in relief the name of the illustrious Dupuytren, was the resection of a part of the jaw practiced in 1812; but does the glory of having first essayed with success this difficult and dangerous operation belong in fact to the French surgeon? No, gentlemen. The merit of this operation belongs to an American surgeon, namely, to Dr. W. H. Deaderick, of Rogersville, Tenn. To convince you of this, refer to the American Medical Recorder, vol. vi, p. 516, and you will see there recorded the case of a removal of a portion of the lower maxillary bone by W. H. Deaderick, M. D., of Rogersville, Tenn., practiced thirteen years before the publication of the observation, that is to say in 1810. Dr. Mott acknowledges the case in a note to his letter to Dr. Liston. Dr. Smith's "System of Operative Surgery" calls attention to it and justly claims it as the first operation of the kind ever performed, being two years before that of Dupuytren. M. South again ("Chelius' Surgery, vol. iii, p. 745,) says: "As will be presently seen, Deaderick was the first who, in 1810, cut away the side of the lower jaw;" and again, page 749: "Deaderick, of Rogersville, Tenn., is justly entitled to the merit of having first, in 1810, amputated a portion of the jaw of a child of fourteen years."

It is still to the Surgery of our country that the honor belongs of hav-

ing first applied a ligature around the Brachio-cephalic trunk and the Common Iliac; parts considered up to that time beyond the reach of our instruments. It was Valentine Mott who, first in 1813, passed a ligature around the Brachio-cephalic artery. The manual operation was skillfully performed, but the result was unfortunate. Four other surgeons have since followed the example of their colleague of New York, but all with as little success.

It is not the same with the ligature of the Common Iliac. This operation was performed by Mott, in March, 1827, for the first time, and was followed by complete success. Crampton, of Dublin, next attempted, in 1828, to imitate his confrère of New York, but was not so fortunate.

In fine, in 1846, the same operation was performed, upon a negro boy, for an enormous aneurism of the External Iliac which filled the right half of the pelvic cavity, by Dr. C. A. Luzenberg of New Orleans. I assisted at the operation, and had again an opportunity of witnessing the rapid *coup d'œil*, the sure diagnosis, the unerring prognosis and the manual dexterity of this great surgeon, to whom there was wanting but a few years more and a more extended field of operation in order to have acquired a world-wide reputation. The patient operated upon was cured without accident.

The last operation which was tried for the first time in the United States, is the disarticulation of the inferior Maxillary bone without lesion of the facial nerve. It was performed in our city, in Condé street, on February 3d, 1847, in the presence of Drs. Luzenberg, Cheeseman of New York, Bradbury, and several others. The Spaniard who was the subject of the operation is still living. Any one can assure himself, with the extremity of his finger, that the glenoid cavity is completely empty, as also is equally evident the absence of paralysis of the corresponding half of the face.*

Other operations not less important nor less difficult, are practiced daily in the United States with results far more satisfactory than those obtained in Europe. Thus the ligature of the Subclavian has been twice performed, with complete success, by Valentine Mott; three times in New Orleans, one without success; a second time under the clavicle, after the

*The Editor of this Journal though not present at the above mentioned operation, was assured by others, among whom was Dr. Cheeseman of New York, that Dr. Mercier, who ignores his own brilliant operations in this paper, was the operator on the occasion. The Editor has seen Dr. M. among other operations place ligatures several times upon the subclavian arteries.

method of Dupuytren, a third time between the scaleni; which two latter were followed by complete success.

Further still, two extirpations of the parotid gland, practiced from 1841 to 1845, by C. A. Luzenberg, in the presence of numerous *confrères*, offer, in the description which has been given of them, so many details and so much precision, that it cannot be denied to them the right of admission into the category of operations in which the entire parotid gland was extirpated.

It is thus in fine that our Surgeons of the South, undertake without hesitation, operations which the great practitioners of the Old World have advised to have expunged from the pages of operative medicine, on account of the accidents and unfortunate results which they have seen from such attempts. I mean to refer to the treatment of strictures of the Urethra by excentric dilatation with Wakley's instruments, or by internal incision with the styletted-catheter of Stafford. In the United States, or at least in the Southern States, we have never to do with those purulent *dépôts*, with those infiltrations of urine with those metastases, with those absorptions of matter, with those diffused phlegmons, with that gangrene, with that putrefaction of the hospitals, with those phlebites, with those most to be dreaded erysipelatous affections, which too often complicate the operations performed in England, in France, in Italy, and in Germany.

In New Orleans, much less than elsewhere, are we exposed to encounter those dreadful complications. We owe it, without doubt, to the climate of our city, the influence of which upon the healing process of wounds does not yield to even that of Egypt. To compare the operative medicine of Europe with that of our Southern States, in reference to their definite results, would form a subject for a dissertation full of attraction and productive in useful results. I must content myself with pointing it out to-day to the generation of young physicians who are about to succeed us.

To crown this series of brilliant and useful discoveries in modern surgery, a final conquest has been added, the most sublime of all, and the most universally profitable. A few months were sufficient to extend its benefits to the remotest portions of the globe, like unto its two elder sisters; the application of steam to navigation and the transmission of thought with the rapidity of lightning by the telegraphic wire. All the

glory of it should be assigned to an American physician. You have already understood, gentlemen, that I refer to etherization in order to abolish nervous sensibility during surgical operations by means of ether and chloroform, for one ounce of which the King of England would have given two of his largest men of war, when Sir Astley Cooper removed from him a greasy tumor of the scalp; and for which Louis XIV, surnamed the Great, would have paid the price of two provinces of his kingdom, the day that Dionis incised with the bistoury his fistula in ano.

* * * * *

In the inventory which I have just made of all the improvements in the old operative medicine, and of all the new surgical conquests achieved by the physicians of our century, those of the old world as well as of America, I feel confident that I have furnished you, gentlemen, with ample proof that every American citizen, who has at heart the happiness and glory of his country, ought to be rejoiced and feel proud at the same time, of the large and important part which the surgery of the United States can justly lay claim to, as its own proper share. And farther, in this progressive march of the surgery of the world towards perfection in the old methods which had fallen in oblivion, or in the discovery of new methods, it is our American surgeons, who, like to our pioneers advancing into the regions of the far West, hold the head of the column and to them is reserved the glory of giving to the rest of the army, the proof that operations, up to them deemed impracticable, could sometimes be attempted with success. To them the honor is due of having enriched the domain of modern surgery with acquisitions so important as the extirpation of the ovaries, the resection of the inferior maxillary, the ligation of the brachio-cephalic trunk and of the common iliac, the resection of the small intestine for several inches of its extent, the disarticulation of the ascending branch of the inferior maxillary bone without lesion of the facial nerve.

How happy should I feel, gentlemen, could I state that in the ascending march of the natural sciences since the commencement of this century, medicine properly so called had progressed *pari passu* with its sister American surgery. Among the causes of this much to be regretted delay, it is necessary to rank the cupidity of some physicians who look upon their profession as only a means of traffic; the want of *esprit de corps* and association among the members of the medical body; the too great

facility with which our schools of medicine confer, each year, a diploma upon a crowd of young men who do not even learn enough to understand that they know nothing; the very decided aversion of our municipal administration to take in consideration the wise suggestions which, more than once, have been made to them by our medical body; the too fatal error of our young physicians who imagine that they carry in the little tin box which encloses their diploma, a mass of knowledge sufficient for their utmost need and believe themselves, from this, justifiable in dispensing with ever opening a book of medicine or surgery; in fine, the imbecility of a venal legislature from which a curer of cancers without the bistoury, the greatest imposter that has ever visited Louisiana, has been enabled to obtain the repeal of the wise law, which, controlling the practice of medicine in our State, and requiring capacity on the part of the physicians practicing the art of curing, gave to society all the guarantees needful to preserve them from empiricism.

The discussion of these different topics would carry me far beyond the limits which the orator of this day ought to ascribe to himself. Let it be permitted me only to address a few words to the rising generation of young physicians who, full of love and ardor for their art, full of bright promises for their future, are to succeed us.

“Our art, says Hippocrates, one day was born of philanthropy; its end is always to do good.” Never imitate that physician of our city, who, however meritorious otherwise, having arrived at the miserable lodging of one of his compatriots, sick of the yellow fever, seized his gold watch which lay upon the sick man’s pillow and bore it away in security for the future payment of his fee. Whereas they have faithfully recorded in their memory the flight of Galenus, beyond the walls of Rome, when ravaged by the plague, because that the fault of this great man has been redeemed by many acts of courageous devotion on the part of the physicians of all ages; in like manner recall not the infamous act of this sordid *confrère*, but only in order to put more ardor in your works of charity. Leave the public hatred to pursue this *miserable*. Aspire yourselves to be enabled to say with Boerhaave: “My best patients are the poor, for God is charged with paying me for them.” Think of the epitaph which they read in England, on the tomb of Dr Fothergill, more beautiful in its simplicity than the most bombastic titles. “Here lies Dr. Fothergill, who, during his life expended two hundred thousand guineas for the relief of the unfortunate.”

It does not suffice that the physician puts in practice charity, all the precepts of wisdom, the contempt of money, moderation, industry, probity, affability, gravity, the just appreciation of the things of life, the relinquishment of all superstitions fears, respect for the divinity to whom medicine without ceasing draws us near; it is needful still further that he be convinced that science is the first duty of the physician; that ignorance when it implicates the life of a fellow being, is a crime, and that, in order to be a good physician, it is necessary that he should divide every moment of his time between study and practice.

“In order to become an excellent physician, says Hippocrates, it does not suffice one to give himself up, from his youth, to the study of medicine, to labor at this science with ardor during some years and then relaxing for the purpose of devoting his time exclusively to practice; but it is necessary to devote his entire life to study and to practice and to abstain from all occupations foreign to his profession; for if one is not deeply imbued with a love for this labor, all natural means, all his faculties fail, become superfluous and the edifice remains imperfect or decays away. It is necessary that the passion for acquiring information should be, at the same time, ardent, sustained, and of long duration; there is needed a perseverance superior to all obstacles and to nourish it, it is necessary to be animated with a species of avidity for knowledge; in fine with an unextinguishable thirst for literary glory.”

Farther yet, it is necessary that the physician should march onward in the path of duty without occupying himself with disappointments which he will at times procure to clients who relied upon a culpable complaisance on his part. He should rival Cæsar's wife, who so far from being guilty, was beyond suspicion or reproach. In fine, he should be, in the widest signification of the expression, an honest man.

It is in following these precepts, gentlemen, that our science will be rendered back to its first dignity, to its *éclat*, to all its consideration, that the medical body of New Orleans will show itself worthy of the divine origin which antiquity was pleased to confer upon it and that you will merit the application of these beautiful words of the Roman orator:

“Homines ad deos nullâ re proprius accedunt, quam salutem hominibus dando.”

It is in preserving the life of a fellow creature, that man draws nearest to his God.

ART. XIII.—OVARIOTOMY SUCCESSFULLY PERFORMED.

BY DR. A. MERCIER,

SURGEON, CIRCUS STREET HOSPITAL, NEW ORLEANS.

On Sunday, December 17th, 1854, Dr. Mercier, assisted by Dr. Cannelle, the attending physician, and by Drs. Weatherly, Wetzel, and myself, performed the operation of Ovariectomy upon Barbara Schaff, a German woman, an eight years' resident of New Orleans. The subject was twenty-eight years of age, and has had four children—the first, ten, the last, three years since. For the last two years she has been suffering from Ascites, probably dependent upon Ovaritis. One year ago, a swelling was observed in the right iliac region, and her menstruation ceased at that period. In June last she was tapped, and six gallons of a white, thick, albuminous fluid were drawn off. At this time, the swelling, which had been observed six months before, was found to have largely increased. It extended from the symphysis pubis to about two inches from the edge of the lower ribs. Palpation of the thin and flabby walls of the abdomen, which seemed to indicate precisely the location of a head, and still more distinctly of ribs for this tumor, conjoined with the fact that there had been a total absence of pain and of the menses since the first appearance of the swelling, suggested to all who examined her then, as afterwards, the possibility of this being a case of extra-uterine pregnancy. The diagnosis evidently lay between this possibility and the greater probability, in consequence of its far greater frequency and the absence of positive testimony, of an Ovarian tumor. Whether the one or the other, Dr. Mercier advised its removal at once. His advice was not acted upon.

The effusion rapidly re-accumulated, and she was tapped six times between June and December 17th, and each time from five to six gallons of liquid were drawn off. The total amount was about thirty-five gallons during the six months.

Preparatory to the extraction of the tumor, which she had finally determined to have removed, though life should prove the forfeit, she was tapped, on the 16th December. Her condition for the operation was favorable in every respect, except that she was anæmic and debilitated.

Delay was calculated rather to increase than decrease this contra-indication. It was therefore determined to operate at once.

At five o'clock, P. M., on the 17th, she was subjected to the influence of chloroform. An incision, nine inches in length, was made over the tumor, extending from the lower ribs to the external edge of the rectus abdominalis. Two small muscular arteries were tied during the course of the incision, and but little blood was lost, either then or subsequently. With some difficulty the adhesions which bound the tumor to the abdominal parietes, the liver, the iliac fossa, and to the omentum, were torn apart with the hand, the intestines, &c., being freely handled for this purpose. Near the pedicle of the tumor the adhesions were so strong for more than an inch in extent that the bistoury had to be resorted to. The tumor presented the appearance externally of an enlarged ovary, and such was its size that, in order to draw it out of the abdominal cavity, several deep incisions had first to be made into it, and a large portion of its fluid contents to be evacuated. Strong, thick twine was then tied as forcibly as possible around the pedicle of the tumor, and it was excised about half an inch above the ligature. The internal parts were freely sponged, and the lips of the wound brought together with five sutures, which included all the parts incised—the peritoneum, muscles and integument. The lips of the wound were further approximated with adhesive straps, and covered simply with lint. Openings were left, sufficiently large to admit a ready egress to any fluid contents of the abdomen.

The tumor, when extracted, measured nine inches in length, and six in diameter. Its weight could not have been less than six pounds. It was of a fibro-cartilaginous character, and composed of a mass of small conglomerated cysts of various sizes, containing a fluid of the color and consistency of glycerine.

The patient was enjoined to lay inclined as much as possible on the right side, a stimulant was administered, and a mixture Tr. Arnica and Syr. Morph. prescribed. Nausea and vomiting were excited by the mixture, and no rest was obtained during the first night.

The following day her condition was decidedly unfavorable. Her pulse was so weak and rapid that it could not be counted, and she complained of much pain in the left iliac region. The mixture was laid aside and beef tea administered. The second night was also passed restlessly, and the pain had increased.

On the third day an enema was administered, which was followed by an evacuation affording immediate relief to the pain; since which time there have been no symptoms of peritonitis. Tr. Arnica and morphine were again administered, and a tablespoonful of beef tea every hour.

On the third night, the patient for the first time enjoyed a sound and refreshing sleep; and the fourth day found her condition much improved in every particular. Since this time she has not presented a single untoward symptom, and each day has found her better than the previous one, under the simple treatment of a nourishing diet.

By the seventh day, the muscular portions of the wound had united, except opposite to the sutures. On the thirteenth day, the ligature came away, and indicated by its appearance that, with a little force, it might have been removed without danger several days before. From this period the flow of serum through the wound, which at first had been considerable, has rapidly decreased.

At the present time, January 3d, 1855, seventeen days after the operation, the patient may fairly be considered to have recovered from the injurious effects of the operation. Her pulse has improved much in strength and regularity, and diminished in frequency to about ninety in a minute; her appetite is good; her evacuations, both fecal and urinary, are natural and regular; her rest is unbroken, and her countenance is a picture of gratitude and cheerfulness. The wound has almost entirely healed, and she is sitting up.

Ovariectomy was first performed in France, in 1776, and from that day up to the present time it, as everything else medical, has had its opponents and its advocates. The practicability of the operation long since ceased to be problematical. Four years is the period assigned as the limit within which ovarian tumors naturally result in death; and statistics of this operation show a mortality of about one in three. An analysis of eighty-one operations for this disease, gives the following results: In fifteen cases extirpation was impracticable, in consequence of adhesions; in five cases there was no tumor; and in six the tumors were not ovarian. Death was the result of the operation in six cases of the fifteen in which the tumor was not extracted. Of the eighty-one cases, only sixty-one tumors were extracted, of which six were not, and fifty-five were ovarian tumors. Of these sixty-one cases, thirty-five recovered, and twenty-six died. The result in the entire eighty-one cases was thir-

ty-two deaths and forty-nine recoveries. More favorable statistics than these cannot be found. The same question as to the propriety of operating for this disease arises as for some other diseases. On the one hand is certain death within a limited period, with more or less suffering in the interval; on the other, speedy death is probable, but a prolongation of life still more probable. The decision may properly be left to the discretion of the patient, without necessitating surgeons to resort to a long ethical discussion.

Dr. Mercier is the first surgeon who has performed this operation in New Orleans, and by it he has certainly not detracted from his reputation as a prudent, dexterous, and most skillful operator. He has justly merited renewed strength in the position he has long occupied, as, if not the first, at least, not the second surgeon of New Orleans. "By their fruits ye shall know them."

STANFORD CHAILLE,

Resident Physician Circus street Hospital.

The first of these is the fact that the United States is a young nation, and that its history is a history of growth and expansion. The second is the fact that the United States is a nation of immigrants, and that its history is a history of the struggle for a better life for all.

The third is the fact that the United States is a nation of free men, and that its history is a history of the struggle for freedom and justice for all.

The fourth is the fact that the United States is a nation of peace-loving people, and that its history is a history of the struggle for peace and harmony for all.

CHAPTER IV

The fourth of these is the fact that the United States is a nation of peace-loving people, and that its history is a history of the struggle for peace and harmony for all.

The fifth is the fact that the United States is a nation of progress, and that its history is a history of the struggle for progress and improvement for all.

The sixth is the fact that the United States is a nation of opportunity, and that its history is a history of the struggle for opportunity and advancement for all.

The seventh is the fact that the United States is a nation of hope, and that its history is a history of the struggle for hope and optimism for all.

The eighth is the fact that the United States is a nation of faith, and that its history is a history of the struggle for faith and belief for all.

The ninth is the fact that the United States is a nation of love, and that its history is a history of the struggle for love and compassion for all.

The tenth is the fact that the United States is a nation of unity, and that its history is a history of the struggle for unity and solidarity for all.

Part Second.

REVIEWS.

ART. I.—*Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of 1853. Published by Authority of the City Council of New Orleans. Printed at the Office of the New Orleans Picayune. New Orleans, 1854: 8vo. pp. 542.*

This document has at length made its appearance. It comes forth in a volume of imposing proportions, bequeathing us something more than

“——— a little book
That we may look upon.”

Its contents may be thus enunciated: First, prefatory and introductory matter, and the indices; secondly, medical testimony; and, thirdly, sanitary reports. The book is interspersed with meteorological and mortuary, location^d charts and tables, and the distribution of this matter being as follows, namely: to the first, 38, and to the second, 237, and to the third, 309 pages. The Sanitary Commission is seen to have been composed of the following named gentlemen, namely: the Hon. A. D. Crossman, then Mayor, and Drs. E. H. Barton, A. F. Axson, S. D. McNeil, J. C. Simonds, and J. L. Riddell; and the following is the enunciation of the subjects of inquiry entertained by the Commission, and the partition of its labors amongst its members, namely:

- “1st. To inquire into the origin and mode of transmission and propagation of the late Yellow Fever Epidemic,” to Drs. Axson and McNeil.
- “2d. To inquire into the subjects of Sewerage and common Drains, their adaptability to the situation of New Orleans, and their influence on health,” to Dr. J. L. Riddell.

“3d. To inquire into the subject of Quarantine, and its use and applicability here, and its influence in protecting the city from epidemic and contagious maladies,” to Dr. J. C. Simonds.

And “4th. To make a thorough examination into the sanitary condition of the city; into all the causes influencing it, in the present and previous years, and to suggest the requisite sanitary measures to remove or to prevent them; and into the causes of Yellow Fever in ports and other localities having intercourse with New Orleans,” to Dr. Edwd. H. Barton.

In contemplating this act of partition of the proposed field of science, it will be seen that Dr. Barton reserves what the lawyers call an *allodial title* to the whole ground. There is no portion assigned to his confrères to which he does not secure to himself the sovereignty. He jumps the fence, and takes back all he has solemnly agreed that his co-occupants shall have for themselves. Every inquiry appropriated to Drs. Axson, Riddell and Simonds, is re-appropriated to Dr. Barton. The extent of research which Dr. Barton has imposed on himself wears a *prima facie* aspect of an immense and yearning desire to do a great favor to the city of New Orleans. But sundry other explanations, more easy of comprehension, intermingle themselves to help us out in our rationale of the world-wide and sky-high jurisdiction assumed by him. These we shall advert to in their proper place. In this book of 542 pages, the reports of Dr. Barton's colleagues occupy but 60 pages!

The department assigned to Drs. Axson and McNeil, though quite swallowed up in this manner, is one calculated to call forth the able and lucid analytical powers of Dr. Axson; and though all the controversies involved in the investigation of his subject remain, as before, unsettled, save only the predetermined fact of the local and spontaneous origin of the epidemic, yet it is impossible to peruse this gentleman's report without bearing testimony to the good taste, logical acumen, and excellent powers of generalization therein exhibited. While we hold that three, out of four, of his conclusions remain unproven, we must say that his report is a production creditable to the medical literature of our city.

Professor Riddell has executed his task as might be expected from a gentleman of his acknowledged scientific attainments. In manner, he is a model reporter on a question of science, proceeding in the most business-like manner, without fustian or clap-trap; and in his use of words he is

as curt as Cæsar. He accompanies his report with engravings illustrative of his scheme of street and yard drainage. We shall not follow the gentleman in review of that scheme. We would merely advert to one question, which is of at least equal importance with any entertained in this sanitary volume, and that relates to whether New Orleans is, or is not, to become too expensive for occupation. This city is now too heavily burdened to endure any taxation having its origin in visionary and hypothetical views touching the ætiology of epidemics. The cords of government are now attaining the utmost tension allowable in regard to the tax-laying power; and we are perfectly convinced that, were the whole scheme insisted on in the sanitary volume carried into practical operation, it would consummate the utter ruin of New Orleans. During the years 1850–51–52, the yellow fever existing in our midst only in a sporadic form, was ignored by the people, and a strong confidence had taken hold of the public mind that we had seen the last of the pestilence. And did the value of real estate improve the least, as a result of this confidence? Not at all; but on the contrary, it experienced a downward tendency. And why so? Because improved property scarcely yielded enough, in many portions of the city, to pay the tax-gatherer. And now after the accumulated ills of two consecutive epidemics resting upon us, an unbounded scheme of taxation is proposed, based on theories in relation to which scarcely a solitary truth has been discovered. It is not our purpose to enter into the consideration of the special merits or demerits of Professor Riddell's scheme of sewerage. It is a subject for extremely cautious legislative investigation. It relates to a leading department of the public service, the cleansing of our city being of the highest importance not only in its relation to general public health, but to the commonest impulses of decency.

Dr. Simonds, in defending the establishment of quarantine, has treated his subject with decided ability. Though we hold quarantine to be of very limited use in any respect, and totally useless in relation to yellow fever, the disease being, in our opinion, local, indigenous, and non communicable, nevertheless, Dr. Simonds has made one of the best possible defences of the system. He makes but little reference to the published testimony of the Commission; and we regret to perceive in an apology which he makes in relation to his report, that this gentleman has during

the whole year been laboring under the misfortune of a partial loss of sight, which has prevented him from reading and writing.

We have thus merely glanced at these three several reports, without analyzing their contents, and we commend their style, spirit, tone, modesty, and comprehensive brevity,—their freedom from rhetorical flounderings and contortions, self-conceit and presumption,—the absence of typographical coaxing, and solemn, empty affectation of originality and discovery; affording a refreshing contrast with the report of their colleague. The motives which develop themselves to the mind of every one who looks into this volume, stand out, and present themselves in two categories, namely: 1st. On the part of Drs. Axson, McNeil, Riddell, and Simonds, a laudable and natural desire, in bringing reputation to themselves, to do good service to the public; and, 2dly, on that of Dr. Barton, to lay the public purse under contribution for the publication of what he unwarrantably denominates a *Report*, but which is really a tedious book, abounding in absurdities, extravagances, and self-glorification, totally unexampled in the annals of official documents of our science. The truth is, Dr. Barton has long been, mentally speaking, in an *interesting condition*, and having excellent reasons to distrust the natural efforts of book-birth, he has deemed it proper to have recourse to the municipal forceps, and the delivery has thus been greatly facilitated. No one will pretend to deny that Dr. Barton has been an adroit manager in this business. The whole Sanitary Commission movement has worked admirably to his purposes. He took possession of the whole ground at once, and this enabled him to perpetrate a book; and, in the meantime, he has so far superseded his colleagues and kept them in the back ground, that no one is now known or spoken of in relation to these sanitary proceedings but Dr. Barton! Persons familiar with Dr. Barton's former and present sanitary projects, might be deceived into the belief that he was laboring under some peculiar kind of monomania; but even were this actually the case, it must be admitted that there is great "method in his madness," as the consummation of this book fully shows. There are evident marks of design. The Commission had a Bartonian origin, progress, and *denouement*. In the introductory and prefatory matter, his colleagues have unfortunately failed to guard and define their literary, and scientific identity, in their juxtaposition with their colleague. They have waived all

objection, protest and disclaimer, and are content to remain comparatively out of view.

Are recommendations to be made to the Common Council? Dr. Barton gives a thirteenth chapter to the subject—giving sixteen recommendations, and “*finally*” a seventeenth, one of which, the eighth, being to *fill up Gormley’s basin, and make a public square of it—a public square out of Gormley’s property!* Yes, he gives recommendations which, if adopted, would give the *Illium fuit* to New Orleans! Is the *project* of a Health Department ordinance wanting? He gives one, elongated to twenty-two sections, his fourteenth chapter being nothing but ordinance from beginning to end. Is money wanted? His fifteenth chapter is all finance; and before he has done the funds have swollen to one million four hundred and thirty-five thousand dollars. A mere drop in the bucket, however, in defraying his proposed expenditures.

But this gentleman has further remarkable advantages in relation to this book-report. It is one great and stupendous *advertisement*, and puts him at the summit of all *advertising doctors*. He strikes, it is true, altogether higher than those sorry M. D.’s who think of five dollars in advance, or adopt the miserable platform of no cure, no pay. On the contrary, his advertisement proclaims no less a thing than the wonderful discovery of an infallible remedy which will positively cure New Orleans of a distemper of more than half a century’s standing—a distemper which discovered Columbus and his men just as soon as they had discovered America. The

“———unknown, terrible and indistinct,
Yet awful Thing of Shadows—”

which has for more than three hundred years eluded the investigation of the medical world, has at length unmasked itself and succumbed to Dr. Barton, and the advertisement of the event is most conspicuously given to the world, as will be seen by the sequel.

In the meantime we believe the Crescent City, repudiating the remedy, and, clinging to life, will be reduced to the necessity of replying to her tax-devising Sanitary Doctor in the language of the poet:

Le médecin que l’on m’indique,
Sait le Latin, le Grec, l’Hébreu,
Les belles lettres, la physique,
La chimie et la botanique,
Chacun lui donne son aveu,
Il aurait aussi ma pratique;
Mais je veux vivre encore un peu.

The late period at which the Report of the Sanitary Commission was received, prevents, at present, a thorough examination of that work. A few remarks upon the Dedication, and the Report of Dr. Barton, will occupy as much time as can now be devoted to the subject. No apology can be necessary for entering upon the examination of a Sanitary Code, involving questions of public policy of paramount importance to society. Bias, error and misapprehension belong doubtlessly to reviewers as well as others, as this paper may possibly prove; but truth will prevail, how much soever it may be opposed, and will suffer no lasting damage from the shock of contending opinions, mingled as they often are with egotism, pride, fallacy and dogmatism.

The Sanitary Commission say, in their dedication, that "they cannot part with the voluminous record of their toils, without an expression of their entire and unwavering confidence, that, if the preventive and remedial measures they have recommended, shall be fully carried out, rigidly enforced and perseveringly maintained by the city authorities, *it would be altogether impossible for the yellow fever to originate here, or to be disseminated as an epidemic here, if brought from abroad.*" An announcement like this, so solemn on "parting with its voluminous records," may be left to its own fate, so long as common sense, reason and science are left free to combat such unwarrantable assertions as that, for instance, in which the Commission declare that their proposed measures will render yellow fever an *impossibility*, and "will exterminate it in any locality." (217.)

Impelled by the inherent properties of Dr. Barton's hypotheses, or by some extraneous and more valuable consideration, which time will unveil, the Commission have adopted and endorsed Dr. B.'s book, miscalled a Sanitary Report. Big book, big pay! The proposed Board of Health (\$8,000 for the chairman), must of course fall into the possession of the Commission, who declare that they can render "yellow fever an impossibility in New Orleans." Think of that, ye who live! Hear it, ye angry ghosts of the dead! Let "the filthiest city in the civilized world" open wide the door of its treasury to the inspired Commission, and yellow fever shall henceforth be "an impossibility." Allah is great! but the Commission

are the true yellow fever killers. Is it not written? “demonstrated?”

Do the Commission speak from experience, or from the history of epidemics? Neither the one nor the other will justify them in a single instance, not even in small pox, in arrogating to themselves such pretended absolute control over desolating epidemics. Omnipotence alone can claim such unlimited dominion in Nature. Human power can no more be certain of rendering yellow fever, any more than death, an *impossibility*. The appearance and the disappearance of yellow fever epidemics at hundreds of places, and at irregular and long intervals, have never been explained by any ascertained atmospheric and terrene conditions, either as antecedents or coincidents of an invariable character. Nor has the presence of the supposed causes of yellow fever been followed by epidemics invariably.

The dominion over nature claimed by the Report, is further exemplified, as follows: “It is satisfactory to know, that we can both moderate the amount of rain liable to fall, and the amount of moisture,” 420—as well as “the North wind,” 422.

The unlimited power claimed by the Commission over the epidemics of yellow fever ranging from the possible to the impossible is one which the profession will be slow to receive and believe with the history of epidemics before their eyes showing the contrary. This claim was doubtlessly intended to be put forth for popular effect. Indeed, one of the parties to whom the Commission awards special thanks for his reliable testimony, which served at least to eke out the book, testified in the public prints of New Orleans, that he too could prevent the epidemic by inoculation! The Commission having omitted this most valuable testimony which was made public in the newspapers long before the report appeared, and which was doubtlessly intended also for the people—not for medical philosophers and commissioners, it will be given to the profession upon the principle *falsus in uno, falsus in omnibus*. Its author has been endorsed with thanks, as already mentioned, by the Sanitary Commission—those sentinels whose duty it is to guard the temple of *Æsculapius* from the intrusions of Charlatany with the flaming sword of scientific truth. This same witness closes his long testimony before the commission in the following meek and humble tone: “I have purposed to give them [my

opinions] with humility appropriate to the subject, I implore indulgence from criticism," &c. Brethren in *Æsculapio!* read the following advertisement which appeared in the Bee, Aug. 18, 1854, and in other journals, and is too long to be copied entire. Its material parts, including its false orthography read thus: ("with humility!")

HIGHLY IMPORTANT DISCOVERY—INFALLIBLE PRESERVATIVE AGAINST YELLOW FEVER—PROPHILATIC VACCINATION.

"Wm. Humbolt, offers his important discovery to the unacclimated, and begs to remind them that they are actually threatened by that terrible malady.

An arduous and profound study of this disease in different parts of America, and especially in Vera Cruz, suggested to me the idea of an attempt toward the discovery of a preservative. Under the impression of this idea I have made numerous experiments, during seven years, on more than two thousand persons; and I have found at last the means of preventing this terrible disease.

I have found a matter or substance, which, introduced into the circulation by some endermic method (as used in vaccination) and followed by the antiseptic syrop of my prescription, for the period of nine days, brings the organism to such a state, that the producing miasms of that disease must be excreted without causing any toxic effects.

This vaccination (to adopt the term,) being made with a very simple substance, will occasion no pain whatever, or cause any notable alteration. It will permit all persons to follow their usual occupations, and only requires abstinence from irritating food and liquids during the nine days prescribed for the antiseptic syrop. After the lapse of said time, the vaccinated will be free from the risk of being attacked by yellow fever.

Persons wishing to make use of this Prophilaetic Remedy or feeling inclined to obtain some further information on the subject, will please call on me from 10 to 12 A. M., at the Drug Store of Espinola & Co., Condé street, Nos. 52 and 54, between St. Philip and Dumaine streets, and from 1 to 6 P. M., at my lodging, Cineinnati Hotel, Chartres street, corner of St. Louis.

DR. HUMBOLDT.

New Orleans, Aug. 18, 1854.

N. B.—Persons desiring to be vaccinated at their lodgings will please send their address to the Drug Store of Espinola & Co."

Having glanced at the Dedication, in which Dr. Barton's plans are apparently and unexpectedly endorsed by the Commission, this gentleman's report will be examined a little more closely, and the more so because he repeatedly "invites the utmost scrutiny." He says: "Looking then to the momentous interests we represent, in this first great sanitary movement in the South—inviting the utmost scrutiny into our facts, principles, authorities, the corollaries we have deduce d

&c.”—218. This “scrutiny” is the more necessary in a document of an official character, which purports to be the foundation upon which is to be erected a new and expensive system of legislation for the overthrow of epidemics. If this foundation be solid, the ink of criticism, printed malice, and the scowl of envy cannot injure it, much less the architect who in caption of one of his chapters writes thus—*“the cause of yellow fever known.”* He who can justly claim the right to put this in the heading of one of his chapters, deserves, and will receive, an apotheosis by universal acclamation. The discoverer of the material cause of yellow fever, not to name that of immorality, (“which has the same paternity,” according to Dr. Barton) must take precedence over the discoverers of the laws of gravitation, the physiology of the circulation, and pain-destroying etherization.

Dr. Barton says: “They (the ample records) all concur to prove without the shadow of a dissent, (both the land and sea causes,) that filth and fresh earth, (the principle involved being probably the same) with atmospheric conditions, has [have] produced, and by sequitum will produce yellow fever—that *it [they] originate it, and that the cause of yellow fever is known.*” In all this statement, which has been made a thousand times from generation to generation, there is no discovery, but much bad grammar and false punctuation, as usual, in almost every paragraph penned by Dr. Barton. In commencing a sentence, he appears to have no idea how it will progress, when it will end, nor whether half a dozen nominatives should have singular or plural pronouns and verbs, so that his *accidence* is right by *accident* only. In this place a few words may be added illustrative of the Bartonian Bathos and Belles Lettres. “Disruption of ordinary catenation—324—Sanitary reform is the talismanic *indicium* and distinguishing amelioration—210—City robbed from the swamps—224—Boundless insalubrity. Fate foredoomed by negligence and ignorance of invariable physical and moral laws, advances to destroy the cherished pride of many ages—226—Spring came—summer leaped into her lap. Here was one branch of the shears prepared for its influence. Our drowsy Councils—335—The dark, murky cholera cloud hung over our devoted city, like a funeral pall, and struck every heart with dismay—264—Give it [the shears of fate] life, intensity, and development—282.”

“The other blade of the shears is the terrene, and embraces all foul, filthy—give rise to this great monarch of disease, (yellow fever) 309–11. It is time to put aside and be done with all this stultifying and misleading mystery and awe, and, boldly facing and defying all carping misgivings, push onr scrutines—311. *Like the pelican foster in our bosoms the poison that is sucking our vitals—382.*” His rhetoric is most impartially administered; for whether the subject be the vile *cloacæ*, “the shears of fate,” or the fall of the Roman Empire, the style is all the same. Such specimens of fine writing glitter on every page of this book, but in no other.

It may be expected that something more definite than has been expressed, as yet, should be said of the literary merits of a work which has been labored for more than a year by many hands, and is intended as a monument creditable to its authors, to the medical faculty, and the city government of New Orleans, which latter organized the Commission at the public expense. This Sanitary Report, or Dr. Barton’s Report, is a solemn, pompous burlesque, unwittingly made on the English language, and all its fundamental principles of construction. If New Orleans is to be officially baptised as the dirtiest, sickliest, and most sinful city in the world, she must be at the same time the most illiterate, if this book is to be her representative in the republic of letters. In a marginal note, Dr. Barton says—“Intelligence synonymous with health”—450. It follows that New Orleans is the most ignorant, being “the sickliest city in the world.”

Excepting Dr. Barton, the Commission is known to be distinguished for scholarship. How, then, does it happen that the Queen’s English has been murdered, and the President’s fellow-citizens in good letters brought into contempt “by authority.” Did the Commissioners decline to aid Dr. Barton? Or did he refuse to accept their offered corrections? Why did not the Commissioners protest against his bad English, incoherent arguments, inaccurate statements, and numerous contradictions, thereby guarding their reputations from an express or implied approval of this Report? Of tautologies, affected verbosity, false metaphors, unwarranted hyperboles, obscure, incoherent, and involved sentences, and fruitless attempts at fine writing and sublimity, nothing will further be said. A few examples of the violation

of the more simple rudiments of the English tongue will show the character of the whole by implication.

Without taking into view the general elements of English composition, a single feature will suffice to show that the reporter does not know the simplest rules of the language, as, almost any part of the Report will "demonstrate." Take for example, small portions of sections 9 and 10, as being, perhaps, the least faulty in the Report, and the following, among other false constructions, will appear officially :

"Delusive assurances * * * *deceives*"—301—*deceive*; repeated in the same sentence—"assurances *produces*"—*produce*; same sentence. "Diligence and surveillance *is* required"—392—*are*. "Extensive inundations to which various parts, &c., *has* been"—296—*have*; repeated in the same sentence. "The number, &c., *are* estimated"—398—*is*. "What *was* called outside cases"—299—*were*. "From various public institutions *was* procured 14,680 cases"—*were*; same sentence. "To these *is* to be added those of the Charity Hospital"—400—*are*. "The proportions * * * *was* impossible"—400—*were*. "Four hundred dead bodies *was*"—406—*were*. "Where *was* buried 638"—406—*were*. "Predominate in Savannah during *their* epidemics"—422—*its* epidemics. "Back yards where all the offals and filth of families *is* concentrated"—424—*are*. "Pure water and an abundance of it *are* as essential"—429—*is*. It is by restoring these, and a removal of the impediments to their free exercise resulting from his congregating in cities, that *constitutes* and *lays*," &c.—434—*constitute* and *lay*. "High civilization and a proper estimate put on human life, *is* known by," &c.—435—*are*. "The offensive materials on its banks *is* derived"—442—*are*. "The fevers of Pensacola, &c., *demonstrates*"—443—*demonstrate*. "The proportion of deaths *were*"—445—*was*.

Dr. Barton professes to have discovered not only the material cause of yellow fever, but the material cause of immorality and irreligion, sin and sickness being, according to his official Report, "twin sisters," having "one and the same paternity," so that physic, hitherto considered as ranking second to theology alone, may now take the front rank, and theology must resign its high claims to superiority and take an inferior position. Æsculapius, henceforth, prescribes remedies of the most comprehensive and fundamental effi-

cacy, whereby both the soul and the body are to be redeemed, regenerated and healed. As this great discovery of the cause and prevention of sickness and sin is a novelty which will make New Orleans famous, being official and "by authority," and nowhere disavowed or protested against by Dr. Barton's co-Commissioners, it may be necessary to use the exact words of the Reporter, including his *italics*, which are sprinkled over his compositions as if they fell out of the sand-box: "Disease and crime have a similar paternity. They are twin sisters; as one so flourishes the other. There is not a doubt in my mind that *the most effective means of advancing the cause of morals and religion among us, would be the establishment of sanitary measures*"—223. At page 354, the marginal note is as follows: "Moral and physical condition dependent on similar circumstances," while the text reads, "The real and true relation between moral and physical degradation, is now beginning to be correctly appreciated under the improved sanitary," &c.; "Ministers of the law, as well as of religion, are discovering," &c.; and such like expressions, are repeated *usque ad nauseam*.

If religion is based on a sanitary foundation, and if irreligion be owing to the neglect of sanitary legislation, the old cities of Europe and of the residue of the world, must be far more pious than New Orleans, which is always, in the Reporter's vocabulary, "the filthiest city on earth," and consequently the most Godless, according to the logic of the official Report.

The Reporter glorifies England, (in which sanitary improvements have only recently commenced), and gives the figures to show, in her three principal cities—the least salubrious of the Empire—that the sanitary condition is three times more favorable than that of New Orleans, and, consequently, three times more religious. It was Luther who said every man carries a Pope in his belly, and of course each thinks himself competent to determine what is the true doctrine.

The propagators of religion in the latitude of Jerusalem have suppressed this great sanitary truth, namely, that irreligion, as well as epidemics, is caused by "disturbing the earth," and by "some atmospheric conditions," and that the extension of religion is based on a sanitary platform, "both having the same paternity." The

Jerusalem teachers do not seem favorable to the doctrine of the Report, that all the afflictions which lay waste the earth, are under man's control, and come solely from the turning up of the earth and atmospheric condition. They even say, "Whom the Lord loveth he chasteneth," that "without chastisement, ye are bastards, and not sons," and much more to the same effect, without alluding to sanitary platforms, "by which epidemics of yellow fever become impossible."

If sickness and sin have "the same paternity," and require the same treatment, as filling, paving with square blocks, &c., the Thirty-Nine Articles ought to be amended—with reverence be it said!—and the Sovereign Pontiff should authorize and inaugurate this materializing sanitary doctrine, lest the faithful fall into not only insolvency, but into a damnable heresy. In the meantime, the City Government of New Orleans may not be in a hurry to endorse the doctrine nor anticipate the decision of the church, but leave the whole to "the shears of fate," or to the limbo of the garret or baser uses.

Those conversant with the history and geography of yellow fever, for centuries, know that it has prevailed in both the New and the Old Worlds, in high, dry, sandy, hilly and cleanly places. In Louisiana, in the great epidemic of 1853, villages situated on the clean, dry and elevated pine terraces, with undisturbed soil, suffered equally with, and, in some instances more than New Orleans.

In the great hive of humanity, where the population is densest, dirtiest, and most destitute of sanitary improvements—whether terrene, subterraneous or atmospheric—namely, Central and Southern Asia, from the Persian Gulf to the Chinese and Yellow seas—from the plains of the Indus to the swamps of the Ganges—from the base of the Himmalaya Mountains to the depressed jungle regions which expand upon lower India and the Indian Ocean, yellow fever does not prevail. It is not contended that dirt, digging, ploughing, making levees, wharves, house-foundations, railroads and canals, do not cause yellow fever, but it is maintained that, as yet, there is no satisfactory evidence (notwithstanding the one-sided evidence in which Dr. Barton delights) to prove that yellow fever is in any manner dependent on these as its cause or invariable antecedent.

It is true that all towns must have some dirt or earth at hand, which by the aid of a certain but wholly unknown atmospheric condition may cause yellow fever, which unknown condition Dr. Barton in his ceaseless but painful efforts to be rhetorical, says is his discovery and has two blades, the one "*terrene*," "of the earth, earthy"—"the other atmospheric" or skyey. These two blades he says are "the shears of fate." Armed with this terrific instrument the scissors of Atropos herself, he utters the following potent argument: "Now skepticism must yield up its doubts, and even sophistry can no longer contest the demonstrated truth. A certain combination of conditions in certain localities produce in thousands on thousands of instances, yellow fever."

"The proposition then is that it requires an extensive disturbance of the original soil, or vast accumulations of decomposable materials to produce an *epidemic*, presuming that the meteorological conditions are present;" 324; these constitute, as he affirms, "the special cause of every epidemic yellow fever." We will here add Dr. Barton's description of "yellow fever weather." "It is characterised by being *very hot in the sun and cool in the shade at the same time*—on one side of the street a broiling temperature, and on the other so cool as to urge to buttoning up the coat. This uncomfortable alternation of chilliness and heat, is productive not only of uncomfortable feelings, but when exaggerated passes into disease—constituting the first stage of yellow fever;" 300. Let the public who can see and feel equally well with Dr. Barton, decide as to whether or not the whole of this is not merely and purely imaginary, and whether the acclimated population, during the ravages of the pestilence, do not enjoy their usual measure of health, as has hitherto always been affirmed and believed.

Dr. Barton in speaking of the salvation of New Orleans, by his method, adds, "the subject itself is not a difficult one * * *. It has been done a thousand times before, with *but one uniform result*." That certain unknown things or "certain combinations of conditions" are necessary to produce certain other things, even cholera or yellow fever, is a mere truism which no one ever questioned. There is a certain unknown condition supposed to exist in India, called by the natives, according to Bishop Heber, "the essence of owl," which causes fevers. This may be true, but it is not proven, or "demonstrated" to use the favorite word by which Dr. Barton establishes

all his fanciful postulates; for his “demonstrations” are assertions; his assertions, hypotheses; his hypotheses being almost always either improbable or impossible. His logic is resistless whenever he attempts either to prove what no one doubts, or “demonstrates” that which is altogether self-evident.

It would be as difficult to discover who opened the first oyster; as to say who, and how many—long anterior to the issue of the Sanitary Report, attributed fevers to the disturbances of the earth’s surface by art, by earthquakes, by volcanoes—to cometary and atmospheric phenomena, to heat, moisture, filth and putrefaction, comprehended for the most part by the term miasma, malaria, or bad air, &c. This doctrine as old as the historic era of medicine is announced by Hippocrates, but not [as his discovery nor as his “shears of fate:” he says: “Some diseases are endemic, peculiar to certain regions, and attacking numbers. Others originate from a peculiar constitution, regimen, locality, or season. Unhealthy situations produce diseases corresponding to the constitution of the atmosphere, that is dependent on the locality. Some diseases arise from marsh and other exhalations, or from the nature of the water, producing calculus or affections of the spleen. The winds are of a beneficial or hurtful character. As the constitution of the year, so are the diseases.”* More than twenty-two centuries later Lancisi, of Italy, revived this doctrine in its “terrene” and “atmospheric combinations,” now named “the shears of fate.”

Dr. Barton’s uncharitable, not to say unwarrantable denunciations of all those able and conscientious citizens, and writers of the medical and newspaper presses, who differ from him in the dogma, “that New Orleans is the sickliest city in the world,” owing solely to its bad sanitary police, criminal negligence of cleanliness, and a willful *suppressio veri*, must have forgotten how this same Dr. Barton when in high feather, as an officer of the Board of Health, announced in his official bulletins, but a few years ago, that “New Orleans was by far the healthiest city in the world,” and offered statistical tables to prove this assertion, having defied all mankind to show figures or

* Translated by J. R. Coxe, M. D., Philadelphia, 1846, p. 105.

facts to the contrary! He even declared officially that "acclimation is worth the risk"—the most extravagant and exaggerated statement ever made, and one which the then editor of the Louisiana Advertiser ridiculed without measure in a series of articles in that paper, contending that acclimation or yellow fever was good for nothing and even infinitely worse than nothing.

Dr. Barton, now that there is no Board of Health for him to be in, avers that "New Orleans is not only the sickliest city in the United States, but in America,—nay in the whole civilized world;" (224.) * * * "Now New Orleans is one of the dirtiest, and with other conjoint causes, is consequently the sickliest city in the Union," (220)—an axiom with the reporter which he repeats habitually. As sin and sickness have according to the report, the same paternity in dirt, sincere confession once for all ought to suffice—more cometh of ostentation. That Dr. Barton has a right to confess, and pronounce the *peccavi* for himself none can deny; but by what code, sanitary or moral, is he bound to confess for the people of New Orleans, and that too in an official document.

Lot, that righteous man, interceded for his fellow-citizens of Sodom, but Dr. Barton confesses fully for his compatriots as being the dirtiest, and consequently the most immoral people known.

The following passage from Dr. Barton's report is illustrative of the highly charitable spirit with which he disposes of all dissenters from the filth platform. After telling us that the whole thing is so very plain that "it requires nothing but visits and familiarity with these festering sores to convince the most skeptical that filth, (crowding is the same thing, for it speedily generates it,) high temperature, and humidity, produces [produce] yellow fever, and yellow fever of the worst form,—that from these *foci* it emanates as by radiation and expansion;" after assuring us "that it has been proven a thousand times before, that one of the most efficient agents in the production of yellow fever is filth, of all kinds," (p. 413,) the following ejection of official words are hurled at the heads of his opponents, in which nothing short of Lynch law is invoked in behalf of our meteorologico-terrene Reporter: "Is there one so isolated that has the effrontery to say he is not interested in what so deeply affects the welfare and prosperity of society—pecuniary, commercial, social, religious? If so, society should arise in its

might and banish the wretch from amongst us; he is no longer fit to participate in the numberless blessings for which we are indebted to the kindness of a merciful Providence."

Notwithstanding the severe but pious discipline here proposed for the correction of unbelievers, we cannot withhold the expression of our admiration at this involuntary sample of the art of sinking, and, with Dominie Sampson, we are constrained to cry, "*prodigious!*"

As this sanitary or Bartonian code is put forth with all the positivism which language admits of, claiming to be the organic constitutional basis of future sanitary legislation and city and State polity, involving the expenditure of millions of the public treasure, it may be "doing the State some service" to examine, refute, or confirm it; which, indeed, the Commission demand in the most defiant tone.

It may be proper to remark as we proceed, that Dr. Barton's sanitary measures possess one inherent quality by which they may be identified, namely, impracticability. A few months ago, Dr. Barton, then a chairman of an official committee, submitted a long preamble and resolutions, "which were ordered to be published in all the papers." The third resolution proposes to make "New Orleans one of the healthiest cities on the continent of America," "by filling up with solid earth all the swamp and low land between the river and the lake." Waiving the objection that, according to Dr. Barton's theory, the turning up of the earth is the cause of yellow fever, it is sufficient to say that, pecuniarily and physically, this "filling up to the lake" is impossible, morally speaking. The late Geo. T. Dunbar, State Engineer, in an official document, dated February 17th, 1840, gave a detailed statement of the cost of filling a small section of the city comprehended between Claiborne, Canal Carondelet, Bayou St. John, Première, and Esplanade—amounting to nearly twenty-two millions of dollars! Mr. Alderman Lesseps, about the same period, in a published address to the Council, proved, by a careful calculation, that in order to give the requisite filling of a surface of 10,000,000 superficial feet, between the Bayou Road and the Canal Carondelet, it would require a period of two hundred and eleven years, allowing an average yield of batture deposit in front of the Municipality, (now the 2d District,) of seventeen thousand five hundred cart loads per year. To fill the whole space, seven miles long and four wide, "from the

river to the lake," would, therefore, require millions of years, but how many let the arithmetic tell if it can.

Dr. Barton's plan of covered drains, sheds along the entire front of the city, square block pavements for the whole city, the filling up of Gormley's basin and the making of a "public square of it," the removal of the forest, the making of avenues and parks of the cypress swamps, levees on lake Pontchartrain, and many other similar improvements, though not new, would exhaust the treasury of the United States, including the California mines. For instance, the amount of streets to be paved with granite block, including the streets to be cleared of round stone pavement and repaved, as recommended by the Report, is, according to the city map, little short of two-hundred miles. This, at the rate of granite paving, supposing there were no embankments to be made, would cost little short of twenty millions of dollars.

The system of filling from the batture accretion, or other source, would disturb the soil, which Dr. Barton repeats, from page to page, as the cause of yellow fever: "A clue was found to the causes of our fatal epidemics; it was clearly demonstrated, in the language of the proposition, that there has been no great epidemic yellow fever in this city without an extensive disturbance of the original soil." 315. The early settlers, who constructed hundreds of miles of levee in Louisiana, "a long time ago," must have had charmed lives to escape the "shears of Fate."

At one single session of the Lafayette City Council in 1849, a resolution was passed ordering about twenty-eight miles of brick sidewalk to be laid, and the whole of the streets to be embanked and graded to the proper level. This led to one of the greatest disturbances of the "virginal soil" that ever took place in New Orleans. In the rear of Magazine street, for many squares, it was found necessary to cut away from the centre of every street from one to three feet of earth, and to make embankments in the front streets in order to obtain the grade. In order to lay the brick sidewalks, it was found necessary to either cut away or make embankments all over the city, and the elevated sidewalks thus made in front of Magazine street converted the unfilled squares into perfect stagnant water-basins.

These excavations were done principally during seasons of the year in which yellow fever prevails. In the meantime, in the same period of the year, acres upon acres were filled up by the citizens, who, in so doing, "disturbed the soil;" and in the meantime, also, the grand draining canal and protection levee in the whole rear of the city, thirty feet wide, was excavated, at the same season of the year, by unacclimated Irish laborers. The amount of ditching, excavation, embanking, filling up, &c., in the city of Lafayette, during the years 1849-50-51-52 was altogether unparalleled—was done by unacclimated laborers, in the summer and autumn—and but little was known of yellow fever. The annexation of the city of Lafayette to New Orleans, in the spring of 1852, put an end to these excavations; and in 1853, when there was relatively little disturbance of the "virginal soil," the now Fourth District, in common with the whole city, was visited by the most awful mortality. In September, 1840, the original soil in the whole front of the city of Lafayette, sixty feet wide and a mile and a quarter long, was disturbed by making a levee, and no yellow fever was produced. The disease waited twelve months after the construction of this Lafayette levee.

Dr. Barton, speaking of the sanitary amelioration of the city of Charleston, holds the following language: "The ponds and creeks have been filled and drained; and the low grounds and lots filled up, levelled and thoroughly drained by underground sewers; a careful avoidance of disturbing the *original soil* of the streets, &c., during certain months, for gas, water, or other purposes; the constant study of her meteorological condition," &c., "that from being one of the sickliest, she has become one of the healthiest cities in America;" 439. Dr. Simmons, however, whom Dr. Barton quotes to prove this conclusion, says one thing, and Dr. Barton says another. Dr. Barton uses the present tense, and Dr. Simmons, using the first future tense, says: "Actively pursuing this plan, I firmly believe it *will be* the cause of making the city not only one of the healthiest among commercial cities," &c. Dr. Barton says she "*is* one of the healthiest," because Dr. Simmons says he "firmly believes" she will become so.

It is curious that Dr. Barton's facts should be almost always fanciful, one-sided or perverted. Dr. Barton not only endorses, in his own name, the superiority of Charleston, as being "one of the

healthiest cities in America," but he endorses this piece of history, also, in the name of the Commission. He says: "These are valuable practical facts, the result of long years of experience, from high authority, (as most of us know personally), and uttered in an enlightened city, where such advice will be appreciated." 440. That the Medical Faculty of Charleston is as "enlightened" as any in the Republic, is not denied. But "the Charleston Port Physician and Chairman of her Boards of Health for near thirty years," quoted by Dr. Barton, does not say that she "has become one of the healthiest cities in America." He only intimates that she "may possibly" become such! Verily, Dr. Barton, when it suiteth his purpose, "de spiseth not prophecying," and very cautious prophecyings at that; for when the Port Physician of Charleston says that sanitary measures "MAY POSSIBLY make us in a great degree, if not entirely, exempt from yellow fever, and should it occur, lessen its virulence," Dr. Barton calls these sayings "practical facts, the result of long years of experience," and transmutes the prophecy into history, for he distinctly says "that Charleston has become one of the healthiest cities in America." Moses and the prophets spoke positively and "no jot or tittle shall fail;" but what are the circumstances which authorize Dr. Barton to change the tenses of Dr. Simmons' words? How is it in reality with Charleston, notwithstanding the words of our very reliable New Orleans Sanitary Reporter?

Without impugning the truth, honor, and lofty character of Dr. Simmons—without so much as inquiring whether he has any emoluments arising from his double office for thirty years, as Chairman of the Board of Health and Port Physician—without questioning the high authority of this gentleman—it is sufficient to say, that he has not asserted that Charleston "*has* become one of the healthiest cities in America," but that "she will *possibly*" become so. As Dr. Barton has positively asserted that Charleston *has* become one of the healthiest cities," and quotes Dr. Simmons as his authority, the passage will be quoted, that the reader may judge for himself.

In speaking of Charleston, Dr. Barton says: "It lies on a peninsula, almost surrounded by the rivers Cooper and Ashley, the neck cut up by creeks and ponds, and extensive swamps in the neighborhood. The ponds and creeks have been filled

and drained; the low grounds and lots filled up, leveled, and thoroughly drained by underground sewers; a careful avoidance of disturbing the *original soil* of the streets, &c., during certain months, for gas, water, or other purposes; the constant study of her meteorological condition by her intelligent faculty, and the establishment and *enforcement* of sanitary regulations have had the effect of so improving her condition, that, from being one of the sickliest, she has become one of the healthiest cities in America. I quote from a recent report on the yellow fever of Charleston, by one of the oldest and most respectable physicians, (who has been her Port Physician and Chairman of her Boards of Health for near thirty years*): ‘In proof, I say, these plans have been progressively going on, and in proportion as the healthiness of the city has been improved, and while the public authorities are gradually, nay, I may say, actively pursuing this plan, I firmly believe it will be the cause of making the city not only one of the healthiest among commercial cities, but may possibly *make us, in a great degree, if not entirely exempt from yellow fever, and should it occur, lessen its virulence and mortality.* From long experience and observation, I regard it the solemn duty of the public authorities to go on with this plan, regardless of expense, not only for the preservation of health, but for the extension of commercial prosperity.’ These are valuable practical facts, the result of long years of experience, from high authority, (as most of us know personally,) and uttered in an enlightened city, where such advice will be appreciated.”

The above specimen of Dr. Barton’s reliability as to his relation of facts, serves to prove that in Charleston, where, as he affirms, sanitary measures have been carried out and “enforced,” and where, alas! yellow fever has increased in malignity, instead of having “become an impossibility.”

It is but a poor compliment upon the enlightenment of the Medical Faculty of Charleston to assume that they are ignorant of the history of yellow fever in that city—ignorant of what Ramsay, Hewatt, Lining, Currie, and many more recent writers have written. Moreau de Jonnès, in his history of yellow fever, (Paris, 1820) assigns a greater number of epidemics of yellow fever to Charleston than to any other place in the North temperate zone. (Tab. Géog. 344.)

* Dr. T. Y. Simmons.

Before Dr. Barton's Report had passed through the press, the epidemic of 1854 had raged without parallel in Charleston; and yet he publishes "that the establishment and *enforcement* of sanitary regulations have had the effect of so improving her condition that she has become one of the healthiest cities in America."—439.

As Dr. Barton insists that Charleston, owing to her great sanitary improvements, "has become one of the healthiest cities in America,"—(439) it may be well to read the following: The Editors of the Charleston Medical Journal for November, 1854, say: "In our September issue, we announced the existence of yellow fever in Charleston, and expressed our fears that it would become in a short time epidemic. Our apprehensions have been fully realized; for, so universal has been its diffusion as to almost entitle it to the appellation of *Pandemic*. Nor condition, nor sex, nor age, nor race, nor geographical situation has enjoyed an exemption from it. Total deaths to the 1st of Nov., 600."

If the account given by the able Editors of the Charleston Medical Journal (of Nov., 1854) shall be fully confirmed, after a thorough examination, which they will doubtless make, it will inevitably follow that the late epidemic in that city is without parallel in the South, where it never has been before "*pandemic*," that is, incident to all classes of the population. The Editors say: "It attacked the native as well as foreigner, and, if the belief in the unity of the fever be adopted, almost, if not quite, in the same proportion; the black race in larger proportion than ever before known; and its geographical distribution has been more extensive than at any former prevalence, the disease never having previously visited the Northern and Western parts of the city. But although the proportion of native population *attacked* was very great, the proportion of deaths among them was *very small*, in comparison with that foreign population.

The above Editorial, in the opinion of many of our physicians, should require some modification. Many believe that old residents were either entirely, or almost without a single exception, exempt from *yellow fever*, though some, even of those, suffered from mild attacks of some form of fever. It is certainly not more than one-third, or even one-fourth, that of foreigners; making, of the 600, 200 na-

tives; a large proportion of the latter being children. The numbers, when published, will however, conclusively settle these points. Not to include the Hospitals, it has generally been conceded by physicians, that during few preceding seasons, have a larger share of recoveries rewarded their attention, than during the past summer in this city. In private practice, so mild did many cases of fever prove under nursing and good management, that very few, comparatively, of these died. Though fever was very general among the black population, the mortality was extremely low. Complete statistical accounts will appear in this Journal, with full and reliable information on all these topics."

The Port Physician's hopes, and Dr. Barton's sanitary facts have proven false. It takes the New Orleans Sanitary Commission to "render yellow fever an impossibility," "*if their advice is followed, not otherwise;*" besides, "it is satisfactory to know, that we can both moderate the amount of rain liable to fall, and the amount of moisture"—420—as well as the "*North wind!*" "It (the North wind) is almost entirely under our control."—423.

The leading article in the November number, 1854, of the Charleston Medical Journal, by William Hume, M. D., Professor of Experimental Science in the State Military Academy, contains a recantation of that gentleman's opinions formerly published upon the causes of yellow fever, as malaria, local filth, &c. As this gentleman is high authority with the Commission (nearly half of the report on quarantine having been copied *verbatim* from him,) it may be proper to give his views of Dr. Barton's hobby-horse on which he has ridden all through the report.

Professor Hume says: "The cotemporaneous occurrence of two events ordinarily suggests to the mind the relation of cause and effect, but that this should be conclusive, the same concurrence should always be observed. If disturbances of our soil is capable of engendering fever at a certain season. we may infer that fever should occur whenever the soil is disturbed at the same season in different years, and that fever should not occur when the soil is not disturbed. Founded upon this view of the origin of yellow fever, there is an ordinance prohibiting all excavations from May to October. This law was based upon observations of the last few years, especially the fever of 1852,

being cotemporaneous with the excavations for the custom house. The most prominent instance of a similar coincidence in our country, was the digging of the Carondelet Canal in New Orleans in the year 1796, and the first appearance of yellow fever in that city.—(See *Thomas and Picornelle*.)

But the fever has continued to appear in that city and no cotemporaneous canals have been dug. In our city we have had several extensive excavations, and no constant concurrence of fever. Commencing with the custom-house foundation of 1852, in connexion with the development of the fever of that year, I have obtained the following details from the records of the progress of the work, which are undoubtedly authentic and may be relied upon. The first excavation on the North wing was commenced on the 8th of January, 1852, forming a pit seventy feet long, thirty wide and ten deep—thus removing 21,000 cubic feet of earth, logs and mud. On the 5th of August, the digging, piling and concrete were finished and the masons were at work, salt water percolating into the pit all the time, which was removed by a trunk and pumps. In July, inundated for three weeks with salt water. On the 5th of October, there were 211 persons employed; of these 196 were Irishmen, 3 Englishmen, 1 Pole and 11 Americans. Up to this period fifteen had been sick and five had died—a mortality certainly not indicative of any local cause of disease, especially when we recollect that these laborers slept in other parts of the city, and were exposed to general infection as well as local. If this pit had been the cause of the disease, we should have expected the first cases would have been among them, but we all remember that the first case was a lady on Brown's wharf, foot of Queen street, and subsequently the disease proceeded up the same to a house in Philadelphia street, and the first case in this house was a little Irish girl, (Mary Ryan, aged 8 years,) who was in the habit of frequenting the wharves to pick chips.—(*Dr. Lynch*.)

If the excavation of 21,000 cubic feet of earth in 1852 could produce such a calamitous result to the city as has been supposed, we have a right to infer that the same result should have occurred by the continuation of the work in 1853. During the same period of 1853, 143,000 feet of earth were excavated and heaped up on the premises; 202 Irish laborers were employed in August and September, yet not a case of fever appeared. The exemption from disease of the workmen in 1853, is conclusive that

the excavation of 1852, had no 'lot or part' in the production of the disease of 1852, and the laborers in 1852 only suffered in common with other citizens, from a cause unconnected with their particular occupation, and we have every reason to conclude that the same calamity would have occurred, if the foundation of the new custom-house had never been commenced in 1852.

The disturbances of the earth, as an original cause of endemic fever, is further weakened by the history of the various gas works. In May, 1847, the gasometer pit was commenced in Church street, and completed in December, 1848. During the summer of 1847, 125,000 cubic feet of earth, wood, shells and clay, were removed and distributed over the city. How many Irishmen were employed in the operation it is impossible to determine as it was a contract, and no record to this effect is to be found in the office. The number was not less than one hundred. In November, 1847, the laying of pipes commenced, and was continued all winter and next summer; and in April, 1848, the city was lighted with gas. Nearly every street was dug into in order to lay these pipes to the extent of miles, yet strange to relate no fever was produced by this profound and extensive excavation. The August and September sun poured its hottest rays upon the heads of at least one hundred Irishmen, strangers to our climate, immersed in a pit ten to twenty-six feet deep, without a current of air to fan their burning bodies; notwithstanding the supposed poisonous exhalations from the earth, not a case of fever was developed. With such an example as this, how can we cling to the creeds of our forefathers in supposing an endemic origin to fevers of their times, arising from excavations, when we witness the contrary in our times. We cannot consent to attribute the origin to such local causes as these, any more than we can refer the origin to an old sink, a cess-pool, a rotten potatoe, or a putrid mouse.

The gas pit in King street, near Vanderhorst, was begun in May, 1849, and finished in November of the same year. The work of excavation progressed during the fatal summer of 1849, yet there was no remarkable fatality in the immediate neighborhood. The nearest case was a German apothecary, Mr. Meyer, who probably took the disease in the lower portions of the city, and brought the germ of it to his home. I say probably, for the following reason:—Mr. M. learned that his pastor, residing in Society street, had yellow fever, and he determined to visit him. He

called at Mr. Panknin's and induced one of the young men to accompany him. On their return about 9 o'clock, P. M., they were overtaken by a shower of rain and took refuge in the market, where they were detained about one hour. They finally returned to their respective homes, viz: Meeting and King streets. Both of these persons took sick and were buried within twenty-four hours of each other; and, if I dare add another probability, by a common infection taken at the same time and place, that is in the market during the rain. I have elsewhere shown that the fever of 1849 took its origin on Union wharves, and subsequently extended to other portions of the city.

During the summer of 1853, a third gas pit was dug in Cannon street, and as no fever was observed in the neighborhood, it is only mentioned confirmatory of the simple fact, that extensive excavations of the earth in Charleston may be executed without involving the neighbors in one common calamity of disease."

The following is taken from the modest caption of Dr. Barton's 7th section:

"Proposition—The upturning of the original soil, together with filth of all kinds—the *sine qua non* of all our epidemics—proofs as far back as 60 years, to the present period—how first *discovered by me*," &c. Of the disturbances of the earth, that in California has been greatest, yet yellow fever has not resulted.

Touching the disturbance of the "virginal soil," we make the following assertion, and appeal to the common sense and experience of Christendom. This disturbance both deep and superficial is constant, unremitting, and continued at all seasons throughout the civilized world, and never has been suspended in either town or country, since the discovery of America—nay, since the Adamic expulsion. That this state of things has been constant and unremitting in and about New Orleans. That Dr. Barton's cause of yellow fever has never been absent from the city, and hence neither he nor any other individual can possibly tell what agency the disturbed earth has had in the production of the disease; for if digging a well in the country can produce yellow fever, digging the foundation of a house, or digging a well in the city can do the same thing. Dr. Barton's whole position is utterly fallacious and incredible, and his great excavation chart shows absolutely nothing. The disturbances of the soil afford equally good proof for and against Dr. Barton's positions. Too

much is proven. He who should contend that public health is promoted by disturbing the surface of the earth, might equally chop logic with Dr. Barton, without either of the parties losing or gaining an inch of ground. We may here remark that Dr. Barton's 13th recommendation forbidding any but the most superficial disturbance of the soil between the 1st of May and the 15th of October, is wholly impracticable. The deep disturbance must take place in the prohibited season or not at all, as high water interferes with excavations in all other seasons. Another instance of Dr. Barton's impracticabilities.

It is most deplorably true that New Orleans has been visited with epidemics. The generation has not yet passed away which witnessed many yellow fever epidemics in the cities of both Europe and America, which were often, and may again be more fatal than any which have visited New Orleans.

A single paragraph from Ozanam's History of Epidemics (T. iii; 236) will show that during the present century, in a single year, the following mortality from yellow fever occurred in the cities of Spain, many of which are high, dry, and well drained :

	POPULA'N.	DEATHS.
Seville.....	80,560	30,000
Cadiz.....	68,000	16,000
Leon.....	32,000	8,000
Xeres.....	30,000	8,000
Santa Maria.....	25,000	6,000
San Lucar.....	18,000	4,000
Port Royal.....	10,000	3,000
Chiclana.....	10,000	3,000
Rota.....	6,000	1,500
Total.....	279,560	79,500

This table "demonstrates" an average mortality of nearly one-third of the whole population, far exceeding any epidemic that ever occurred in New Orleans.

Admitting the charge, that New Orleans is the most sickly spot on the globe, it does not hence follow that its people are the dirtiest, and, consequently, the most immoral and irreligious—these evils having "the same paternity," according to the Sanitary Report. It may be suggested, that the Grand Jury of the Parish of Orleans will, or might have before it, one of two alternatives, namely: to present the

city as a nuisance without parallel, or the Report as a gross libel, unworthy of the acceptance, much less the sanction, of the City Government. New Orleans, which is probably no dirtier than New York and many other cities, has been singled out, and, without a particle of proof, proclaimed officially as being the dirtiest city in the world. What has been charged to the whole population, if published concerning a family or an individual, would be an actionable calumny. Such an accusation against this city ought to crimson the cheek of any people not wholly devoid of common decency.

“We are permitting,” says Dr. Barton, “every village of yesterday to outstrip us in the race of population, of wealth, of public monuments, of social improvement and intellectual enjoyment; and at last, but what is first in importance, HEALTH, we stand positively lowest in the scale. 387. All the facts we have garnered, &c., will insure to New Orleans an exuberance of health, and cause her to rival in salubrity the healthiest large cities on the globe.” 388.

“Poverty, filth, intemperance and crime have a similar paternity. Disease originates from them, and, taking the wings of the morning, it spreads itself to the uttermost parts of the earth. 384. Yellow fever—we have no further excuse for its continuance among us, and I do seriously think, that it is as much the duty of the civil authorities to keep this city free from yellow fever, as it is to keep it exempt from any other controllable calamity.” 386.

The question, which is the dirtiest city in the world? is one very much like those questions propounded in debating societies, in which the premises, arguments, and conclusions are equally good for both sides, for anything one can know. It is true, however, that the Reporter “demonstrated” and decided this question but a few years ago, as above mentioned, in his official character as President of the then Board of Health, and in a report from his own hand, “that New Orleans is by far the healthiest city in the world, and that acclimation is worth the risk.” A city that has expended many millions in sanitary improvements, and still owes a balance of nearly nine millions, and which is still the filthiest in the world, ought to be avoided by all who wear clean linen and dread contamination, taking the official report as authentic evidence.

Dr. Barton sums up his denial of acclimation in this laconic generali-

zation, (Ind. xviii): "Mortality not for want of acclimation." * * * The mortality of New Orleans is not, according to Dr. Barton, in any manner dependent on the presence of the unacclimated: "There is neither necessity nor propriety in denominating this *want of acclimation*. The accusation is no less a slander upon the climate than it is upon decency." 392. "Hence, then, away with the nonsense about the difficulty of acclimation, which only tends to blind the ignorant." 394.

If this be true, ninety-nine in every hundred citizens long resident in New Orleans must be indecent as well as slanderers. If the natives and acclimated be no more exempt from yellow fever epidemics than new-comers, all the mortuary certificates ever written in this city are either forgeries or falsehoods. Dr. Barton is the first writer who has presumed upon the credulity of the public so far as to advance the dogma that the city creole and recent immigrant are equally liable to yellow fever, and equally contribute to the mortality. Dr. Barton's grand deduction, (in italics, of course,) is in these words: "Hence, then, *the true test of the salubrious condition of a country must be in its friendliness to the stranger—the facility of its being reconciled to the requirements of his constitution*, and not merely to the native, the acclimated." 393. This test, so dogmatically put, is contradicted by universal experience, which proves that neither animals nor plants, men nor monkeys, when transported to remote and different climates, flourish equally with the natives of the place.

To point out all the absurdities, inaccuracies, and perversions with which Dr. Barton's Report abounds, would require more time and space than can be devoted to this hurried notice. To show, however, how totally unreliable this document is on naked points of fact, within the observation of every person in this community, we shall merely notice a position it takes in relation to the numerical prevalence and malignity of the epidemic of 1853. The Reporter would have us believe, in opposition to common sense and common observation, that this numerical prevalence and malignity was governed by the unpaved districts and filth-spots laid down in his "sanitary map" of the city. Every one who paid the commonest attention to the epidemic of 1853, knows that the numerical prevalence in the different wards of the city was none other than in the direct ratio of unprotected per-

sons therein resident; that no "acclimated" person in any of the filth-spots of the sanitary map ever took, or dreamt of taking, the yellow fever; that the mildest, as well as the most malignant cases were found, side by side, on the dirty beds of the immigrants, in these localities, while some of the most splendid mansions in the city were not exempt from the black vomit and death. No one will deny that the mortality in the filth-spots of the city was greatly increased by the following "general causes: 1st, by destitution and exposure; 2d, by the low vitality of the ship-worn immigrants. But the numerical prevalence was no way affected, nor was, so far as is known, the specific character of the disease at all governed, by these causes. There was no escaping the disease, nor was there any known means of predetermining the intensity of the disease in any case. But Dr. Barton had his binomial equation, *air plus filth, equal yellow fever*, to work out; and his never ending "demonstration" (*demonstration* is the favorite word by which he characterizes all his hypotheses,) has a strange appearance when eliminated of its equivalents and stripped of its co-efficients.* Not a single witness amongst the New Orleans physicians, who testified before the Commission, confirms Dr. Barton's official assertion about the numerical prevalence of the disease; but nearly all deny it, either by implication or in express terms. Nor did any one here confirm his views in relation to the effects of disturbing the surface of the earth. Any restriction touching this disturbance would not only put an end to improvements in New Orleans, but would suspend most of the avocations of civilized life. The filling of lots and squares in New Orleans can only be done, and has always been done, in the yellow fever season of the year, as in no other season can the necessary earth be procured.

A word in relation to the published testimony, of which the second part of this volume consists. We gave it a close examination, with a view of appreciating its bearing on the doctrines of the Report, but we cannot now go into its merits. Suffice it to say that it is most remarkable how little of this part of the report goes in any way to confirm Dr. Barton's views. The testimony may be thus classified:

*It would seem, from Dr. Barton's labored attempt to decry dirt, that he thinks there must be somebody in New Orleans more partial to this article than himself. In his solemn platitudes in this behalf, he contends with imaginary opponents. He proves what no one ever doubted

1st, that of witnesses who came before the Sanitary Commission, consisting of New Orleans physicians, who have said but little, and nothing confirmatory; 3dly, special pleadings and arguments of believers in the doctrine of the Reporter; 2dly, the testimony collected by Professor Riddell, mostly non-professional, to establish his position that the town infected the country; 4th, the miscellaneous reports and letters from abroad. The display of italics which appear whenever certain doctrines are favored, shows either a remarkable scrutiny of the testimony, or a miraculous coincidence among the witnesses.

Dr. Barton, speaking of the condition of the city in 1853, with reference to his meteorologico-terrene ætiology, says: "this early connection of the atmospheric element with the physical, showed, in combination, a foreshadow of what was to come, and enabled me to give a warning as early as the middle of May, in the Academy of Sciences, in this city, of the disastrous consequences that were to follow, and to some scientific correspondents. How that prediction was verified, I now proceed to point out." "Combination of the atmospheric element with the physical!" As though "atmospheric elements" were not "physical!" But this is not the point—neither need we speak of the dangers in which the above construction places his "scientific correspondents." We would merely say a word in relation to the prophecy or "prediction" of the Reporter. If Dr. Barton really, as he would have us believe, knew that these "disastrous consequences" would occur; that in less than four months, thirty thousand persons, in and about New Orleans, would be smitten with the pestilence, and that there would be a great South-Western epidemic, why did he confine this awful disclosure to the Academy of Sciences merely and to a few scientific correspondents? He alone was in possession of the secret; no one else had drawn, or could draw the meteorological horoscope. Why did he not call together a universal mass meeting, and, exhibiting his zig-zag lines and decimal fractions, implore the people to listen to his warning voice? Why did he confine his disclosures to the savants of the "Academy" and his learned "correspondents." Was ever such an opportunity of immortality thus sacrificed? a crown of glory, within grasping distance, thus refused? Unparalleled stoicism! He saw the yawning blades of "the Shears of Fate" opening upon the devoted city of New Orleans, and the first intimation of his prediction is

in the Report before us. He told it to the savants! He did not even awake the "drowsy Councils." Why did he not proclaim it upon the house-tops, and in the highways? No uninspired man ever before obtained such an insight into the future. He ought to have known that there were at least 30,000 unacclimated persons in the city, the result of five years accumulation. He ought to have known that the mortality would be frightful. The prediction did not even startle the "Academy" nor the "scientific correspondents." They wait for the prophet to do homage to himself, and to blow an astounding blast on his own trumpet, after the fact. Why does not the Academy of Sciences speak? Why did they not then speak? Why was not the remarkable prediction proclaimed by them to the world?

But, further, having had the most ample opportunity of observing the epidemic of 1854, we assert, and are fully prepared to show, that the disease was never surpassed here in malignity; and no one can doubt that all the conditions, whatever they may be, existed, in their most unmitigated form, in 1854. Doubtless, then, while the "shears of Fate" were being sharpened for the slaughter, the prophet spake again to the "Academy of Sciences" and to the "scientific correspondents." Why did they not hear his voice, and proclaim his words? Nay, why did he not himself prophecy aloud to the people?

A Commission which can "control the North wind," "the quantity of rain," and "make yellow fever an impossibility," might equally speak a couple of suns into existence, for the purpose of warming the Polar regions. The Sanitary Report, which proposes not only to make epidemics "*impossible*," but to improve religion, does not seem to recognize the doctrine, that Divine Providence has frequently sent epidemics upon the world regardless of pavements, gutters, drainage, and the upturning of the earth's surface. Not to name more recent examples, the following will suffice to show that there is a Power higher than that of the Commission: King David (1 Chronicles, chap. xxi) incurred the wrath of Heaven, and the alternatives of famine, pestilence and the sword were tendered him, for his statistical presumption in numbering Israel. David chose pestilence as his punishment, in these words: "I am in a great strait; let me fall into the hand of the Lord; for, very great are his mercies. But let me not fall into the hand of man. So the Lord sent pestilence on

Israel, and there fell of Israel seventy thousand." And it was not till in his own sovereign will "he repented him of the evil," and said "it is enough, stay now thy hand," that the epidemic ceased.

The following passage is copied not only for its rhetoric, but more especially for the purpose of showing what Dr. Barton and his co-Commissioners can and will do for the dirtiest and vilest of cities, New Orleans—"if their advice is followed—otherwise not:" "Here, in the midst of filth of all kinds, are the true birth-places of disease; and it was equally clearly shown how much it was in our power to remove them, and that it was our duty and interest to do so, not merely on account of present prosperity, but even from a higher consideration, the promotion of the great interests of religion. Our reputation abroad for salubrity is ruined; reform is now only left us. All cities, wherever situated, whether in the old world, the highly favored seats of renowned monarchs, covered with marble, drained and watered by works that are still the wonder and admiration of the world, or in the new, the selected marts of far enriching and enterprising commerce, have been subject to epidemics; which, like the monaxysinal diseases, the trials of infantile life, stand as tests of the constitution. Many quail and sink under the trial. New Orleans has stood many such trials, she is now at a crisis, and it will depend upon her people to say whether she shall recuperate or not. Her consulting faculty (this Commission) have pronounced her entirely susceptible of cure, *if their advice is followed*, otherwise not. A new era has manifestly sprung up, it is signalized by the appointment of this first Commission of inquiry into the real, not suppositious, condition of New Orleans. We look upon it as a proof that the great reformation so much needed, and without which no permanent prosperity is to be expected for this city, is about to commence." * * * "Nothing else is now wanting to repair the errors of the past, and it does no violence to probability to foretell from them the most brilliant future. If the Sanitary Commission shall succeed in convincing their fellow-citizens of this, and that the same principles are applicable to our city as to all others, which is the true practical object of their appointment, then that appointment will have been superfluous." * * * "Our city may be restored to salubrity; that her reputation for perennial pestilence shall be no longer applicable; that upon the broad founda-

tion of SANITARY MEASURES we can erect a monument of public health, and that if a beacon light is erected on its top, and *kept alive by proper attention*, our city will soon be second to none in this first of earthly blessings; the appointment will not have been made in vain."

This solemn, graphic, and sublime close of the rhetorical part of the Report has no parallel in fiction. With one foot on the sidereal Heavens, and the other on this dirty terrene globe, New Orleans lying between the legs of the colossal sanitarian, he holds in one hand a scroll, inscribed "Sanitary Report"—"Yellow fever epidemics rendered impossible!" "The quantity of rain," and "the North wind controlled!" In the other hand see "The shears of fate!" one blade of which, the terrene, is run into the ground—the other into the sky. The "sickliest of all cities" lies between the jaws of this monster, while Dr. Barton tenders the dread alternative—destruction! or the acceptance of his control over this devoted city, whereby it shall become "the healthiest"—"*otherwise not!*" Let no profane caricaturist placard this solemn tableau upon the walls of this devoted city, lest Dr. Barton should withdraw his protection, and leave epidemics among possibilities hereafter.

After this solemn and pathetic farewell, in which New Orleans is graciously vouchsafed perpetual protection if it will submit itself to the safe-keeping of the high potentates of the Sanitary Commission "*otherwise not*"—the Report plunges at once into legislation, finance, and Boards of Health.

M. MORTON DOWLER, M. D.

ART. II.—*On the Nature, Signs and Treatment of Child-bed Fevers, in a series of Letters addressed to the Students of his Class.* By CHARLES D. MEIGS, Professor of Midwifery and the Diseases of Women and Children, in Jefferson Medical College, Philadelphia, &c., &c. Philadelphia: Blanchard & Lea, 1854. Pp. 362, 8vo.

[COMMUNICATED.]

We were somewhat acquainted with the views of this writer on the subject of Child-bed Fevers, previous to perusing the present volume, both from the Introductory Essay which he published in "The

Select Medical Library," in 1842, and the notes which he appended to his translation of "Colombat on Diseases of Females," and published more recently.

The present is a much larger and more pretentious exposition of the same subject, and may be supposed to embody his more matured views and experience. From a hurried examination, we cannot say that we have been able to see that he has added much to what he was previously concerned in publishing to the people on this side of the Atlantic; indeed, the present may, in a great measure, be regarded as a re-issue of these same writings, no doubt amply *Meig-nified*, if we may be permitted to follow his example and coin a term.

Passing over his motives for writing the present work, with the history of puerperal fevers from the ancients down to our own day, which occupy a considerable space of the volume, we come next to his notions on the blood, and especially on its condition in this and kindred diseases. He seems to be prepared to ignore all that has been more recently, by the aid of the microscope, added to our knowledge of this important fluid. Like the famous Professor of Padua, he will not consent to look through the instruments which reveal the new wonders, but satisfies himself, it would seem, in reviling the doctrine.

The blood, he remarks at page 70, has been charged with becoming so changed as to become a common carrier-of-all-sort-of-things, like an Adams & Co.'s express wagon; and again, "you shall hear," he says, "scientific people talk of the blood circulating within the living body, and permitting the manifestation of the most perfect health and power, though all the while loaded with foreign materials and modified to that degree as to cause the most extraordinary suddenness, the greatest violence, and an irrepressible tendency of disease to destruction of the life!" * * * * *

"I cannot bring myself to believe that because a woman is pregnant, or lying-in, her blood may lose its specific character and become something else, which is not blood, but which still carries on the functions of the blood, and either destroys her life through child-bed fever, or after bringing her down to the gates of death, instantly, in the twinkling of an eye, ceases to trouble her, and allows the recovery of the most perfect health, even in a very short time." * * *

“It is a dogma to say that the blood is poisoned, and that an entity foreign to the body is within it, exercising on the organism its pathogenic force. I say it is a dogma, and a dogma is only an ‘I think.’ To say is not to know, but only to believe. A dogma is a settled opinion, but a settled opinion is not a knowledge, but only a belief. Now, as to foreign matters in the blood, you can only think them to be there, you cannot know it.”

It is in this, the veriest strain of rant, that he would dispose of all the well-attested observations of Virchow and Kilian, and a host of other microscopists, who have detected abnormal constituents in the blood in parturient females; and who, on well established grounds, regard the altered condition of the blood in child-bed fever as a primary, rather than a secondary result, or phenomena, and from which the others follow. Hear him further *à la Carlyle*: “Think, think for yourself, O, student of Medicine! Make yourself a free citizen of the Republic of Letters, that you may know of yourself and not of another, what is the truth.” But how is the poor student of medicine to know what is truth for himself, in many important particulars, if not in the same manner that the student of natural philosophy would be told to proceed; the latter has to depend on calculations, and give credence to well-established observations made by competent masters: he is not expected to, because he could not, make all these for himself; nor in medicine can the student either afford to do without the assistance of those who have preceded him and made the science what it now is; and the *ipse dixit* of whom it were well for even “American students” to respect, rather than follow their own immature judgments or observations on either healthy or diseased tissues and products. The “dear young gentlemen” must be very young indeed, and deeply imbued, too, with the predominant idea of young Americanism, if they do not feel already that such a style of teaching is not the one best calculated to prepare them for being intelligent and useful practitioners, however much it may tend to gratify a prurient vanity among them, at once dangerous to themselves and annoying to those with whom they are sure to come in contact in after life.

After telling us at page 76, after the manner of Oken, that “the blood is the fluid body, and the body is the fixed or rigid blood,” and something more about the relation of the blood vessels (endangium)

which is “the solid through and by which all the induction of life or nerve-force, into the elements of the young blood is effected, in order to raise them to strength and perfection, and it alone sustains the life of the older or maturer globules. Hence, a condition of that induction is that the blood shall move from space to space within the inducing or hæmatomic surface. To rest is to die: to cease to be endowed with oxygen is to die; and to be removed from the presence and influence of the endangium, is equally sure to bring about the death of the blood, which lives in and by it, as its perpetual, watchful, particular providence and protection, its life-giver and sustainer.” * * *

“The vital state of the blood is, therefore, in constant dependency on the vital state of the endangium. Is the tissue healthy, the blood is so likewise; is it sickly, exhausted, dying, the blood changes its phases along with those of the power it depends upon.”

On this we would only shortly remark that the tendency of modern researches in both physiology and pathology, is towards the establishment of the opposite of the doctrine here inculcated; and in this to approximate to the old notions regarding the vitality of the blood. As to the claim here set up for the inner coat of the vessel being the life-giver and sustainer of the blood, this superiority of endowment will be seen to be simply absurd, if we reflect that in the development of the embryo, the primordial cells, from which are formed both the blood and the vessels—the former from the nucleus, the latter from out the cell walls—a process which goes on simultaneously, and which is effected by the vital force inherent in the cells themselves, which is thus transfused to both the blood and the tissues, and is the “particular providence” which traces out the path and destiny of both alike. There is nothing in all this to give countenance to the assumption that “the true blood-vessel gives life or nerve-force to the elements of the young blood. The nuclei of the primordial cell is certainly, for all we can know to the contrary, at least, equal in histogenic importance to the cell wall.

Based upon this notion of the superior importance of the blood-vessel, we have the doctrine maintained that “child-bed fevers are nothing more nor less than instances of endangitis; cases in which the membrana vasorum of the uterine and crural, and even the cava-veins are the seats of an inflammation, called child-bed fever, milk-leg, *dépôt*

laiteux, &c., &c." * * Further on he says (page 77) "When I speak to you of blood in diseases, as depraved, as vitiated or dissolved; as ruined and incompetent to carry on the functions appertenant to it; as the antagonism of the solids; I desire you to understand me as speaking of diseases of the *membrana communis* (*endangium*). I do not more conclusively refer the dysenteric diarrhœas, croups, &c., to a state of the gastro-intestinal, or pulmonary or laryngeal mucous membrane, than affections of the blood to affections of the blood membrane (*endangium*). All scarlatinas, measles, variolas, varicellas, erysipelas, gout, rheumatism, and many forms of child-bed and other fevers, have their prime seat in the blood membrane, (*endangium*) and are but so many varied expressions of its diseased conditions."

This is solidism run mad, and, like its opposite, exclusive humoralism, in medical discussions, is sure to lead astray whoever takes hold of it to explain all pathological phenomena.

The intimate connection of the solids and the fluids—that which is fluid to-day may to-morrow be solid, and vice versa makes it altogether improper, because it is false—to set up any exclusive preference like that which is here set up as if in perfect mockery of the Oken views quoted above, and which otherwise would seem to be recognised as the doctrine of our author. For our own parts far from being carried away by the reasoning of our author in favor of the solids being the primary seat of the affection more particularly under consideration; or influenced by the ridicule with which he assails the humoral pathology of the same, we are much disposed to yield more importance to the latter, as the more correct explanation of the order of progress in a large class of diseases; and this as we have already hinted, is the tendency of the teachings of our best writers on pathology both here and elsewhere. Dr. Meigs seems to be fully alive to this fact, and expects nothing but "a smile of contempt" from "the self-sufficient physiologist" of the present day; but he is ready to meet this, as well as the scorn of "many a professional martinet," in the full assurance that posterity will do him all justice, and place him and the "*Endangium*" high in their regards henceforth and forevermore; a consolation which the dreamers of false doctrines have died sharing equally with the masters of truth, in instances not a few disclosed in history. On the subject of the aptness of the lying in woman to take on this special diseased action, as proved in many an unfortunate

and wide spread epidemic among them, we have seen already how gallantly our author comes to the rescue of the fair in this interesting moment of her existence, and how he endeavors to establish her claim to a clean bill of health, which by many, he avers, has been most unjustly refused; yet in his enumeration of the risks attendant on child bearing the real practitioner rather than the flimsy gallant is found giving utterance to such sentiments as the following: "Such instances must be met with by every person considerably employed in the practice of midwifery; being accidents that must, and will attend the trials of parturition. They have been observed in all ages and nations, and must be met with hereafter, as long as the nature of woman and the physiological laws of parturition remain unchanged." (p. 115.) * * * * *

Such causes acting upon a constitution rendered susceptible by pregnancy, by labor and its accidents, stand ready like the hair trigger of a rifle, to hurl the mischief at the slightest provocation, throughout all the parts within the pelvis and the abdomen." * * * * * "And the fact undeniably, is, that no accoucheur who thoroughly understands his mission as a professional man, is ever completely exempt from a feeling of uncertainty and anxiety as to the safety of his patients in child-bed."

Of one thing he, however, permits her to remain consoled with, and that is the assurance, contrary to the usual teaching, that in no circumstance does she prove while suffering from this disease herself a focus of infection, or contagion, to infect other parturient females about her. And, notwithstanding all that has been said, and supposed proved, of obstetricians being the unwitting instruments of communicating the infection, which was thought to cling to their persons or clothes, yet he can it seems console himself with the following gratifying facts, which he relates with not a little of egotism.

"I have practiced midwifery for many long years; I have attended some thousands of women in labor, and passed through repeated epidemics of child-bed fever, both in town and in hospital. After all this experience, however, I do not, upon careful reflection and self examination, find the least reason to suppose I have ever conveyed the disease from place to place, in any single instance. Yet for many years I carefully considered whether such transfer, by a third person, might be possible and carefully read the statements of various authors to that effect. In the course of my professional life I have made many necroscopic

researches of child-bed fevers, but did never suspend my ministry as accoucheur on that account. Still, I certainly was never the medium of its transmission ;" (page 102.) This is positive enough.

Blundell in his lectures on midwifery remarks, in reference to this subject: "There are some men who entertain a lurking belief of the infection of this fever, notwithstanding all their intrepid declarations to the contrary; not that in these declarations it is their intention to deceive, but there is a curious phenomenon of the human mind, well known to those who have studied it, and which consists in fancying we believe that to which we give no credence, and the contrary; a state of mind which is soon discovered to ourselves and others, by placing ourselves in a position which calls for the operation of the faith or belief, when infidelity becomes manifest;" and an opposite illustration of this is then related, which need not be repeated here.

Enough has already been quoted to show Prof. M.'s views as to the causes of child-bed fever. With regard to the introduction of pus from the inflamed womb, or the veins in its neighborhood, into the general blood torrent, we have a repetition of what Dr. R. Lee and others have said upon that subject; the whole as confidently stated as though he were entirely ignorant of the more recent experiments made by Mr. Henry Lee, and since repeated by others, which go to show the improbability, if not also the impossibility, of pus injected into the vein of a healthy subject, getting into the circulation as previously supposed; and he also overlooks the important experiments, and reasonings, of Dr. — Mackenzie on the subject of phlebitis which go to prove the notions of Dr. Lee to be, at the best, exceedingly questionable on this matter. The altered condition of the blood being in these shown to precede the phlebitis, rather than to follow, views which are in the main adopted by Churchill and others, but which are here entirely unnoticed, and the old notions insisted on, as if they were unquestionable, with all the dogmatism imaginable. Without much of punning it may be said, both of the matter and spirit of this portion of the work, they "are lees [Lee's] frae end to end."

With regard to the treatment of child-bed fevers, which our author still continues to advocate, it is entirely that so forcibly laid down by Dr. A. Gordon. The best we can say of it is, that if it was suitable in Gordon's day when this, and several other diseases, assumed a much more

sthenic character than they now are seen for the greater part to do, and as a consequence may have required a degree of active depletion such as the same now but rarely require—it is neither the treatment which in the old world, or the new, is now deemed by the great body of medical practitioners as the one to be followed, at least as a general rule; blood-letting as a chief remedy has long since passed out of favor. In the exanthematous diseases, in erysipelas, as well as in typhus, remittent and yellow fevers, the severe antiphlogistic measures which in former years were carried out are not found to suit so well in the types which these different diseases for the most part assume in our day, and hence a change in the treatment has been made, and, we must add, with great and corresponding advantage. But while our author would perhaps admit, that in the above diseases the milder treatment here indicated is such as will best fulfill the intention of the rational practitioner in child-bed fevers, however, which depend in the first place, he holds, an inflammation of the tissues require, in order to lessen the tendency to expansion, free blood-letting. The idiopathic fevers he holds are “affections dependent on states of the encephalic pole, while inflammatory diseases and disorders of the organs are results of failure or derangement in the organ-poles of the body.” “They are diseases of opposite poles,” and if the treatment which would best suit in each is not “as opposite as the poles,” then, it should be.

The sulphate of quinine we know has been used in large doses, and with the best advantage, in the idiopathic fevers, and given, too, in erysipelas in the same way; and there is ample testimony to its efficacy in it also. Its controlling action, when combined with morphine, in child-bed fevers, we have had several opportunities of testing, and can speak of it, as we can speak of it in remittent and yellow fever, as of undoubted efficacy; while, in our own experience, depletion has been as little required in the cure of the former as in the two last named complaints. We feel confident that these diseases, as they present themselves in this latitude, at least, are more likely to yield to this treatment than the severe depleting systems, which not long since found such general favor; and, that venesection, which still in the case of child-bed fever has many advocates, though few so bold as our author, will come to be much less practised when this disease, like the others referred to, comes to be regarded as mainly depending on some altered condition of the blood, which requires to be corrected.

ART. III.—*A Systematic Treatise, Historical, Etiological, and Practical, on the Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian, and Esquimaux varieties of its population.* By DANIEL DRAKE, M. D. Edited by S. HANSBURY SMITH, M. D., formerly Professor of the Theory and Practice of Medicine in Starling Medical College, Ohio, and FRANCIS G. SMITH, M. D., Professor of the Institutes of Medicine, in the Medical Department of Pennsylvania College, Philadelphia. Second Series, Philadelphia: Lippincott, Grambo & Co., 1854, pp. 985, 8vo.

Professor Drake, after a long and eventful life has but recently descended to the tomb lamented by the whole medical profession, being regarded by some as the most eminent medical writer that this Republic has produced. He had passed through many storms—had reached the culminating point of his fame, when his sun set majestically without a cloud in “the great Interior Valley of North America,” whose medical history he had so ably illustrated in a large volume destined to immortality as being the best expositor of the primordial state of society in the West.

Enemies and rivals, as well as friends, are apt to bestow unqualified commendations upon the dead, and especially upon one who has like Dr. Drake, merited the gratitude of the country whose literature he has enriched by his contributions.

Although Dr. Drake's second volume or series, mentioned at the head of this article, will doubtlessly be received with alacrity as being of a practical character, yet there can be little doubt, that the first will prove, from its historical data the more enduring monument to his fame. His editors have doubtlessly done their duty ably, fully, nobly, disinterestedly, both as it regards the arrangement and completion of his unfinished manuscripts. It is evident, however, from his first volume, and still more from his second, that if Dr. Drake had lived to complete his second series, his plan would prevent his work from becoming a standard one in pathology and therapeutics, because the many hundred doctors whose desultory conversations and fugitive publications he has detailed with infinite pains, do not afford complete and satisfactory results. Memoranda taken from

conversations held upon the hill tops, river banks, coasts, bayous, cypress swamps, cane brakes, cities and plantations, must be defective. It may be questioned whether in the most learned societies, conversations and extemporaneous debates, deserve to be inaugurated into octavos intended for posterity. It is to the pen—the cautious, slow, thoughtful, considerate pen which pauses, reconsiders, retraces its steps—advances, reconnoitres the whole field of operation and then, (to change the metaphor) instead of extemporaneous shots, sends its well aimed volleys into the camp of charlatany, prejudice, ignorance and error.

Pathology, therapeutics, physiology, and pathological anatomy, and even the methods of interrogating nature, are progressive, and ere the bones of Drake moulder to dust will, like the early sociology which he has so graphically depicted, assume new aspects. Even facts in medicine grow old in a certain sense, as to their value or use in new combinations of society and physical alteration incidental to a new country. Were Hippocrates of Cos, Sydenham of England, or even the late Dr. Rush of Pennsylvania, to alight among his successors in the art of healing, the surgery, the pathology, the therapeutics, the physiology, the histology, the morbid and microscopic anatomy of the present era would surprise and astonish his "honest ghost." A bottle of quinine, or a stethoscope, would be incomprehensible—painless surgery a miracle—steam vessels and cars superhuman, and telegraphic dispatches outstripping time itself, impossibilities. Hippocrates should he leave the Elysian Fields for earth, would have to go school to Young Physic, and be laughed at too for his ignorance. As time rolls on, the same spirit of progress now in operation will have created a new Medicine, which Drake and his cotemporaries had not known.

The genius of Drake is descriptive, historic, observant, vast, and appreciative, but without striking originality. His fame does not rest on new and profound generalizations and specialities in physiology, ætiology, pathology, or therapeutics, but upon comprehensive views of the art and science of healing in its connections with climate, temperature, rain, humidity, winds, basins, waters, rivers, lakes, seas, valleys, prairies, hills, mountains, soils, vegetation, and geology; the hygiene of towns, plantations, trades, and races; the social institutions, manners and habits of the "great interior Valley of North America." This able work is now, and probably will ever be the most valuable representative of the origin and

progress of the medical mind and social aspects of society in the West, from its foundation to the middle of the 19th century. The corner stone was laid and the structure of civilization was inaugurated under Drake's eyes. Millions of the Caucasian race have displaced the savages, and the wilderness has blossomed as the rose, in one life time.

It is by patient investigation in the fields of observation, experiment, ratiocination and induction, concentrated into comprehensive monographs, that medicine must advance until a complete system or "Treatise" shall be achieved—an event which the most sanguine can scarcely hope to see actualized amid the ever fluctuating masses of humanity swayed to and fro by internal and external influences little appreciable, and often new. Probably neither Hippocrates nor his successors for two thousand years, ever witnessed either yellow fever or cholera epidemics.

Hippocrates often mentions *black urine* as characteristic of the fevers which he treated—an appearance not now witnessed. He attended the son of Parion, near the Temple of Diana, for a fever which ended fatally on the 120th day; he also attended Herophythus for a fever which did not reach a crisis until the hundredth day. Although there is nothing definite given by which these cases can be identified with the fever recently called typhoid, yet, from their duration, it is probable that these cases were either typhoid or typhus. Another patient of his, Pythion, who resided above the Temple of Hercules, appears to have died on the 10th day, of congestive remittent, which on the banks of the Mississippi, usually runs its course in a shorter time.

As to medical science, the ancients have failed in description, reasoning, and in writing. The moderns leave to their successors complete histories, many discoveries, and well-reasoned, if not perfect monographs, representing the facts and deductions of their era. Dr. Drake's book will prove to his successors a historical treasure, such as ancient medicine has never presented.

At the same time his work is scarcely up to the level of knowledge in pathology, therapeutics, pathological anatomy. His views on these subjects, taken often from desultory conversations, as before indicated, held with hundreds of doctors during his travelings, are, as might be expected, not of the most satisfactory character for "A Systematic Treatise." He who sets out to travel over half a dozen of States, determined to ask every medical man what his opinion is, may make a large book, if not a

profound "treatise." A traveler will meet many who will agree with him, like Polonius, calling a thing "a cloud, a weasel, or very like a whale;" and, indeed, Swift asserted that the surest way to get credit for wisdom is to agree with every one who gives you his opinion.

The narrow limits of this Journal forbid all attempts to give an analytical examination of the contents of Dr. Drake's work. Every practitioner should procure this book, which, with its predecessor, is, as a whole, unrivalled in its line of research. This is one of the few works which, without lowering the high character of medical science, may be profitably read by all persons of sense and education.

The following lines express his views adverse to the importation and malarial theories of the origin of yellow fever, which fever, nevertheless, he regards as being of local origin :

"Such is the malarial theory of this formidable disease; that which is held by a majority of the physicians of the South; a theory which inculcates cleanliness and free ventilation in houses, yards, alleys, streets, boats, ships, and wharves, to obviate the development of civic malaria, which being prevented, the fever, according to the theory of an importation of ferments to act on that malaria, would equally fail to appear.

But I cannot regard this theory as finally established, although obnoxious to fewer objections than the theory of ferments; for—

1. The whole pathological history of yellow fever *seems* to constitute it a specific fever, differing from an autumnal remittent too far to admit of its being a mere variety of that disease.

2. It has occurred in localities where civic malaria can scarcely be admitted to exist, as in Washington and its vicinity, in the State of Mississippi; in Opelousas; and in the navy-yard on Pensacola Bay, where a white sand-drift constitutes the surface of the ground.

3. It has been produced by exposure to the air, which has escaped from goods sent from a city where the disease prevailed. But I know of no facts which go to show that autumnal fever is ever produced in that manner. It is scarcely necessary here to repeat that there is no well authenticated example of its spreading from this source.

4. While the condition of a town, to all visible appearance, continues the same, the disease will appear and disappear, precisely as it

does while the commercial intercourse with a place continues unchanged.

Until these objections are removed, the cause of this disease should be kept *sub judice*. We are not compelled to choose among conflicting theories, and declare that which is best supported to be established. It may be more probable than any other, and yet not be true."—(Chap. vi. 298.)

EDITOR.

ART. IV.—*Statistical View of the United States, embracing its Territory, Population—white, free colored, and slaves; Moral and Social Condition, Industry, Property, and Revenue; the Detailed Statistics of Cities, Towns and Counties; being a Compendium of the 7th Census, to which are added the results of every previous Census, beginning with 1790, in comparative tables, with Explanatory and Illustrative Notes, based upon the schedules and other official sources of information.* By J. D. B. DEBOW, Superintendent of the United States Census. Washington: A. O. P. Nicholson, Public Printer, 1854, pp. 400, royal 8vo. From the author.

This is a very elaborate and complete work, arranged with great skill, expressing the numerical, industrial, social, vital, physical, and moral condition and resources of the Republic, in several hundred compact and comprehensive tables, accompanied with annotations and well-reasoned illustrations. The typographical arrangement is such that the matter of two or three volumes has been compressed into one, which is far more valuable than the original colossal volume, the 7th Census, of which it is a compendium, with many additions.

Of this valuable work, the House of Representatives of the United States ordered one hundred thousand copies to be printed, under the direction of Professor DeBow, Superintendent of the U. S. Census.

With the completion of this Compendium, Prof. DeBow's official labors in the Census Bureau appear to have been finished. His industry, talents and sagacity, displayed in works on the Census, his Industrial Resources, and his Commercial Review, have merited and received from his countrymen, and more particularly from his fellow-citizens of New Orleans, much commendation. Professor DeBow, in aiming at the use-

ful rather than the shining, shines nevertheless. He possesses that excellent trait of an editor, namely, a perfect willingness to allow his contributors to outshine him in his Review. EDITOR.

ART. V.—*Gazette Hebdomadaire de Médecine et Chirurgie, Bulletin de l'Enseignement Médical ; publié sous les auspices du Ministère de l'Instruction publique : No. 1 to No. 60 ; Nov. 24th, 1854.*

This journal contains the Imperial decrees and documents concerning the medical affairs of France, which, however, occupy but a small portion of its pages. It is a very valuable expositor of the medical sciences in that country, and appears well informed in the medical matters of other lands. It is edited by Dr. A. Dechambre, and surpasses all the French journals of medicine in its typographical appearance, being somewhat smaller than the *Gazette Médicale de Paris*.

In the 52d No. (Sept. 29th, 1854) Dr. A. Dechambre, *Rédacteur en Chef*, translates from the *New Orleans Medical and Surgical Journal*, for May, Dr. Boling's Article on Phosphorus—to which is appended an editorial note by M. Dechambre, who says: "Dr. Boling's paper is important, because it establishes experimentally, what chemistry enables us to affirm *à priori*. The alcoholic tincture of phosphorus is a very bad preparation, being still less charged with the active principle of that substance than Dr. Boling himself has allowed. It is known that phosphorus *plunged in water*, is oxidized by the air contained in the water, and communicates to that liquid a phosphorous acid odor. It is probable that in alcohol it is the same, and that after a contact of fourteen days, especially if the bottle be not full, the phosphorus becomes oxidated, and takes the form of vapor. The oxidation becomes still more complete when the tincture is poured into water, augmenting daily with the exposure of the tincture to the influence of the air."—(Page 941.)

The titles of the articles and the names of the authors whose contributions appear in the *New Orleans Medical and Surgical Journal*, commencing with the May number, are republished in the *Gazette*.

From this and other cotemporary journals, it appears that the medical mind in France is occupied chiefly in discussing uterine displacements, uterine cancer and the uses of the microscope. EDITOR,

ART. VI.—*The principal forms of the Skeleton and of the Teeth*, by Professor R. OWEN, F. R. S., &c., Philadelphia: Blanchard & Lea, 1854, pp. 329.

Seldom has the learned world seen a book so philosophic in its anatomy and its physiology, and withal so gorgeous in its typographical and pictorial illustrations, as that of Professor Owen on Odontography, a treatise on the comparative anatomy of the teeth—their physiological relations, modes of development and microscopic structure, published in London ten years ago, in two royal octavo volumes, at a cost to the American student of fifty dollars.

The cheap, but beautiful little work named at the head of this article, which can scarcely cost more than one or two dollars, far exceeds the learned author's Odontography, in its scope, execution, and useful information.

In 1848, Professor Owen's Monograph upon the Transcendental Anatomy of the Skeleton, entitled, "the Archetype and Homologies of the Vertebrate Skeleton," (pp. 203) appeared. The fundamental idea of this work is easily traced to Oken.

The work on the Skeleton and Teeth forming a part of Orr's popularized circle of the sciences but really adapted to the highest order of the medical mind is based upon, but rivals its predecessors.

The people are great—lords, commons and congressmen are mighty men, but anatomists, physiologists and profound naturalists alone will find in this book easy reading, but all others whether ladies or gentlemen will find this book difficult to understand.

Notwithstanding the innumerable works upon anatomy, it will not be easy for the student to obtain a satisfactory definition of, or a treatise upon the philosophy of that science. Perhaps Mr. Owen has profited more by the German mind which he seems to ignore, than from his own originality or powers of generalization. The *Æsthetics* of anatomy, so to speak, the abstract and the sensuous, the transcendental and empirical, are mingled in the German philosophy like the heterogeneous mixture of the caldron in the witches' dance, in Macbeth. Kant's transcendentalism, Goethe's *Metamorphoses*, Oken's *Ideal Skel-*

eton, Carus' Philosophical Comparative Anatomy, represent the spiritualism, and the Pantheism of Germany, where many great thoughts and some great errors abound.

The osseous skeleton, the central though in some animals the external frame is the most permanent structure of the individual, being the fundamental point of departure in anatomical science, while its primordial diversities serve as the basis of classification for the philosophical Zoologist both as it regards extinct and existing animals, revealing nature's plans of development and organization, the harmonies of which, the poet Goethe, and the naturalists, Saint Hilaire, Cuvier, Owen and others have studied with great success. In a transcendental and teleological point of view as illustrating the adaptations of nature in her purposes or finalities, the skeleton is pre-eminent, as seen in its mechanical support, protection and leverage afforded to other tissues, as also, in its modes or laws of development by the multiplication or repetition of its primary organization conformably to a typical unity.

EDITOR.

ART. VII.—*Viaggi Di Pietro Della Valle, Il Pellegrino.*

Pietro Della Valle, the famous Oriental traveler, writing from Ispahan in 1619, gives the following account of the process of emasculation in that country, showing how summarily surgical interference and aid may be dispensed with, and with what impunity the most horrible mutilation may be perpetrated. This traveler, who from the extent of his journeyings, (which have lately been republished, in the original, in London, in two large volumes,) received the name of "Il Pellegrino," holds the following language, which I have rendered from the original Italian for the New Orleans Medical and Surgical Journal: "I will relate you a great curiosity in point of natural philosophy and medical science. I was thunderstruck to see how light a matter was made of the affair of emasculation, or to speak more precisely, the cutting away of the whole external organs of generation, which is done without producing death. It is inflicted as a punishment in certain crimes and sins of sensuality—as the violation of the persons of women and like transgressions, in the manner of the ancient Egyptian punishment as described by Diodorus Siculus, which consisted in shaving away with a knife the entire genitals of any person found guilty of forcible carnal connection with any respectable woman. So far from dying after the operation, they are very easily cured, by medicating the surface simply with ashes. The Vizier Mazanderan, who entertained me at Ferhabad, had the misfortune to undergo

this punishment, and accordingly he himself related me the facts in the most candid manner. It having been reported to the King that he had robbed a young person in a certain house, in view of an act so unworthy of his station, and suddenly to give an example to the other ministers, he was subjected to this operation, in which the whole of the external genitals were cut away, the King sending away his young wife and providing her another husband. But his other and older wife did not choose to depart, but remained in his house, making him a good and sisterly companion. But the truth came out, and it was discovered immediately that the accusation was false, when the King was much moved and softened, and orders were given that he should be medicated with great attention, and the wound kept for several days covered with ashes, and in a close dark chamber. He was cured, and he found the King rather gave than took away in the event, for the King indemnified him, giving him immediately high rank, and loaded him with favors; and so forgiving a person was the mutilated man, that he spoke of his misfortune only as the work of his enemies, who had misled the King; that the King ever since had shown him so much favor, that every day he prayed God for his sovereign's life. This was certainly marvellous to me, for had it been my case I would have repaid the sovereign in quite a different manner. A Vizier in Eseref, when I was in that place, inflicted with his own hand this punishment on one of his servants, with a single cut, he being accused of forcibly entering the room of a young girl; and I myself saw the poor fellow walking about the next day, to and fro, and he medicated himself with ashes. So little care was taken of him that his recovery, which took place, was a marvellous thing to me." Pietro also says, that adults who undergo this operation do not lose their beards, and the youth who undergo it never acquire one. His friend, the Vizier, was amply endowed in this respect, having a long blond beard.

M. MORTON DOWLER, M. D.

ART. VIII.—*Principles of Comparative Physiology.* *New American, from the Fourth and Revised London edition. In one large and handsome volume, with over three hundred beautiful illustrations.* By WM. B. CARPENTER, M. D., F. R. S., F. G. S., Examiner in Physiology and Comparative Anatomy in the University of London, &c., &c. Philadelphia: Blanchard & Lea. 1854, pp. 752, 8vo.

Principles of Physiology. Designed for the use of Schools, Academies, Colleges, and the general reader; comprehending a familiar explanation of the Structure and Functions of the organs of Man, illustrated by comparative reference to those of inferior animals. Also, an *Essay on the Preservation of Health.* With 14 quarto plates, and over 80 engravings on wood, nearly 200 figures. By J. C. COM-

STOCK and B. N. COMINGS, M. D.. New York: S. S. & Wm. Wood, 1855, pp. 110, 4to.

In his conversations with Eckerman, Goethe said: "That a man should be able to make an epoch in the world's history, two conditions are essential—a good head and a great inheritance. Napoleon inherited the French Revolution; Frederick the Great, the Silesian war; Luther, the errors of the Popes; and I, those of the Newtonian theory."

Dr. Carpenter has these two conditions, the "good head, and the great inheritance," and, accordingly, he has made an epoch (as a compiler) in the history of modern English physiology.

"The learned world," says the author of the Rambler, "has always admitted the usefulness of critical disquisitions; yet he that attempts to show, however modestly, the failures of a celebrated writer, shall surely irritate his admirers, and incur the imputation of envy, captiousness and malignity."

That Dr. Carpenter's ponderous quintuple systems of physiology should be universally esteemed, and truly too, the best which have emanated from the English medical press, is as lamentable as it is true.

Dr. Carpenter's works may not be criticised without incurring some risk of the penalties which the above quoted maxim of Johnson sets forth. Inasmuch as these colossal volumes have passed through numerous editions, and have received superlative commendations from reviewers, and the sanction of the medical profession, the author may smile at the critic's "drawn dagger and defy its point." In physiology, whether original or pirated, success, more than mere originality or merit, satisfies the world, and the author as well. The latter, not to name the publisher, desires no greater ovation, no louder pæan, than the eager clamor for new editions, *ad infinitum*.

Compilers, some of whom appear bent on popularizing physiology, (the most difficult of the medical sciences,) have been commended by critics, as pre-eminently qualified to select from all sources whatever is valuable in science, so that whatsoever they reject is like a squeezed orange, juiceless and useless. It has been supposed that compilers are less swayed by biases than the investigators of specialties. An error this is. It is an error which ought not to rise into credit. It is during original investigation that the strong and the weak, the clear and the cloudy, the certain and the uncertain, the positive and the possible, manifest themselves and afford the cultivator of science just grounds for forming synthetical and analytical judgments, enabling him to compare, to appreciate, and compile understandingly, with enlarged views, and with the greatest success, the experimental researches, deductions and discoveries of others, as well as his own.

Professor Carpenter has "inherited" the great thoughts, the experimental researches, and profound generalizations of monographic physiologists of the first half of the current century; and he is one of

the most respectable and talented of the tribe of compilers who dream that they can make the science of sciences an easy acquisition to sciolists. "We have a Republic, but no republicans," said a French orator of the last generation. Ladies, gentlemen and school children, outside of the profession, are now engaged in reading popular physiologies. Such physiology is likely to diminish the number of physiologists, by levelling downwards, or by taking a step backwards.

Non-professional persons of education and leisure may, and should know much of medical science, much of chemistry, pharmacy, pathology, therapeutics, medical jurisprudence, and public and domestic hygiene; but physiology is based on a knowledge of special, comparative, and histological anatomy, and anatomy must be learned from dissection, not from pictures; hence physiology, which is intrinsically difficult even to the anatomist, must be incomprehensible, for the most part, to the young ladies and gentlemen in attendance at the public schools.

It is to be feared that the second work named at the head of this article, able and beautiful as it is, will not effectuate its mission among "the schools, academies and general readers."

It cannot be denied, however, that Drs. Comstock and Comings have produced an able and splendid work, if not a "royal road" to physiology.

EDITOR.

NECROLOGICAL NOTICES.

COMMUNICATED FOR THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

Died, in Natchez, on the 20th November last, Dr. Francis Chappellier, long a practicing physician and surgeon of that city. He was a native of France, born at Neufchateau, Department of Vosges, on the 1st January, 1805. He graduated in the University of France, in 1828, and immediately commenced the study of medicine and surgery. In 1838, ten years afterwards, he received his diploma as Doctor of Medicine. He emigrated to the United States, and, in 1840, located in Natchez, where he continued until his death, with the exception of a short absence in the winter of 1852 and 1853, for the benefit of his health. He enjoyed in an eminent degree the respect, confidence, esteem and friendship of every member of the medical faculty of Natchez, and died lamented by all those who had the good fortune to be acquainted with him. As a surgeon and physician, thoroughly educated, he had no superior in the State. The community and the profession have sustained a great loss in his death. His relations, consisting of his mother, two brothers and a sister, continue to reside in France. He died in the Christian faith, and the funeral ceremonies were celebrated in St. Mary's Cathedral, from whence his remains were borne to the City Cemetery.

Extract of a letter from Dr. Francis Chappellier, dated October 1st, 1854, to Samuel A. Cartwright, M. D.:

"When I left Natchez for your city, in 1852, I looked like the shadow of a man. There was hardly any flesh on my bones; the skin usually dry, but nearly as white as snow. I was very feeble—had no appetite—my lungs being expanded with extreme difficulty. The least exercise shortened my breath and produced the most intense spell of coughing. I then expectorated, once in a while, some purulent matter, but most generally phlegm. At night my cough was the worst. I do not believe I slept once two hours in succession. I informed you of my arrival in your city. You hurried to see me, and advised me to repair to a sugar-house as soon as possible, as the rolling season would soon be over. I went to Mr. Lapricis' plantation, where I was received as if I had been a son or brother of the family. The breathing of the vapor was rather pleasant to me. I thought that I was breathing more freely. I drank plentifully of the juice of the cane, as hot as possible. The vapor did not oppress; on the contrary, as I said already, I felt more relief after inhaling, from the first day to the last. My appetite increased, and I was even hungry between meals. I also became more fleshy. I was certainly much better when I got back to New Orleans. I felt strength in my limbs that I had not experienced for many months previously. I did not stay long enough on the plantation, but it could not be helped.

"The treatment of bronchitis and consumption by the vapor is bound to succeed if used in time. This is my candid opinion, and I gave it to every one as such, even to the most incredulous. You know yourself that I had a prejudice against it. I followed your advice rather as an experiment, and found it turned out to my advantage, although I remained but a short time in the sugar-house. My situation is now such that I will attempt nothing more, but let nature have her course."

FOR THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

The Medical Profession in this State has sustained a loss which will not be repaired in many long years. The death of Dr. PINCKNEY BELINGER, on the evening of June 19th, last, was a sad and mournful event to many friends, to whom he had become endeared by many acts of personal and professional kindness, and to whom his place will never be fully supplied. The good and the great are always the first to fall. The subject of this notice was a native of Grahamville, South Carolina, and a member of one of the largest and most respectable families of that State. He came to the State of Florida early in her history, and practised his profession through the entire Seminole war (which lasted eight years) with great success, having had many serious wounds to treat, inflicted by the rifle balls, poisoned arrows and tomahawks of a savage and ruthless foe. Although his health was feeble when he immigrated to this State, yet he continued to discharge the arduous duties of his profession for nineteen years, doing a large and laborious country practice. DR. BEL-

LINGER, like a true philanthropist, considered himself as the property of the noble profession which he adorned, and requested that a post mortem examination should be made of his body, and published, for the benefit of the profession.

Accordingly, six hours after death, assisted by Drs. Taylor, Gorman and Gaillard, I proceeded to make the post mortem, with the following results:

Cranium not examined. Lungs healthy. External appearance of the abdomen much larger than natural, and presenting to the touch evidence of effusion and induration. On cutting through the walls of the abdomen, about two quarts of bloody serum escaped, and revealed to the sight a large tumor, filling up the entire cavity, and apparently formed from diseased enlargement of the mesenteric glands. The tumor seemed to be fleshy, and filled with tubercles in all stages of softening, and was firmly attached by muscular adhesions to the spine, from the diaphragm to the promontory of the sacrum, embracing the aorta, the pancreas and kidneys, posteriorly; and anteriorly, the duodenum, which passed through the tumor, the latter causing contraction in its course for about six inches. All the abdominal viscera were entirely displaced; the small intestines were forced into the left hypochondriac region, and the colon was pressed out flat, between the tumor and abdomen. The tumor also had strong attachments to the right pubis and ilium, and to the rectum. The right testicle was enormously enlarged, weighing over one pound. The nature of this enlargement seemed to be simple hypertrophy, having caused no pain or inconvenience, except from its weight. It was connected with the abdominal tumor by muscular attachment. The tumor, when removed from the abdomen, weighed a little over five pounds, and we thought a sufficient quantity of bloody pus escaped from it into the abdomen, to make three pounds more. The liver, natural in size, was studded with tubercles, in all stages of suppuration. The gall-bladder was filled with black bile.

History of the case: DR. BELLINGER was of bilious lymphatic temperament, and suffered much from bilious disorders. In the latter part of his life he was often troubled with vertigo, which was attributed to indigestion, but must have been caused from pressure of the tumor on the aorta. He also suffered a great deal from constipation, and frequently said he had a stricture in his bowels. The duodenum, which passed through the tumor, was thereby obstructed; its calibre was reduced to the size of a goose quill. During last winter, a carbuncle formed on the right hip, which was open for five months, and the post mortem revealed a connection between this carbuncle and the tumor. Dr. B. was not confined to bed entirely until two weeks before his death, which seemed to be from exhaustion of vital power. THOMAS M. PALMER, M. D.

Monticello, Florida.

P. S. We have been greatly annoyed in this part of the world with dysentery, which has been very stubborn, but not very fatal. Our treatment has been nux vomica, acetate of lead, opium, tannin, &c., with small doses of castor oil, spirits turpentine and tinct. opii, mucilaginous drinks, &c., &c.

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FOR MARCH, 1855.

ORIGINAL COMMUNICATIONS.

ART. I.—*A Glance at the "Reply" of SILAS AMES, M. D., to Experiments with Phosphorus, &c., by WM. M. BOLING, M. D., of Montgomery, Ala.*

IN the New Orleans Medical and Surgical Journal, for January, 1854, is a paper by Silas Ames, M. D., on the treatment of Pneumonia, in which phosphorus, in a certain form and mode of administration, is spoken of as a remedy. In the May number following, of the same Journal, are published some experiments by myself, with the article in question; and in the November number, is a reply by Dr. Ames. This reply is now under examination—in the course of which, it may possibly be deemed proper to revert to the paper on Pneumonia.

Dr. Ames tells us that he was influenced, in undertaking his reply, less with the view of engaging in the easy task of pointing out defects, which he thinks exist in the mode in which my experiments were conducted, "than to establish, or confirm in the minds of others, a just confidence in the remedial powers of phosphorus." With the confidence of Dr. Ames in its virtues, his motive was laudable. With the entire distrust entertained by myself as to its value, as administered by him,

the effort on my part would perhaps be equally laudable, to inspire in others a like distrust, that injury, negative it may be, might not result from a misplaced confidence. I am not fully confident, however, that I can on the present occasion lay claim to any such intention, or rather to be influenced entirely by such a desire: for if there are any who, having read my former paper, (and those who have not, will probably not read this,) may have failed to be satisfied of the total inefficiency of the article as used by Dr. Ames, I am sure that nothing further, that I can say, will lead them to such an opinion: neither can I resist the impression, that any who may have failed to be firmly convinced of its value by Dr. Ames' first paper, will be likely to have their faith increased by his second; while it is scarcely probable that those who were already convinced, needed it to strengthen their conviction.

Dr. Ames commences by saying: "The experiments thus prompted, led Dr. Boling to three principal conclusions—

First. That phosphorus is not a sedative.

Second. That it is not a stimulant; and

Third. That it is not poisonous when given in an alcoholic solution.

These conclusions, though it perhaps might not be very unsafe to admit them, at least the first, *are not mine*, though so stated by Dr. Ames; who also says that they are "broadly expressed" in my paper, which, however, I cannot find to be the case. The first object of my experiments, it will be remembered, was to ascertain whether phosphorus, given in the form of alcoholic tincture, in certain doses mentioned, and administered in a certain manner, was, as stated by Dr. Ames, a sedative. In the course of the experiments, it became an object to ascertain whether, 2d, the same article, administered in the manner and in certain doses also mentioned, was stimulant, indirectly, however, and through a local irritant action upon the stomach. If his observations as to such more readily appreciable effects were erroneous; if it should be apparent that he erred in this particular, where, as it seems, there would be less difficulty in avoiding error; what assurance can we have that he may not have fallen into error in regard to the curative operation, where the difficulty of reaching a certain conclusion seems so much greater, the evidence being about the same. It may also be stated that my experiments referred not merely to phosphorus in the form of alcoholic tinctures, but to those tinctures prepared in the manner that the tinctures used by Dr. Ames, prior to the publication of his paper on Pneumonia, were prepared. In regard to phosphorus in no other form than in that of the tinctures

spoken of by Dr. Ames, and administered as in his prescription with water, were my inquiries directed. What might be the properties of the article in other forms or modes of administration, or even in the form of tincture, prepared by any other process than that by which the tinctures used by Dr. Ames were prepared and otherwise administered, I did not hazard a decided opinion. What may be its action, when given in a form to secure the administration of a known definite dose, and one sufficient to produce an appreciable action or effect at all, I am not prepared, from any observation I have made, to say. In view, then, of the aim with which my experiments were instituted, had I deemed it proper to have "broadly expressed" the conclusions that I thought might fairly be deduced from them in the form of propositions, they would have been something like the following:—

First. That the alcoholic tinctures of phosphorus, prepared as those used by Dr. Ames were, and administered in the manner spoken of by him, and in the doses in which they are asserted by him to be so, are not sedative.

Second. That the same articles, so prepared and administered, are not stimulant, indirectly through a local irritant or poisonous action upon the stomach, in the doses in which it is asserted by Dr. Ames that they are so.

Third. That the alcoholic tinctures used by Dr. Ames, are not probably the best preparations of phosphorus, "by which to secure with any certainty its full and peculiar operation, *whatever this may be*, upon the system?" which latter proposition is stated in my former paper. This opinion, not at the time predicated more upon the very minute quantity of phosphorus held in solution by alcohol, (because, if desirable, any objection, founded upon this fact, might, to some extent, at least, be avoided, by increasing the quantity of the tincture given,) than upon the *uncertainty* of the dose,—the possibility even, we may suppose, that at times no phosphorus whatever may be received by the patient, owing to the loss from change of form and evaporation, at the moment of combination between the tincture and the water with which it is mingled, preparatory to administration,—is sustained by other authority. In a note by Dr. Dechambre, of Paris, editor of the "*Gazette Hebdomadaire de Médecine et de Chirurgie*," to a translation of my paper on phosphorus, published in that journal, is the following remark:—"Ce travail est important comme établissant, à l'aide d'expériences, ce que la chirurgie permet d'affirmer *à priori* le teinture alcoolique de phosphore est une très

mauvaise préparation, encore moins chargée en principe actif que ne le suppose l'auteur lui-même." It will appear in the progress of the examination, that the statement made in the last part of the last sentence is, virtually, as regards the use of the remedy in Pneumonia, a fact; and that the quantity of phosphorus held in solution in the alcoholic tincture is really less than I admitted it to be in my former paper.

He who may have, or for the time may assume or be allowed the management and disposition of an adversary's forces, and fails to secure a complete victory should be regarded as but an inexpert tactician, or as being placed himself in a disadvantageous position, be it from the inherent working of his cause, or something else, as a matter of course under such circumstances, the different bodies would be arranged and placed to suit the different forces to be brought to bear against them, and the different modes of attack that would be deemed most available. Dr. Ames it will be seen, whatever may be thought of his subsequent management, and the conclusion of the contest, did not lose sight, at the beginning of the advantages of such a procedure; but most methodically composed and arranged my position to suit the arguments that he intended to bring against them.

In connection with this subject of the deduction and arrangement by Dr. Ames, of conclusions for me to suit himself, and though a small matter as bearing upon it, or as having some analogy with it, attention may be called to the fact, that he also makes my grammar for me, wrong, and then does me the favor to correct it. On the second page of his reply, he quotes from my paper the following sentence with the correction ("it") by himself. "In conclusion, I would here remark, that though prior to these experiments, I had taken and given a good deal of the alcoholic solution of phosphorus, I have never in any instance given them ("it") in a case of acute or dangerous disease."

Now in my paper, strange as it may seem there is at this point no grammatical error; certainly at least, not the one introduced by Dr. Ames. The word "them," relates to "solutions" as it is there written, referring to the two solutions, the saturated and the diluted, and not to "solution," as Dr. Ames quotes, and then corrects it. No one of course, would think of referring back to see whether an error of the kind existed or not. I took it for granted myself, as attention was so pointedly directed to it by Dr. Ames, that it did exist, and it was by accident, and not while examining especially for it, I discovered that it did not. Had there been such an error, it would have been considered by but few,

as of sufficient importance to warrant or require pointed attention ; and had Dr. Ames, been as acute in the detection of trivial errors, as his management in this affair would seem to evince that he was anxious, he might surely have discovered a real error of the kind, perhaps more than one. Such there are I have no doubt, in the paper reviewed by him, as well as in both of our papers. This same thing is repeated by Dr. Ames, in making another quotation from my paper, further on in his reply.

But to return. The conclusions then, not being mine, not made by myself, but by Dr. Ames, for me ; (his *own* work) the examination might very legitimately be closed at this point, and all the logic expended, and the ingenuity displayed in their attempted refutation, be very properly regarded as so much ammunition wasted, or rather consumed in the perpetration of a species of *felo de se*. But it may not be uninteresting in a cursory manner to pursue the subject further.

Dr. Ames objects to my experiments with healthy subjects on several grounds ; among others, for the reason as he says, that sufficient care was not taken to "avoid those normal causes influencing the state of the pulse, which being *common to all persons*, of whatever age, are *almost constantly in operation* during our waking hours." Such being the case, it would be a most difficult matter, even with reasonable care, to avoid such causes entirely, nor does it seem probable that they could have been, in all of my experiments, more actively in operation, than in those of Dr. Ames himself, nor has he explained in what way they were, in his opinion so. Indeed, on the contrary, it would seem probable that the disturbing causes were less numerous in some of my experiments, than in his. In some instances, to be sure the pulse was counted in the erect position of the patient, by design both when the subject was and was not taking the medicine, but the fact was distinctly stated ; and this was the case in only a part of the experiments. We are not informed by Dr. Ames, what were the precautions taken by himself to avoid the causes, normal or abnormal, influencing the state of the pulse, so constantly in operation ; and with the sick especially, so calculated to lead to doubtful, or possibly even erroneous conclusions. Dr. Ames further objects because, as he concludes, there was influencing my subjects, some "other cause," than those just alluded to, "operating more powerfully to disturb the pulse." It would be curious if such were in reality the case ; that both of the subjects selected should have thus been operated upon by some mysterious influence, escaping my observation,

and different from the influences generally in operation upon others. Possibly it was so, and without the evidence of a series of experiments upon other subjects, exhibiting the condition of the pulse at different intervals, I would not be willing to give a positive opinion to the contrary, while in the absence of any such series of experiments, one could scarcely be certain of being right, in giving a positive opinion in the affirmative. This objection it is proper to state, is based upon certain variations in the pulse, in my subjects, at different intervals, from unobserved or apparently inappreciable causes. Dr. Ames does not refer to any positive authority or experiments to show that the variations of the pulse to which he objects, are unusual, but merely remarks that physiologists are "universally silent" as to such changes. Now, even though it might be the case, that physiologists are universally silent, as to such variations, it does not by any means follow that no such variations take place, any more than the failure of a traveler to mention a particular individual, when speaking generally of the people of a place, would be proof conclusive that no such person was in the place. His affirmative conclusion, therefore, is deduced from a negative premise. In referring to an instance in which the pulse was found quicker when counted at one time, than it had been at an earlier hour in the day, Dr. Ames says, "We find the usual order of the diurnal change reversed, the pulse increasing in frequency instead of diminishing as the day advanced." The time of counting the pulse last, was during the digestion, as was evident from the report of a hearty dinner, which may possibly have had something to do with the acceleration of the pulse at the moment; but these professed physiologists especially, if Dr. Ames should be included among the number, are by no means unanimous as to this "usual order of the diurnal change," referred to by him, and in consequence of the reversal of which, he is so much dissatisfied. On the contrary Mr. Rochoux (*Dic. de méd.*) speaking of the pulse, says, "en général, *la fréquence augmente graduellement du motion de soi.*" Lo, too, Dr. Bostock, referring to certain experiments upon the pulse, says, "we are, however, scarcely able to draw any conclusion from them, except that the pulse is *less* frequent in the morning, and that it has a general disposition to become more frequent, as the day advances." In certain experiments again to be referred to, performed by Dr. J. M. B. Harden, of Georgia, the pulse was generally found to be more frequent in the afternoon, than late in the evening, at night, or in the morning. Now the hour at which the pulse of my subject was felt last in the afternoon, and found more fre-

quent than at a previous counting, and from which Dr. Ames says the "usual order of the diurnal change" was reversed, was 4 o'clock, P. M. From late in the evening, during the night, no doubt, the pulse is less frequent than during the day, but instead of diminishing in frequency, as Dr. Ames seems to think it should have done in my subject, as the day advances, until late in the evening, I believe it will be found that its frequency is increased. The average range of the pulse in Dr. Harden's experiments was at 7 A. M., 64; and at 3 P. M., 79. It would seem then that not in my subject, but in the imagination of Dr. Ames, was the "usual order of the diurnal change reversed;" and in view of some of the opinions held by him, in regard to the manner in which the pulse should act, it would not by others probably be regarded as at all strange, if at times he should meet with the record of examples in which it did not act in accordance with them, or to suit his fancy. Dr. Ames says, that the several sources of variation in the frequency of the pulse, are "well known, and consequently appreciable if the cause be enquired into," but it would certainly be a very difficult matter for an observer to appreciate the mental emotions that might lead to variations in the pulse of his subject, while under examination. Even a transient thought may lead to a quickening, perhaps but momentary, of the heart's action.

Though there are but few experiments that I can find on record, calculated to elucidate the question, physiologists, it appears, are *not* universally silent as to very much such changes in the pulse, in persons in health, as were shown in my subjects. In the experiments of Dr. Harden, already alluded to, very considerable variations from what would be regarded as its average or more common standard, are to be found. During his experiments, of which he was himself the subject, he took his usual meals, but at the times more particularly referred to, he seems to have been subject to few, if any, disturbing influences, that were not probably in operation upon my subjects, and that are not generally in operation upon other persons in health. At other times the variations were perhaps more considerable than in the instances below given, but there may have been at such times, additional disturbing influences in operation. On the 8th of November, the subject being recumbent, the pulse is, at 7 A. M., 60; at 1 P. M., 65; at 3 P. M., 72; at 7 P. M., 64; at 11 P. M., 54. On the 9th, at 7 A. M., 58; at 1 P. M., 70; at 3 P. M., 80; at 11 P. M., 60. On the 16th, at 7 A. M., 60; at 1 P. M., 72; at 3 P. M., 70; at 7 P. M., 60; at 11 P. M., 54. Neither, in all cases, are the usual appreciable

disturbing influences found to produce the variations that might be expected. Thus, while at 1 P. M., on the 7th, the pulse is at 64, after a ride of ten miles, at the same hour, on the 9th, it is at 70, the subject having been at rest. While at 3 P. M., of the 4th, after an ordinary dinner, the pulse is 87, on the 2d, at the same hour, under apparently similar circumstances, it is 72, and on the 15th, at the same hour, 68. At 7 P. M., on the 7th, the pulse is 72; at the same hour, on the 8th, it is 64. At 3 P. M., of the 8th, it is 72; at the same hour, on the 9th, it is 80. The want of accordance, too, in the relative frequency of the pulse, as felt at different times in these experiments, in the different positions, is very considerable, and indicative in itself of variations from trivial or inappreciable causes. Thus, while on the 3d, at 7 A. M., the pulse is 60 lying, 60 sitting, and 68 standing; on the following day, at the same hour, it is, in the different positions, 60, 68, and 80. While at 7 A. M., on the 4th, it is 59 lying and 71 sitting; at 3 P. M., of the 10th, it is 74 lying and 72 sitting. While at 11 P. M., on the 15th, it is 54 lying and 68 standing; on the 16th, at the same hour, it is 54 lying and 58 standing. While at 7 P. M., of the 16th, it is 60 lying and 70 standing; at the same hour, on the 15th, it is 60 lying and 80 standing.

As the indications of the presence of this "other cause," whatever Dr. Ames may have supposed it to be, appear to be about the same in the experiments just referred to as in my own, and as there is so little probability that any such mysterious influence should have been present, in the circumstances under which they were made, the inference naturally arises, that the thing is altogether a suggestion of the doctor's own imagination—a creature of his fancy—having otherwise no real existence.

Dr. Ames seems to think it very strange, that on one occasion in my experiments, in which the boy Sam took 500 drops of alcohol, his pulse, counted an hour and a half after, was not found to be increased in frequency. According to the *experience* of Dr. Ames, if *very* large doses of quinine—larger even than may be necessary, when given in disease, to reduce the frequency of the pulse twenty, forty or even fifty beats in the minute—be given in health, "it may be that the pulse will not be affected at all, and is just as likely to be made faster as slower." Now, if the pulse may present such very eccentric variations, as a general thing, under and from the action of a remedy so powerful as quinine, as it would appear from the result of Dr. Ames' experience with the article that it does; *sometimes not varying at all, sometimes rising and sometimes falling* under its influence, without the presence of this "other

cause," may we not suppose it equally possible, that as an occasional thing, or in a solitary instance, a moderate dose of a stimulant remedy might fail to be followed by an accelerated action of the heart. I would here state, though the remark may have no bearing whatever on the question, or perhaps, if at all, may be regarded as favoring the point that Dr. Ames would establish, that, judging from my own observation, there is rather more uniformity, as a general rule, in the effect of quinine upon the pulse of persons in health, than accords with his experience. Almost invariably, I am inclined to think, an effect upon the pulse is produced by what would be regarded here in the South as a medium or full dose. It usually is increased in frequency a few beats for a short time, and then falls a number of beats below the range at which it may have been when the medicine was given. But, according to Dr. Ames, there is nothing like uniformity. It may not be affected at all and is *just as likely to be affected one way as the other.*

Though not a matter of any very great importance, I will here call attention to an erroneous statement repeatedly made by Dr. Ames in regard to the quality of the alcohol taken by Sam. Thus, he says that on one occasion he took half an ounce and on another an ounce of *anhydrous* alcohol. Now, on the occasion referred to, Sam was taking the *diluted* tincture, containing *one* part of anhydrous alcohol and nine parts of the common commercial alcohol, which, so far from being anhydrous, is never so, and is very often very decidedly *hydrated.*

It would have rendered the record of my experiments, more extended than it was my wish at the time to make it, to have noted every trivial circumstance or influence among the "normal causes" alluded to by Dr. Ames, that might possibly have been in operation at the time to occasion variations in the pulse; and even with every desire and the utmost efforts on my part to do so, I fear I should have failed to meet his requirements, unless they had previously been distinctly stated. Instituting the experiments, it was my wish to ascertain whether the action of the medicine, as recommended was sedative or not, and I conducted them, as seemed to me at the time, in such a way as was most likely to bring the matter to a fair test. On one point I was careful—to wit: that the circumstances should be as near as possible the same, when the experiments with and without the phosphorus were made; the administration of the latter excepted; and in some of them, if not all, it will not be doubted, I think, that the causes referred to, were as certainly and effectually avoided, as it is probable they were or could have been,

in the experiments of Dr. Ames. It would be bringing the matter to what Dr. Ames would perhaps regard as a satisfactory test, for he and myself to select several subjects—say each of us two—and to place them under conditions in which as much as possible the sources of fallacy to which he alludes might be avoided; he, himself, indeed, to arrange and regulate all such points; and then for us to test his doses of the tinctures of phosphorous, as prepared up to the time of the publication of his paper on Pneumonia, upon them. In such a trial I would merely ask, that the enumerations of the pulse should be made as often when the subjects were not, as when they were, taking the phosphorus, and that the circumstances should be as near as possible the same, the use of the phosphorus excepted, in both series of experiments. The effects of the article both as a cumulative agent and as a poison, as set forth by Dr. Ames, might also be attended to at the same time. Should it turn out upon such a test that I have been deceived in regard to the action of the article, I shall have at least as a set-off to the mortification that such a demonstration may be supposed possibly to occasion, the gratification that may arise from the supposition subsequently, that possibly the remedy in question as recommended, might prove serviceable.

It is somewhat difficult, it may be admitted, upon the healthy subject to conduct a series of experiments with any particular medicine with the view of testing its action upon the system, in such a way as surely in all cases, to avoid every possible source of fallacy. There are so often, agencies in operation that may not be noted, which may produce possibly, effects that may confuse or modify, or even be taken for, those of the article under examination. But if these sources of fallacy are at times difficult to avoid in the healthy subject, the sources of doubt, must in instances be much more numerous, and much more difficult to avoid in the sick, laboring under violent acute disease, where the real antecedents of any change are often so uncertain. In one point of view then at least, if experiments upon the healthy subject are not invariably reliable, as to the action of medicines upon the system, how much less so, in some respects must the experiments of Dr. Ames be, made upon the sick. Here in addition to most of the disturbing influences in action upon the healthy subject, there are many others also, and very important ones, in operation calculated to lead to doubt; and on this account, among other reasons, experiments upon the healthy subject, where some of these disturbing influences may be avoided, have been regarded as a legitimate means, not always conclusive of course, of confirmation or contradiction in regard to

the supposed operation of articles of the *Materia Medica*—of new ones more especially. Such experiments are, of course, more necessary, and of more value and importance, in regard to articles, whose influence upon the system is not manifested by any very striking or characteristic indications, such as will make the presence of their operation observable above, and as it were, in spite of other disturbing influences; and in some parts at least of Dr. Ames' last paper we are led to think, even as he views them, the evidences of the operation of his remedy, are neither very striking, conspicuous, nor characteristic. Dr. Ames speaks of phosphorus as a sedative, but he gave it in his experiments in conjunction with quinine and aconite, two very powerful sedatives, under the operation of which, the influence of another sedative agent, unless of extraordinary power, would certainly not be very apparent, which a part of the influence of the former might very readily in mistake be ascribed to the latter, especially by an observer prepossessed with the belief that this was curative, and through a sedative action; the latter idea, it may be, having its origin in the fact that the other agents that he had found useful in the disease (Pneumonia) in which he had adopted the belief that it was curative, were of this class; the former, as we may hereafter possibly find cause to believe having something of an *à priori* method of development. He speaks too of its producing considerable disturbance of the stomach, shown by "nausea or vomiting, burning heat and a feeling of oppression at the epigastrium," &c., which symptoms, as a result of his remedy, would be easily appreciable under other circumstances, but less readily in experiments such as he made with it, which were mainly in a disease in which the symptoms enumerated are very frequently present; the article being given too in conjunction with another remedy—aconite—well calculated to develop several of them, even when not already existing. How with certainty then, discriminate between the "nausea or vomiting, &c.," supposed to be produced by the phosphorus, and the same as produced by the other agents given in conjunction with it, or as occurring in connection with or growing out of the disease itself; or be certain that the part attributed to it, might not belong to other causes? To determine a question thus rendered doubtful, it would, of course, greatly aid to administer the remedy under circumstances, in which the causes of doubt should be as much as possible avoided; for instance to the well, or in a series of cases, to the sick by itself, that thus avoiding the operation of other agents, effects produced by them could not be attributed to it. Such a course, as mentioned, would seem to be more essential, in regard

to an agent, the manifestations of whose operation, are not very striking and characteristic, and whose action and influence had not already been to some extent ascertained, say in other and milder affections and by long and repeated use, in various ways. That no such results as those attributed to phosphorus by Dr. Ames, follow its use, even in doses so much larger than those used by him, where the other probable causes of the symptoms mentioned are not in operation, in conjunction with it, show it would seem very clearly, that they had their origin under other influences, and that they were erroneously ascribed to it.

In the paper of Dr. Ames, on Pneumonia, his estimate was, that the quantity of phosphorus held in solution to the ounce of his *mother tincture* was about four grains. In the course of my experiments in the administration of phosphorus, I instituted also some experiments upon it, with the view of testing its solubility in alcohol. From these I arrived at the conclusion, that the quantity held in solution in an ounce of the saturated tincture was about one grain, which would make the dose recommended by Dr. Ames, about the one-sixteenth thousandth part of a grain, supposing the patient to receive at the time of administration the full amount of phosphorus contained in the alcohol, before its combination with the water, which however there is good reason for believing is never the case. In his reply Dr. Ames, gives the particulars of some experiments performed at his suggestion, from which he arrives at the conclusion that his saturated tincture contains about six grains to the ounce; which would make his dose, somewhere between the one two-thousandth and the one three-thousandth part of a grain, but nearer the latter than the former; certainly not itself a very formidable looking quantity, instead of the one sixteenth-thousandth at which I had estimated it.

The plan adopted in these experiments by Dr. Ames, was different from that pursued in the preparation of the tinctures used by him prior to the publication of his paper on Pneumonia, and were it worth the while, might on that account be very fairly objected to. This latter was simply by the process of *maceration*; the phosphorus as found in the shops being used, and no artificial heat employed. In his recent experiments as to the solubility of phosphorus, the plan adopted was this—the phosphorus was first reduced to powder by the process of Le Roy; that is by violently agitating it, while melted in hot water, and then suddenly cooling it. A given known quantity of this powder was then placed in several vials, containing each an ounce of anhydrous alcohol, and after being *digested* for some time, the portion remaining in each was again weighed,

and the quantity held in solution, estimated by the loss. The experiments were performed during the summer season.

Now it is possible, that independently of the mere facilitation of solution by the preparatory disintegration of the phosphorus, its actual solubility may have been modified by the process of pulverization; by the heat for instance, employed in it. Phosphorus it is known, is found in several different States, in which it exhibits different physical, and possibly chemical properties; and some of these different conditions are owing to different degrees of temperature to which the article may have been submitted. The solubility of the different kinds, as chemists tell us, differs. One kind is more poisonous to artizans working with it, than another; and the poisonous action of the article is diminished by carrying it through a heating process. Seeing that its properties are otherwise modified by such means, it is not impossible that its subsequent solubility may also be so affected. It does not follow then that the results either physical or therapeutie, should be precisely the same, in the two different methods of preparation. Still it is not necessary to dwell on this, or to attach any importance to it, as the result will show.

There was an obvious, and indeed, as performed, unavoidable source of fallacy in the experiments of Dr. Ames, in regard to the solubility of phosphorus in alcohol, growing out of the impossibility of estimating precisely the loss in weighing the powder after digestion—the weight being modified or affected by the degree of moisture, as also the quality of the fluid, alcohol or water, with which it might be moistened—the loss by combustion, &c.; for it would be very difficult to collect by filtration, or any other process, owing to the combustibility of the article, especially in the heat of summer, all of the powder, in a dry state. Indeed, the inequality of the results in the different experiments of Dr. Ames, would seem conclusive as to the inaccuracy of the process. In view of these objections, I was induced to make a new series of experiments, in which every possible source of fallacy should be avoided.

The only particular in which my experiments as to the solubility of phosphorus, given in my former paper, differed from the method pursued in the preparation of the stronger tincture used by Dr. Ames, prior to the publication of his paper on Pneumonia, was in the proportionate quantity of phosphorus on which the alcohol was allowed to act. In my experiments, considering the aim in view, of necessity it had to be less. A large quantity of phosphorus, in the process at first pursued in the preparation of Dr. Ames' tincture, was placed in a small quantity of

alcohol. In my experiments, a smaller proportionate quantity of phosphorus was of necessity placed in the alcohol. Provided there was as much in the latter as the alcohol would dissolve, the only difference as to the result, would be a question of time. In the case in which the larger quantity was macerated in the alcohol, the point of saturation, owing to the more extensive surface presented for the solvent to act upon, would be reached at an earlier period than when the smaller quantity was used, and this could be the only difference. The ultimate result would be the same. The larger, no more than the smaller quantity, could affect the inherent solutive capacity of the alcohol. Whenever, then, a point should be reached, at which, when even but a small quantity was used, the solution ceased; and more especially when a portion of that already dissolved might cease to be held in solution, the inference would be a fair one that the point of saturation had been attained, and that the quantity held in solution was the measure of the solvent power over it, of the menstruum.

The following experiments, among others, were performed, with the view of ascertaining with accuracy, if possible, the quantity of phosphorus that one ounce of alcohol would hold in solution.

First. A grain of phosphorus was placed in a vial, (which was afterwards carefully sealed,) containing a fluid ounce of anhydrous alcohol, on the 13th of May, 1854, by Mr. Glockmyer, of the firm of B. R. Jones & Co., of this city.

Second. A grain of phosphorus was placed in a vial, (which was afterwards carefully sealed,) containing a fluid ounce of anhydrous alcohol, on the 13th of June, 1854, by Mr. Garside, a pharmacist, in the house of Coxe & Hutchings, of this city.

Third. A grain of phosphorus, with a fluid ounce of anhydrous alcohol, was placed in a vial, (which was afterwards carefully sealed,) by Mr. Thiep, of the firm of B. S. Thiep & Co., of this city, on the 1st of August, 1854.

I did not test, nor see tested, the alcohol, in either of these experiments, and therefore cannot vouch for its purity; but in relation to the last of the three, I had the assurance of Mr. Thiep, that the alcohol was such as he was in the habit of using in the preparation of the saturated tincture of phosphorus; and it is at his establishment that Dr. Ames' tinctures are prepared.

The result is about the same in the three experiments. In all, the process of solution having advanced to a certain point, appeared to cease,

and a portion—about the same in each vial—remained undissolved; and this condition of things continued till the commencement of cool weather in the fall, when the phosphorus which was dissolved during the hot summer weather, no longer retained in solution on account of the reduction of temperature, commenced settling in the form of a granular powder at the bottom of the vial. At the present time, (January, 1855,) the precipitate, together with the portion that has never been dissolved, in each vial, as near as an estimate may be formed by the eye, is about half a grain—more or less.

Satisfactory and conclusive as to me these experiments seemed, on the appearance of the reply of Dr. Ames, I had some others performed—among them the following: and, as it will be seen, in such a manner as to secure any advantages that the process he adopted could possibly have, with the avoidance of the several sources of fallacy necessarily pertaining to it. They were performed at my request, by Mr. Walker, apothecary, in the house of B. R. Jones & Co., of this city: “a gentleman remarkable for the care and accuracy of his pharmaceutical manipulations.”—The alcohol used, which I had considerable difficulty in procuring of such purity, on being tested, was found of sp. gr. 794, at 60° F. The phosphorus was procured from the establishment of B. S. Thiep & Co.; and care was taken, before weighing it, to remove the external whitish crust.

On the 19th of November, one grain was placed in a vial, which was afterwards carefully sealed, containing a fluid ounce of this alcohol. It was then carefully digested several hours in a water-bath, the temperature of which varied during the time, from 110° to 150° F., till the solution was *complete*, and then set aside to cool. On examination a day or two after, a very appreciable quantity of phosphorus was found precipitated in the form of a granulated powder, as in the other experiments.

This, I think, cannot be regarded otherwise than as conclusive, that the saturated tincture does not contain, or hold in solution, a grain to the ounce, during our usual Fall and Winter weather. A portion of the precipitate in all these vials, it is but reasonable to suppose, will be re-dissolved during the warm weather of the coming Summer, but again precipitated in the Fall; the quantity held in solution depending greatly, of course, upon the temperature. *But viewing the matter as connected with the treatment of Pneumonia, which is a disease here of the Winter rather than of the Summer season*, the strength of Dr. Ames' saturated tincture should be estimated, not by the quantity of phosphorus that may be dissolved in the alcohol by the process of *digestion* or held in solution during

the heat of Summer, when there is but little Pneumonia met with, but by the quantity held in solution during the Winter season, when Pneumonia is more common. The estimate made in my former paper, as to the quantity of phosphorus, as regards the purpose in view, contained in the ounce of the saturated tincture, then, to wit, one grain, from which it follows that Dr. Ames medicinal dose is the one-sixteen-thousandth ($\frac{1}{16,000}$) of a grain, is not too low. Professor Eberle denies that phosphorus is at all soluble in alcohol, and though, as regards this fluid at a high temperature, he was mistaken, the above experiments show that if his opinion was based upon experiments performed in the Winter temperature of his residence, (at one time Philadelphia, and subsequently Cincinnati,) the results must have been such as to justify it, very nearly at least. Merat and Deleus say, "le solution alcoolique, *tonjours peu chargée*, quoique M. Labarraque ait *pretendu* qu'elle pouvait contenir par once jusque à 1 gr. $\frac{1}{2}$ de phosphore," &c. M. Labarraque was possibly correct in supposing that it might contain a grain and a half to the ounce, if he meant at a high temperature, while the belief of the authors mentioned, that he was mistaken, was well founded, considered in reference to the Winter temperature, or that of the season in which Pneumonia prevails.

The above experiments enable us, I am inclined to believe, to estimate with as much accuracy as the subject is susceptible of, the size of Dr. Ames' dose; unless we could also fall upon some method of ascertaining with precision the quantity lost by evaporation, in the combination of the tincture with water preparatory to its administration; when possibly it would be discovered that it was reduced to about—nothing. The loss of the supposed active ingredient, in this way, no one will doubt, who has ever witnessed the process of mixing the saturated tincture with water, must amount to a very considerable portion, at least, of the very little there is of it in the half drop doses of the diluted tincture.

Dr. Ames tells us that prior to the publication of his paper on Pneumonia, he had not made the size of his dose a subject of inquiry. This, even without such an avowal, is the conclusion that would probably strike almost every person at once, on making an estimate of his dose; and with many, if not all, it would be difficult to resist the impression that, had he made it a subject of inquiry, before the publication of his paper, he might have been thereby led to hesitate—to hold back a while—to make further investigations and variations of his experiments; among other things, perhaps, to have made trial of his dose upon the healthy subject, for in-

stance, where the possibility of mistaking phenomena connected with the disease, or resulting from the operation of other remedies, for the effects of the phosphorus, might be avoided; and, in the event of his so doing, also that he might have been led to such a modification of his opinions relative to the effects of his remedy and its dose, as would have prevented the promulgation of the "new medical fact."

It would be difficult, I think, indeed, after a perusal of the details of my experiments, notwithstanding the ingenuity of Dr. Ames' objections to them, to resist the impression that phosphorus, as administered by him, is not a sedative; and in the Reply he tells us that he never said it was much of a sedative. Let us see how this compares with the tenor of what is said on the subject, in his paper on Pneumonia; first, however, presenting what he says in his reply on the subject, in his own language.

"By referring to my paper on Pneumonia," he says, "it will be found that I have never spoken of phosphorus as an active sedative agent over the action of the heart. In all that is there said of the *dose* of phosphorus, no mention is made even of its sedative powers; and this, it may be seen, is in striking contrast with what is said of the saturated tincture of aconite, in speaking of the dose of that article. As regards the latter, a great deal of pains was taken to point out that its remedial and poisonous action was the same, namely *sedative*, and that, on this account, great caution was required in giving it." He further says, that his references to it as a sedative were in attempting to account for its curative action, and that its sedative is spoken of only in connection with its medicinal action, which is contrasted with its *poisonous* action, which is *not* sedative.

Now, in the present discussion, it could scarcely be deemed a material point, whether the sedative action ascribed to phosphorus, was spoken of by Dr. Ames in connection with the *dose* or not; and he certainly placed it under its more appropriate head, in treating of it while engaged in his explanation of the *modus operandi* of the article. Dr. Ames tells us that he took a great deal of pains to point out that the remedial and poisonous actions of aconite were the same, to wit, sedative, "and that on this account great caution was required in giving it." Well, he certainly took as much pains, it must be admitted, to point out that the remedial and poisonous actions of phosphorus were not the same; in fact that they were "antagonistic"—the one being sedative, the other indirectly stimulant, in consequence of a local irritant action upon the stomach; "and, on this account, that a great deal of caution was required in giving it." But

surely there is no proof here presented that he did not attribute to it a decided sedative action. The latter he did not speak of as dangerous, to be sure, because it was limited by the former. Thus—"Its *sedative* or contrastimulant, is its medicinal or therapeutic effect. Its *poisonous* effect is the reverse of this, namely, *highly* stimulant, *by reason* of the local inflammation it excites. In *this* way is brought about the *antagonism* between its effects in large and small doses."

Well, its medicinal or therapeutic effect he seems to think is very considerable—a great remedy, "equalled by but one other" in Pneumonia—and this is its relative effect, which *ergo*, it follows as a matter of course, he should regard as very considerable. Again—"there is a *point* at which it *ceases* to be medicinal or *sedative* and becomes poisonous or *stimulant*. Thus, it is not possible to produce by it, the *extreme* depression which follows large doses of aconite, for when the *dose is enlarged for this purpose* beyond a certain point, a new and opposite action is *immediately* set up, *by* which the power is lost."—"Unlike most other therapeutic agents, its medicinal and its toxicological actions are in a *certain degree* of the development of the latter, *antagonistic*, so that in *proportion* as its toxicological powers are brought into exercise, so are its medicinal virtues diminished, and *thus* it is found that its curative effect is not in the *ratio* of the quantity administered."

That the sedative action, moreover, as he states, was not represented by Dr. Ames, as existing to a *poisonous* extent, is most surely the case, but for the very good reason that, according to his explanation of the various effects of the article, and its *modus operandi*, a stimulant or poisonous action, counteracting the sedative action, would be developed, before the dangerous point of sedation could be reached, owing to the very prompt and powerful local irritant influence upon the stomach, should the dose necessary to produce the proper manifestation of its sedative action be moderately exceeded. It would be impossible therefore to sink or be lost in the yawning *charybdis* of sedation, because the *scylla* of stimulation, upon the stomach and bowels, was always rearing its destructive breakers in the way. But then the influence from the preceding fairly is, that were it possible to so administer it as to avoid this irritant action upon the stomach, in large doses, or if in any case, from any cause whatever, such action might fail to occur, the dose being carried beyond the limit of its medicinal sedative operation, then, under the above explanation, what terrible depression might we not anticipate! Did any such operation pertain to it, it ought, of course, to proceed to a poisonous ex-

tent, whenever it being exhibited in large doses—this counteracting effect failed to be developed; and in degree proportionate with the augmentation of the dose, up to such point. If but moderately manifested under a close dose of half a drop, it ought surely to be very sensibly developed under doses, in this quantity several hundred times multiplied. But not only in my experiments, though the counteracting stimulant influence was not developed, did it fail to manifest a sedative action in any poisonous degree; to discover which was not however the object chiefly in view—but it also failed to manifest any such effect, even to a degree that might be regarded as medicinal, or any effect of any kind at all.

Dr. Ames, however in his reply, admits in one place, that in his paper on Pneumonia, he did speak of “its sedative influence over the *general* circulation,” but, a prior remark would lead to the inference, that he did not mean to say it had any such action, in a degree sufficient to overcome “any of the ordinary physiological influences over the pulse;” which it surely seems, is about equivalent to saying that he said it was a sedative, and was not a sedative.

Can a medicine with propriety be said to have a particular operation, (say as a sedative, or as a cathartic, or as an emetic, for instance,) in any given case or condition, otherwise, or for other reasons, than because it may be capable of overcoming the ordinary physiological influences, resisting such operation, in the case or condition specified? And is it not, mainly at least, in virtue of such special powers, that medicines are classified? Is not the possession of such powers implied in the definitions in any classification founded upon the effects of medicines upon the system? Professor Giacomini,* speaking generally of the class, thus defines sedatives or contrastimulants:—“Nous regardons comme hyposthénisantes toutes les substances qui introduites dans l’assimilation organique, changent tellement l’organisme vivant que la force vitale reste abaissée au dessous du rythme normal, ou du degré où elle était avant leur application.” And of stimulants:—“Nous appellons hypersthénisantes, toutes les substances qui étant introduites dans l’assimilation de nos tissus, changent tellement la manière d’être de l’organisme vivant que la force vitale d’élève au dessus du rythme normal ou du degré où elle était.”

Suppose, for instance, one should at one time say that a specified article

* Quotation made from the French version of his work.

was a cathartic, in doses named, which, on repeated trial, failed, not only in such doses, but in other and much larger ones, to have any such effect; and on being reminded of it, should answer:—" Ah, yes, I did say it was a cathartic, but I never said it was capable of overcoming *any* of the ordinary physiological influences resisting a cathartic action;" and what would be the legitimate inference? That it was a cathartic, or that it was not a cathartic?

But, let Dr. Ames speak for himself, that we may see what he really does say in his paper on Pneumonia, in regard to the sedative operation of phosphorus. Speaking of a class of remedies useful in inflammation, combining the properties of "a *sedative* to the heart's action, and a stimulant to the contractile force of the capillaries," he says:—" *Phosphorus* is put in the class, solely because of my own experience, and that of a few others, of its *immediate sedative or contrastimulant* influence on the *general circulation*, when given in a dose large enough to produce any sensible influence of any kind on the action of the heart, but still not large enough to excite inflammation, or a high state of irritation of the stomach and bowels. Its sedative or contrastimulant, is its medicinal or therapeutic effect."

Although from the wording of the latter part of the first of these two sentences, it might admit of a cavil, the true meaning intended is evident. Indeed, on examining the papers of Dr. Ames on Pneumonia, it will be found, I think, impossible to arrive at any other conclusion, than that his "new medical fact" consists mainly, if not exclusively, in this announcement of the sedative action of phosphorus over the heart. Repeatedly, at least, its operation as a cardiac sedative is spoken of; and the idea is presented and conveyed in different pointed remarks, and strengthened by incidental allusions, that its limitation is found in the *secondary or indirect* operation of the article as a stimulant; and such an influence, so far as I can discover, and I have read and re-read the papers with the view to such a discovery, is not contradicted by a single assertion or intimation to the contrary. The idea is not incidentally, merely, it will be seen, presented in a single instance or in such a way as to admit of explanation on the supposition of an oversight; but the assertion is repeatedly made, in various shapes, distinctly stated, and emphatically dwelt upon.

"My opinion," says Dr. Ames, in his reply "of the extent and *kind of sedative* power it exerts in disease is expressed in the following extract;" made from his paper on Pneumonia: "Its action on the lungs seems, from its effects, to be especially directed to the minute bronchial

tubes, and the air cells; and in inflammation, to the *capillary vessels*, rather than to the heart."

Let us refer then to his views, as given in full in his paper on Pneumonia, as to the nature of this action on the "capillary vessels," that, if possible, we may ascertain precisely the "*kind* of sedative power" to which he refers. In pursuance of this object, it will be necessary to notice certain opinions presented by Dr. Ames, in the paper just mentioned, in regard to the mode of the curative action of remedies in inflammation, while abbreviating, nothing essential to the full development of his views will be omitted. He says, "It is well known that under certain circumstances the remedies for inflammation are required to be of a stimulating nature.

The pathology of inflammation explains this seeming paradox. The phenomena of inflammation, are now known to be derived from engorgement, or "repletion in excess" of the capillary vessels carrying blood, the repletion being itself dependent on a deficiency in the organic contractile force, which in health propels the blood, in part at least, through these vessels."

According to this view of the proximate cause of inflammation, he continues the remedies for it, "ought to be *stimulants*, at least in their *local* action on the part inflamed." This would be true, he goes on, "of all remedies for inflammation, if the organic force of the capillary vessels were the only force concerned in circulating the blood." In view however of the part which the contractile action of the heart performs in it, some active and efficient remedies for inflammation are on the list, which are in no sense stimulant, and which act exclusively by reducing the injecting force of the heart. These are the pure sedatives, and blood-letting is the best representative of the class. Leaving these out of consideration, because their action is *indirect*, he proceeds to divide the other remedies for inflammation into three classes. The first are the diffusible stimulants, and alcohol is the best representative of the class. "They are applicable only in those states of the system, where *local* inflammation co-exists with a depression of vital powers, a deficient action of the heart and of the nervous force of the capillaries."

Those of the second class stimulate the nervous system generally, and through it the hearts' action also, but moderately, but at the same time have an especial action on the organic force of the capillaries—the latter more than compensating for the slight additional injecting force of the heart imparted by them.

“The third and last class consists of such medicines as combine the properties of a *sedative* to the heart's action, and a *stimulant* to the contractile force of the capillaries;” and in this class, Dr. Ames places aconite, antimony, *phosphorus* and quinine.

The action which, according to Dr. Ames, *phosphorus* exerts upon the “capillary vessels,” has here been traced out, and the “*kind of sedative* power it exerts in disease,” as thus ascertained, discovered to be a *stimulant* power.

Now admitting for the moment the possibility of a reconciliation of terms, or of a latitude in their application, by which Dr. Ames might show that, in saying a particular action was one of stimulation, it was very clear that the idea was conveyed that it was one of sedation; and that such was the intention, there is still in the way of such an explanation the fact, that according to his theory of the mode of the curative action of his remedy upon the capillaries in inflammation, the operation must of necessity be, and was a *stimulant* one; moreover, that a *sedative* action upon the capillary vessels, in a state of “repletion to excess,” the antecedent of which is a “deficiency in the organic contractile force,” as stated by Dr. Ames, would of necessity be injurious, and the reverse of that required.—And it should also be borne in mind, that while referring us to his views of the action of his remedy upon the capillaries, which turns out to be *stimulant*, as explanatory of the extent and *kind of sedative* power it exerts in disease,” it is placed in the class of medicines which “combine the properties of a *sedative* to the heart's action, and a *stimulant* to the contractile force of the capillaries.” Truly, the doctor seems here to have very curiously and amusingly involved himself in his own subtleties.

It has been shown that Dr. Ames, though telling us in his paper on Pneumonia that “*phosphorus* is put in this class solely because of my (his) own experience and that of a few others, of its *immediate sedative or contrastimulant influence on the general circulation*,” in his Reply, virtually states, that he did not mean that it would produce the impression upon the system, in virtue of which, only, could it with propriety be so placed, or called a sedative; that, though he called it a sedative, he did not mean that it would act as a sedative; in other words, that it would overcome the ordinary “physiological influences over the pulse,” so as to reduce its action. But here he invests it with still more remarkable attributes, if possible, and tells us that the “*kind of sedative* power it exerts” is, “*mirabile dictu*,” stimulation. He refers us to his views in regard to its action on the capillaries, as explanatory of his opinion of the “*kind of*

sedative power it exerts;" and in tracing this out, we discover it to be a *stimulant* action.

The views of Dr. Ames in regard to the sedative or therapeutic action of phosphorus, as contrasted with its stimulant or toxicological action, will probably be still further developed by quotations to be made from his two papers, in the course of an investigation of his opinions and statements in regard to this latter, in which it next becomes proper to engage.

The third conclusion deduced and presented for me by Dr. Ames is, "that phosphorus is not poisonous when given in an alcoholic solution or tincture."

Now, as I could not be unaware of the fact that phosphorus is a poison—nor yet, that however small the quantity of the active principle contained in the solution, a sufficient quantity of this might possibly be given to reach the poisonous dose—nor yet, that even supposing for the moment that all the solution contained might be driven off, lost, or changed in mixing it with the water, it might possibly be given without previous combination with water, in quantity sufficient to reach a poisonous dose—nor yet, though believing that the alcohol itself should constitute the proper limitation of the dose, rather than the phosphorus, that this necessary or proper limitation might possibly be exceeded to such an extent as to reach a poisonous quantity of the latter—it follows that the inference deduced should not be as to whether a poisonous dose of an alcoholic solution of phosphorus might or might not be given, but have reference merely to Dr. Ames' opinions on the subject, which indeed was the point under consideration. As to whether his statements in regard to its poisonous action, administered as he administered it, and in the doses in which he said it was poisonous, might be correct or not. Were Dr. Ames to assert that certain very marked poisonous effects were very uniformly developed by the use, say of aconite or any other powerful poison, in doses of the decillionth of a grain, and another should argue that the assertion was not probably correct, for the reason that in much larger doses, say even the millionth of a grain, not only did it not produce the effects ascribed by him to it, but no appreciable effect whatever, it would scarcely be a fair representation of the case, or a legitimate inference, to assert, that it was said not to be poisonous in any dose. This third conclusion, then, would be—"Phosphorus is not poisonous, given as Dr. Ames directs, and in the doses in which he says it is so."

Dr. Ames speaks of the "facility of doing mischief" with his preparations, and, alluding to his doses, tells us that the evil of developing the

poisonous properties of the remedy "can hardly be avoided, in giving it in much larger ones" than those which he recommends; of course, meaning that a patient taking such larger doses would not be very likely to escape injury. After also stating that in his doses of half a drop, even of the diluted tincture, it "occasionally produces some very sensible effects upon the head and stomach," (and on another occasion he speaks of "the *peculiar activity* of its physiological manifestations, in a *much more minute* quantity than is contained in the dose recommended," to wit, a half a drop of the diluted tincture,) he further remarks, alluding to doses of from half a drop to two drops of the saturated tincture, that it "cannot be continued, in the smallest quantity just mentioned, for any great length of time, without inducing considerable disturbance of the stomach, shown by nausea or vomiting, burning heat, and a feeling of oppression at the epigastrium, and that in the larger quantity, though a *single* dose, or *perhaps a few* doses may be given with impunity, it cannot be continued for any great length of time with ordinary, or at least with a proper exercise of prudence."

The consequences of the dose of two drops, given oftener than "a single dose, or perhaps a few doses," must have seemed awful indeed to Dr. Ames, and, as if language would fail to convey a sufficiently vivid picture of the terrible mischief, and words were inadequate to its portrayal, he leaves the task to the imagination of his readers, after leading them on to the highest state of excited expectation. He tells us first that the medicine cannot be continued in doses of half a drop for any great length of time without inducing considerable disturbance of the stomach, as shown by certain symptoms enumerated, and then, that in the larger quantity of two drops, though a single dose, or perhaps a few doses, may be given with impunity, it cannot be continued any great length of time with "ordinary or at least a proper exercise of prudence." He had already told us that the doses of half a drop could not be continued any great length of time without inducing considerable disturbance of the stomach, shown by *nausea or vomiting, burning heat, and a feeling of oppression* at the epigastrium; which being the case, *they* even, of course, could not be so continued, "with ordinary, or at least with a proper exercise of prudence." What then must be the consequences, which we are thus left to imagine, of the two drop doses, so continued. This, together with the direct expression of opinion given by Dr. Ames, in the quotation just made, and others presented while on the subject of the sedative action of his remedy, will give us an idea of his views in

regard to its poisonous action, and the awful consequences therefrom to be expected in the doses of which he speaks. It will be seen that such poisonous action is not referred to as merely of occasional occurrence from any "eccentricity" in the operation of the remedy, or of possible development in consequence of idiosyncrasy of the patient, but as a regular and rather uniform effect, generally to be looked for whenever the remedy might be given in the doses mentioned and to the extent stated. This is the conclusion fairly deducible from the general tenor of his remarks, as also from his direct statements. After remarking thus far upon its necessary and uniform poisonous action, thus given, he goes on to explain its *other* qualities as a poison. "So far in regard to *activity merely*; but in estimating the proper dose, several *other* things are required to be taken into consideration, having reference to certain *peculiarities* in its operation. *First*, the *eccentricity* of its action as a poison."—"Secondly, its effects are cumulative;" and in the *third* place, the *antagonism* already referred to between its medicinal and toxicological operation is mentioned.

This "eccentricity" would seem to be ascribed to the action of phosphorus by Dr. Ames, for the reason that it has sometimes been given with perfect impunity, in doses a great deal larger than such as at other times have been known or supposed to produce serious results. He himself offers as explanatory of it, the supposition that in such cases as the former, the phosphorus had undergone some chemical change by which it had been rendered inert; and quotes authority in support of the opinion; or more strictly speaking quotes the authority, and concurs in opinion with him. Dr. Ames, in his paper on Pneumonia, does not speak of ever having observed any such eccentricity of action, in any of his own experiments with it, nor would the admission of its occurrence under his observation, or in his experiments be consistent with the supposition, that he himself, in all cases, had used an article that had not undergone this chemical change, rendering it inert.

To this extent it has been deemed proper, to present the opinions of Dr. Ames, both in regard to the sedative or therapeutic and the stimulant or poisonous action of phosphorus, as preliminary to a notice of his arguments against the inferences that appear to me fairly deducible from my experiment, and which I here present, in shape somewhat different, but in substance, I believe, the same as formerly given.

First. That not only no sedative effect, but *no* appreciable effect of *any* kind being produced upon the healthy subject by the tincture of phos-

phorus given in doses many hundred times as large as those in which Dr. Ames says that in disease it produces a therapeutic or sedative, and other "very sensible effects," it is more probable that he was deceived, (his experiments being made under circumstances rendering misapprehension probable or possible,) than that it does, given as he gave it, have any sedative action.

Second. That not only no such poisonous action as is said, by Dr. Ames, to follow its use in doses mentioned, by him, when given to the sick, nor any appreciable effect of *any* kind resulting from its administration to the healthy subject, in doses many hundred times larger than those in which he states that it produces such an action, it is more probable that he was deceived, (his experiments being made under circumstances rendering mistakes so probable) than that it really does, given as he gave it, produce such poisonous effects.

The *third* it is unnecessary to repeat.

It would also be proper to bear in mind, that Dr. Ames, as has been already mentioned, in another place, speaks of the "peculiar activity of its *physiological* manifestations, in a much more minute quantity than is contained" in his half-drop doses; referring to the diluted tincture.

It is regarded, I think, as a settled point by the professor, that as a general rule, the effects of remedies of an active character, and more especially of poisons, upon the system, are of the same nature in health as in disease; and in experiments in the former state, the actual or positive power of medicinal agents can generally be ascertained correctly. In such experiments too, the confusion and want of certainty, necessarily arising from the mingling of the symptoms of disease, or such as are produced by other remedies with those produced by the medicine under experiment, are avoided; while in experiments upon the sick, in which a number of remedies may be administered at the same time, the actual or positive power of any *one* particular medicine, not already well known, can rarely with certainty and precision be estimated or distinguished; more especially of such as are not very decided, striking and peculiar in their operation. The medicine that will purge or vomit, or that will depress or exalt the vital actions in disease, will be likely to do the same in health; and the article that will poison a sick man, with but a moderate augmentation of the dose at most—at least as compared to the difference between the medicinal dose of Dr. Ames, and the doses given by myself, will surely poison a well one. To these statements generally, there will be found but few exceptions, if any, to some of them none.

Speaking of experiments upon the healthy subject, and of their many advantages, Professor Giacomini remarks: "on observera sur quel tissu, quel organe, l'action du médicament porte de préférence, de quelle manière l'équilibre vital est rompu, par l'action du médicament; quels sont, en un mot, ses effets *purs et simples*, sans l'intervention d'aucune cause extérieure."

In regard to the effects of sedatives in particular, he remarks: "Le véritable et constant effet des remèdes contro-stimulants chez l'homme sain est l'hyposthénie c'est à dire un abaissement gradué des forces." And again: "Les remèdes contro-stimulants produisent chez l'homme *malade les mêmes effets* que chez l'homme en santé." And again: "Les hyposthénisantes vasculaires ont pour effet presque constant chez l'homme sain, de retarder, de ralentir d'affaiblir les contractions du cœur."

There are, however, rare and exceptional instances, met with perhaps but once, in many thousand, in which owing to idiosyncrasy on the part of the individual, some particular remedy will produce effects, anomalous, and altogether different from those that it usually produces upon the great body of mankind. There are also in like manner other instances equally rare, of persons upon whom, certain articles may fail to produce their ordinary effects, even when the usual dose is largely augmented. A very extraordinary case of the kind, is that of the child mentioned by Dr. Ames, to be referred to more particularly hereafter, who ate up at a single meal, a full ounce of calomel. Such, however, are exceptional cases. They are not found to afford any rule, and are as a matter of course, allowed no weight, in estimating the doses necessary to operate upon, or influence the systems, of the great mass of mankind; nor in ordinary medical parlance, in regard to such matters, are they even taken into consideration. When known, of course, such peculiarities are respected in the individual, but this is all. Though the inhalation of a few particles of ipecacuanha may produce asthma in one, or half a grain of calomel, severe ptyalism in another; of each of whom, there might perhaps be found one example, in many thousands, in estimating the effects of these remedies, and regulating the dose accordingly for others, could we expect, say the $\frac{1}{16000}$ of a grain of ipecac, to produce emesis? Neither has it been deemed proper, because an individual, in consequence of some constitutional peculiarity, or "eccentricity" in the action of the article, owing to *inertness from chemical change*, (supposing such a thing possible) or any other cause, might eat up, say half-a-pound or so of calomel, without effect; to dose others, not protected by such idiosyncrasy, with an

article, not inert from chemical change or any other cause, accordingly; or to estimate the general effects of the article when good by such cases, but rather so by its effects upon the generality of mankind.

It is also the case, that the susceptibility to the operation of certain remedies, is in some instances modified by disease. In some cases the susceptibility to the influence of a particular remedy, is exalted by the morbid, state, so that the impression of a given dose will be more marked than it would be in health. This variation is not very considerable; not differing it is probable, more than as one to four, eight, twelve, or sixteen, in different cases. On the other hand, particular morbid states, equally also diminish the susceptibility of the system, to the action of certain remedies, and larger doses will be tolerated than in health, or even in other morbid states. It is true, also, that in some instances, the manifest effects of certain remedies upon the system, in health and disease, are apparently, not precisely the same. But there is no example that I can call to mind, of a remedy, which, as a general rule, will produce *marked and appreciable effects in disease, and a prompt curative influence in acute disease in a particular dose and mode of administration, and yet not only not manifest any of the peculiar effects said to result from its action in disease, when given to the healthy subject, even in its so-called medicinal dose, several thousand times multiplied, by no effect whatever.*

Let us test the reasonableness and probability of such a proposition, in regard to phosphorus, by calling to mind the results that would follow the use upon the healthy subject, of any other active and dangerous article of the *Materia Medica*, in the smallest dose in which it will produce any "very sensible effects" in disease augmented in anything like the proportion in which my doses of Dr. Ames' tinctures of phosphorus, given to the healthy subject, were augmented above his medicinal dose. Setting aside, as a matter of course, the rare and exceptional cases of idiosyncrasy alluded to, and what would be the inevitable consequence? One drop, or perhaps half a drop—say even a quarter of a drop—of the dilute hydrocyanic acid, is about the smallest dose capable of producing an appreciable, though not "very sensible effect," upon an adult subject, laboring under disease. Now what would be the necessary consequence of the administration of a dose of this article, of good quality, to the healthy subject, in the above small dose, even but two or three hundred times, saying nothing of one as many thousand times multiplied? Death, instantaneous and inevitable! About the twentieth, or say the thirtieth of a grain of morphia, might possibly produce an appreciable effect upon a sick

person, of adult age; but what would be the consequence of this small dose, say but four hundred even, to say nothing of one as many thousand times multiplied, given to a man in health? Death, of course. Dr. Ames tells us, that a child six or eight months old, can generally take one-fourth of a drop, say every third or fourth hour, of a well made saturated tincture of aconite, prepared from the root of good quality, without inconvenience. This is perhaps the smallest dose—though let us reduce it to the eighth of a drop—with which any very sensible effects, it is probable, could be produced upon a subject of the age mentioned. But let us multiply this dose, say but four hundred times, which would perhaps be about half a teaspoonful, instead of four thousand times, which would be, say about five teaspoonsful, and what would be the fate of the healthy infant that might take it? Death, instantaneous and inevitable, even were the dose but half my estimate of it.

It will be remembered that in my experiments with the tinctures of phosphorus, published in a former number of the *New Orleans Medical and Surgical Journal*, I administered, on several occasions, to different subjects, doses from one thousand to four thousand times as large (and Dr. Baldwin, it will be remembered, has given still larger ones,) as Dr. Ames' medicinal dose, from which he says he has seen produced, when given to the sick, "very sensible effects upon the head and stomach," and—*my subjects survived*. Not only were they not instantaneously destroyed, but no "sensible effects" whatever were manifested.

Now how are we to reconcile the faith of Dr. Ames in the marvelous efficacy of phosphorus, in doses several thousand times smaller than such as it has been ascertained that it is without effect upon the healthy subject, with this marked and equivocal manifestation of action of the other active and dangerous articles of the *Materia Medica*, upon the healthy subject, in doses even but moderately increased above the medicinal dose? Be it remembered, that Dr. Ames does not inform us that his preparations were carried through any process of "dynamization," with a view to the development of latent powers. Should it be answered, that though, in ninety-nine cases, owing to "eccentricity," such doses might be given to the healthy subject without effect, yet, in the hundredth, the poisonous action would probably be developed, then it would seem but fair to suppose that from the operation of the same influence, the proportion between the cases in which the therapeutic action might result, and those in which it might fail to be produced by the medicinal dose, would be about the same.

Even among remedies not regarded as among the more dangerous, surely from the smallest dose capable of producing an appreciable effect and a prompt curative influence in acute disease, so multiplied and given to the healthy subject, there would result, to say the least, a very decided and manifest effect.

Phosphorus, of course, is a poison given in a sufficient dose, but it is possible to descend to such a minuteness of dose that the most subtle and deadly of poisons might produce no appreciable effect, even in the quantity many thousand times multiplied. The 60th of a grain of aconitine, it would seem, is a dangerous dose, capable of producing very appreciable effects upon the system, yet the $\frac{1}{16,000}$ of a grain, many times multiplied, even of this powerful article, no one will doubt, might be given without appreciable effect.

As to the operations of poisons, and of those more especially having a local irritant or destructive action upon the coats of the stomach, I have not been able to find in the works of writers on Toxicology, any remarks—omitting allusion to the *exceptional* cases already spoken of—that would imply any very material difference in the action of these agents generally, upon the well and upon the sick, and I have consulted a number of authors upon the subject. The definitions of Orfila, Foderè, Mahon, Mead, Anglada, Plenck, Devergie, Frank, Flaudin, and others are before me, and in none of them do I find any intimation of such a difference.

Dr. Ames, speaking in another place of his dose—a half-a-drop of the diluted tincture—says “it is large enough for ordinary purposes in the treatment of Pneumonia, while it *is not too large to be perfectly safe.*” He further, however, tells us that another physician—one of course who had had some *experience* with it—speaking of the remedy, told him he “doubted the propriety of giving it even in this dose in cases where there is a gastric complication.” Par parenthese. It would be curious if, on comparing notes, it should appear that this is the same physician who, though he had used the tincture of phosphorus for some time, told me in the course of a conversation on the subject, that he did not believe that, according to the method of administration advised by Dr. Ames, the patient received any phosphorus, but rather that it was *all* lost by evaporation on combining the tincture with water. But to return: Now, in view of the fact of the frequency of gastric complication in Pneumonia, as met with in the South, taken in connection with the assertion of Dr. Ames that his dose is “*not too large to be perfectly safe*” in this

affection, consistently, of course, he cannot regard the "*experience*" of his friend, (directly opposed to his own,) according to which his "perfectly safe" dose is not perfectly safe, as of any value. Notwithstanding the great importance which he attaches to "*experience*," he thus admits, that the inferences from it are not always reliable; that what is called *experience* cannot always be depended on. How then can he consistently call upon others to receive without question, this thing *experience*, when he himself is unwilling to do so. Giving, as he does, the cold shoulder to the *experience* of his friend, and denying its conclusiveness in the case of another, how can he expect his own *experience* to be received, against probability, as conclusive. It is really difficult, too, to reconcile the confidence of Dr. Ames in the perfect safety of his dose in Pneumonia with other statements made by him in regard to it. Thus, of phosphorus, he speaks of "*the peculiar activity of its physiological manifestations, in a much more minute quantity than is contained in*" the dose that he recommends, to wit, a half a drop of the diluted tincture. It is fairly presumable, of course, that in speaking of its "*physiological manifestations*," he has reference to its action upon the *healthy subject*. Now, in Pneumonia, Dr. Ames tells us, the susceptibility to the poisonous action of his remedy is greatly exalted. How, then, can he feel such confidence in the perfect safety of his dose of the remedy in it, such being the case, consistently with his *experience* of the "*peculiar activity of its physiological manifestations in a much more minute quantity*," under circumstances in which, of course, this exaltation of the susceptibility to its action could not exist? Or even supposing, that in speaking of its *physiological manifestations* he may not have had reference to its action upon persons in health, but that he has reference to effects observed in disease—in Pneumonia itself—still, even under this supposition, the same question will naturally present itself.

Dr. Ames, after remarking upon what he terms fallacies in my *experience*, takes up the first conclusion, as deduced and presented for me, and says: "The inference" (that phosphorus as given by himself does not act as a sedative in disease) "is fallacious, because there is no necessary connection between it and the facts on which it is based. It might be true that phosphorus is *not* a sedative in health, and yet it might be true that it is a sedative in disease. Medical *experience* has furnished innumerable examples of the general truth, that negative facts concerning the physiological effects of a medicinal agent afford no reliable proof as to its therapeutic action.

But does medical experience furnish any example of a medicine that will produce prompt appreciable therapeutic and poisonous effects in disease, in doses many hundred times smaller than those in which it will produce no appreciable effect whatever as a general rule in health? The inference it should be remembered, would not be deduced merely from the fact that in my experiments it failed in the dose of Dr. Ames, to act as a sedative, but from the fact, that in his dose many hundred times multiplied, it failed to act at all. Does medical experience, it may be repeated, furnish any parallel to an example such as this would be, in view of the correctness of the belief of Dr. Ames in regard to therapeutic and poisonous action of phosphorus in disease with the absence of all effect whatever in health, given as explained, under the different circumstances? The rare and solitary instances which are met with, that might seem at a superficial glance to favor such a view, it must be remembered are exceptions, in which owing to idiosyncrasy in the subject or "eccentricity" in the remedy, results different from such as it generally produces are manifested, instead of its usual effects; while the effects to which reference is here made, are those spoken of by Dr. Ames as produced generally by phosphorus, under the circumstances in which he gave it, independently of its "eccentricity," nor yet arising from idiosyncrasy. As the inference, it is not pretended, would be advised from the absence, in my experiments upon the healthy subject, merely of symptoms pertaining to the "eccentricity" spoken of by Dr. Ames, (such being properly omitted as exceptional and extraordinary,) but from the absence of other common and more uniform effects produced by the remedy, as he says, independently of this, the latter, being irrelevant, cannot be legitimately used to account for the non-occurrence of these more common effects, nor to explain it away.

Admitting the fallacy of the conclusion, for the moment referred to by Dr. Ames, but merely as deduced by himself, it by no means follows that the converse of the proportion is thereby established; that because "it might be true, that phosphorus is not a sedative in health, and yet it *might* be true that it is a sedative in disease," therefore it is a sedative in disease.

"The rule"—continues Dr. Ames—"is reversed as regards positive facts observed in the same way. If for instance, phosphorus had proved to be sedative in these experiments, then the conclusion would have been legitimate and proper that it is also sedative in disease, and this without any other proof. And why?"

Now it really does seem difficult to understand how the conclusion could be less legitimate and proper, that because a particular article in a given dose, is without effect in the healthy subject, that, therefore, it is not sedative in disease; and more especially in doses comparatively very minute—than that because it was sedative in health, therefore, it must be sedative in disease. In the one instance not less, or more than in the other, the inference as to the probable effect in disease, would be deduced from the apparent effect, or non-effect, which amounts to the same thing under the circumstances, in health. After the above interrogatory, Dr. Ames proceeds: "Solely in virtue that experience has long ago established the general truth of such inferences from positive fact; the law being subject, however, to many exceptions, that the manifest effects of a medicinal agent are generally shown in disease as in health."

Tell me, would the complete failure of an article to produce any effect, be less a "positive fact," as regards the legitimacy of deduction, than the operation of another in a particular manner? Dr. Ames continues—"But no such law applies to negative facts, and hence it is, that while in the one case the experience of medical men has furnished before hand the necessary experimental evidence to establish the truth in many instances of such an influence from positive facts; such an inference from negative facts, lacking this stored up proof, requires to be supported by an especial and direct experimental proof, in every instance. In the one case, the facts indicate a certain proof; in the other, but a probable one, at best. To establish the latter, it is required that the indications from the facts obtained in experiments on well persons, shall be verified by experiments on sick ones."

Now fancy one attempting to establish the truth in this instance, by verifying the *indication* from my experiments on the healthy subject, which was a nullity, by experiments upon the sick, and that, too, by giving doses several thousand times smaller than the doses with which this "indication" was obtained. While no one, surely, could doubt the result, the certainty of the *verification*; and while, at least as a general rule, it could not, unquestionably, be regarded but as an unnecessary waste of time to assure one's self that a negative result, thus obtained, would be verified by the test proposed; where is he, even though endowed with all the sober gravity and solemn dignity of the Great Mogul himself, who could resist a smile, on witnessing the process? Surely, experience has not more fully established the correctness of the general law, "that the manifest effects of a medicinal agent are equally shown in disease as in

health," than it, with common sense, has established the correctness of the law, that a particular preparation, which, in a given dose, is without action or manifest effect upon the system, in health, will, administered in doses many hundred times smaller, also be without effect or manifest action in disease, nor does there appear to be any stronger "stored up proof" of it.

As in my experiments there were no positive effects developed, the absence or nullity of effect, may, for all practical purposes, be regarded, in this instance, as the "manifest effects;" indeed, we are reduced to the necessity of adopting it as such. This being the case, by the law laid down by Dr. Ames himself, liable, as he states, to exceptions—but, without exception, it is probable in instances precisely parallel to the present—"that the manifest effects of a medicinal agent are equally shown in disease as in health," we are necessarily brought to the conclusion, at which I had already arrived, that his doses of phosphorus were without manifest effect, on the diseased subject. The inference, then, appears quite legitimate, according to Dr. Ames' own logic, from the *want* of effect in health, to the *want* of effect (and *à fortiori*, in comparatively minute doses,) in disease.

Dr. Ames says that instances "in which it is shown that decided and even powerful curative effects are brought about by the aid of medicines, which have either no *poisonous* action at all on well persons, or are given in doses too small to affect them sensibly, or in which the curative action is apparently the very opposite of their sensible physiological action, are equally numerous and familiar."

Now the most of this quotation may be readily admitted, or rather allowed to pass without a strict examination; still it has but little bearing on or relevancy to the question. That it should have, it should be made to appear by it, that powerful curative effects are brought about, not merely by medicines that have no poisonous action on well persons, or in doses too small to affect them sensibly; but that these powerful curative effects should be brought about by doses which do produce, very sensible and even without great caution, perhaps poisonous effects upon the sick, and yet fail in these doses many hundred times multiplied, to produce any sensible effect at all upon the well. The quotation from Dr. Ames to be fairly appropriate and parallel, instead of reading, "no poisonous action, &c., on well persons," should read—"Instances are equally numerous and familiar in which it is shown that decided, prompt and even powerful curative, as also without care, very appreciable poisonous effects are brought about by the aid of medicines upon the sick, in doses many

hundred times smaller than doses in which it has been ascertained that not only no such poisonous action, and no operation similar or analogous to the one from which the curative effects are said to result, but no appreciable operation of any kind whatever are produced by them in health;" and thus fairly presented, it would be perfectly safe to deny its general truth. As presented, by Dr. Ames, the parallelism is deficient in the very points essential to the argument for which he would use it, though owing to the ingenious obliquity with which the fallacy is introduced and managed, it might very readily escape observation. If there are instances in which an active therapeutic and poisonous agent, will produce prompt curative effects in acute diseases, in doses smaller than would be requisite to produce a sensible effect of any kind whatever upon the well, the disparity will surely not be found anything like so great as between Dr. Ames' doses and my own, and the point of their sensible effect upon the well, will be found not greatly above their point of curative or poisonous effect upon the sick. Even the smallest curative dose of Dr. Ames, however produced "some *very sensible effects upon the head and stomach,*" (and it is not more probable that he was mistaken in this, than as to its curative operation,) and yet in my experiments upon the well, no effect of *any* kind, in doses so much larger. Dr. Ames himself, however, it is but fair to call to mind, speaks of "the *peculiar activity*" of the "*physiological manifestations*" of his remedy, in "a *much more minute* quantity than is contained in the dose" of half a drop of the diluted tincture.

After the remark, as quoted above, as to the manifest effects of medicinal agents in health and disease, Dr. Ames selects two examples for illustration; and says,—"*Lemou-juice*, it has recently been discovered, exercises a speedy and efficient curative agency in acute rheumatism, an inflammatory affection in which contra-stimulants or sedatives are necessarily the only effectual remedies. Now is there anything in the physiological effects of lemon-juice, which would indicate this therapeutic effect? Is it a cordial sedative, administered to persons in health? I believe not. No mention is made of any sensible physiological effects from it, by either of the few authorities I have consulted, though among them is the elaborate work of Dr. Pereira, the last edition, where one might expect to find them, *if any where*. Certainly, it has no considerable power in this respect, if any at all; and yet it is said to reduce the force and frequency of the pulse in this disease, with a degree of power not equalled by the most active sedatives known to the *Materia Medica*. Let us suppose,

then, that when *this* discovery was first announced—a discovery of such value, that it has been said, on high authority, in England, to mark an era in the history of practical medicine,” (in that case, of almost equal value to Dr. Ames’ discovery, the “new medical fact,”) “physicians, instead of testing its value in the circumstances of disease, in which its sedative power was affirmed, had set about to determine whether this was true or not, by giving it to well persons, to try, in fact, whether it would cure rheumatism, by giving it to persons who had no rheumatism to cure, what the *à priori* inference from such experiments would be, as well as its value when made, is obvious enough.”

This seems right strong. But, let us state the case so that it shall bear fairly upon the point at issue. The dose of lemon-juice, recommended thus for the cure of rheumatism, is about two ounces, say three times a day, or six ounces in the 24 hours, if I remember aright; and this is under, I think, rather than over the average quantity. Now, suppose that Dr. Owen Reese, in speaking of it in this particular dose, had said that it was sedative; that this sedative was its medicinal effect; that this medicinal action was not in the ratio of the quantity administered, except within narrow limits, because of a counteracting poisonous operation, should the quantity necessary for its medicinal action be slightly exceeded; that it was difficult to avoid this poisonous action; that even his smallest dose produced “very sensible effects upon the head and stomach,” &c.; and all in virtue of “*its activity merely,*” *independently* of certain eccentricities of action; and suppose, further, that in his experiments for the cure of rheumatism with the lemon-juice, he had prescribed it in conjunction with other remedies that were known to be useful in the cure of rheumatism; were powerful sedatives, produced very sensible effects upon the head and stomach, *all* such as he had ascribed to the lemon-juice; and still further, that not only were the doses of lemon-juice recommended extraordinarily minute, but that the method of administration was such that of necessity a considerable portion—and *how much unknown*—of the active principle, the citric acid for example, must be lost; and then suppose that another physician should test the action of lemon-juice, in such a way as to avoid the possibility of mistaking the effects of other agents for effects produced by it—upon the well, for instance—not only in the dose recommended, but in this dose many hundred times multiplied, and should find that not only did it not produce any *one* of the *several* effects attributed to it by Dr. Reese, but had no appreciable action of *any* kind upon the system, even in this augmented

dose; what the inference—*à priori* or *à posteriori*—“would be, as well as its value when made, is obvious enough,” truly, as Dr. Ames remarks. What, under the circumstances, would be the most reasonable inference? That Dr. Reese had attributed effects to the lemon-juice that were fairly attributable to the other agents used in conjunction with it, or that his lemon-juice—setting at defiance all the experience of the past in regard to all other powerful remedies and poisons, and all natural agencies and influences, all analogy—really was capable of producing all these various effects ascribed to it, when given to a person who had rheumatism, and yet produce no manifest effect whatever upon the healthy subject, in such comparatively immense doses—say from two or three hundred ounces in the day, to sixty and some odd gallons at a *single* dose; which would bear the same proportion to the dose of Dr. Reese that my doses of phosphorus bear to that of Dr. Ames? Think, as to the possibility of giving *even lemon-juice* to the well, without effect, in doses so vastly augmented above what, on the sick, it would produce such various and remarkable results, even were it possible to find a stomach of sufficient capacity to contain them? It will be observed also, however, as we proceed, if I mistake not, that Dr. Ames, through an oversight of course, *strengthens* his argument by *weakening* the lemon-juice. In the quotation just given, it will be remembered that, speaking of this article, he asks the question, “is it a cordial sedative administered to persons in health?” and states that *no* sensible physiological effects are said to arise from it by authors that he has consulted; among others, Pereira. Now, for all practical purposes, lemon-juice may be looked upon merely as a form of administration of citric acid. This is its chief constituent. Merat and Deleus say of the latter: “Il est employé en médecine aux mêmes usages que le suc de citron.” Speaking of lemon-juice, Pereira remarks, that on account of the difficulty of preserving it, citric acid may be substituted for it. Of the latter, he tells us that Orfila places it among the irritant poisons, but that he, himself, is not acquainted with the effects of *large* doses of it on man. “*Small* quantities of it, dissolved in water, form an agreeable beverage, which allays thirst, diminishes preternatural heat, checks profuse sweating and promotes the secretion of urine.” Speaking of the *physiological* effects of the acids as a class, including of course the citric acid or lemon-juice, he says that they, “in *moderate* doses, at first allay thirst, sharpen the appetite and promote digestion, check preternatural heat, *reduce the frequency of the pulse.*” Professor Giacomini, of Padua, speaking of citric acid in a separate form, and as a lemon-juice, classes it

among his *hypsthénisants* or conrostimulants, and covers nearly half a page in describing its effects upon the healthy subject. Galtier, speaking of his "médication tempérante ou rafraichissante," says: "Les changements *physiologiques* que forment le principal caractère de cette médication, consiste dans la diminution de la chaleur, de la soif, dans le *ralentissement* de la circulation," &c. "Les médicaments tempérantes ou rafraichissantes appartiennent presque exclusivement au règne végétal; ce sont ou des acides, tels que les acides tartarique, citrique," &c.

Foy, referring to the second genus of the second order of his second class, "tempérants," defines them, "médicaments propres à modifier la trop grand activité des organes en ralentissant la circulation et en diminuant la production de la chaleur animal," &c.: "Les tempérants les plus usités sont, parmi les végétaux l'orange, le citron," &c. It would be unnecessary to consult authorities further on this point.

This lemon-juice would seem to be rather an unfortunate selection for Dr. Ames to have made, for illustration, in several respects. "Medical experience furnishes innnumerable examples," of medicines newly introduced and of old ones revived, and presented before the profession with high pretensions, as possessed of some particular quality or power, and their claims urged with the utmost zeal and confidence, not merely by the authors themselves of the supposed discovery, or from the results of experiments "by a single individual on a few subjects, or in the same place, but by a number of physicians in a considerable number of subjects, some of them living in places widely separated from the others," and which yet under the proper ordeal—weighed in the balance—have been found wanting, condemned by the general verdict of the profession, and assigned to merited neglect and oblivion. While such instances are of almost every day occurrence, the really valuable and important discoveries, verified by proper trial, in therapeutics, as in other things, are rare and at long intervals. The present indications are, that such will be the fate of the lemon-juice-cure for rheumatism; the discovery of which it seems there has been some one silly enough to say, is of such value as to "mark an era in practical medicine." Though presented to the profession with very considerable confidence, and recommended by its simplicity and the facility of its administration, even now is its reputation rapidly on the wane, if not already set. In the very valuable and elaborate work recently published by Dr. Fuller, in London, it is stated that though for some time it was generally adopted, "after an ample trial it has been discarded as uncertain." A professional friend, recently resident physi-

cian in St. Joseph's Hospital, Philadelphia, tells me that after a fair trial in that institution, its use as a remedy, had been abandoned. And another friend tells me that after a similar test it had been discarded in the Pennsylvania Hospital. Dr. Reese's patients it appears, were ill, for forty days, and the average duration of treatment was 35 days.

In further illustration of his argument, Dr. Ames speaks of a case in which a patient, laboring under chronic hypertrophy of the spleen, took "the enormous quantity of an ounce of quinine in the course of four days, in doses of 40 grains, given three times daily," without any material change in the pulse, though "what change there *was*, indicated an increase, rather than a diminution of its frequency;" and he calls to mind the fact, that an effect of this remedy, in certain febrile affections, is to reduce the frequency of the pulse. It seems, however, that, in the case in question, *some effect* was produced by the doses used; and had none been produced, the case would still have to be regarded as an exceptional one, for it is well known that quinine, even in very much smaller doses than those mentioned, does, as a general rule, produce very appreciable effects upon the healthy subject. An average dose of quinine, among physicians of the South, with a view to its medicinal sedative action in febrile and inflammatory diseases, we may say is about ten grains, given as often as every eight hours. The quantity taken by the patient spoken of by Dr. Ames, was four times as large, and he calls it "*enormous*." To deserve the appellation, however, as applied to the doses of the tinctures of phosphorus used by myself—even to the comparatively small doses in my own person—as compared to the preferred medicinal dose of Dr. Ames, the quantity should have been something larger. Let us see. The quantity given by Dr. Ames is three drops, about, of the diluted tincture, in the 24 hours. I took, for eight days, fifteen drops a day of his saturated tincture, each drop being equal to ten of the other, amounting, in the aggregate, to fifty times as much per day. His patient, then, instead of taking 120 grains of quinine per day, should have taken 1,500 grains, or over three ounces; and instead of 480 grains, or one ounce, in four days, 6,000 grains, or a little over twelve ounces, in the same length of time. And in regard to the single dose: to deserve the term "*enormous*," as compared to an average medicinal sedative dose of quinine, in the same degree that a dose given by myself, in one instance, to-wit—200 drops* of the saturated tincture, equal to 4,000 doses—would deserve to be so

* Dr. Baldwin, it will be remembered, as mentioned in my former paper, has given still larger doses than this.

called, as compared to the medicinal dose of Dr. Ames, the patient should have taken, instead of forty grains at once, the nice little dose of *forty thousand grains, or eighty-three ounces*, and the small fraction of 160 grains over; or something *more than five pounds*; thus, $1 : 4000 :: 10 : 40,000$.

(To be continued)

[CORRECTION.—For the phrase—*du motion de soi*, page 582, read—*matin au soir*.]

ART. II.—*On the Nature of Malaria, and Prevention of its Morbid Agency.* By JOHN GORRIE, M. D., of Apalachicola, Florida.

THE atmosphere is observed, from its sensible effects, to have a powerful influence over the terrestrial creation. Beneath its widely extended dome all animated nature takes its origin, and derives its materials of growth, health and enjoyment. Floating in incessant motion around the earth, it sweeps up, and is the grand recipient of the products of Nature's operations. Viewed chemically, it must be regarded as a perpetually working laboratory, in which spontaneous distillation, sublimation, condensation, composition and decomposition of all aërial fluids, pursue their eternally recurring revolutions. In its physical composition, it is the most complex of all natural bodies. To its well known aërial elements, it unites mineral vapors from the earth, the products of every kind of combustion, and all the volatile exuviaë resulting from the functions and decompositions of animal and vegetable structure.

In the various constituents and properties of this wonderful fluid, we recognize the agents and forces which produce all change on the earth's surface. Though these act with great energy upon the inorganic materials of our planet, their influence is more conspicuous over the two great kingdoms of animal and vegetable life. The air is not only the medium in which the offspring of nature live and move, but it is the rich storehouse from which vegetable organization directly, and animal indirectly, derive their being and nourishment. Alike the agent and object in the great system of vital chemistry, it enters directly, in the condensed form of solids or liquids, into the structure of vegetation. And if in the composition of animals it does not work so direct a part, it is equally indispensable to them; without it, respiration is impossible; all the other functions must cease, and death be speedily inevitable. To its connection with, and influence over organic structure, may be traced the various stages of physiological action, in the germination, flowering and fruiting

of vegetation—the generation, nutrition and maturity of animal life, and in both their elementary composition—their carbon, oxygen, hydrogen and nitrogen.

But this fluid, so essential to the numerous and varied operations of the world, in all its phenomena of change, life and growth—“this most excellent canopy, this brave overhanging firmament, this majestic roof, fretted with golden fire,” and fraught with every good, is the direct source of the greater portion of “the ills that flesh is heir to.” Careful and enlightened observation justifies the almost universal conviction, that the atmosphere often contains a poisonous impurity, more subversive of human health, and more fruitful of human suffering, than the union of all other physical evils.

Providence seems to have opposed, in the small quantity or peculiar properties of this impurity, an insuperable obstacle to a knowledge of its precise nature. Chemists and naturalists have pursued the deeply interesting and important subject, through all the gaseous and volatile products of nature, that are considered detrimental to the animal economy—such as the combinations of carbon, sulphur, the metals, selenium, phosphorus and nitrogen, with hydrogen, cyanogen, with other compounds of carbon and nitrogen, fungous and animalcular life, and the suspected agencies arising from the emanations of assemblages of human beings—without approaching its discovery. In such experiments and inquiries, no evidence has been furnished that either the chemical or physical properties of the air are altered during the prevalence of the most malignant epidemics. Nor, when we consider the limited quantity of air the chemist is able to operate on, the manifestly great subtlety of the impurity, and the apparent impossibility of obtaining a tangible quantity for examination, is it probable that a philosophical knowledge of it will ever be attained through these means.

Where philosophers have failed, physicians have scarcely deemed it necessary to try to succeed. More interested in the cure, than the cause of disease, they have inferred and acted upon the existence of the effluvia constituting *Malaria*, without waiting for its positive demonstration.—Content to apply that converse process of reasoning, which is so generally all that can be used in the elucidation of natural phenomena, they have been satisfied with the evidences afforded by the sallow visages, weakly constitutions, and destructive diseases that prevail among the inhabitants of certain regions, that a deadly poison may be infused in the atmosphere, without caring about the minute tests that the analytic chemist requires.

Considered as the cause of idiopathic fevers it is important, in view alike of their prevention and treatment, that we should understand all that can be known of the nature of Malaria. Though analyses in the chemical laboratory are alone inadequate to detect the character of the attenuated exhalations of which it is composed, it is not literally true that the atmosphere affords no evidence of an extraneous material to which periodical fevers may be ascribed. The testimony tending to such a proof may be regarded as indirect and direct; and, though in neither conclusive, both point to the commonly understood nature, and the species of substances to which Malaria belongs. In the first class we may place the appearances of organic matter always to be found in the waters of marshy and swampy places, as well as those of rivers and wells of sickly localities. The substance when boiled, or even suffered to stand at rest in a wooden pail, after giving off a volatile matter, coagulates and separates in flocks—thus indicating that it is albuminous.* Independent of any sensible emanation, the presence of this matter would of itself indicate that, under the process of evaporation or sublimation, portions of it must be carried chemically or mechanically into the atmosphere; and the offensive odor it often emits demonstrates it to the sense of smell.

The direct evidence of organic matter in the atmosphere, though showing its presence in very minute quantities, is more satisfactory. Numerous experiments upon moisture condensed from the air of insalubrious situations prove that it often contains organic matter in a putrid condition. By filling a metallic or glass vessel with ice, and placing it in the open atmosphere of a higher temperature, chemists have been enabled to separate the aqueous vapor, and the condensable matters it may hold in admixture or solution in larger quantities than by any other known process. Managed in this way, each portion of the surrounding air as it comes in contact with the vessel, deposits the whole or a large part of its moisture, and by the change produced in its own density, independent of the influence of any more general current; descends and gives place to a new portion which affords a similar deposition; and thus is furnished an unlimited quantity of aerial vapor for observation or analysis. When conducted in suitable places, these experiments seldom fail to furnish

* The water of the wells of this town, situated in a sandy alluvial but highly malarious region, becomes, especially in summer, after standing a short time in a wooden vessel, thick and ropy, as if full of the spawn of frogs; and, at the same time, is charged with animalculæ. The process of deposition which speedily follows the separation of this ropy or flocculent matter, acting like the white of egg in clarifying wine, carries down with it much of the insect and other impurities, and renders the water, previously offensive to sight, taste and smell, clear and wholesome. This matter is manifestly albumen.

evidences of decomposing organic matter in the air; and though they may not impart to us absolute ideas respecting the nature of the cause of Malarial diseases they certainly afford indications which may lead to useful conclusions on the subject.

But whether perceptible or not by our senses, *Malaria* manifests properties which are considered sufficiently cognizable by the understanding to indicate that it is material. Every view of its properties shows that it is volatile and an emanation from the surface of the earth. We infer the former alike from several of its important characteristics, and the known constitution, affinities and chemical properties of the atmosphere. Its obedience to the motion of the winds; its diffusibility under the heating influence of the sun; its reputed interceptibility by groves and gauze screens; its admitted poisonous reception through the respiratory organs; and the certainty that infectious diseases are neither engendered nor propagated when atmospheric impurities may not rationally be considered to abound mark the properties of a volatile fluid. That it is of earthy origin we deem within the bounds of probability, from the known action of the atmosphere upon terrestrial matter, and the detection in it of numerous substances heterogeneous to those elements so viewed as essential to its composition. These substances are found, or may be supposed to include every form of gaseous and volatile combination known to us, and others which have as yet eluded the researches of the most patient investigators; among which may no doubt be placed that product or specific combination, whose peculiar and energetic qualities are the cause of endemial and epidemical diseases. To this conclusion we are further justified in arriving from the similarity of soil, the elements of climate, and consequently the character of the emanations which attend and seem necessary to the production of Malarial maladies; and from the analogy or identity all over the earth of the diseases referred to *Malaria* for their origin.

Though undoubtedly volatile some of the properties of *Malaria* indicate that it differs from ordinary gases and vapors. Obeying the motion of the atmosphere and evidently diffusible and expansible by the sun and other sources of heat, it yet has a tendency to limit its range and sphere of activity to certain strata of the atmosphere. While indicating the properties of a very attenuated fluid it is incapable, like hydrogen, or other gases, of ascending to the higher regions of the air. Surrounded by hills or enveloped by vegetation, it possesses the singular property for a volatile fluid, of clinging with great tenacity to low marshes, depressed

plains, and other places in which it is generated. Independent of such circumstances, its sphere of horizontal, like that of its perpendicular prevalence, is often circumscribed within very narrow and well defined boundaries, as to the neighborhood of a pond, a mill-dam, a sluggish stream, one side of a street, a single ship of a fleet, and even to a part of a hospital or house. It is thus incapable of that uniform and speedy diffusion in the air, which Dalton has shown is an invariable law of gases and vapors; for while these fluids in disregard of chemical affinity and in opposition to the law of gravitation combine intimately and mechanically with the whole accessible atmosphere, Malaria, instead of manifesting the same properties, is always most concentrated as well as most potent, the nearer it is to the surface of the earth.

It is not absolutely necessary to the objects of this essay that it should offer anything new, either in subject or argument, on the material nature of Malaria. To the main subject under consideration—the prevention of the morbid effects of Malaria—it matters very little whether it consists of a gaseous or vaporous poison, a vegetable, animal, or product conjoined of both species of organic matter, fungi, animalculæ, or any other conceivable substance. But, though not indispensable in a practical it is desirable in a scientific point of view, that we should know all that can be learned about it, and, therefore, I may be excused for adding a conjecture on the subject to the multitude already before the world.

The properties, whether manifested to the senses or ascertained by reflection, that we are accustomed to assign to Malaria, present so many analogies to those of volatile oils that I deem it probable it is one of that class of bodies.

These substances form an interesting branch of organic chemistry, which has not yet been sufficiently studied to enable us to determine their number, to elucidate their connection with other organic substances, or to trace their affinities to air and other mineral bodies. Though they are found in all the natural kingdoms, they may be considered, like Malaria, as primarily the exclusive product of organization.* They are chiefly obtained from the vegetable kingdom, but are also found in considerable numbers, either as natural products or compound chemical radicles of animal matter. Albumen, from both kingdoms, and other organic products, as gelatine, in the process of fermentation, putrefaction and decomposition, always furnish them.† Though differing from each other

* Turner's Chemistry, page 644. 6th American edition.

† Turner's Chemistry, pp. 475, 578.

their properties are distinctly characteristic. Many of them are highly poisonous, as Dippel's, or the empyreumatic oil of the older pharmacologists, the fusel oil, the oils of cantharides, tobacco, bitter-almonds, tansy, cherry-laurel, &c. Exposed to the atmosphere at natural temperatures, most of them emit a pungent and fragrant odor; but in some this property is very offensive; in others it is slight; and it is probable there are a few in which it is not perceptible. They are slightly soluble in water, and in contact with its vapor, and in proportion to its tension, are evaporable and condensable without alteration. To some of them aqueous vapor appears indispensable to their preservation in the air, because their components are held together by so weak an affinity that, like the fixed oils, when boiled, they are decomposed by the action of the dry constituents of the atmosphere.* Set free, or formed in water, they are often united with other evaporable substances contained in organic matter, which cause them to putrify and emit a nauseous smell, till freed from it by the action of the oxygen in the atmosphere upon the organic matter.† Mixed with dry clay or sand, and exposed to high natural temperatures, they are partially or wholly decomposed.‡ They adhere to organic, fibrous and fleecy substances, as wool, cotton, or leaves of vegetation; and this property is taken advantage of in the arts so as to collect the more scarce and delicate kinds. They are for the most part obtained in minute quantities, and their emanations are generally so attenuated, that though distinctly perceptible by the senses, it would be impossible to identify them by any chemical analysis of the air in which they are contained.

In these physical properties of volatile oils we see a striking resemblance to those recognized as belonging to *Malaria*, and pathology lends some further support to the view that they may be identical. The entire novelty of the subject precludes many data; but that diseases are produced through the agency of volatile oils, existing in the air in quantities too minute for detection by chemical analysis, is seen in the effects of the emanations from the *Rhus Vernix*, the *Manchineel*, &c. It is a singular coincidence between the mode of action of the former and that of *Malaria*, that it is more marked when the poison is effused in connection with vapor.||

* *Ure's Dictionary of Arts, &c.*, p. 910.

† Odors have frequently been perceived to be attendants upon Malarial epidemics and endemic and during the prevalence of an epidemic yellow fever, in this place, a highly offensive odor, very much like that of volatile oils from several species of *Crassica*, accompanied with the well known one of putrid vegetation, was daily observed on the springing up of the sea breeze.

‡ *Ure's Dictionary of Arts, &c.*, p. 911.

|| *Thatcher's Modern Practice*, p. 486.

All the phenomena attending Malarial diseases indicate that some specific toxicological agent is absorbed by the blood from the air we breathe, and produces its general morbid influence through its action on that liquid. Demonstrable evidence of this may be considered as found in the peculiar and well marked odor which the blood acquires in typhus, plague and yellow fever, and which, to an acute observer, is sufficient to distinguish them from all other diseases; the same or a similar odor is exhaled with the sweat in these diseases. They are both very offensive, of a decidedly organic character, commonly referred to a volatile animal effluvia, and certainly possess properties which we might consider as those of a volatile oil. Viewed in this respect the action of Malaria is like that of camphor or hydrocyanic acid—the former of which is a volatile oil, solidified instead of liquified, and the latter has as many of the properties of an organic volatile oil as of an acid—which we know are absorbable by the circulation, and when absorbed impart their peculiar odors to the blood. The symptoms of many diseases arising from eating or receiving into the circulation putrid or poisonous organic substances are very like those imputed to Malaria; thus we find rigors and fever from dissection wounds, bilious vomitings from the bites of serpents, a deep yellow hue of the skin and urine, gangrene and effusion of black matter in the stomach and bowels, from eating poisonous fungi, fish, decaying cheese, &c., all of which possibly, and even probably, depend upon the presence in the offending matter of a volatile oil.* Finally, according to Magendie and other experimental pathologists, the injection into a vein of putrid organic substances produces symptoms and lesions very much like those arising from the action of Malaria. We have seen that volatile oils are always accompaniments of decomposing organic bodies, and we may therefore attribute the effects produced by its injection into a vein with as much propriety to the oil as to any other substance.

This cursory view of the nature of Malaria may be a subject worthy of future and close examination, but in regard to the objects of this essay, its introduction must be looked upon as a mere incidental occurrence. Whether we look upon Malaria as cognizable by or imperceptible to the senses, as a tangible liquid, gas, vapor, or emanation from volatile oil, or, divesting it of all materiality, consider it as a quality of the atmosphere, may be of no practical consequence, from our total inability to control it. But there are certain circumstances, indispensable to the generation and existence of Malaria, which are neither beyond the reach of positive

* See Cyclopædia of Practical Medicine. Article Toxicology.

examination, nor, as will be presently shown, human control. All the observation and reflection which physicians have given to the subject produce and justify the conclusion that *Malaria* is indissolubly connected with three concurrent appendants—decaying organic matter, moisture and heat. It would seem as if no one, nor indeed no two of these were capable of producing the morbid agent, any more than one or two of the constituents of gunpowder of producing explosion; the concurrence of the three circumstances in the one being as necessary as the three components in the other to develop its appropriate effects. It follows as an inevitable consequence, that the subversion of any one of these circumstances must be the subversion of *Malaria*.

The evidence of organic decomposition is not at all times co-existent with the effects we attribute to *Malaria*; still, physicians trace their connection from having observed, throughout a series of ages that certain forms of disease prevail with a frequency and virulence in some proportion to the quantity of the former. Indeed, so well convinced are they of this connection, that many do not hesitate to affirm that autumnal and periodie fevers bear a precise relation in number and severity, all other circumstances being equal, to the quantity of this matter co-existing. How far this is strictly true facts are not confirmatory; but, besides the evidence from the presence, the connection is proved by the results attending a removal or ultimate decomposition of organic remains. Cities and large rural districts that had once been highly destructive of human life from the action of *Malaria*, have become entirely exempt from its operation under the application of hygienic means for destroying organic remains, or in the progress of the soil from a state of nature to one of high artificial cultivation.

It is common to consider that the decay of vegetation works a more important part than that of animal matter in the production of Malarial poison; and it is certain that wherever we find soils rich in vegetation, accompanied with humidity and high natural temperature, we see its effects; while the evidence of similar results, from an abundance of animal life, or decay, is not so clear. But it is also certain that the elementary composition of both species of matter is nearly or quite identical—their emanations are alike volatile and gaseous, are known to possess properties that cause them to be blended with and diffused in the atmosphere, and to affect its respirable qualities injuriously. There is, therefore, no reason for supposing that they differ in regard to the quality of the virus they produce, or that the effluvia of the one, in equal quantity, is more or

less dangerous than that of the other. Moreover, wherever vegetation is abundant animal life in some of its forms is equally co-existent; the decay of the one can scarcely take place without the death of the other; and their putrid remains must co-operate in the same destructive result.

Malaria being thus connected with and dependent upon organic decomposition, it is evident that the first and greatest principle in any scheme for preventing its effects ought to be to remove or supersede all such decomposition: "*sublata causa tollitur effectus*;" and only in the event of such a course being found impracticable ought we to think of acting upon the other circumstances attending its existence.

Next to organic decomposition aqueous vapor seems to be the most essential property of the atmosphere to both the production and maintenance of Malaria. Humidity and its fluctuating condition as vapor in the air are modifying elements in the constitution of every climate, and are looked upon as performing important parts in the production of every disease. Besides the physical addition humidity furnishes, it is a constant cause of atmospheric change; it performs an essential part in the increment and decrement of natural temperature; and it exerts electrical influences, while, though not always perceived by the senses, are believed to be in constant action upon the welfare of man. But though, from these properties, it is regarded as capable of being injurious to human life, we must infer from its universal prevalence, in either a liquid or vaporous state, that it never is so in more than a slight degree; and that, indeed, it is necessary, in certain definite proportions, for the regular performance of the functions of the animal economy. Reflection, however, on the effects excess must have on the cutaneous and respiratory functions affords reason for believing that it may cause the system to languish so as to predispose it, to be easily acted upon by other exciting causes of disease. But when in its visible state either as water, rain or fog—it is not redundant, or exposure to it is not continued for a great length of time, it is by no means inimical to the human frame.

But however slightly injurious or harmless the presence of simple humidity may be, the case is materially altered when it assumes the vaporous form, and is in the condition to co-operate with the other recognized circumstances attending the production of Malaria. As water it may be beneficial by preventing the corruption of organic matter, or, when formed, by holding in solution, without the possibility of their doing injury, its otherwise noxious products; but in the state of vapor it possesses powers by which it is well adapted to excite, set free and convey the Ma-

larious effluvia. As vapor it possesses the property in common with water of holding gases in intermixture and solution; and there are evidently cases in which it raises solids and liquids by pure mechanical suspension. This is obvious with earthy particles, the odor of which is often very sensible during the evaporation which follows a shower of rain, may take place equally with animalculæ or spores, and as already mentioned occurs with liquids in the distillation or evaporation of volatile oils.

That simple atmospheric vapor may exert an auxiliary action in the development of periodic fevers is highly probable from its uniform abundance during their prevalence, and the debilitating effect its check to the functions of cutaneous and pulmonary transpiration produces. But it is as an adjuvant or constituent element of *Malaria*, that its effects are most marked and most injurious. Observations on this poison concur in showing that if it is a ponderable body it must exist in combination with vapor; and it is as an agent operating upon and combining with organic emanations that we are to look upon the latter as a necessary circumstance in the production of fever. I have intimated that volatile oils are universally products of organic decomposition, and that they manifest properties common to them and *Malaria*, and sufficiently numerous to render it very probable some one of them is identical with that cause of physical evil. The most conspicuous of these common properties is their affinity for vapor. Considering *Malaria* as a volatile oil its combination with aqueous vapor admits of an explanation of that singular proclivity to obey the law of vaporous diffusion and yet be limited to a low altitudinal range.

The limited sphere of action of *Malaria* depends, no doubt, upon several causes—all connected with its decomposition a similar limited diffusion. In regard to the latter it is well known to meteorologists that the quantity of hygrometric moisture diminishes very rapidly as we ascend in the atmosphere; and as its transporting capacity diminishes in a similar ratio, *Malaria*, from this cause, must attain a speedy limit to its elevation. If *Malaria* be a volatile oil or a substance of analogous properties, and one of those that undergo decomposition by contact with dry air, it is evident that as soon as it is freed from its aqueous connection it must be changed, and ceasing to be *Malaria* cease to be noxious. In addition to these probable causes of the limited diffusion of *Malaria* there can be no doubt that the operation of diminished atmospheric pressure comes into play to prevent either vapor or air from supporting it above a certain height. Nor is it unworthy of consideration whether after certain

degrees of rarefaction, vapor and the specific volatile matter of Malaria may not lose all affinity for each other, and leave the latter to the law of diffusion and dilution [in the atmosphere] of common gases. As a cause of fevers the combination of Malaria and vapor are evidently held together by a weak attraction; for multiplied observations show that a moderately dry, or a moderately cold atmosphere often renders it inert.

Supposing this view of the connection and mutual dependence between Malaria and atmospheric moisture to be correct, it would seem only necessary, if physical effects invariably depend on physical causes, to free that portion of air used in respiration from moisture, to decompose and divest it of Malaria, and insure to man an exemption from its effects.

An agent equally important with organic decomposition and aqueous vapor in the production of Malaria, though forming no part of its substance, is atmospheric heat. Considered alone, but in a state of high tension, it is a source of discomfort and inconvenience to the Caucasian race, and if long continued, acts injuriously upon the constitution. In connection with its invariable accompaniments—a diminished supply of oxygen, and an increased quantity of moisture in the volume of air used in respiration—and independently of any specific poison, it is found to predispose the system to disease, and is sometimes a sufficient cause of actual indisposition. While excessive, it is an evil in constant action; no length of exposure, or habits of the individual, will neutralize its influence, or prevent its effects. It may be considered as enervating, for its unfavorable action is increased by its duration: the common experience of those living in climates in which the winters are cool, and the summers attain a tropical temperature, proves that the heat of the commencement of the hot period can be better withstood, than the lower one of its termination; and the inhabitant of the equator finds the heat more oppressive in old age than in youth, or the meridian of life. That strange disease—insolation—is seldom so sudden in its attack as its popular name implies, but seems to be the result of a gradual absorption of solar heat by the nervous system.

Our observation of the effects of natural temperature on the animal economy, leads us to the conclusion that when moderate, it is necessary to maintain the functions in healthy activity; but when in excess, it is similar in its operation to undue stimulus from other causes, and induces debility. The symptoms of such a stimulation are shown in an excessive secretion of bile, sweat, and other products of overcharged glandular

action; in an incessant transpiration from the cutaneous and pulmonary surfaces, by which the capillaries are compelled to maintain a constant action; in the production of that nervous condition, of whose pathology we are wholly ignorant, called sun-stroke; by all of which the system is enfeebled, and predisposed to be easily acted upon by exciting influences. To the exhausting action of heat upon these functions, we may attribute the uniformly lax conformation of muscular fibre, the slender and weak frame, and the indisposition to mental and physical activity, which characterize the inhabitants of warm countries.

The natural remedy for the discomfort and inconvenience, as well as the morbid condition of the animal system, induced by simple heat, is a diminution of natural temperature; and the discovery and application of a process by which this object could be effected, cheaply and abundantly, would be the discovery which, at the present day, promises to confer the greatest benefit on mankind.

Atmospheric heat, though it readily induces debility, and predisposes to, and actually produces disease, is, unaided by a more specific morbid agent, incapable of producing any form of epidemic fever. For this purpose, it is necessary that it should assume a concurrent action with the other circumstances attending the extrication of *Malaria*. In the influence that solar heat exerts over these, its injurious effects are distinctly marked. Whatever may be the precise nature of the concurring causes of fevers—particularly intermittent, remittent and yellow fevers—it seems to be generally conceded that a highly essential circumstance to their efficiency is a certain degree of atmospheric temperature. It is true, that assent to this position is not universal; unmodified, it has been often doubted, and even denied; but examined in a comprehensive manner, there is scarcely an etiological opinion more generally adopted by physicians. On the other hand, the influence of solar heat has been often exaggerated; it has been considered as the cause, *per se*, of Malarial fevers, while we know that so far from being the direct cause, it operates sometimes as a preventive or terminative of these diseases. Numerous writers on *Malaria* inform us that in the West Indies, on the coast of Africa, and sometimes in this country, its severest form of disease—that of epidemic yellow fever—often commences and ceases before the heat of the season has attained its maximum. Now, independent of a law of definite duration, which it is probable yellow fever, in common with other epidemics, obeys, the desiccating agency of the sun may be exerted in exhausting from the soil the material elements that enter into the compo-

sition of the morbid poison, and so dispersing, attenuating and diluting them with common air, that they cease to be injurious. We have seen that volatile oils mixed with dry clay or sand, and exposed to high natural temperatures, are decomposed; so the desiccating and heating action of the sun may enable these earths to decompose the sources of Malaria. In endeavoring to explain the part that vapor performs in the connection with, and diffusion of Malaria, I have said that the diminished atmospheric pressure of elevated regions induces an expansion of the particles of Malaria which may subvert its combination; so the rays of the sun may produce a rarefaction which places its particles at too great a distance for their mutual attraction, and, hence, of existence as Malaria. In this way the sun disperses the fog or haze which covers the low lands of warm countries during the night, and often gives, from his rising to his setting, a comparative or even total immunity from its effects; while the absence of his influence, by allowing their reconcentration at night, renders them highly deleterious.

As the most prominent element in the constitution of climate, and as the stimulus to the growth of vegetation and the myriads of animals that subsist upon it, as well as the potent accessory to their decomposition after death, solar heat must be regarded as the chief agent in the production of the matter that enters into the composition of Malaria. The evidences of such an agency exerted to create and give energy to the other constituents of Malaria, though like all others in relation to it they are chiefly negative and presumptive, are deemed satisfactory. Malarial diseases are exclusive attendants upon or followers of hot weather, for except in the instance of typhus and some typhoid maladies, where Malaria seems to manifest itself after long accumulation and modification in a latent state, they never break out in the winter season of the temperate zones; and it is very doubtful whether even in summer they ever penetrate as far as the arctic circle. But on the other hand, we find as we direct our enquiries from the temperate into warmer latitudes, Malarial fevers become more common and more violent, displaying the greatest virulence in the equatorial regions.

The influence of heat over Malarial diseases is further rendered apparent by the circumstances under which its intensity increases or diminishes their prevalence. Incapable of existing in high latitudes, from their low temperature, it is also found that in elevated situations the cause of fever acts with less energy from the same cause. It would seem that every variety of Malarious disease requires for its origin a temperature pecu-

liar to itself, but high in proportion to its malignity. Intermittents never rage with epidemic generality; but in an atmospheric temperature above the mean annual heat, the fever which presents the remittent form requires a longer continuance of a similar degree of heat; and the yellow fever is never seen in temperate climates but under a tropical temperature.

Duration of heat like intensity, has a marked influence over the character of Malarious disease. Besides determining the precise form of some of its varieties, it is evidently owing to the long continuance of high temperature that the latter part of summer and beginning of autumn are the most sickly seasons in Malarial climates, subject to great alternations of temperature. This correspondence between the intensity and duration of heat, and the various forms of endemic and epidemic Malarial fevers is so striking, that, though the abundance or paucity of the other causes must modify the result, it is possible to determine the general character of the prevailing disease by the thermometer. A mean temperature of 60° F., continued for a month, seems, from a view of all the phenomena, necessary, even in the most marshy situations, for the production of the milder form of intermittent fever; a temperature of from 65° to 75° F., during the same period, for remittent; and in no year or part of the Northern hemisphere, where the medium temperature for the months of June, July, August and September, has been below 77° F., has yellow fever ever been found. With the assigned continuance of these temperatures in places where moisture and putrefaction are abundant sporadic cases of the respective maladies will generally arise. But if, with the duration, the temperature rises above the number of degrees mentioned, and the concurring causes of disease exist, they will excite miasmata in increased quantities and virulence, and the diseases will assume the general form of epidemics. But though heat is so essential an element in the production of Malarial diseases, it must be considered, as already mentioned, that high natural temperature may, and artificial heat certainly will diminish or put a stop to them; so that we may conclude they are dependent upon a certain limited range of temperature above or below which they cannot exist.

The spontaneous termination of Malarious diseases is not less marked by temperature than their commencement. Though in the epidemic form, they have periods of duration and decline, which might show that they are rather coincidences with than effects of high temperature, yet all the forms are amenable to influences of the opposite character. In countries subject to frost, the progress of the whole class is generally checked

by its advent, and they disappear till the succeeding summer has set in motion the various causes concurring in their production. Where frost never exists, as within the tropics, no season is wholly exempt from every form of Malarious disease; indeed, in many districts they prevail, at least sporadically, throughout the year.

Thus the testimony in relation to the connection between atmospheric heat and Malarial diseases, while it shows that a certain range is an indispensable co-operator in their production, proves that it is not the cause itself; for its excess or absence is sufficient to prevent or put an end to the other causes of the evil, and, hence, to the evil itself. This being the nature of the connection, it is obvious that if the temperature of that portion of the atmosphere in which we habitually dwell, and which we respire, can be maintained above the maximum or reduced below the minimum at which Malaria is produced, it must, as an inevitable consequence, and as in the subversion of organic decomposition, and the condensation of hygrometric moisture, cease to exist.

Malaria has been vaguely supposed, by some physicians, capable of originating every malady in the long catalogue that compose their systems of nosology. However unfounded such a generalization may be, it is very probably the cause of idiopathic fevers, cholera, dysentery, hepatitis, and a multitude of other complaints referred to climate for their origin; and certainly, its presence aggravates every ailment. Though its evil effects are most manifest in tropical and warm countries, it is by no means confined to them. It may be assumed, that wherever the summer season attains a heat above the mean temperature of the earth, there the evidences of its injurious influence will be found to exist. Even where, from the general coolness of climate, and the apparent absence of putrefying matters, as in our Northern cities, its presence cannot be identified, its baneful effects are acknowledged, because they are similar to those of the unhealthy regions, which are unquestionably dependent upon poisonous exhalations.

As a cause of disease, Malaria is subtle, diffused, and must be very general in its operation; where it exists, it is liable to be received by every system; none who breathe it can be certain of escaping its danger; no age, sex, constitution or condition can impart exemption from its influence; no habits, care or caution will secure any one from its assaults. But, though so general in its influence, it varies in the energy of its attacks, even where the developing circumstances are the same. It is everywhere remarkably unfriendly to recent immigrants from a colder country, and

in proportion to the coldness of their native land. What the peculiarity of constitution may be, which renders the people of Northern latitudes more obnoxious to Malaria than either the natives or those who have become assimilated to it by a long residence in a warm country, we do not know; but it is a subject of great importance, and worthy of the fullest consideration. It is certain that we find the same exposure to the causes—predisposing and occasional—of Malaria will produce a deadly fever in the stranger, while the native or old inhabitant of the region may remain in apparent good health.

But while this fruitful source of disease manifests its malignant effects most suddenly and most severely on persons who have moved from cold or temperate into hot climates, it is not without deleterious effects upon the inhabitants of the tropics and tropicoid regions. Children and youth, up to a certain age, nowhere enjoy the same advantages in regard to protection, as native adults; but, during the prevalence of some epidemics, are as subject to the active form of the disease, as strangers to the climate. And though its effects may not be indicated in the native adults by an open form of fever, or other plainly cognizable disease, they are sufficiently obvious to be detected by the careful investigation of the physician.—Insidious in their operation, they yet plainly show that the natives of Malarial regions do not enjoy the robust health of the inhabitants of mountainous, or other healthy countries. To a spectator accustomed to the appearance of the latter, “the whole population of a Malarial district looks like a community of invalids.” “The sallowness of their complexions, the listlessness of their looks, the attenuated figures, are but too evident signs, even where organic disease has not set in, that the disordered state of the functions, which goes under the name of cachexia, exists.” Under long continued exposure to Malaria, the liver and spleen become enlarged and inflamed, cachectic symptoms increase in intensity, a slow, insidious fever ensues, languor increases, the mind is incapable of continuous labor; and, sooner or later, premature death closes the scene. And though it generally happens that adult natives and long residents of warm climates are seldom attacked with yellow fever, yet, as they are subject to the derangements of the organs and system above mentioned, and are liable to the intermittent and remittent fevers, incontrovertible testimony is afforded, that there is no real exemption from the morbid agency of Malaria.

It is also found that the climate which, from conformity of constitution, maintains the indigenous inhabitant free from the severer forms of

Malarious disease, may be highly injurious to the native of a similar climate, as for instance, Charleston or New Orleans to the inhabitant of the country in its neighborhood, or Vera Cruz to the native of Havana; and *vice versa*. In each of these instances, a change of residence from one place to another, during the prevalence of an epidemic, will expose the person making it to the liability of taking the disease. It is also observed, that the slightest change of circumstances removes the immunity from the action of Malaria which long habit may have given.—Recurrence of the poison, after a long absence, in the place of residence, or exposure to it in an unaccustomed form, may cause it to rage with equal violence in the native or permanent inhabitant, as in the stranger.

Much of the foregoing is trite and well known to every physician; but it is so because it is true and important; and there can be no valid objection, in an essay designed to demonstrate how an evil may be prevented, to a repetition of facts which show its nature and magnitude. Malaria has a wider range in the production of disease, suffering and death, and must be looked upon as more subversive of human happiness and prosperity than any other physical evil; and therefore every thing which may contribute to render general the security from its ravages, and to produce the attendant material comforts which the inhabitants of temperate and healthy climates enjoy, should it be listened to with patience.

Hitherto it has not been in our power by any care, caution, science or ingenuity to obtain an efficient and practicable preventive of these evils: nor, indeed, have the measures intended for their cure been more satisfactory to the philosophical physician. Though the effects of Malaria, when presented in the mild form of intermittent fever, are considered by some physicians to be controllable by a specific remedy, yet, experience, the great test of truth, obliges others to doubt whether anything within the resources of their art exerts a real and salutary influence over them. Easily and promptly relieved by Peruvian bark and its essential salts, there is yet no disease, at least in its first or acclimating attack, in which a permanent cure is more difficult or uncertain to the physician, or the duration of which is more harassing to the patient than intermittent fever. Like a number of other remedies—as colchicum in gout and rheumatism, iodine in bronchocele and other scrofulous ailments—the preparations of cinchona seem to have the power of suppressing rather than of eliminating the cause of the disease. With undoubted power of reducing the poison to a latent state, which the system may tolerate without inconvenience for a time, it sooner or later leaves the Malaria to develop itself in free and

sensible re-action. Recovery under the use of any medicine is so rare or doubtful that its occurrence may be considered accidental, or at least is certain only when the cause has concurred in exhausting itself. It may be truly said that there is no occurrence within the scope of medicine that more discourages the physician, places him more on a level with the quack, or excites on the part of the public more doubt of the efficiency of his art than his inability to insure a permanent cure of this apparently trivial disease. But is it surprising that so little control is excited over it, or that in its treatment the most learned physician and the ignorant quack should be on an equality, when both repose with the same calm content upon a specific, the use of which was taught by an untutored savage, and the operation of which science is unable to explain. How can we expect to remedy an evil where its cause is always present to renew it and is withal so obscure as to have eluded the researches of all classes of philosophers?

If this difficulty in effecting the cure of the milder and typical form of Malarial diseases be true, every medical man must admit that yellow fever, and even the higher grades of remittent, and the fever called congestive, are utterly beyond his control. In the milder cases of all, recoveries take place independent of treatment; in those of intermediate severity, with its aid, or in spite of it; but the severest admit of no control, submit to no empire, and run their course inevitably, and the more rapidly from attempts to arrest them, to a fatal termination. Taken as a class, there are no diseases over which medical art has less power; and if they are less mortal in the present than in previous ages, the improvement has arisen more from dispensing with pernicious medicines, and abolishing pernicious practices, than in ascertaining any positive methods of controlling their fatality. The unmanageable character of the yellow fever particularly strikes, at its first appearance, the most learned and experienced physician with doubt and despondency, while its general termination in death appalls the heart of the stoutest and most indifferent philosopher. Justly confident of the utility of their profession, as physicians generally are, and proud as many of them may be of their individual skill, all must acknowledge that the higher grades of Malarial disease work alike, and place on the same level their scientific labors, and those of the presumptuous empiric.

Although the imperfection of medical science places the cure of Malarial diseases beyond our control, yet Providence has compensated for the infliction of the evils, by imparting to us the power of subverting or

entting off a part or the whole of the causes on which they depend.—The beneficence that has endowed us with faculties for preventing suffering, must be viewed by man as among the highest attributes of the Creator. How much to be preferred to even the most successful treatment of disease, must that power be which makes certain the impossibility of its existence! How decided is that improvement in the art ministering to the afflictions of the human body, which substitutes for an uncertain, imperfect or inadequate cure, the means of preventing disease! Who can doubt, that if even the domestic suffering, loss of time, suspension of industry, and expense which disease, in a greater or less degree, inflicts upon every family, could be set aside, it would be infinitely better to anticipate and supercede the action of a poison, than to resist it after its effects have been produced? That art can do much to counteract our natural tendencies to disease, there are numerous affirmative proofs; and, therefore, attention ought to be turned as much to the subject, as in all past time it has been directed to the cure of disease.

[To be continued.]

ART. III.—*Pterygium*: by C. S. FENNER, M.D.. of Memphis, Tennessee.

IN the Southern States; Pterygium is one of the most frequent affections of the eye to which the male adult is subject. Northern authors who have written on the disease, have evidently seen but little of it; consequently their description of its pathology and treatment is not as full as it would have been had the disease been more common in Northern latitudes. Mr. Lawrence devotes several pages to Pterygium, and says, "It has been most frequent within my experience in those who had passed much time in hot climates, particularly the East or West Indies." He has never seen a case sufficiently advanced on the cornea to impede vision, or to justify its removal by a surgical operation. Mr. McKenzie in his excellent *Treatise on the Eye*, gives a brief description of Pterygium, but I should judge his knowledge of the disease is to be derived more from German authors, than from personal observation. Other writers have described cases as Pterygium, to which it bears no resemblance. Mr. Tyrell gives a case having its base on the cornea, and its apex at the caruncula lachrymalis. Mr. Travers describes and delineates as Pterygium,

a union of the conjunctiva of the upper lid with the cornea. Mr. Wardrop represents a fleshy Pterygium projecting between the lids, partially covering the cornea, involving the conjunctiva and semilunar fold in a diseased mass. He and Dr Monteath both say they have seen the disease in very young infants. True Pterygium never occurs in infancy or childhood. During a practice of eleven years in the South-west, I have seen not less than five or six hundred cases of Pterygium, and have watched the progress of the disease in all its various stages for a series of years. I propose briefly, to give a description of the morbid growth, as it occurs here, according to the result of my experience. I believe that in the Southern States, South of latitude thirty-four, one male adult in every hundred, living in the country, has this disease. This may seem an exaggeration, but I will venture to say there is scarcely a physician practising in the locality named, that cannot instantly recall to mind, three, four or even more cases within the immediate range of his practice.

Symptoms and Progress: Pterygium usually commences at some period between the ages of 21 and 30; more frequently after 25 than earlier. A small elevated shining point is noticed at the edge of the cornea; occasionally after exposure to extremes of heat or cold, or after fatiguing the eye by looking at minute objects, a few red vessels are seen, running parallel from the caruncula lachrymalis to this point. This redness lasts for a day or two, and then disappears, the elevation at the edge of the cornea remaining visible. The red vessels re-appear on every slight exposure of the eye to fatigue, and lasts for a longer or shorter time, until eventually they become permanent; before this occurs, the form of the Pterygium can be distinctly traced, even when its vessels cease to convey red blood. The time from the commencement of the disease to the permanent redness of its vessels, varies from two to five or six years, more frequently the latter, if it makes its first appearance after the age of 25. The base of the membrane now gradually increases in breadth, while its apex extends on the cornea towards its centre, thus assuming its characteristic triangular form. The Pterygium is now subject to attacks of inflammation, extending to the ocular conjunctiva, whenever the patient is exposed to exciting causes, the most frequent of which are, riding some length of time facing a cold wind; exposure to the light and heat of the sun during the summer season; reading by artificial light; exposure to night air; irregularity of living; &c. During these inflammatory attacks the vessels of the Pterygium are enlarged, and convey an unusual quantity of red blood; the ocular conjunctiva becomes red, its vessels assuming

the peculiar net-work form of catarrhal ophthalmia, accompanied with considerable photophobia, lachrymation and mucous discharge. There is a sensation of roughness, as of sand beneath the lids, attended with severe smarting, but none of the deep-seated, circum-orbital pain that attends purulent and rheumatic ophthalmia, nor is there any swelling of the lids. The inflammation usually lasts from two to ten days, and then gradually disappears. The more frequent these attacks, the more rapidly the vascular membrane extends. Its growth seems to be caused by a morbid deposition beneath the conjunctiva, and adherent to it, rather than form a thickening of that membrane itself. This deposit elevates the tunica adnata from the cornea, so that when it is removed by an operation, all that portion of the cornea covered by the diseased membrane, is denuded of its natural covering.

As the disease advances, the patient finds himself unable to read, or see small objects distinctly by candle light; this occurs long before his vision is at all, or but slightly impaired during the day, and is one of the most constant symptoms, although unnoticed by any of the authors that have written on the disease. The flame of a candle loses its pyramidal, tapering form, and resembles a ball of fire, with innumerable rays of light diverging in every direction; sometimes a bright luminous halo surrounds the flame. These appearances undoubtedly are caused by the elevated edges of the corneal portion of the Pterygium, irregularly refracting the incident rays of light striking them, throwing some into the eye, while its corneal surface being smooth, opaque and shining, receives some of the reflected rays from the iris, again reflects them, a portion of which passing through the pupil, impinges on the retina, thus confusing vision. The enlarged state of the pupil at night, doubtlessly facilitates this result by admitting a larger number of doubly reflected rays. The disease now slowly advances on the cornea, while the base increases in breadth until it sometimes covers nearly a fourth of the eye, and is lost in the palpebral conjunctiva.

A Pterygium with a broad base never extends beyond the centre of the cornea. Several years since I saw in an elderly lady a Pterygium with a narrow base, that had passed a short distance towards the centre of the cornea and then shot above the pupil, nearly to the sclerotica on the opposite side, thus leaving the field of vision clear. My friend Dr. Malone, of Columbus, Mississippi, sent me a case in 1852 of Pterygium occurring in a mulatto man, some 35 years of age, having embedded in its substance, a serous cyst, of the size of a small bean, protruding between

the lids. I punctured the cyst, evacuated the water, and then removed the growth in the usual way.

This disease may affect both eyes, or may be confined to one, the latter being rather the more frequent. Double *Pterygium*, or two on one eye, is very rare; I have seen but three cases, the most marked of which came under my notice March, 1852. A negro man, about 50 years of age, was brought to me, with double *Pterygium* on each eye, growing from the internal and external angles; those from the valvular semilunaris being slightly the larger. Both advanced over the pupil, nearly meeting in the centre, leaving just a perceptible clear portion of cornea, allowing him sufficient vision to be of some service in the field. I removed the internal *Pterygia*, intending at a subsequent time to excise those at the external angles; but his master dying in the meantime, the patient was removed, and I did not complete the operation. Beer, who saw a great deal of this disease, and speaks of having cured 386 cases, met with but two instances of double *Pterygium*, and, in a single instance, three on the same eye. The latter I have never seen. Some English authors say four on the same eye is not an unfrequent occurrence. I think they have mistaken for *Pterygium* a condition of the conjunctiva, sometimes seen in the eyes of plethoric persons, particularly those addicted to alcoholic stimulants, where the conjunctiva is thickened, and has enlarged blood-vessels running from the circumference towards the cornea. I have seen such condition of the parts, accompanied with true *Pterygium*, growing from the caruncula lachrymalis.

Pterygium crassum, and *tenue*, a distinction made by authors, are one and same disease; the latter term being applicable when the vessels of the *Pterygium* cease to convey red blood. *Pterygium pingue* is an entirely different affection, and is inappropriately named; it is merely a thickening of the conjunctiva, near the nasal edge of the cornea, and is never productive of any unpleasant symptoms, except a slight deformity, which, if annoying, can readily be removed with the forceps and scissors. It is much more common in negroes than among white persons.

Causes. It is difficult to ascertain the causes that produce *Pterygium*. A majority of cases occur in those who are considerably exposed to the open air; hence, it is not often found in persons who have spent a large portion of their lives in cities. Females are rarely affected; I have met with but three or four cases, one of which occurred in a mulatto woman.

South of latitude 34°, the disease is very common, while it gradually

becomes less frequent as we go North. It is much less frequent in the Northern part of Mississippi, than in the middle and Southern counties, while in Tennessee, I have seen comparatively few cases. I am inclined to think the commencement of the disease is connected in some way with exposure to the heat and light of our long continued summer season; doubtless the many frequent and sudden atmospheric changes, by causing the Pterygium often to inflame, assist very materially in advancing its growth, when without such an exciting cause, it might have remained nearly quiescent in its incipient state. Negroes are seldom affected, while it is common in mulattoes, perhaps more so than in whites.

Some authors attribute Pterygium to chronic ophthalmia. This is a mistake. In reviewing Howard on the eye, in the *New Orleans Medical Journal*, No. 4, Vol. 7, in reference to a statement of the author, (which seems taken almost verbatim from Mr. Cooper's reference to Scarpa's opinion on the subject,) that "Pterygium is perfectly free from pain, nor is there any new production of the eye, as there would appear to be, but only an alteration of the thin transparent membrane that covers it, and which is converted by chronic ophthalmia into a thick opaque tunic," I use the following language: "We have seen, in all their various stages of advancement, several hundred cases of Pterygium, and we can scarcely recollect a case in which the membrane had extended on the cornea, that was not at times attended with more or less pain. So far from Pterygium being the result of chronic ophthalmia, our own observations teach us that nineteen persons out of every twenty, laboring under this affection, never had either acute or chronic ophthalmia, until the Pterygium had obtained a sufficient size as to be itself a source of irritation, causing the eye to inflame from very slight causes. True, those persons with large Pterygium are subject to frequent attacks of conjunctivitis, on exposure to cold, particularly cold winds; but it is the Pterygium that is the predisposing cause of the inflammation. The successful removal of the Pterygium relieves these attacks of inflammation, which would not be the case if this membranous excrescence was only the result of inflammation." Since the above was written, several intelligent physicians, whose opportunities for observation had been extensive, assured me that such was the result of their experience. Although chronic purulent ophthalmia, with granular lids, is very common, comprising fully one-half the cases of ocular affections that come under the notice of the surgeon, yet in eleven years' practice, I have not seen a single advanced case of Pterygium accompanied with granular conjunctiva. The inflammation excited

resembles catarrhal ophthalmia, and never permanently injures the eye, more than to assist the further development of the vascular membrane, already existing.

Treatment.—The treatment of Pterygium may be divided into medical and surgical. In favor of its medical treatment, I can say but little. A great variety of caustic, astringent, and stimulating substances have been recommended or proclaimed as specifics, but I believe they are seldom of any benefit, and generally do much harm; indeed I have never seen or known of a cure effected by any local application, and am satisfied that the growth of the membrane, is often greatly accelerated by the irritation and inflammation, resulting from their use. Sometimes the membrane after advancing three or four lines on the cornea, and annoying the patient for a long time, suddenly becomes white and shining, from the absence of red blood, ceases to inflame, and remains in a quiescent state for many years. I saw a gentleman from Alabama a short time since, with a Pterygium extending one third across the cornea. He stated that the membrane had not grown, inflamed, or given him any pain, for fifteen years. A highly respectable physician of Lowndes county, Mississippi, consulted me in 1849, in reference to the propriety of removing two large Pterygia, one growing from the inner canthus of each eye. He had suffered severely from the disease, so much so that he found it difficult to pursue the practice of his profession, particularly attending to night calls. Before he had fully made up his mind to submit to the operation of excision, the disease suddenly ceased to annoy him, all redness disappeared, the membrane became white and shining, and he was highly gratified to find himself able to attend to all the duties of a large and laborious practice. I saw him again in 1853, and learned that the disease was giving him but little trouble, nor had it perceptibly grown for four years. Cases similar to the above are frequently met with, but after remaining quiescent for a while, the membrane generally inflames, and continues to advance on the cornea, until it seriously obstructs vision, but never to the extent of entire loss of sight.

Surgical Treatment.—The only effectual means of curing Pterygium, is to remove it by a surgical operation; this will generally succeed when the base of the membrane is not unusually broad, although cases are occasionally met with in which the disease will return and require to be excised two or three times before the cure is final and complete. When the disease extends to the centre of the cornea, with its base covering nearly a fourth of the eye, and is lost in the palpebral conjunctiva, a

perfect cure cannot be expected; still, such cases may be greatly benefited by removing the membrane, and repeating the operation two or three times, at intervals of six months, should the disease continue to return. My method of performing the operation is as follows: An assistant supports the patient's head, and elevates the upper lip, either with his fingers or the spring speculum. (To do this properly, neatly, and without giving pain to the patient, requires considerable tact and manual dexterity, which can only be acquired by practice; hence, if possible, it is better always to have the same assistant, not only in this but in other operations on the eye.) I direct the patient to turn the eye as far out as possible, so as to have the Pterygium on a stretch. With a very delicate pair of bulldog forceps held in the left hand, I seize the entire membrane midway between the caruncula lachrymalis and the cornea, and slightly raise it from the eye, the ring finger resting on the malar edge of the orbit, at the same time depressing the lower lid. A delicate, sharp-pointed knife, held in the right hand, is now thrust under the membrane at a point equi-distant from the caruncula and forceps, with its flat surface resting on the sclerotica, its edge looking towards the cornea, with a drawing stroke of the knife, the Pterygium, to its apex on the cornea, is shaved from the eye. The knife is now laid aside, and the base of the membrane severed with the side-curved scissors, without injuring the caruncula. If any threads or parts of the membrane remain, they should be raised and clipped off with the scissors.

I have seldom found it necessary to confine the patient after the operation. I direct him to bathe the eye frequently in warm water, and avoid such exposure as would be likely to induce inflammation. Coagulable lymph is effused, organizes, adheres firmly to the sclerotica, and in a few days there appears a membrane nearly as large as the one removed. This is gradually absorbed, leaving a normal conjunctival coat. Should this not take place, a second removal ought to be resorted to. The denuded portion of the cornea soon granulates, becomes slightly opaque, and is gradually covered with its natural transparent membrane. When the cornea is more extensively implicated this termination is not to be expected; some opacity will remain, but of less extent than the Pterygium originally covered. If too little of the membrane is removed, the operation will do but little good; vessels will soon be seen shooting on the cornea, and the disease will directly occupy the same surface as at first. If too much is removed, unpleasant results will follow. Dr. Hays, in his edition of Lawrence on the Eye, with that discriminating judgment

that characterizes everything emanating from his pen, makes the following remark, the correctness of which I can testify to from personal observations; he says, "We must caution the practitioner against the extirpation of the entire basis of the Pterygium, especially when it proceeds from the internal angle of the eye. Instances have come under our notice in which very great inconveniences to the patient followed a neglect of this caution." I saw a gentleman in 1853, who had a Pterygium removed the year previous, including the caruncula lacrymalis. Although the disease was effectually relieved, the motions of his eye were greatly impeded by a kind of frænum or cord remaining after the cicatrization of the wound. He could not turn his eye out as far as he had been accustomed, and every effort to do so was accompanied by a painful sensation like the stretching of a tendon. By performing the operation in the manner described above, the caruncula is left undisturbed, while sufficient of the membrane is excised in most instances to effect a cure. Patients with Pterygium, as with most other diseases requiring surgical assistance, after submitting to an operation or treatment leave for their homes, often at a considerable distance, and are frequently lost sight of altogether, so that the surgeon remains ignorant of the final result of his efforts. It has been my lot to see a considerable number of persons whom I had operated on for Pterygium, at periods varying from two to seven and eight years afterwards, and I have generally been gratified to find the cure remaining permanent.

ART. IV.—*Excision of a Steatomatous Tumor: reported by W. L. GAMMAGE, M. D., Rusk, Cherokee County, Texas.*

In offering a report of this operation, I am actuated more by a desire to point to the importance of a clear excision of the theca or sac of this class of tumors than by any other purpose, as the history will illustrate.

The subject was a negro boy *æt.* 27, of a nervo-bilious temperament, of a robust and well developed form, but much emaciated and enfeebled from the excessive drain which had continued for two years, the whole outer surface of the tumor having daily discharged from 8 to 16 ounces of pus. He exhibited no hereditary taint, nor indeed any evidences of a pre-disposition to cancerous or scrofulous affections.

History of the case: Boy says about 5 years since he had a fall from a horse in which his back was bruised; this gave him considerable pain

for some time, but finally got well; about one year after the fall, he discovered a lump on his back about the size of a partridge egg, near the spot of the original contusion. It sensibly grew until it attained the size of a turkey's egg. He was then placed in the hands of Dr. L., who operated; wound healed kindly, but the tumor rapidly returned, and in the course of 24 months grew to the immense size which it presented when it came to Rusk to be excised. About this time, however, the boy was placed in the hands of Dr. * * *, who, after beginning an operation by making two lateral cuts with the scalpel upon the sides of the tumor, declined proceeding, and abandoned the case. His prognosis as to the boy's recovery being, as I am informed by attending physicians, *unfavorable*. The boy was then sent to C. B. Raines, M. D. and myself, who after preparation of the system for a few days, proceeded to operate in the presence of Drs. Hicks, Bazkin and Johnson. Chloroform was given by inhalation until complete anæsthesia was brought on, when the operator, Dr. Raines, commenced by making an incision through the integuments, cutting upon the right side of the tumor from above downwards and leaving a flap, directed the knife into the tumor until it was dissected off to its centre all along upon the right; the knife was then passed to the left side, and the dissection continued until the whole was removed; several arteries were cut which we ligatured very readily with the assistance of Dr. Johnson, to whom I now take occasion to return our thanks for his kindness.

Upon examination we found that the radix remained, and upon placing the fingers at its apex, and making sensible pressure downwards, an opening was discovered, which a few strokes of the knife proved, to be the space between the sac of the tumor and the longissimus dorsi and trapezius, upon which muscles the tumor seemed, and indeed proved to be, adherent: the radix then, *with the entire sac*, was dissected from the theca of these muscles, and thus the operation of excision was concluded: proving clearly to all the attending physicians that every part and parcel of the offending substance was removed. It is well enough to mention here that a very distinct sinus was discovered between the *theca* of the muscles and the sac of the tumor, which sinus contained in its centre, a shred-like membrane resembling fine silk paper wisped or rolled upon itself, and surrounded by a minute quantity of pus-like matter which seemed to communicate with the body of the tumor.

The wound was then dressed after sprinkling it thoroughly with equal parts of sulphate of copper and alum pulverized. The dressing consisted of strips of adhesive plaster drawn tightly over the wound to bring the

flaps together, a plaster of basilicon ointment over these, then a pledget over the plaster, and a bandage over the whole.

Treatment: One teaspoonful red peruvian bark, half a wine glass port wine three times per day; bowels to be regulated by the use of cream of tartar and sulphur. The wound healed rapidly and perfectly; in seven weeks boy dismissed, and no prospect of a return of the tumor; general health perfectly restored.

Description of the Tumor: The tumor weighed five pounds seven ounces and three-quarters; the body of the tumor a species of honey-comb; its cells filled with adipose matter; the peduncle, which was about six inches in diameter was composed of fibrous matter very firm and consistent.

There can scarcely be a doubt but that the first operation would have been as completely successful as this of ours, had the surgeon removed the sac, instead of leaving it behind to form the nucleus of a new deposit.

January 22nd, 1855.

ART. V.—*Case of Compound Fracture of the Humerus,* by GEORGE S. D. ANDERSON, M. D., of Alexandria, La.

HENRY C. SMITH, aged about 33 years, was accidentally shot with a musket loaded with buckshot on the night of the 11th November, 1854. One of the shot struck his left arm just below the insertion of the deltoid muscle, and fractured the Humerus. The shot embedded itself in the posterior muscle of the arm. A physician who lived near, was immediately sent for. He did not visit the patient then, but told the messenger that it was unnecessary for him to do so, as nothing could be done for the wounded man till inflammation would come on and subside. He, however, directed a poultice to be applied to the wound every two hours, with laudanum to be poured over every second one sufficient to cover the entire surface of the poultice.

Three days after the accident, this apostle of Esculapius visited his patient, but made no alteration in the treatment. Inflammation had come on, and he probably wished it to subside before he would make any attempt to remove foreign bodies, if there were any in the wound, or before he would attempt to reduce the fracture.

November 27th. Was called to see the above mentioned patient. His arm was very much swollen, and discharging a large quantity of thick, yellowish matter from the wound. The matter seemed to have

burrowed amongst the muscles, as a great deal was squeezed out by compression made by encircling the arm below the wound with my hands. On moving the elbow crepitation was distinctly audible. The forearm was also very much tumefied, and pitted on pressure. His hand was not much swollen, but he could not use either his hand or fingers. In a word, he had no use of any muscle of that entire extremity. A bandage was applied, commencing at the free extremities of the fingers and continuing it to the shoulder, leaving the wound, which was left uncovered by the bandage. The arm was now brought alongside of the body, in which position it was secured by bandages applied in a manner similar to that for fractured clavicle. The only topical treatment was fresh lint to the wound, which was to be washed twice a day with warm soap suds. On my next visit, however, I learned that the lint had been discontinued after a day or two after I left, and a poultice just large enough to cover the wound had been applied. I did not alter this, but suggested that as the object was to exclude the air and keep the place moist and soft, simple cerate would have been better.

December 3d. Found patient much better. Very little swelling of any part of the extremity, the most of which was in the forearm. Can use hand and fingers, and slightly flex the forearm on the arm, evincing that the biceps muscle had not been injured by the shot which had entered the arm at its outer border. There was but a small quantity of matter discharging at the wound, and this was more laudable than when I first saw the patient. The bandage was quite loose in consequence of the subsidence of the swelling. Crepitation on moving the elbow. The arm was slightly curved forwards, making a concavity from the shoulder to the elbow posteriorly. The bandage was therefore tightened, and three splints applied and secured in their places by another bandage from the elbow to the shoulder.

December 5th. Patient still improving. The swelling in the arm had so nearly or entirely subsided that it was necessary to apply another bandage. This was put on over the last. I saw the patient no more.

It was nearly four weeks after my last visit to Mr. S. that I was again in his neighborhood, and learned, to my great satisfaction, that the cure of his arm had been complete, and that he had left a week previously on a visit to Alabama. The bandages remained on from the time they were put there till the morning he left for Alabama, when, as the fractured ends of the bones had united and the wound had healed, all dressings, &c. were discontinued, and the bandages and splints were taken off.

When I first visited my patient, I found him very much emaciated and sallow, from the profuse discharge of pus from the wound. I therefore ordered a nutritious diet, with wine, and brandy if necessary. Happily, a nutritious diet was all that was necessary, as the purulent discharge was so much diminished by the above treatment as no longer to be a source of exhaustion to the patient.

There were no constitutional remedies used. Castor oil was directed to be given if the bowels should become constipated. This was intended only to prevent fæcal accumulations.

I have every reason to believe that sooner or later amputation would have been necessary to save the patient's life, if the case had gone on without an alteration in the treatment prescribed by his first attendant. Callus never could have formed, and the matter so copiously secreted would have continued to insinuate itself between the muscles till nothing but amputation could have saved his life.

The points of interest in this case are—*First*, His first attendant is an old practitioner, and says he was educated in New York and spent some time in a hospital in that city. *Secondly*, His treatment was extraordinary and unheard of; and *Thirdly*, The amendment that immediately followed appropriate treatment, so that within four weeks after it was commenced the cure was completed and the man had set out on a trip to a distant State.

Believing this case of sufficient interest to be published, I have reported it for the benefit of junior practitioners.

ART. VI.—*Case of Mrs. Watkins' cure of Recto-Vaginal Laceration*, by DR. J. MARION SIMS, of New-York; reported by M. S. Watkins, M. D., of Jackson, Miss., the Husband of the Patient.

WE were married on the 15th day of May, 1828, and almost immediately thereafter, Dysmenorrhœa, indicating a morbid condition of the uterus, made its appearance. On the 8th of January, 1831, she gave birth to her first child, and on that occasion had the misfortune to sustain a complete Recto-Vaginal Laceration, tearing asunder the sphincter ani muscle. From this time her health gradually declined, and for a period of twenty years, she suffered all, and more than all the distressing affections which we might naturally conclude would arise from the condi-

tion which I have mentioned above. Without going into all the minutia, I will state some of the most prominent affections under which she labored. Dysmenorrhœa, engorgement and displacement of the womb, ulceration of the os uteri, occasional dysuria, leucorrhœa in its most aggravated form, accompanied with that most distressing affection, Vaginal Pruritus. To these may be added, indigestion, with the usual concomitant symptoms, diarrhœa incessant, harassing exhausting, raw-beef tongue, aphthæ and inflammation of the mucous membrane lining the alimentary canal. Such were the distressing symptoms, presenting themselves under different phases at different times, but always advancing like a ruthless enemy, panting for its prey.

Such was the condition of my wife, utterly prostrated in body and mind, when in the month of February, 1853, she was placed under the charge of Dr. Sims.

After an absence of nearly two years, she returned home some ten days since, and is now a living monument of the triumphs of surgery. The Recto-Vaginal fissure is completely re-united, including the sphincter ani. The uterus is replaced, and performing its healthy functions, and her general health is now so good that she is more fleshy than I have ever known her, and she is now busily employed in the discharge of her domestic duties.

But I should do great injustice, did I not state, that last summer she would most unquestionably have died, in New-York, from absolute prostration, had she not have been removed by Dr. Sims from the city to the hydropathic establishment of Drs. Wellington and May, at Orange, New Jersey. Here she was subjected to cold water practice, and her health so much improved that on her return to the city, Dr. Sims was enabled to perform his last surgical operation, which terminated the management of her case.

The recovery of my wife, may, I think, be fairly attributed to the surgical skill of Dr. Sims, and the recuperative tendencies of hydropathy. I say recuperative tendencies of hydropathy, for certainly in the case of my wife, the warmest admirers of hydropathy will hardly contend that her case could, by any possibility, have been cured by the cold water treatment, without the efficient agency of Dr. Sims.—*February 6th, 1855.*

February 23, 1855.—I am happy to say that the health of my wife is good, and that the uterine function, from which I apprehended most danger, is not only re-established, but more healthy than it has been for twenty years.

Dr. Sims is getting to be a man of note, even among the many magnates of New-York; and much, very much of his time is occupied by the reception of visitors in his office, of persons, particularly of physicians, who come to seek information from him. He has been laboring with much zeal to effect the establishment of a Hospital, for the reception of females who are laboring under diseases peculiar to their sex, and I have no doubt that his efforts will be crowned with success; but as some time must elapse before suitable buildings can be prepared, arrangements are now making, and will shortly be carried into operation, to commence his system in temporary buildings, prepared for the purpose. Dr. Sims, (as I learn through my wife,) has lately performed a new operation for Ovarian dropsy—an operation which, I learn, has never been performed before. Whether he has succeeded or not in effecting a cure, I have not yet heard. That he will triumph over all opposition, I have no fears.

ART. VII.—*Review of the Epidemic Yellow Fever of 1854, as it appeared in Charleston, South Carolina: by B. DOWLER, M. D.*

THE following correspondence, which appeared in the *Charleston Medical Journal* for January, 1855, is highly interesting, both in a scientific and moral point of view. It is interesting to know that the irruption of Yellow Fever, in Charleston, in 1854, did not transcend the old landmarks which had circumscribed its march, and formed its peculiar features and differential characteristics for more than three centuries. It did not, as was at first reported, attack natives and strangers equally. But three adults born in Charleston fell victims to that disease, out of the large number that perished, amounting to 621. Even these three are not known to have been born of acclimated parents, nor does it appear that they were known to have been constant residents in that city. The forty-one children who died, may have all, or a large proportion of them, been born of unacclimated parents, recently arrived from the rural districts, or from foreign lands. Among foreigners, constituting a small proportion of the total population of that city, 458 died; among the natives of the United States, exclusive of Charleston, 119 succumbed, while the natives of the city contributed but 3 adults and 41 children to the mortality resulting from the great Epidemic of 1854. This small number, distributed over the Yellow Fever season, would scarcely have excited attention, had there been no strangers present.

There is another feature in this correspondence, the moral aspect of which is creditable to the late editors of the *Charleston Medical Journal*. Having done good service in the cause of science as editors, their last act on withdrawing from the conduction of the *Journal*, is one rectifying an erroneous or rather conjectural statement of theirs in the preceding number of the *Journal*, concerning the mortality of Yellow Fever among the natives of Charleston. The lovers of truth lose not, but gain in moral dignity as well as in science, by the correction of errors even though unintentionally committed.

“Letter to the late Editors.

“CHARLESTON, December 23d, 1853.

“MESSRS. EDITORS:—In the number of the *Journal* for *November*, you make some remarks on Yellow Fever, which may lead to erroneous conclusions. After giving a tabular statement of the mortality from the fever up to the 1st of *November*, 1854, you remark:—‘Many believe that old residents were either entirely, or almost without a single exception, exempt from yellow fever; though some, even of those, suffered from mild attacks of some form of fever. It is also the opinion of a large number that the proportion of deaths among natives was certainly not more than one-third, or even one-fourth, that of foreigners; making of the 600, 200 natives—a large proportion of the latter being children.’ It is to the last sentence of this extract that I would direct attention, viz:—to the statement *that one-third or one-fourth of the deaths which occurred from the yellow fever of the past season were of natives—a large proportion being children!* From a careful analysis of the cases recorded by the City Registrar, I find that the proportion of natives is greatly overstated in your remarks. The following table includes all the deaths recorded by that officer up to the latest death from yellow fever, viz: up to the week ending *November 25th*.

“Total number of deaths, 621, viz:—

Foreigners, - - - - -	458
Natives of the United States, exclusive of Charleston,	119
Natives of Charleston, (3 adults and 41 children,) -	44
	621

“It thus appears, that instead of the deaths among the native population amounting to *one-third* or *one-fourth*, they were only *one-fourteenth* of the total number; instead of being about 200, they were only 44, and instead of a *large proportion only* being children, 41 out of the 44 were children, and only 3 were adults.

“This correction is the more necessary, as it would otherwise leave the impression that we natives had lost the great safeguard which our acclimation gives us; because, on the outbreak of fever in future years, we too, if your statement be true, would have to tremble before the pestilence; because, anxiety would take the place of the confidence in which

we now live, while hundreds of those foreign to our soil and our climate are dying around us; and because it would teach our native population to fly from their homes, and from the duties and responsibilities which should bind them here, whenever a severe epidemic declares itself in our midst.

“But there is, also, another reason why this erroneous statement should be corrected. It is this. Persons regarding it as true, will say that it was not yellow fever which prevailed here last summer; and they will allege, as a reason for this, that yellow fever respects natives, while the fever of last summer destroyed a large number (200 you say) of these. There is already too much proneness towards error of this kind. Because one epidemic differs from another of the same disease, in intensity and in extension, it is regarded as a different disease. This would overturn the foundations of our knowledge, would render vain and nugatory the facts recorded by our predecessors, and remove those prominent landmarks by which we are guided, and which enable us to form our diagnosis of different diseases. That the epidemic of the last summer, in our city, differed in certain of its features from other preceding epidemics of yellow fever, is undoubtedly true; but it did not differ from them more than epidemics of other diseases (scarlatina, for example) differ among themselves. The main features of the disease remained the same, and the main prominent points in its history were the same. One of the most important of these is, that the adult native is exempt from its severe forms, that he does not die of the disease; and in this our epidemic of 1854 forms no exception to those which have preceded it, for, out of 621 deaths, there were but 3 or 4 native adults. Children, of natives, have always been regarded as only partially acclimated—not as altogether exempt, but as measurably so from its attacks, and this is also shown by the epidemic of the past summer, for out of 621 deaths by yellow fever, in our city, only 41 occurred among the native children.

“You will pardon me, Messrs. Editors, for thus venturing, in your own Journal, to criticise your statements, and to arraign your facts. But I know you too well to presume that you are not always ready to correct errors, into which you have inadvertently fallen, or to imagine that you will not willingly publish truths so important to our city, and to the history of its most dreaded disease, as that I have endeavored to draw out from its mortuary records.

“With great esteem yours,

“P. C. GAILLARD, M. D.

Reply.

“It is to us a matter of great regret that the concluding paragraph to which Dr. Gaillard’s strictures refer, appeared in our editorial remarks on the Yellow Fever, in the November issue of the Journal. The statements therein made were rendered unnecessary by what was said in the *first* paragraph, and in consequence it was ordered to be omitted by the printers. It was intended that our editorial remarks should end with

the tabular statement of the *whole* mortality; it not being our wish to enter upon a statistical history of the fever at that time. But, as an instance of the numerous blunders that occur in type setting, it was allowed to remain, and, to our surprise, it was published.

“Even in *those* remarks, however, it seems that in placing the number of deaths of natives at or about 200, it was *distinctly* stated to be wholly *conjectural*. The remarks in question were penned some time before the epidemic had ceased; consequently *before complete* returns of the relative mortality of the two classes, strangers and natives, could be obtained. We admitted the incompleteness of the statistics; and in default of this completeness, we therein promised our readers that we would, at a future time present them with a fuller and more reliable sketch of the epidemic than we were then able to do.

“We thank Dr. Gaillard for having furnished, in his comments, a correct statement of the comparative mortality of strangers and natives; in which we are gratified to find that the number of the latter falls far below our estimate.

“CAIN & PORCHER.”

No extra-tropical city equals Charleston in events illustrative of the characteristic habitudes of Yellow Fever, during a very long period.—The Yellow Fever Epidemics, with which the seventeenth century closed and the eighteenth opened, in that city, up to 1703, were probably not so severe as that of 1854, after the lapse of one hundred and fifty-one years.

Increased interest attaches to this last Epidemic, from the premature and erroneous conclusions which were drawn and propagated concerning its ravages among all classes of the population of Charleston, irrespective of natives and strangers—of the acclimated and the immigrants. These hasty conclusions, advanced before all the necessary data had as yet been obtained, whereby they might be established or overthrown, are fortunately without foundation, as the above mentioned documents prove.

In the *Charleston Medical Journal*, of January, 1855, Professor Hume, M. D., Alderman and-so-forth, repudiates, in the opening article, at length, the doctrine of the domestic origin of Yellow Fever, which he had formerly advocated. He says that during the last thirty years Charleston has suffered twelve times from Epidemics of this malady; “that the greatest and most extensive excavations that were ever witnessed in Charleston, took place in the summer of 1848, yet a healthier summer was never known; and that no excavations were accomplished in the summer of 1854, yet the fever has been more fatal in its results, more numerous in its attacks, and more extensive in its ramifications, than was ever known in Charleston.”

Prof. Hume, who believes in the contagiousness or transmissibility of Yellow Fever from person to person, continues:—

“Cases were carried from Savannah, and possibly from Charleston to Augusta, and the same phenomena of propagation occurred there, that did in Charleston and Savannah. The infection spread through the town, involving hundreds in disease and death, being the same disease as that prevailing in Savannah and Charleston. In the year 1839, the same occurrence took place, which was attributed to a very low state of the river, and the accumulation of city offal in its dry bed. In 1854, the same causes have not existed, the river is full, and the offal is elsewhere; and the negligence of the inhabitants was soon awakened to the true history of its introduction, and common observation soon taught them the mode of its propagation. Augusta must now be considered as capable of receiving and propagating the infection of Yellow Fever, notwithstanding the differences of topography between her and the cities of Savannah and Charleston. * * * * If we suppose the surface of the Savannah and Congaree rivers level, we notice Columbia seated one hundred and ninety-four feet above the abutment of Columbia bridge, while Augusta lies not more than twenty feet above the abutment of the Augusta bridge. Columbia is more than one mile from the river, on its eastern bank, while Augusta lies contiguous to the river, on its southern bank. These relative positions, with respect to elevation, direction and distance from the rivers, enable us to trace nearly as great a difference between those two places, as we have seen to exist between Aiken and Blakville.”

In regard to the Yellow Fever of Charleston, Professor Hume remarks:—

“The belief in local causes was so predominant, that the people would rather attribute the origin of a case to a mud-puddle in the street, than to a case of fever opposite. Unfriendly as we are to mud-puddles and low lots, we would rather see one thousand of them in the city, than see one infected vessel at the wharf; that puddles and low lots aid powerfully in propagating the disease, we are fully convinced, and that they should be abated we have often urged; but let us cover our city with ten feet of pure sand, we will not counteract the danger, nor materially mitigate the risk of foreign introduction, and subsequent self-extension of Yellow Fever. There is always moisture and organic matter enough in our atmosphere, to constitute the vehicle of extension; and although it is our duty to diminish the quantity of each as much as possible, it is hopeless to presume that we will ever be entirely successful. Fifty-four years of experience in internal medical police, should satisfy the most credulous that its efficacy still lies in hope and false promises, quite successful when the fever is not brought to us, but a total failure when an infected ship arrives in our harbor. * * * *

“It may be remarked, that we have no control over temperature; true, but we have control over the introduction of the poisonous foreign

element, which vitiates our own healthy air, and it is to this end we must address our efforts. Every possible contingency of admixture must be guarded against, with as little injury to commercial interests as our safety necessitates." * * * *

"From the observations made upon the course of the Yellow Fever during the present season, nothing has been brought to light which could induce a belief that it was self-productive in our city or climate. The observed facts tend to show that the increase of temperature from May to August, only render our climate *propagative*, not productive of the fever. The tree grew when the seed was planted, but the seed was not created in our climate. The germ was brought, nourished and cherished among us, and our soil and climate took no other part in the matter than if the seeds of a tropical plant had been sown in our gardens. It grew so long as soil and climate suited its exigencies, when these failed, it withered and perished. The reduction of the solar temperature in October effected this condition, and the fever soon ceased its ravages.

"From the history given above of the concurrent existence of the Fever in a West India vessel, about ten days before the development of the same disease in a sound Northern vessel, we are induced to infer that proximity to the infected vessel was more likely to produce the disease than any known cause on the wharf. The city at that time was exempt from the disease and quite healthy. The first cases of propagation were among the crews of adjacent vessels, and occurred precisely in the same manner as was observed to take place in the city, *i. e.* from house to house, until it reached its ultimate limits. Personal transportation and diffusive expansion were both in active operation during the summer, and to these two causes it is that we owe the epidemicity of the disease. It was in the concurrence of its first importation, a soil and climate suitable for its propagation, and a people susceptible of its influence, that we can trace its origin and subsequent increase and multiplication. We need not inform intelligent readers of our utter incapability of altering the soil, or of modifying the climate, but we may insist upon the possibility of excluding the germ from our shores; and it is in this single effort that all our hopes are concentrated, to relieve us from a pestilence which walks in midday, and will not be excluded so long as human life is esteemed to be less valuable than the gains of commerce. An officer who obeys instructions is not responsible for a failure, and if the Yellow Fever has been imported into Charleston at different times, the fault is in the law, rather than in the execution of the law. The Act of Assembly, passed Dec. 20th, 1832 is defective in a very important point. It only takes cognizance of vessels which are 'foul and infected' with any 'malignant and contagious disease,' but takes no notice of those vessels which become 'foul and infected' after their arrival, and after they are moored at the wharf. To establish the fact that a vessel is 'foul and infected,' the port physician must see the disease on board; but how often does it occur that a vessel passes quarantine inspection, is

pronounced sound, and in a few days after the disease manifests itself. She is now beyond the jurisdiction of the port physician, the decree and certificate of health cannot be revoked, and the captain or consignee is not bound to report the foulness and infection after the port physician has decreed that there is none."

The above extracts, indications of Professor Hume's opinions, are reproduced in this Journal upon the principle that one should hear both sides of a question of great public concernment. The cases, facts, and special pleadings, in *extenso*, supposed to be corroborative of these opinions, are excluded for want of room; nor do they seem new or conclusive as it regards the contagiousness and the importability of Yellow Fever, and the possibility of excluding it by quarantine, but tend rather to show the fallaciousness of some of its reputed causes, as bad drainage, filth, disturbances of the earth's surface, &c.

Professor Hume's researches upon the geography of Yellow Fever during the year 1854, in South Carolina and the contiguous States, prove that the epidemic raged in places altogether different in topography, dryness, elevation, drainage, soil, filth, temperature, and hydrographical characteristics. To some of these conditions he attributes much importance, as being more or less favorable to the diffusion of the Yellow Fever contagion. He says:

"We have long suspected a moist atmosphere to be the medium of the diffusion of disease, and the suspicion is somewhat verified by the behavior of the fever in Blackville and Augusta, when compared with Aiken and Columbia. To those acquainted with the topography of these places, it is only necessary to advert to the high and dry position of Aiken, the deep gorge adjacent, and the incapability, from the general inclination of the surface to this gorge, for water to remain on the surface. Blackville is considerably lower, upon a flat level, and the depression in its midst was formerly a pond, and is now artificially and imperfectly drained. The amount of moisture in the air must necessarily be very different over these two situations, dependent upon local causes. The capability of the air, in both places, to receive equal quantities of moisture, the temperature being equal, are alike, but the amount furnished will be very different, the moist surface of Blackville furnishing a much larger supply than the dry surface of Aiken, and for a much longer period. The clay of Aiken is retentive of moisture, while the sand of Blackville evaporates it freely—hence the air of Aiken will be comparatively dry, while that of Blackville will be moist. We cannot pretend to affirm that the propagation of the fever in Blackville, is dependent upon the probable excess of moisture, nor that the non-propagation, in Aiken, is dependent upon its superior dryness, but, in the absence of further information, we are justified in adverting to those affinities which proclaim

a striking similarity between known malarial and non-malarial localities. Blackville has strong local resemblances to Charleston, and can propagate the disease; Aiken has no resemblance to Charleston, and cannot propagate the disease—hence it is reasonable to infer that the propagation is dependent upon some of those," &c.

The equivocal manner in which disputants treat the subjects of contagion, infection, transmissibility, miasma, the importation of fomites, disinfection, and quarantine, is calculated to keep up a ceaseless, fruitless, and inconclusive logomachy. Latitudinarian interpretation of facts, vague and indeterminate uses of words, theoretical biases, and foregone conclusions, together with the intrinsic difficulty of comprehending these subjects, will account for the diversity of opinions which prevail concerning the ætiology of yellow fever, dengue, dysentery, cholera, influenza, and many other epidemic maladies.

Professor Hume's ratiocination is by no means strictly in accordance with the ancient landmarks of contagion. It is not contact with the sick nor the immediate emanations from their bodies which constitute the sole basis of his theory. He allows contingent and intermediate causes to perform the work of direct personal presence and emanation. When the disease cannot be accounted for by immediate communication, he calls in the aid of atmospheric agency, in diffusing the contagion to undefined distances without affording any very evident type or known analogy. Nevertheless, he speaks doubtfully. He says:

"We next proceed to examine those cases which must be presumed to arise from diffusion, as we are unable to trace an immediate communication with an infected vessel. Where close proximity exists there is no difficulty in admitting diffusion; but where the distance increases, our doubts increase in the same ratio. The assumed limit to the distance to which diffusion can extend, is the strong position of those who advocate the domestic origin of Yellow Fever in Charleston, for it is evident if diffusion cannot extend one thousand feet, and a patient is found at that distance from the infected spot, he could not have derived the disease from the infected spot, hence it must be from a nearer source, and that source probably a circumstance which has existed for years, and never proved detrimental until the new and additional source was brought to the city. It has been truly remarked that the inhabitants of low lots and moist places were the first infected, but it was also remarked that later in the season the high and dry lots were equally infected. It is not a legitimate conclusion, that the low lots originated the disease in the first part of the season, any more than that the high lots originated the disease in the latter part. If this could be proved, the advent of infected vessels to our port takes no lot or part in the infection of adjacent and neighboring vessels; then how are we to explain the curious fact, that only certain vessels

become infected, and those vessels only having communication with others recently arrived from infected regions, and with the disease often among the crew?" * * * * *

"Even admitting that the intensity of the miasm is diminished according to the established law, in proportion to the square of the distance, a sufficiency may remain to produce its peculiar effects; and as that peculiar effect is produced at a considerable distance, it is vain to say that it cannot be produced. * * * * *

"From the details above, and the melancholy recollections in the minds of our Charleston readers, ask yourselves the simple question,— 'Whether it is questionable that a person or persons, coming into a healthy city, could propagate the disease so as to vitiate the atmosphere of a whole city, and create an epidemic.' For ourselves, we reply that in the face of the facts observed and detailed, it is *un*-questionable that a person or persons, affected with Yellow Fever, coming into a healthy city, *can* so vitiate the atmosphere of the whole city, as to propagate the disease, and create an epidemic. We have traced the first cases of our Epidemic to vessels from the West Indies. We have seen the identical disease on board, then in the adjacent vessels, afterwards at a greater distance, and towards the end of the season at the most distant inhabited portions of the city. The primary cause of the disease, as well as the disease itself, were brought to our city in these vessels; as the cause expanded in our impure, heated air, the disease as the effect, expanded in proportion. The cause took the precedence, the disease followed. The disease was but the visible sign of the progressive expansion of an element recently incorporated with our atmosphere, and up to the period of arrival of these vessels, was non-existent as an elementary component portion of our air; or, if existent from previous arrivals, was kept in check by circumstances adverse to its expansion. * * * But we thank God the curtain is now withdrawn, the hundred eyes of Argus have watched the whole process of introduction and subsequent propagation, and we trust that the present generation, at least, will believe what man have seen, and what all have heard."

Havana has often thanked God and cursed Charleston, after this manner, from generation to generation. More than half a century ago, Dr. Oyarvida, of Havana, published a work in the Spanish language, to show that the Yellow Fever in that city did not originate therein, but was imported from the *United States*; and that when a person is exposed to this imported contagion, without contracting Yellow Fever, it is owing to the mercy of God.

Dr. W. T. Wragg's Report on closing the Roper Hospital of Charleston, S. C., after the Yellow Fever epidemic of 1854, made to the trustees, and read before the Medical Society at its annual meeting, and unanimously recommended for publication by both of these bodies, is a highly instructive document—extracts from which will now be subjoined:

“The doors of the hospital were thrown open on Friday, the 9th of September, and by the evening of that day 14 patients were under treatment. On the evening of the 10th, there were 25 patients in the wards, and the daily number of admissions continued rapidly to increase till the 20th, on which day 21 patients were admitted.

“The greatest number in the hospital at one time was about 70. This was on the 20th September, and on that day the influx of patients was so rapid, that, for a time, it was impossible to provide beds fast enough.

“The total number of patients treated was 254. Of these 198 were males and 56 females.

“131 were from Ireland; 51 from Germany; 29 from the United States; 15 from England; 4 from Scotland; 3 from British America; 3 from Poland; 2 from France; 1 from Spain; 1 from Switzerland; 1 from Denmark; 1 from Norway; 1 from Sweden; and 11 unknown.

“The ages of these patients were as follows:—3 were under 10 years of age; 40 between 10 and 20; 142 between 20 and 30; 46 between 30 and 40; 18 between 40 and 50; 2 between 50 and 60; 1 between 60 and 70; and 1 was 96 years old.

“The length of time these patients had been in Charleston was as follows;—2 of them had been but 2 days; 1 but 5 days; 3 but 1 week; 23 between 1 and 2 weeks; 21 between 2 and 3 weeks; 4 between 3 and 4 weeks; 28 under 6 months; 45 between 6 months and 1 year; 36 between 1 and 2 years; 21 between 2 and 3 years; 9 between 3 and 4 years; 14 between 4 and 5 years; 6 between 5 and 6 years; 6 between 6 and 7 years; 3 between 7 and 8 years; 2 between 8 and 9 years; 1 between 9 and 12 years; 1 between 12 and 14 years; 1 between 14 and 15 years; 1 between 15 and 16 years; and 1 had been 35 years in the city; he was not, however, a Yellow Fever patient.

“In 15 cases the length of residence was not ascertained.

“Their occupations were as follows:—50 labourers; 46 servants; 37 sailors; 18 painters; 17 clerks; 8 carpenters; 8 bakers; 6 seamstresses; 6 coachmen; 5 bricklayers; 5 shoemakers; 4 washerwomen; 3 coachmakers; 3 cabinetmakers; 2 guardsmen; 2 machinists; 2 blacksmiths; 2 paperhangers; 2 pedlars; 1 cook; 1 gasfitter; 1 tailor; 1 cooper; 1 dyer; 1 watchmaker; 1 nurse; 1 merchant; 1 eigarmaker; 1 farmer; 1 fireman; 1 sausagemaker; 1 butcher; 1 plumber; 1 stewardess; 1 tanner; and 12 unknown.

“The part of the city in which the patients resided was accurately noted, but this was found to cover so large a portion of the entire area inhabited by our population, that I can only give a general idea of the divisions of the city most severely scourged.

* * * Of the 19 admitted on the 1st day of illness, 4 died and 14 recovered, 1 being unknown—giving a mortality of about one-fifth. Of the 44 on the 2d day, 8 died and 31 recovered, 5 being unknown—giving a mortality between one-fifth and one-sixth. Of the 34 on the 3d day, 12 died and 20 recovered, 2 being unknown—giving a mortality of about one-third. Of the 16 on the 4th day, 5

died and 8 recovered, 3 being unknown—giving a mortality of about one-third. Of the 24 on the 5th day, 13 died and 10 recovered, 1 being unknown—giving a mortality of over one-half; and of the 14 on the 6th day, 10 died and 3 recovered, 1 being unknown—giving a mortality of over 3 to 1. While of the 14 on the 7th day, only 6 died, 7 recovering and 1 being unknown—giving again a diminishing mortality of less than one-half, which we find continuing—for of the 1 on the 8th, there was no death. Of the 6 on the 9th, there was 1 death to 5 recoveries—giving a mortality of one-sixth. Of the 3 on the 10th, there were 2 deaths to 1 recovery—giving a mortality of two-thirds. Of the 2 on the 11th, both recovered. Of the 1 on the 14th, 1 died; and of the 3 on the 15th, none died.

“Thus it will appear that in rather the largest number of cases (80) the duration of the 2d stage was 1 day; and that in a number not much less (67) it lasted 2 days; and, further, we will see that this latter term was the extent of duration for this stage in 150 out of the 155 cases collated. We may, also, remark that 4 days was the extreme prolongation of this phase of the disease.

“With regard to the duration of the 3d stage, it would be difficult to assign a limit, since into this fall all the cases which run on to local congestions, and these congestions are not unfrequently protracted.

“The next stage of the disease is that of the 3d stage, which is characterized by a very low temperature, the patient being insensible of malaise, &c., for a considerable time, and then gradually becoming feverish—and that 101 offered the usual symptoms characterizing the approach of Fever.

“Examined with a view to ascertain how often chill was the incipient symptom the following figures are made out. In 232 cases it was ascertained that 104 commenced with chill and 128 without.

“A brief reference to some of its most striking characters may not prove uninteresting. The symptom to which I will first refer is pain in the back. It was present in *every single* case that came into the Hospital; was one of the most distressing to the patient, and often persisted with little or no abatement till convalescence was far advanced. This may strictly be said to be the only symptom that never was wanting. It was intense in degree and extended around the loins and flanks towards the lower portion of the abdomen, occasioning great distress in the space between the umbilicus and pubis. The invariable presence of this symptom, its severity, duration, and evident correspondence with the most serious derangements of all the glandular and secretory functions of the abdominal viscera, induced me to assign it an important place among those from which the nature of the disease in question may be inferred. To this point I will again have occasion to refer.

“Headache must be placed next, as least often absent, though in rare instances it was wanting, or was but slight in degree throughout the

disease. Accompanying the headache there was generally pain in the eye-balls, which, in many cases, was severe and sensibly increased both by touch with the finger and on motion in the orbits. But it is to be noticed that there was never any increased sensitiveness of these organs by the stimulus of light.

"The degree of intensity which the headache attained in some cases was appalling. In these instances it seemed to presage the approach of congestion, or other deterioration of the brain, which occurred in some of our cases within a very few hours after the first invasion of the Fever. The cases of this kind were characterized by a train of symptoms, and by results peculiar to themselves, as I shall have occasion to point out further on.

"The next symptom in point of frequency and importance, was pain and weariness in the limbs—a sensation which sometimes extended to the whole muscular system, giving an indescribable sensation of fatigue which was often less endurable than actual pain. This symptom, when connected with pain in the head of the violent degree above alluded to, often went on to a sensation of numbness or tingling of the flesh, which always, and with good reason, excited the utmost fears of the patient. It was invariably the precursor of early dissolution.

"Next to this symptom, in frequency, came nausea and vomiting, and where these existed it generally, though by no means always, happened that later in the disease tenderness of the epigastrium came on. The cases thus characterized (as was remarked above of those in which headache predominated) formed a group with peculiar symptoms and results.

"The flushed eye was seldom absent, and the congested skin generally accompanied this symptom, but not always. The flush of the eye sometimes became an actual and permanent ecchymosis, and the congestion of the skin often extended from the face and neck downwards till it covered the whole body, rendering it so discolored, particularly in the latter stage when jaundice had come on, as to change the original hue of the skin, causing the fairest to be as dark as mahogany.

"The temperature of the skin, and the other deviations from the healthy standard, are worthy of note. It is not possible to indicate any invariable condition of this organ, as belonging to Yellow Fever. In some cases, it was scarcely changed from the natural standard of heat and moisture. In some it was hotter than usual, and in a few rare cases below the healthy standard from the very beginning of the disease. I have endeavored to prepare a tabular statement of the condition of the skin, marking its temperature and moisture in each of the three stages of the disease. An unavoidable difficulty, however, in the way of making such a table, consists in the fact that some of the cases continued to retain their peculiarities, in these respects, throughout the different stages of the Fever; and, therefore, in order to make the statement accurate, must be allowed to figure in more than one column. This will account

for the line of totals footing up higher than the whole number of patients treated:—

The skin in the	1st stage.	2d stage.	3d stage—was	
Hot and dry in	120	22	9	151
Hot and moist in	60	14	1	75
Cool and dry in	00	1	44	45
Cool and moist in	00	34	30	64
Natural temp. and dry in	1	18	45	64
Natural temp. and moist in	11	37	00	48

Total number of observations, - - - - - 447

“ I remarked above, that in a few rare cases the temperature was below the natural standard. I have not separated these from the 12 cases grouped under the two heads of natural temperature in the first column of the table, because the deficiency of heat was not so decided as to be very easily distinguished.

“ Among those who were hotter than is common, even in Fever, the thermometer went up to 108° when placed in the arm-pit, and 105° on the cheek of a few of the hottest.

“ It will be seen by reference to the table just given, that heat of skin was not always accompanied with dryness. Perspiration poured from the surface most profusely in some of those who were the hottest. Though this moist state of the surface was, as a general indication, considered favorable, it was by no means sufficient to decide the prognosis. In a later stage of the disease, however, when at the termination of the febrile paroxysm the skin was moist and warm, the indication was decidedly favorable. Even the cold and moist skin was, at this stage, more favorable than the cold and dry surface.

“ The pulse admits of no more accurate classification, as a diagnostic symptom of the Yellow Fever, than the temperature. In some cases it was not perceptibly altered from the healthy rate. In some it was strong, bounding, full and quick. In some it was frequent, small and weak; and in others it was large, soft, and soap-bubbly; occasionally it was slower than usual.

“ These remarks apply to the first stage especially. As the fever went off, the pulse invariably lost its force and volume, though it sometimes remained frequent.

“ I have constructed the following table, with view of showing the comparative frequency of some of the more important characteristics of the pulse in each of the three stages of the fever:—

The pulse was in the	1st,	2d and 3d stages, viz:		
Full in	114	53	26	193
Small	50	94	119	263
Quick	169	106	116	391
Slow	12	49	36	97
Weak	72	133	135	340
Strong	63	9	7	79

The pulse was in the	1st,	2d and	3d stages, viz:	
Frequent	130	35	53	218
Infrequent	1	95	77	173
Natural	17	00	00	17
Total number of observations,				1771

"An inspection of the last column of this table, will show the important fact, that want of energy and vigor were the most frequent characteristics of the circulation. Thus it was quick, weak, small and frequent, in the order in which these conditions are here named. It may also be remarked, that the pulse was full in 26 and strong in 7 cases in the 3d stage; this is to be accounted for by the violence of the local determinations, resulting after the Yellow Fever symptoms, proper, had passed off. Again, in the 2d stage, it was full in 53, and strong in 9; this was also the result of local determination.

"The tongue afforded the same variety as the pulse. In a few cases it seemed not to be changed from its natural appearance. Generally, however, it was altered. Sometimes it was clean, moist, swollen and tremulous. It would be difficult, I conceive, for any one not conversant with Yellow Fever, to discriminate this tongue from that of the habitual inebriate. And if the watery eye, the nervous pulse, the stupid face and stammering speech did not suffice to complete the delusion, the observer must be gifted with uncommon perspicacity.

"In another state of the tongue this organ was moist and brown; sometimes it was dry and brown; and several cases presented a very remarkable appearance, the whole surface of the tongue seeming to be overlaid with a snow-white velvety coating. Sometimes it was red at the tip and edges, and sometimes dry, red, and cracked to such a depth that blood would ooze from the creases. Occasionally it was glossy, dry and red, and sometimes so swollen that it was utterly impossible for the patient to speak, or even to put it out of his mouth.

"The following table will show the frequency with which each of the most peculiar symptoms presented by the tongue was met with. As in the table of the pulse given above, it must also be borne in mind for this, that the observations were made during the progress of the disease, and that the number of cases in which the tongue was found to be in a particular state, is not necessarily the same as the number of patients in whom it was observed. The following table will show the frequency with which each of the most peculiar symptoms presented by the tongue was met with. As in the table of the pulse given above, it must also be borne in mind for this, that the observations were made during the progress of the disease, and that the number of cases in which the tongue was found to be in a particular state, is not necessarily the same as the number of patients in whom it was observed.

The tongue in the	1st	2d and	3d stages,	
was swollen in	4	29	31	64
Dry	52	23	14	89
Bloody	00	3	31	34
Whitish	44	33	18	95
Brownish	94	53	39	186
Moist	109	109	110	328
Red	43	26	33	102

The tongue in the	1st	2d	and 3d	stages was
Velvety and white	23	2	3	28
Black	1	1	19	21
Natural	26	10	7	43
Glazed	4	2	00	6
Cracked	00	3	00	3
				999
Total number of observations,	-	-	-	999

"I would observe that though the symptom, so much dwelt on formerly, of tenderness at the epigastrium, was often totally absent throughout the entire duration of the disease, yet nausea and vomiting were seldom wanting; and it is of the matters thrown up that I would first speak.

"The fluids thrown up in the early hours of the disease were occasionally tinged with bile. But this was by no means general. Often there was no color perceptible, or the tinge was bluish. But, generally, the taste to the patient of this fluid was acid and not bitter. The substance to which this acid taste was owing, was found to be an excess of free hydro-chloric acid, and to the presence of this acid in such large quantities was, doubtless, owing the blue color which the vomited fluids so often presented, as well as the darker hue of these matters when the fearful black vomit made its appearance. To the early presence of a superabundance of this acid was, probably, owing the undigested state of the last meal eaten by the patient previous to his attack, which was generally thrown up pretty much as it had been swallowed, even many hours after it had been in the stomach."

Dr. Wragg says: "A suppression of urine was of fearful consequence. It was present temporarily in a large number of the cases during the early part of the first stage, and in 15 it was persistent, continuing three days. The urine though scanty, yet often but slightly altered, in the early stage of the fever, it offered deviations from the healthy standard in the second and third stages of great importance.

"First: It was found to eliminate from the system an immense quantity of bile in those cases in which jaundice succeeded as one of the secondary symptoms.

"Second: It was found to eliminate off free hydrochloric acid in a considerable amount. This was shown when the tests for that substance were used, and it is to be believed, from the frequent presence of this acid in the stomach and intestines during the early stage of the fever, that its existence in the urine takes place earlier and is more frequent than we are yet aware of. It has not been possible for me to institute a comparison between the number of deaths and recoveries in cases where this state of the urine was made out. It appears to have been coincident with great danger to the patient, but it will be a point of much interest to be investigated hereafter, whether its presence in the urine at this stage of the disease is not, as in the case of bile under the same circumstances, proof of the existence of an eliminative action.

“Third: There were, in a few cases red sedimentary deposits consisting of urate of ammonia, colored with purpurine.

“Fourth: Large quantities of organic debris, apparently epithelial scales, cells, &c., were found in a few cases.

“For these results of chemical analysis of the urine, I am indebted to Dr. Ford, whose careful researches though not sufficiently corroborated by repetition, will, I am disposed to believe, when completed and given to the public, be found to open some new views of this disease.

“In further reference to those symptoms which were of most importance in the disease, I will now direct the attention of the Trustees to the frequent occurrence of hemorrhage. It was noted in 86 cases. I say 86, but although I have set down only 10 cases of Epistaxis as occurring in the first stage of the fever, it is proper to state that it was reported to have existed in several others, before they came in; which, if taken into the account, would considerably increase the per centage of hemorrhage. I have thought best, however, only to tabulate those in which I was myself witness of the existence of this symptom.

“Of the 86 cases there was hemorrhage in the

	1st.	2d.	and 3d, stages from the	
Stomach	0	1	3	4
Lungs	0	1	0	1
Nose	10	18	11	39
Tongue	0	21	19	40
Urethra	0	2	0	2

Total 86

showing an average, with the figures given, of about $\frac{1}{3}$ hemorrhagic cases (in some one of the stages,) out of the whole number of patients treated. Though the numbers given make the frequency of this symptom the same from the nose and mouth, it is probable (for the reason just stated above) that the former is rather more frequent than the latter. The value to be attached to these symptoms varied accordingly as they come on earlier or later in the disease. Thus epistaxis in the beginning of the febrile paroxysm often relieved the violence of the head-ache, and ceasing with the arterial excitement, was found to have acted beneficially. While the bleeding from the mouth, coming on at the point of disappearance of the fever, and unaccompanied with any other unfavorable symptoms, seemed to perform the part of a critical evacuation, and thus to relieve the oppressed functions of the system. The appearances that indicated danger in either of these hemorrhages were the profuseness of the discharge, the offensive smell accompanying it and the setting in of that train of nervous symptoms which was so frequently found to terminate the disease.

“In constructing the above table I have deviated from the plan adopted for those of the skin, pulse and tongue, taking in this only the actual number of cases in which hemorrhage was observed, and classing them according to the part from which it was most profuse. It, therefore, is

proper that I should state that when the hemorrhagic tendency existed no part was safe from its invasion. Thus, in addition to those mentioned in the table, blood was seen to issue from the gums, intestines, ears and from cuts, bruises and blistered surfaces. In one case the small incision made for evacuating the contents of a thoroughly matured boil continued to bleed so profusely as to weaken the patient seriously, and it could only be arrested by an ingenious application of adhesive plaster coated over with collodion.

"In 149 cases there was jaundice. This symptom commenced in the first stage very frequently, the yellow eye being its first indication. In these cases the yellow suffusion generally spread over the face, neck and body. But, in a much larger proportion it did not become well marked till the febrile stage was passing off. In the greater number of cases it was of short duration, yielding readily and quickly to the advance of convalescence, but in a few it was intense in degree, general in extent and tedious in duration. In some of the protracted cases it cleared away slowly and gradually, so that the patients were still affected with it when they left the hospital. In these cases the bile seemed to be passing off entirely by the kidneys. In others it disappeared in a few hours; the skin of the patients becoming changed in hue between the morning and afternoon visits, so as to leave them scarcely recognizable. In these cases I remarked that this sudden change was synchronous with the discharge from the bowels of a black tenacious inodorous matter in greater or smaller quantity. This matter sometimes resembled bluff mud and sometimes was moulded into the form of the intestine, passing away in long ribbon-like lengths. I was not able to ascertain, positively, the composition of this matter by analysis, but believe it to be bile mixed with thick mucus. I may add that the appearance of this matter in the stools was hailed as a sure augury of convalescence.

"Reference was made above to the various local determinations observed as sequæ or results of the disease. I would now invite attention to two, which I have already adverted to while speaking of the early symptoms, and which, being observable from the beginning, gave peculiar characteristics to the train of phenomena about to follow. From these I was early enabled to prognosticate the fate of the patient with some approach to certainty. These determinations were marked by very different symptoms throughout, and the fatal termination (when this occurred) was characterized by equally different phenomena. In one set of these cases the determination was to the *stomach*, in the other to the *head*. * * * * *

"Another of these cases was remarkable for a symptom which I never saw before in any form of the disease. The patient presented the combined features of the two forms of gastric and cephalic determination. He did not retain a dose of medicine or a particle of food, and was laboring under a strange hallucination, imagining the most impossible things, when, three days before his death, priapism came on, and persisted to the last hour of his existence.

“Of the 254 cases treated in the Hospital, black vomit occurred in 74. It was found in the stomach and intestines in 3 additional cases, on post-mortem examination. In many cases, the patients entered the Hospital throwing it up; and, in a very considerable number, it occurred within a few hours of their admission. 9 recovered after throwing it up, and 63 died. The period of the disease at which it appeared, varied considerably. In some cases it occurred within 36 hours after the first invasion of the fever. In others not until the 5th day.

“Of the treatment pursued in the Hospital, I have but little to say. I depended on no specifics, but endeavored to treat the symptoms as they arose. A mild mercurial aperient at first, where the patient had not already been purged; calomel and quinine, in medium doses, neutral mixtures with paregoric to act on the skin, and allay irritability of the stomach; very moderate doses of snake-root and salts, if further aperient effect seemed indicated after the treatment just mentioned had been continued two or three days, or castor oil, in small quantity, in lieu of the snake-root and salts; tonics early administered, and made more stimulating as the stage of debility came on; blisters to the epigastrium, when tenderness or vomiting indicated their use, and to other parts when debility or local determination seemed to indicate them; nutrition as soon as it could be borne; and, in addition to these substances, stimulants of the most active, as well as the most inviting kind; and some opium in some cases. All of the mixtures and preparations usually relied on for the relief of obstinate nausea and vomiting existed; such, in a few instances, as the means I relied on for combating this fearful disease. I never used calomel with the intention of producing salivation; and when I discovered symptoms of the setting in of this condition, I at once discontinued the medicine. My experience this season tended to confirm the views I have previously entertained in opposition to the ultra calomel plan of treatment; for though salivation occurred in a certain number of cases, it was particularly remarked that no indication of benefit resulted, and several of the fatal cases were from among those who gave unmistakable evidence of mercurial impression. Quinine was very freely used; at first, in combination with calomel, for its anti-febrile qualities, and afterwards with brandy, camphor, porter, &c., for its tonic effects. Dovers' powder, carbonate of potash, James' powder and paregoric, were chiefly relied on for cutaneous action. The general warm bath, mustard pedeluvia, cold douche, ice applications, &c., were freely employed, and gave us the most valuable results. Packing in wet sheets was tried in some of the hot, dry cases, but I am not prepared to say that any very marked advantage was obtained, beyond the decided comfort the patient experienced during their employment. For the arrest of hemorrhage, creasote was the most effectual article I employed.

“I cannot say that my experience in the use of any of the articles recommended for the arrest of black vomit, resulted in establishing confidence in their use. In occasional cases good results seemed to follow the use of some of them, but these were so often found to fail in others

that I cannot say that any one of them deserves the reputation of a general or certain remedy for this symptom. Creasote, turpentine, morphine, prussic acid, mustard, musk, &c. were tried, and although all seemed to give relief in occasional instances, yet they failed far oftener than they succeeded.

"But I will not pursue this part of my subject further. I repeat that no reliance was placed on any specific, and though I tried all that I conscientiously thought I might use with safety or a hope of success, my chief dependence was on the application of such general principles as seemed to find their required conditions in the symptoms before me.

"The result was that of the 254 cases treated, 92 died and 162 recovered—giving a mortality of a little over one-third.

"In terminating this, the concluding part of my duties as physician of the Hospital, my task would be but imperfectly performed if I did not avail myself of the opportunity it affords me of publicly expressing my high appreciation of the services rendered the institution by the young gentlemen who so kindly aided me. To Drs. Ford and Chisolm, I am indebted for the most valuable assistance, the former having had charge of the male wards from the opening of the Hospital to the present hour; and the latter having taken the same position in the female wards, and occupied it as long as there were any patients there. These gentlemen voluntarily assumed these duties, and arduous as they were unflinchingly fulfilled them, thus rendering the most valuable services without hope or expectation of any remuneration. To Drs. J. Ford, Prioleau, Girardeau and Miles, who during the period of the greatest pressure had temporary charge of a ward each, I am also indebted for most important assistance.

Dr. Wragg's views of the anatomical seat of Yellow Fever as being in the sympathetic nerve, are put forth as merely conjectural by himself. They may, however, prove true. Morbid changes may be hereafter discovered in the nerves. Conjectures are endless—possibilities boundless. Direct post-mortem observations, as well as analogical reasonings, render it improbable that the morbid alterations of the sympathetic nerve, or of the spinal cord, constitute the essential anatomical and differential characters of this malady. Analogy in pathological anatomy is not favorable to the assumption that the lesion of a disease must be in a tissue which innumerable *post-mortem* examinations prove to be either wholly, or at least more rarely altered than any other, the osseous alone excepted.

"By reflex action from this important nerve, (the sympathetic)," says Dr. Wragg, "the whole spino-cerebral system is brought into disordered action, and hence the train of general symptoms familiar to all." The lesion of this nerve, here assumed, is wholly hypothetical, being

unsupported by anatomical examination, while the symptoms enumerated as proofs of this postulate, as nausea, vomiting, altered secretions, the arrest of digestion and assimilation, together with pain, are of the most indefinite and general character, seeing that they occur in numerous other maladies little resembling Yellow Fever.

The principal fact upon which Dr. Wragg relies as proving his view of the pathology of Yellow Fever, is the central pains felt by the patient. Without questioning the assertion that these pains are seated in the ganglionic centres of the sympathetic nerve—without affirming that pains just above the eyes, along the back, and in the legs are far more common and predominant than in the ganglionic regions, it may be sufficient to say that most authors deny or doubt that this nerve possesses any sensational function at all; while others suppose that whatever *sensory* endowment it may have, is not proper to itself, but derived from the spinal nerves. It might reasonably be supposed that this nerve during morbid alteration, as tumefaction, induration, softening, ulceration, and so forth, would become the seat of acute pains. But of all tissues of the body, this nerve is the least subject to such alterations; probably not a score of cases have occurred in a century. In the Yellow Fever of New Orleans, according to the writer's very extensive experiences, not a case has been found in the dead body showing a well defined alteration in this nerve, including its plexuses and ganglions; his attention had been directed to this nerve, particularly to the solar plexus, and semi-lunar ganglion, by a publication which had proclaimed these as the seats of morbid change in Yellow Fever.

In that valuable work on Pathological Anatomy, just issued from the press, Drs. Jones and Sieverking, say: "A few cases are on record in which the ganglia of the sympathetic system were found more or less deformed and altered in structure. The actual demonstration is yet reserved for future inquirers. Bichat repeatedly examined the nerves of the viscera in different diseases without discovering any lesions. With the exception of a single case, he has found the semi-lunar ganglion intact in cancers of the stomach. In a case of periodic mania, he found this ganglion of the size of a small nut, with a cartilaginous centre."

Dr. Wragg's paper is one of rare merit, presenting a luminous example of the numerical method skilfully and successfully applied in the elimination of truth.

ART. VIII.—Review of European Legislation for the Control of Prostitution.

From the New Orleans *Daily Delta*, it appears that the Board of Aldermen of the City of New Orleans, at their meeting on the 5th of December, 1854, took into consideration the subject of Public Prostitution in this City, as the following report will show:

“*Police Committee.*—A petition from the Second District requesting that the nuisance of public women be interdicted, and that they be subject to the same rules and regulations as are in force in Paris, which was adopted.

“On motion, Messrs. Gillmore, Durell and Adams were appointed to serve on Police Committee.”

This initiative taken in sanitary police by New Orleans is a legislative novelty in the United States, and is a sufficient justification—if a justification be asked—for the publication of this article—the chief purpose of which is to direct serious attention to a mass of valuable information derived from a foreign journal as will more fully appear in the sequel—information which the American Medical Press has passed over silently, which is of great and increasing importance to the well-being of society, and, which, the *New Orleans Medical and Surgical Journal* ventures to re-produce, not without the hazards of censure from the sentimental, fastidious, æsthetical philosophers, who may disapprove in print, what they tolerate in conduct without animadversion.

All right thinking men, and women too, who entertain a hope of effecting reforms in social and sanitary police, will seek for and gladly accept the necessary information whereby they may be enabled the better to guide themselves in their benevolent purpose, whether it be the reformation, control, or punishment of a class whose aberration from the straight line of virtue, must be deplored, pitied, condemned, but not excluded from human sympathy.

———“Consider this—
That in the course of justice, none of us
Should see salvation.”

In this Republic, wherein majorities rule, it is of the utmost importance that the public mind should be enlightened in matters pertaining to social, sanitary, civil and legislative polity; that a firm conviction of the justice, utility, necessity and practicability of laws, should precede their enactment—otherwise, the statute-book becomes inoperative or mischievous.

It is believed that in Insular Europe, and in America, wherever the

English language is spoken, no legislative measures, having in view the adoption of regulations similar to those enforced in Paris, and many other cities of Continental Europe, for the control of prostitution, have been effectuated.

Optimism, moral cowardice, affected or real modesty, or an inherent repugnance in the mental constitution of all nations speaking this language, have either repelled legislative action in this behalf, or tacitly submitted without an attempt to regulate, restrain or ameliorate a great moral, physical and health-destroying evil. The optimist consoles himself with the unprogressive, if not fanatical belief, that the moral, social, physical and sanitary order of the universe, cannot, should not be disturbed, seeing that it must eventuate in the good of the whole. A philosopher of this school, after a thorough syphilization, with or without the loss of a nose, might be sceptical whether such an individual evil, which he may have contracted or inherited, could in any degree contribute to the well-being of society, or the harmony of the spheres.

If doubt, timidity, despair, silence, or the avoidance of the contemplation of an existing evil, could lessen it—or if such a mode of treating it did not virtually aid in its extension, then “masterly inactivity” would be, if not the proper remedy, yet an allowable one.

Neither the past nor the present of humanity afford the most hopeful any ground to believe that prostitution can be wholly prevented. It exists. It is virtually an institution. It is not the less an actuality because it is ignored in the organic law and in the statute book and condemned by the decalogue. Neither virtuous abstractions, nor the deductions of moral science, nor the hopes of paradise, have proved paramount in swaying the passions of the great mass of mankind. Theft, murder, treason, prostitution and other crimes and nuisances detrimental to the well-being of society, how repulsive soever they may be, should not be left to their native tendencies without control, restraint, and punishment. Preventives may be impossible, while correctives may be highly advantageous. Houseburning is not preventable, but this is no valid reason why arson should not be punished, nor conflagrations be extinguished. Fanatical expectations of realizing the end without using the means—the substitution of declamation for legislation and sentimentalism for action, have in all ages proved the *vis inertia* to the march of human improvement.

Prostitution, public and clandestine, is a crying evil and has been boldly met, controlled, tolerated, but not persecuted by the philantthro-

pist, legislator and the moral philosopher of the old world, and cannot be much longer ignored in the new. Whether the indirect toleration accorded to prostitution on the continent of Europe, be incompatible with the purity and modesty of the English language, remains to be seen. Already, however, the ablest and most conservative Quarterly in England—The British and Foreign Medico-Chirurgical Review—has unhesitatingly proclaimed its opinions in favor of the Legislative Control of Prostitution.

Dr. T. S. Holland's papers, in that journal during the year 1854, have scarcely been alluded to in America; yet a studious silence cannot impair his massive logic, nor derogate from his claims as a moral not less than a medical philosopher. Under the sheltering example of this distinguished periodical, and in view of the initiatory action of the Board of Aldermen of New Orleans, not to name the exigencies of the subject itself, it is proposed to re-produce condensed extracts from the above named papers—the papers themselves being for the most part extracts from and concentrated analyses of eight works published recently in continental Europe—some of these works are unrivalled monuments of learning in hygiene, and medico-legal jurisprudence. As, for example, that of the apostle of female sinners, Parent Duchatelet.

Dr. Holland, the reviewer of the works above alluded to, has, moreover, obtained unpublished documents from the Continent, illustrative of the Control of Prostitution, and has given a greater amount of information than has ever before been compressed into the same number of pages. Here, as in legal proceedings, the best evidence in the case should be produced, and not that of inferior authority. Hence, the editor of the *New Orleans Medical Journal*, who has not seen all of the works referred to by Dr. Holland, offers the researches of the latter, which no fastidious person ought to read, without first having looked his door.

Dr. Holland's researches, extending to the existing condition of prostitution in Algiers, France, Austria, Hanover, Bavaria, Belgium, Portugal, Italy, Spain, Denmark, Great Britain and Ireland, cannot fail to instruct and guide physicians and legislators, whose first duty it is to obtain reliable, accurate and practical information, without which the best intentions may prove altogether abortive, utopian, or injurious. The golden rule, in moral as well as in medical treatment, is to do no harm—a postulate which requires more knowledge than is generally supposed, concerning prostitution.

Perhaps in nothing has the genius of man transcended that of woman,

more than in the natural, and more than all in the ideal—indeed, it might be said, in the morbidly exaggerated history of the passion of love. His creative pen has given to Fiction a potency greater than Truth—to Imagination a predominance over the exigencies of Nature, and has made the Ideal virtually omnipotent, having invented innumerable perils and snares for female virtue, not the least of which is a thin-veiled, half-concealed sensualism, glimpses of which is seen, ever and anon, in almost every modern romance and fashionable novel, by which mental debauch, secret pollution, or open, shameless prostitution, are stimulated into unnatural activity, by which the smouldering passions, and even the almost extinguished cinders of age are re-kindled, and cherished as *souvenirs* of the sensual past.

Ignorance, imbecility, neglected education, bad examples, vanity, a passion for ornamental costumes, jewels, extreme poverty, domestic troubles, ill-treatment of friends, abandonment by lovers and husbands in the army, navy, commercial marine and elsewhere, and even the pious purpose of saving aged parents, and infant brothers, sisters and children from starvation, have led to prostitution, especially in Europe, as authentic records show—examples of which may be seen in the works of Parent Duchatelet.

It has been observed that prostitutes have shown indirectly, yet unequivocally an aversion to their own course of life, by endeavoring as far as possible, with few exceptions, to place their children beyond its baneful influence.

Society inflicts pains, penalties, and exclusions un-relentingly upon even repentant and reformed prostitutes, little calculated to encourage, sustain, and confirm them in a virtuous course of conduct—little in accordance with the dictates of reason, and altogether contrary to the genius of Christianity—while it is but too ready to forgive, forget, and even look approvingly upon moral delinquencies equally atrocious in the male sex. The unfavorable antecedents, treacheries, deceptions, temptations, and extenuating circumstances which in many instances have contributed to draw women from the path of rectitude, might justify the extension of charity, pity, and forgiveness towards transgressors of that erring but most miserable class.

This subject will be resumed after having given the extracts above alluded to.

“ *The Control of Prostitution in Berlin.*—Under the chief of police, and by the advice of Dr. Behrend, there has been formed a commission for moral police, consisting of—1st, The chief of police as president; 2d,

The medical counsellor of the central police board; 3d, The chief physician for sanitary police; 4th, The chief physician for moral and humane police, under whose immediate direction comes whatever relates to Prostitution; 5th, Ten physicians, to each of whom a part of the city is assigned, whose duties are to examine the women living in the brothels of their districts, and to attend in rotation every day for two hours at the office of the commission, in order to examine the women who present themselves; they are also called by the police to all cases of severe accidents, violent deaths, &c., and attend the police gratis; 6th, Four surgeons assist the physicians.

"This commission has divided Prostitution in Berlin into two kinds, the tolerated or public, the non-tolerated or secret; and to the first of these we will now direct attention:—

"*Tolerated or Public Prostitution.*—Whenever any one desires to open a house for the reception of Prostitutes, application is made at the office of the commission for a copy of the 'Request,' which is to be filled up, signed, and returned to the office; and as this document contains some important regulations, we give a complete translation of it:*

"'I request from Commission for Moral Police, permission to let in No.—, in—street, furnished rooms to women who live by Prostitution. If this request be granted, I hereby bind myself to fulfil the following conditions:—

"'1. I shall consider this permission as a concession which the commission can at any moment withdraw or modify, without my having the right to inquire their reasons for so doing.

"'2. I will not admit any woman into this house without having received, for her in particular, the official form of permission from the commission; nor will I allow any other persons, excepting the woman for whom I have received such permission, to live therein; and if I act otherwise, I shall pay to the commission 5*l.*

"'3. I promise not to have any other women servants, and not to employ as a servant any one who has not attained forty years of age, under a fine of 7*l.* 10*s.*

"'4. I promise not to allow any woman or any man under twenty years of age to enter this house, under a fine of 7*l.* 10*s.*

"'5. In the aforesaid house there shall be no noise or tumult, whereby the neighborhood may be inconvenienced; and if I have given rise to such noise, or if it appears that, in the event of its being caused by others, I did not do everything in my power to prevent the same, I shall pay a fine of from 15*s.* to 15*l.*, besides remunerating in full for all damages that may have been made during the tumult.

"'6. I promise not to keep any spirituous drinks in this house, nor to allow any to be brought into it, nor to suffer any dancing or music therein, under a fine of from 15*s.* to 7*l.* 10*s.*

"'7. I promise that the street door shall be kept shut during the

* These and following regulations are not admitted into general circulation, and we are indebted to the kindness of Dr. Behrend for copies of them.

day and night, and if it be at any time found open I will pay a fine of 15s. to 30s.

“8. I promise that the windows shall be left and retained in the condition which is ordered and approved of by the commission; and I will pay a fine of from 15s. to 30s. for every arbitrary alteration or neglect of these arrangements.

“9. I promise not to make any alteration in the interior or exterior of this house, without previously acquainting the commission and obtaining their permission to make it, under a fine of 15s. to 7l. 10s.

“10. I promise that none of the women who live in this house shall appear at the street door, nor in any public garden or other place of public amusement, nor in any dancing rooms nor public walk; and if one or more of them are seen in any of these places, whether they be there with or without my knowledge, I will pay a fine of 15s. to 30s.

“11. I promise that none of these women shall go on a journey out of the city, or on any party of pleasure, without having previously obtained the permission of the commission, and its being made as they direct; under a fine of 15s. to 7l. 10s.

“12. I engage, out of the agreement that is made between me and these women, to provide them with lodging, board, attendance and clothing, all of which shall be subject to the inspection of the commission, whom I will inform of all changes made in these respects; under a fine of 15s. to 60s.

“13. I promise to have a list of prices printed, a copy of which I will give the commission; and in case of my demanding or receiving more than is therein stated, I will pay a fine of 15s. to 60s.

“14. I will not allow any of these women to incur debt for more than three pounds, under a fine of 30s. to 60s.

“15. I promise not to use any bodily punishment with these women, nor to confine nor use any violence towards them, under a fine of from 15s. to 7l. 10s.

“16. I promise not to allow any one to enter this house from one o'clock at midnight until the morning, under a fine of 15s. to 60s.

“17. I promise that the women shall live in all respects with, and have every right contained in the 'Book of Regulations;'* that they shall preserve the greatest personal cleanliness, and if any of them become sick, I will immediately inform the attending physician, as well as the commission; I will especially direct my attention to the discovery of syphilitic disease and of scabies in these women; and, should either come to my knowledge, I will immediately inform the attending physician and commission; further, I will not in such a case allow any one to visit the woman until she be examined by the physician or removed to a hospital. For any transgression of these points I will pay a fine of from 15s. to 15l., in addition to which, I will defray the expenses of any one who may thereby become diseased.

* See p. 90 for these regulations.

“18. I shall inform the commission if any of these women become pregnant; and if I omit to do so, I will pay a fine of from 15*l.* to 30*l.*

“I promise that the examination of the women and of the house can be made at any hour of the day or night by the commission, the attending physician, or police officers; that I will in every way facilitate the making of these examinations, and provide for the physician the prescribed instruments, vessels, &c. For every omission, or even neglect in these respects, I will pay a fine of 15*s.* to 60*s.*

“20. I promise to obtain from each of the women living in this house, with the exception of servants, from six to nine shillings per month,* and pay the amount half-yearly to the chief fund of the police; should any woman refuse or neglect to pay this monthly subscription, I engage to pay the same, considering her as my debtor.

“21. This is to the effect, that the monthly subscription entitles the woman, when affected with syphilis, to free treatment and support in hospital, and that the owner of the house has no claim on this money.

“22. I promise that, in case any of these women are ill of any other than venereal disease, if they become pregnant, &c., I will provide them with medical attendance and support, or the commission can deduct the expenses from the security money.

“23. On the granting of this request I will pay once and forever, to the chief fund of the police, the sum of fifteen pounds, and will not under any circumstances demand that this money be returned to me; with this one exception that, within a half-year from the granting of this request, I be obliged from unforeseen and unavoidable circumstances, to give up the permission.

“24. In order to secure the payment of the fines, I promise, within three days from the granting of this request, to deposit in the chief fund of the police the sum of forty-five pounds, as security, which is to be returned on the conditions contained in clause 23, or in the event of my giving up this house and acting towards the women as directed, of which I will give the commission at least three weeks' notice. For this I shall not seek to have this 45*l.* returned to me, if I retain one or more of these women, and for them I shall submit to the regulations of the commission.

“25. All the above-mentioned fines, &c. are completely independent of the legal punishments for offences and crimes; I am amenable to the common laws against secret prostitution, against public prostitution, imposition, secret delivery, the production of abortion, &c.; and should I, for any offence or crime, suffer legal punishment, I shall consider it as just, if the commission withdraw their permission. Further, if I thrice wilfully, break the regulations of this contract, or act in direct opposition to the orders of the commission, they have the right not only to withdraw this permission, but I hereby forfeit all claim to the security money, which is, in that case, to be used for the purposes of inspection and cure.

* This sum is not stated in the original, but it varies between the amounts given above.

“ 26. I promise to submit to the opinion of the commission on all points connected with this contract, and in case that I consider myself aggrieved by that decision, I submit to the jurisdiction of the chief of police, whose judgment shall be final; if after that I have recourse to the civil law, I thereby lose the right of retaining the permission.

“ 27. The commission has the right of receiving all fines incurred under the regulations of this contract, without having recourse to the usual forms of law; and I engage to raise the security money to its original amount within three days after it has been reduced by the deduction from it of the fines.

“ Finally, in the event of my failing to fulfill the last condition, I hereby forfeit all claim to the forty-five pounds security.’

“ It may not be out of place to offer here a few remarks on this form of the request. It will be observed, that these conditions are offered to the commission, for though the form has been drawn up under their direction, still it is a proposition offered to and not emanating from them, which, if acceded to, they can at any moment withdraw, and thereby close the house; while the 4th clause prevents boys or very young girls from being admitted. The 6th clause is not put in force, as beer is sold in all these houses, and there is but little difficulty in obtaining brandy and other spirits; still it is a useful regulation, as it enables the commission to punish the owner of the house if any one is proved to have become drunk on his premises; the restriction regarding dancing and music is never enforced. The regulation respecting the closing of the street door, clause 7th, is so strictly acted up to, that it is at all times necessary to ring to obtain admission, and on entering, it is immediately closed, as it matters not by whom it has been left open or even unlatched, if found so the owner is fined, and it is one of the duties of the police to enforce this regulation. With respect to the windows, clause 8th, the lower sash is firmly screwed into the frame so that it cannot be opened, while a wire-gauze screen extends half way up the window, and renders it impossible for those within to be seen from the outside; the upper sash can be opened to ventilate the room, and the shutters are closed when there are lights within. The value of these and following regulations must be apparent; we therefore pass on to the 14th clause, in which the owner of the house promises not to allow any of these women to incur debts above three pounds, but, unfortunately, the small fine of 30s. to 60s. completely fails to ensure the fulfilment of this promise.

“ It may appear strange, that we should attach the greatest importance to the most stringent enforcement of this promise, for the public, who generally consider, or at least only see the one great fault in these women, often conclude, that, as they are understood to have lost their character, they must as a consequence have no character, except it be a bad one. Some fear, and others wish not, to look deeper than their external demeanor, lest perchance they may discover that these women are not altogether prostitute, while the majority, even of our own profession, find

it much easier to allow a part of their fellow-creatures to live and sicken and die under the unrelenting scowl of society, than to study a question which society appears to have proscribed. For our part, we fully anticipate, and are prepared to meet, the opposition that any attempt to establish a system of control and protection of prostitutes will excite; but if we can prove by statistics and probabilities, that it has been, and will be attended with benefit, opposition in this, as in every other discussion that has truth for its basis, will only tend to excite argument, which will, we confidently believe, establish the necessity of the system, and inevitably ensure it support.

“While visiting these houses, with our kind and truly humane friend, Dr. Behrend, we have asked these women why they do not seek some honorable occupation, as they can at any moment leave the house, no matter how heavily they may be in debt, [see regulation No. 1,] and the invariable answer was, “I must first pay my debts.” But the debts increase, and prostitutes do not understand Insolvent Debtors’ Courts, and therefore languish on from year to year, until a hope of recovery is lost. Hence, any system that has for one of its objects the reformation of these women, should enact, that it be a high, very high offence to give them credit; for as long as they can incur debt, so long will they remain prostitutes, even under the most favorable system of control. But to return: clause the 18th obliges the owner of the house, and the woman is equally bound by regulation No. 14, to inform the commission and the attending physician when pregnancy occurs. In such cases, the speculum is not used after the third month, and at the expiration of the seventh she is obliged to leave the house; but as she will not be received into hospital until the termination of the eighth month, how is she to live in the interim? For this, as the State has no provision, *her fellow-prostitutes come to her aid*: a voluntary subscription is made in every brothel in Berlin, and the money thus collected is given to her; on it she lives until her admission to hospital; it helps or supports her while nursing her child, or pays for its being nursed, while she returns to her wretched life to pay off old debts, and help others in her turn. With this redeeming feature in her fallen nature, it cannot justly be said that the trail of the serpent has utterly effaced all traces of the beauty and nobility of the woman’s heart.

“Clause No. 19 has the effect of obliging the owner to preserve complete order, and the women are most scrupulously cleanly. It is provided in the 23d clause, that the applicant for the permission shall pay fifteen pounds to the chief fund of the police, and he is informed that the entire of this money is to be given to the institution for repentant females, while the interest of the forty-five pounds, lodged as security, is also given to the institution. Thus, the first act of one about to open a brothel is to denounce his mode of life by helping to support an asylum for repentants. It is provided in clause 25, that if the owner of the house be found guilty of any offence against the common law, he shall be doubly punished, first by the civil courts, and secondly by the permission being withdrawn, or

by losing, in some cases the security. Lastly, in order to prevent any disagreements that may arise between the owner of the house and the commission, or between the former and the visitors or the women, from furnishing interesting (?) reports for the daily press, the 26th clause endows the commission with the power of judging all complaints, and a final appeal can be made to the chief of police.

“If the request is granted, the applicant gets a copy of a book printed by order of the commission, containing regulations for his conduct; but as many of these are similar to those already given in the form of request, we shall notice only the additional rules.

“Regulations for the Person who is allowed to provide Lodging and Board for Prostitutes.

“He shall not allow billiards, cards, or any other game to be played in his house.

“A printed list of prices must be hung in each room, and the commission must be furnished with a copy. (We give the following as a specimen of these tariffs:—Entrance 6d., for which a cup of coffee is given; coffee, per cup, 4d.; use of a room for fifteen minutes, 3s.; for thirty minutes, 5s.; for one hour, 9s.; and these prices include the company of one of the women for the time stated.) If there is a higher demand made from a visitor than that stated in the tariff, on his reporting it to the commission, the owner of the house is fined in accordance to clause 13 of his contract.

“The agreement between the owner and each of the women must be written, one copy to be kept by the owner of the house, a second to be given to the woman, and a third left with the commission. (These agreements are generally to the effect that the owner gets two-thirds of what money she gets in conformity with the tariff, and for this he provides her with lodging, board, clothing, and attendance.)

“That the owner of the house must provide an examination table of a certain form, two or three specula, several pounds of chloride of lime, and for each woman, besides bed and body linen, he must furnish a washing-stand, &c., a vaginal syringe, and two or three sponges. (Each woman must have a separate bedroom, so that there cannot be more women than there are sleeping apartments in these houses; this most sanitary arrangement, though not in the ‘Book of Regulations,’ is invariably enforced by the commission.)

“If it be necessary that a woman take outdoor exercise, or if she goes out on business, she must be modestly dressed, and the owner of the house shall provide a trustworthy man to accompany her and see that she does not stand in the street or remain out longer than is necessary for her health or business.

“If a woman wishes and determines to leave her unlawful course of life, the owner dare not make any attempt to dissuade her from so doing, nor dare he prevent her, as soon as he is acquainted with her desire, not even if she be his debtor. (The commission further enforce, that if she

be entirely unprovided with proper clothes, he must furnish her with a suit of such as is worn by servants, and send her, at his expense, to her native city, no matter how remote it may be.)

“ ‘ If a woman wishes to leave this house, in order to continue her debauched life elsewhere, she must first have fulfilled the conditions in the written agreement existing between her and the owner, or she may leave in accordance with a new agreement voluntarily made by him; but of this the commission must get notice.

“ ‘ It is expected that the owner of the house will assist the commission in their efforts to bring these women back to an honorable course of life, that he will endeavor to prevent secret prostitution, and to trace syphilis to its origin. (It must be evident that this regulation is only useful inasmuch as it expresses the objects for which the commission has been constituted.)

“ ‘ The request having then been granted, and the owner provided with a copy of these regulations, the house is finally inspected, and if the arrangements are approved of, permission is given to open it.

“ ‘ A woman wishing to enter such a tolerated brothel, must apply to the commission, with proof of her having attained the age of twenty, and being free from debt, as it is the endeavor of the commission to ensure that no one is obliged to have recourse to prostitution to free herself from debt. The regulations are read to her, she is informed that if she enters a brothel, it becomes for her a kind of prison, in which she must submit to the regulations she has just heard; and such arguments as the nature of the particular case suggests are used, to induce her to change her resolution. In the event of her adhering to her wish, she is examined, and if found healthy, her name, age, residence, birth-place and personal appearance, are noted, so that she can be readily identified; she then obtains the written permission to enter such a brothel, and is given a book containing the following regulations, in which her name is written in full:—

“ ‘ *Rules for the women who have not been induced by the most urgent persuasions to leave their debauched course of life, and are, therefore, placed under legal inspection.*

“ ‘ 1st. The person can, at any time, leave the house, in which, by permission of the commission, she obtains board and lodging, as soon as she has the earnest intention of leading a lawful and honorable course of life: nothing can oblige her to remain, neither obligations nor debts to the owner, or to any other person; and in such a case, she shall have the necessary protection and assistance from the commission, or from the civil police of the district in which she lives.

“ ‘ 2d. If a woman wishes to leave a house in order to continue her debauched life in another, she can only do so after having fulfilled the terms of the written agreement between her and the owner, or by his permission; the commission reserve to themselves the right of making exceptions to this rule, in the event of the woman desiring to leave on

account of her having been bodily ill-used by the owner, or for other important and well-founded reasons; but of all such changes, the commission must get notice.

“3d. If a woman obtain the aid of the commission, and after leaving a brothel, under pretence of following an honorable course of life, devote herself to secret prostitution, she shall be confined for three months in the House of Correction; and at the expiration of that period, she shall be detained there until she desires and obtains an honest employment, be given into the charge of her family, or sent to her native city.

“4th. The police of each district shall, from time to time, inquire if the women have any cause of complaint, which they or the attending physician will receive and communicate to the commission.

“5th. The woman is hereby seriously cautioned against entering into much debt, as she is responsible for all liabilities, and is thereby brought into a state of dependence, which greatly increases the difficulties of her reformation.

“6th. The owner of the house must be obeyed in all that refers to the carrying out the regulations of the commission relative to the order and decorum of his house; the women must not appear at the street door or at the windows, nor attempt to attract the passers-by with words, gestures, &c.; and, should any woman act contrary to these regulations, she shall, for the first offence, be punished with imprisonment for three days on bread and water, and for each repetition of the offence, with eight or more days.

“7th. That they shall not appear in the streets, or in any place of public amusement, under a penalty of three days' imprisonment.

“8th. That any necessary out-of-door exercise must be made in conformity with regulation 14th, p. 91; and she incurs imprisonment by any breach of these directions.

“9th. She shall not practice any deception or extortion against those who visit her, for which, as well as for theft, procuration, reception of stolen goods, fraud, &c., she shall be punished with more than usual severity.

“10th. That she shall preserve the greatest personal cleanliness; that during each menstrual period she shall not allow any one to visit her; that if she be in any way ill, has any swelling, ulcer, discharge, &c., she shall immediately inform the owner of the house and the attending physician. In the event of her acting contrary to this regulation, and thereby assisting in the extension of disease, she shall be sent to the Work-house for from six to twelve months.

“11th & 12th. In these regulations, she is ordered to pay particular attention to the detection of gonorrhœa, syphilis and scabies, and is referred to the last pages of the book, where there are instructions for the detection of these diseases in both sexes, and also for the discovery of pregnancy.

“13th. This rule is a repetition of clauses 20 and 21, p. 88.

“ ‘14th. If she suspects or knows that she is pregnant, and does not inform the owner of the house and the attending physician, she shall be most severely punished, according to the law against the concealment of pregnancy.

“ ‘15th. After each menstrual period, she shall take a bath, or wash the entire body; and after every coitus she shall wash, and inject a solution of chloride of lime; the syringes, sponges, solution, &c., will be provided by the owner of the house.

“ ‘16th. She must submit to, and on no account be absent from the ordinary examinations of the visiting physician, nor from any extraordinary examinations, which can be made as frequently as, and at whatever time, the commission may direct.

“ ‘17th & 18th rules are to the effect, that the commission will act as arbitrators between the women and the owner; but that, so long as they remain in this house, they are under the control of the commission, whose regulations if they violate or refuse to obey, they shall be placed in confinement.

“ Having entered a tolerated brothel, the prostitute is visited twice every week by the attending physician, on which occasions the examinations are thus conducted. A woman enters the room used for the examination, gives the book of the regulations, in which her name is written, to the physician, who examines her hands for scabies, then the mouth and pharynx; lastly, the vagina—invariably, excepting during the periods of pregnancy, with the speculum; her book remains with the physician.—Another enters, and thus the examination is continued; finally, the physician counts the books, to see if he has examined all the women in the house, and writes his report. This is forwarded the same day, to the chief physician; and if any one has been reported diseased, she is immediately sent to hospital, which she cannot leave until a notice of her being perfectly cured has been sent to the commission.

“ Such are the regulations for tolerated prostitution, and the commission has most humanely enacted, that no debt or obligation can retain the women in the house as soon as they intend to enter on a proper course of life; but they do not leave, except in rare cases, and often return to their old habits; to prevent which, the third of these last regulations has been instituted. We are, however, disposed to believe that punishment and reformation stand much less frequently in the relation of cause to effect than is generally supposed. Punishment may prevent a repetition of the act, from fear of the consequences; but in the majority of cases, the ingenuity is taxed to discover how the crime can be repeated without detection; and if this supposition be applied to cases such as the present, it will become highly probable that the most lenient measures would be most effectual in reforming the immoral tendencies of these women. True it is, that in all efforts at reform, we are often obliged to argue and work as it were backwards, directing our energies to prevent the effects, in the hope of being thus indirectly enabled to remove, or at least oppose obstacles to the action of the cause; but it would be probably more effec-

tual if we directed more attention than has been hitherto done to discover and remove the causes of crime. The caution in Rule 5, p. 92, against their entering into too much debt, on account of its placing obstacles in the way of their reformation, can have no effect on those who do not desire such a change. It matters not how anxious a woman may be, either to reform her life or avoid the payments of her debts, she will not venture to leave as long as she is in debt to the owner, as she is well aware, that although she can leave to-day, he can prosecute her to-morrow for debts incurred in his house. How can she meet his demand? Her most direct, easiest, and habitual way of obtaining money, is now interdicted, under penalty of a long imprisonment, as laid down in Rule 3, ²p. 91. The commission cannot pay her debts, or they would be holding out a premium for crime; thus she is, in point of fact, obliged to remain until she has paid her debts. There is but one means for avoiding this evil, namely, by declaring the women non-responsible for debts incurred while living in such a house; forbidding the owner, under a heavy penalty, from giving them any credit; and enforcing that all clothes worn by the women shall be provided for them gratuitously by the owner of the house. Under this or some other regulation, by which it would be rendered impossible for prostitutes to obtain credit, we might reasonably hope for the reformation of some, and the moral improvement of many.

“We shall next consider—

“*Non-tolerated or Clandestine Prostitution.*”

“The class of women who come under this division are those who live separately in their own apartments, and correspond to ‘*Les Filles Isolées*’ of Paris. After having reported and brought under the control of the commission the most notorious of these women, the civil police proceed in their search, and if a woman is observed to be frequently in the street at night, dressed in a manner very disproportionate to her station in life, should information or other circumstances cause her to be suspected, an inquiry is made into her occupation, means of subsistence, those who visit her, with whom she associates, her general character, &c.; a report is then laid before the commission, and if they consider there is good reason to suspect her of living by secret prostitution, she is brought to the office of the commission and examined; if found in health, her name, age, residence, &c., are entered in the Red Book, or book of the suspected, and she is cautioned that the police know her mode of life, and, if she does not alter, she will be placed under the commission. If she comes a second time under the notice of the police, for drunkenness, &c., or if, on the first examination, she is found diseased, after being sent to hospital she is enrolled on the Black Book, or Book of Control, which contains her personal description and history, with the reasons for her being inscribed. She must now attend to be examined, once a week, at the office of the commission; and if she fails to be present at the appointed time, she incurs imprisonment from one day to four weeks, ac-

ording to the frequency of the offence. These examinations, made by the physician that day on duty, in the presence of the chief physician, who has to countersign all orders to hospital, &c., are conducted in the following manner. On entering the waiting-room, each woman is given, by the police officer in attendance, a small book, in which her name, residence, birth-place, age, religion, size, height, complexion, color of her hair, eyes, &c., are inserted; with this she enters the inner room, gives the book to the physician, who proceeds with the examination as stated at p. 92; he then marks her book, each leaf of which is similar to the following: Date. Result. H. for healthy. A. B., signature of physician.

“On leaving, she returns the book to the policeman, who, seeing it marked “healthy,” allows her to depart. Meanwhile, another woman has been examined, found diseased, and her book is thus filled up:

Date. Result. S. for syphilis. G. for gonorrhœa, or Sc. for scabbies.

“On giving the book thus signed to the policeman, he informs her that she must go to hospital; and at the termination of the examinations, all those who have been reported diseased are conducted there under the care of one of the officers of the commission. As the women return their books to the policeman, he marks them as having attended in the registry; and should one be found to be absent, she is, on the same day, arrested and placed in confinement. If one of these women be ill, she must send information to that effect to the office; she is visited the same day by the physician of that district, and sent to hospital: but if she has feigned illness, she is forthwith arrested; and this has been so strictly acted upon, that the attendance is almost invariably regular.

“Such are the regulations for public and secret prostitution in the city of Berlin. We have given a detailed, and, we believe, complete account of this system, as, after a careful study of the subject, and having seen the systems of Austria, Belgium, France, &c., in operation, we found it to be the most efficient and humane.

“It may not be uninteresting here to mention the relative number of prostitutes in Berlin.

In 1849 the population of Berlin was - - - - -	423,902
The male population over 16 years of age was - -	134,772
The number of military (not included in the above)	19,030
Total males	153,802
The number of tolerated brothels is now - - - -	20
The number of prostitutes in these brothels - - -	225
The number of ‘non-tolerated’ prostitutes under the superintendence of the police - - - - -	540

“If we say, then, that there are 765 prostitutes actually known to the police, this gives one prostitute to every 201 males (including the military). As, however, the total number of clandestine prostitutes is not yet

known to the police, the relative proportion of prostitutes is somewhat more than this.

“It is at all times exceedingly difficult to demonstrate the effects of a control such as that we are discussing, as it has been hitherto found impossible to determine accurately the amount of syphilis that exists among the mixed and migratory inhabitants of capitals. We have fortunately been favored with a statistic of the cases of syphilis among the troops in Berlin, which fulfils all that is necessary to secure the correctness of conclusions deduced from it. It must be observed, that the number of soldiers is fixed, and that the same corps are permanently on duty in Berlin; further, these statistics have not been collected for the purpose of proving or disproving the efficacy of the system of control, as the registry of the Military Hospital has afforded the required data.

“*Report.*—To the Royal Commission for Moral Police in Berlin:

“In answer to the letter of the Royal Commission, dated April 30th, 1853, I have to report that, among other things, we have observed, during the last few years, a remarkable diminution of syphilis among the garrison. While in the year 1849 there were 1423 cases of syphilis among the troops—

In 1850 there occurred 670 cases.

1851 “ 526 “

1852 “ 332 “

In the first quarter of 1853 “ 59 “

Also, in respect to intensity, the disease forms a most favorable contrast with that of former years. In my opinion, the above numeral proportion furnishes the most sufficient proof of the utility of the existing sanitary regulations.

“DR. STUMPF,

Chief Physician to the Garde de Corps.

“BERLIN, *May 3d*, 1853.”

“There has been a diminution of 753 cases, or more than half, on the first year, when the examination of the tolerated prostitutes, or those living in brothels, was made regularly; a diminution of 144 on the second year, of 194 on the third, or that of 1852, in the February of which year began the regulations for non-tolerated prostitutes; and calculating of 1853 according to its first quarter, we get a diminution of 96 cases for that year.

“Between the 1423 cases that occurred in the year 1849, and the 332 that presented themselves in 1852, we have the enormous difference of 1091 cases; and we are justified in calculating, that had there been no control for prostitution, the frequency of syphilization would not have diminished; hence, in 1850, 1851 and 1852, there would have been 4269 cases, whereas, under the control, there have occurred only 1528: therefore, there has thereby been saved from infection no fewer than 2741 soldiers, during a period of three years; yet a complete control has only existed for eleven months.

“Let us turn, for a moment, to the disease in women, and we shall find that during February, 1852—this being the first month of the inspection over the non-tolerated—about 30 women were examined every week, and the cases of syphilis amounted to 29 per cent. per month; while in April, 1853, about 540 women were examined weekly, and the amount of syphilis had fallen to 5 per cent. per month.

“With facts before us such as these, the beneficial effects and direct humane tendency of a control over, and examination of prostitutes, is no longer theoretical or problematical; it has been found to protect the women from the ill-treatment that they have almost invariably more or less suffer from the owners of brothels in Britain, while it facilitates their reformation, and at the same time protects the public health.

“Having so far described the means whereby prostitution is controlled in Berlin, we are naturally led to inquire into the origin of the system. Dr. Behrend’s work, written in 1850, in reply to certain questions proposed by the Minister Von Ladenberg, appeared originally in ‘*Henke’s Zeitschrift für die Staatsarzneikunde,*’ has been reprinted under the above title, and furnishes us with the desired information.

“The public documents which refer to this subject being few and imperfect, the historical details have been compiled from Fidicin’s ‘*Geschichte der Stadt Berlin.*’ Interesting as such details are to those living in the Prussian monarchy, we shall only remark that the early part of the fifteenth century was the date of opening the first tolerated tax-paying brothel, and that in 1846 the women were obliged to wear a mantle of a particular form. The oldest regulations, dated 1700, remained in force until 1792, and consisted of fourteen clauses, in some respects similar to those now in operation, the first of which read thus:—‘This occupation is by no means legally permitted; it is only endured as a necessary evil.’ (p. 20, rule 1.)

“We in this country tacitly admit the statement contained in this clause, that prostitution is a necessary evil—otherwise, why permit it to exist? and, by so doing, tolerate it, if not in word, at least in fact. The following table, compiled from several parts of the first division of this work, presents, in some degree at least, a statistic of the amount of this class of crime in Berlin, at different periods.

Year.	Number of brothels.	Number of women living in brothels.	Number of prostitutes not residing in brothels.	Total number of prostitutes.
1780	-	100	-	800?
1796	-	54	-	257
1808	-	50	-	433
1844	-	26	-	305?

“Necessarily inaccurate as these numbers are, they most probably give us a glance at the relative, though not at the actual, frequency of prostitution; and allowing for the many changes which the social system of Prussia has passed through from 1780 to 1844, our author is led to

the conclusion, 'that every such attempt (to prevent public prostitution) has had as its result an increase of clandestine prostitution, with all its sad results.' (p. 155.)

"From 1700 to 1845, one or other form of regulations and control existed until the latter year, when, against the so-called legalization of crime, an outcry was raised by those who looked upon life from a pulpit only, and believed that the actions of men could be regulated conformably with their own dogmas and theories, nor ceased to complain until the last day of December, when all the brothels were closed, and the occupants, with those who lived by clandestine crime, were sent to their native towns, or conveyed out of the Prussian states. This system was continued till 1850, when the plan described in our last Number succeeded it. The result of this attempt, in 1845, to abolish Prostitution forms the subject of the second part of the work under review; and in reply to the first question, 'Has Prostitution in Berlin diminished or increased since 1846?' it is stated, that while in 1839 between 600 and 700 clandestine prostitutes were known to the police in 1840, consequent on the number of brothels having been restricted, they increased to about 900; while in 1847, or a year after these houses were closed, 1250 of these unfortunates were known to the authorities; and in 1850, the date of publication of this memoir, the average estimated number was as high as 8000. How great a difference between these thousands and the statistics in hundreds that we publish for 1852! Our first proposition then, to those who, on principles or presumed facts, oppose the establishment of regulations for, and a control over prostitutes, is—given about 8000 unfortunate fellow-beings under the abolition system of 1850, and less than 800 while the toleration of 1852 is in operation—to find on which side lie truth, humanity, and christianity?

"Has syphilis diminished since the abolition of brothels?" Such is our author's next inquiry, and to it the following statistics of 'der Charite' return a direct reply: (p. 181):

Year.	Number of cases of syphilis in Women.	Number of cases of syphilis in Men.	Total.
1845	514	711	1225
1846	627	813	1440
1847	761	894	1655
1848	835	979	1814

"As respects the intensity of the disease, it can only be expressed in numbers by representing the average number of days occupied in treating the cases that presented themselves at the hospital during the above years; and such a table we extract from p. 182:

Year.	In Men. Days.	In Women. Days.	In both sexes. Days.
1845	26 $\frac{6}{7}$	42 $\frac{8}{9}$	34 $\frac{2}{3}$
1846	30 $\frac{1}{2}$	51 $\frac{1}{2}$	40 $\frac{7}{8}$
1847	34 $\frac{1}{9}$	43 $\frac{2}{3}$	38 $\frac{2}{3}$
1848	33 $\frac{1}{3}$	53 $\frac{1}{6}$	43 $\frac{1}{2}$

"We have given these tabular details, in the hopes of carrying to the reader's mind the conviction of the truth of the conclusions which Dr. Behrend has drawn from these and other statistics—that after the closing of brothels in 1845, syphilis increased, became more severe in type, and (nor do we see any reason for doubting that our author is equally justified in making this statement) extended widely into the best families, while unnatural crimes became very frequent."

"Turning at this point, for confirmation of the baneful effects of closing the Berlinian brothels, to statistics derived from other than the public hospitals, we find in Dr. Neumann's pamphlet much valuable information, based upon the statistics of 'der Berliner Gesundheitspflegeverein,' a society composed of the workmen of all trades, and similar in many of its objects to our trades unions, which, though now abolished by order of, and for reasons best known to, the government, was instrumental in working out many social as well as medical problems. Among the latter is found the question involved in the title of the second work before us; and as neither the limits nor the objects of this review allow us to follow the author through his interesting details, which, coming from one already known in reference to the medical statistics of Prussia,* will be read with interest and advantage by those engaged in such investigations, we shall make but one extract from this *brochure*, the value of which is increased by the details having been derived from those of the laboring classes who were treated by the medical officers of the society, and are reported by one who holds opinions in very many respects opposed to those of Dr. Behrend.

	Average monthly number of members.		Of every 1000 members there were syphilitic.		Of every 100 sick there were syphilitic.
2d half of 1849	- 4901	-	- 5.32	-	- 13.51
1st half of 1850†	- 6177	-	- 4.59	-	- 11.90
2d half of 1850	- 8112	-	- 3.55	-	- 7.78
1st half of 1851	- 9483	-	- 3.20	-	- 7.49
2d half of 1851	- 10,525	-	- 2.83	-	- 6.98

"The value of this statistic, as proof of the beneficial effects of a control, appears to us to be as evident as is the difference between 5 and 2 per cent.

"It is not unimportant to trace, with Behrend, the influence which the stringent regulations, and final closing of the brothels in 1846, had on the number of illegitimate births. The results of his extensive tables are:

"That in the first period (1838 to 1841 inclusive), for about every 60 legitimate children, there were 10 born out of wedlock.

"That in the second period (1842 to 1846 inclusive), for every 54 legitimate, 10 illegitimate children were born.

"That in the third period (1847 to March, 1849, inclusive), for every 52 legitimate, 10 were illegitimate.

* Zur medicinischen Statistik des preussischen Staates, von Neumann, Berlin, 1849.

† Here began the control.

“Hence, in the first period every seventh child was born out of the marriage state, while in the third period nearly every sixth child was illegitimate.” (p. 202.)

“As the number of illegitimate births forms an important element in all inquiries into the morality of a country, a glance at the Registrar-General’s Annual Reports may prove not unimportant or out of place; as those who return from a continental tour, shocked with the immorality in which France is believed to be double-dipped, appear to be unaware that in England ‘the proportion of children born out of wedlock was (in 1846) 6.7 per cent. It was 7 in 1845, and 6.7 in 1842.*

“According to the same authority, at p. 10 of the fifth Report, the proportion of illegitimate births in France is as 71 to 1000; and it appears from the above that in England, in 1845, they reached 64 per 1000. If we then take into account that a far greater number of illegitimate births would be reported legitimate in the latter than in the former country, it will be evident that the number of births out of wedlock is at least as high in moral England as in presumed immoral France! †

“The effects of closing the houses of prostitution were:

“1. A very considerable increase of clandestine crime.

“2. Great increase in the frequency and intensity of syphilis.

“3. A depression of morality, showing itself in the very frequent seduction of immature girls, in the increased profligacy of the married, and in the greater number of illegitimate births.

“4. That the public safety and tranquillity were much endangered.” (p. 206.)

“The third part of Behrend’s book contains, together with their opinions, the cases given by Parent-Duchatelet of Paris, Ponton of Lyons, Tait of Edinburgh, and others, relative to the impossibility of abolishing prostitution by any forms of law; and as it is the duty of all governments to protect society, our author, in general terms, considers by what means a control can be best established. The recommendation of the formation of a ‘permanent commission,’ a Magdalene asylum, a hospital and clinic for syphilis, closes a work which we cannot lay aside without recording our admiration of the untiring, scientific, and, what is far more honorable, humane exertions of Dr. Behrend and Herr Von Hinkeldey, the present President of Police, to the latter of whom Prussia is chiefly indebted for her existing sanitary system; and though the names of those who endeavor to relieve a class scarcely sinning oftener than sinned against, may never be heard among the fashionable loungers of our drawing-rooms, yet such philanthropists may fearlessly appeal to time and eternity for a judgment.

* Ninth Annual Report of the Registrar-General, 1848, pp. 19, 20.

† Of 248,554 children registered, 15,839 were illegitimate; so 1 in 16 children born in England is not born in wedlock. I can discover no grounds for supposing that less than 65 in 1000 English children are illegitimate. The proportion in France is 71 to 1000.—(Fifth Report of Registrar-General. Second Edition, p. 10.)

“Having completed our account of the Berlinian system, and closed two of the books under review, we resume our sketch of this class of crime as it at present appears in Europe; and before leaving German territory, add a word or two respecting

Prostitution in Austria, Hanover, and Bavaria:

“There exists, as far as we could ascertain, and Professor Sigmund most kindly assisted us, neither printed nor documentary evidence relative to prostitution in Vienna, Prague, &c.; neither in these cities, nor in Grätz, Innsbruck, Pest, Lemberg, or Craeow, is there any control over prostitutes, further than, if complaint be made against them, they are punished, perhaps somewhat more severely than if they had not led such a life. Where brothels exist, the police are particularly watchful to prevent a breach of the common law; but as to medical regulations, there are none; and for statistics relative to the extent of syphilis, we refer to the reports of the hospitals and the elaborate articles by Professors Sigmund,* of Vienna, and Waller,† of Prague.

“In Hanover and Bavaria, also, we failed to find any form of sanitary regulations relative to this subject; but in the latter country, those who are found affected with syphilis, can, according to an old statute, be punished with imprisonment; if declaration be made that any woman has propagated the disease, she must submit to be examined by Professor Escherich, judicial physician, and if found syphilitic, she is placed under treatment, and can be punished for having concealed the disease. It may not be uninteresting to observe, that in some of the small university towns public prostitutes are only known as rare visitors; as by some statute, whether of the kingdom in general or university in particular, we cannot state, these women can be driven from the town. Yet the result of inquiries that we have made, in Gottingen, for example, to which town these remarks apply, rendered it but too evident that the students were more frequently diseased after each of these visitations, while the spread of the disease continued long after the *public* prostitutes had disappeared. The lesson this teaches is self-evident.

Prostitution as it is Controlled in Bruxelles.

“Bruxelles, so far-famed for her statistics and sanitary measures, and so justly proud of her ‘Congrès Général d’Hygiène,’ cannot, so far as our short stay enabled us to judge, boast of having carried out her sanitary regulations relative to prostitution as completely as Berlin has done. How surprised were we to find, that some physicians would not be seen visiting a brothel of the third class; we had, indeed, and shall long have, to learn that there are prostitutes too low, *alias* too poor, to be visited by the medical officers of a sanitary system.

* Aertzlicher Bericht, iiber das k. k. allgemeine Krankenhaus zu Wien im Solar-Jahre, 1850; § 23; 1851, § 43; see also Deutsche Klinik, Nos. 21 to 29, 1851; and for last report, Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien, 1853, Mai Heft.

† Vierteljahrsschrift für die praktische Heilkunde, Prag.

“Having visited the houses of prostitutes in Bruxelles, in company with the chief medical officer, we presume these houses were fair specimens of others in the same city; and, as regards cleanliness, general regularity, and respect to the officers of the commission, there is room for many improvements. We say this reluctantly; for we brought from Bruxelles very many pleasing remembrances. Having already completed our account of the control used in Berlin, we shall proceed to give an outline of the regulations in force at Bruxelles, so as to enable our readers to judge between them.

“The brothels of Bruxelles are of two kinds; first, ‘*les maisons de débauche*,’ in which live ‘*les filles des maisons de débauche*; second, ‘*les maisons de passe*.’ which are visited by ‘*les filles éparses*,’ and in which they keep their appointments. These houses are distinguished, one from the other, by different colored lanterns hung over the doors; and if the women who walk our streets were permitted to enter only such houses of reception in which no prostitute lived, they would then be placed under circumstances in one respect similar to those of ‘*les filles éparses*’ of Bruxelles.

“All classes of prostitutes are ordered to be examined twice a week; those who live in brothels of the first and second class are visited by the physicians; while the very poor—we mean the women of the third class, and all those who do not reside in brothels—are obliged to attend at the dispensary. If the latter class attend regularly for four weeks, they are exempt from all tax; if, on the contrary, their attendance be irregular, they can be imprisoned from one to five days. Any one of the women who do not live in brothels, can be examined at her own dwelling, provided that she pays, at the dispensary, three shillings and fourpence, for which she receives four visits, which will be continued as long as the payment be made in advance. Thus the prostitutes of the first and second class brothels are saved the inconvenience of attending at the dispensary, as are also those who live in private lodgings, and who would much rather pay the prescribed amount than be seen going to the office as common prostitutes; while the half-starved, ill-dressed, pauper of the third class brothel, must wait at the dispensary until examined, and then return to, shall we say, her home, where none but her companions, the poorest of both sexes, and an occasional police officer, are ever seen. The prostitute who comes to the dispensary may have been once resident in a first or second-class brothel, and was then visited by the physician; it is she, as being poorer, that must now visit him.

“It must be evident that the regulations of a sanitary commission should apply equally to all who come under the denomination ‘prostitutes;’ the physician should see no difference between the silk-dressed and slip-shod harlot. The regulations in force at Bruxelles most unwisely make such a difference; and even in the practice of British hospitals, we have seen such distinctions but too commonly made. In truth, a poor prostitute pays a bitter retribution for her faults.

“The medical staff of the dispensary in Bruxelles is composed of, first, a ‘superintending inspector,’ whose duty it is to be present in the dispensary when the examinations are being made, and to visit the houses once a fortnight at least; second, two ‘medical inspectors,’ who, during alternate months, examine, one the women in the brothels, the other, those who attend at the dispensary. The date and result of each examination are marked on a card belonging to each woman, in the registries kept in each brothel, and at the dispensary. If a woman be found affected with syphilis, or any other contagious disease, the owner of the brothel must immediately send her in a car to hospital, and on the cure being completed, she is returned her card, and may resume her former life.

“Such of our readers who are anxious to learn more of this system, are referred to an extract of the regulations of 1st July, 1844;* but as we are returning homewards, in our search among the sanitary systems of the Continent,

Prostitution as it is at present Controlled in Paris,

next claims our attention.

“There is no city in which the history of this subject has been so fully considered, as in that where the second edition of Parent-Duchatelet’s great work† appeared, in 1838. Since that period, many important alterations have been made; and, thanks to M. Trebuchet, chief of the medical police, and M. Duval, chief of the ‘Dispensaire de Salubrité,’ we possess some information, together with the printed forms relative thereto, which, like those of Berlin, not having, as far as we can learn, heretofore appeared in any other form than as instructions for the use of the staff of this fifth office of the first division of the prefecture of police, require special consideration.

“The act of ‘registration’ consists in an entry being made of the name, age, birth-place, residence, previous occupation, and motives which induced to prostitution. This is, in almost all cases, made at the request of the women, as the office does, not oblige any one to acknowledge herself as belonging to this unfortunate class, unless she has been frequently arrested for open debauchery, or when, on being attacked with contagious disease, she refuses to submit to the measures which it is the duty of the authorities to enforce, in order to preserve public order and health.

“As it conduces to the interest of every member of society to protect public health and morals, and as the government consider it immoral and degrading to receive the earnings of crime, the entire expense of the control over, and treatment of, these women is defrayed out of the public treasury; prostitutes in Paris are therefore exempt from tax, are punished by imprisonment, and never by fines.

* Congrès Général d’Hygiène de Bruxelles, 1852, p. 430.

† For review of first edition, see *British and Foreign Medical Review*, vol. v. 1836, p. 333; and vol. vi. 1837, p. 49.

“The women choose, at the time of registration, to which of the two classes of prostitutes they will belong; they are then registered as ‘*les filles isolées*,’ who have separate dwellings, or as ‘*les filles de maison*,’ who live in the so-called houses of tolerance; and they can pass from one class to the other after making a declaration expressive of their desire.

“A woman on being registered as one of the former class receives a card with her name, age, general appearance, residence, &c., on the reverse of which are printed the following

“*Duties and Prohibitions imposed upon Public Prostitutes.*”

“The public prostitutes, ‘*en carte*,’ are bound to present themselves to be examined, at least once a fortnight, at the *Dispensaire de Salubrité*.

“They are ordered to show this card (the one we are now describing) whenever required by the officers or agents of the police.

“They are forbidden to induce to debauchery during the day; they cannot appear in the streets until a half-hour after the time fixed for commencing to light the lamps, nor at any season before seven o’clock, P. M., nor remain after eleven, P. M.

“They should wear simple and decent apparel, such as cannot attract attention either by its richness, color, or extravagant form.

“They are forbidden to appear in their hair.

“They are expressly prohibited from speaking to men who are accompanied by women, or children; and from addressing any one in a loud voice, or with importunity.

“They shall not, at any hour, or under any pretext whatsoever, show themselves at their windows, which ought to be constantly closed, and furnished with curtains.

“They are forbidden to stop in the public thoroughfares, form groups, walk in companies, go and come within too short limits, or be followed or accompanied by men.

“The neighborhood of churches and temples to within at least sixty-five feet, arcades, public gardens, and all deserted or dark streets and places, are closed against them.

“They are expressly forbidden to frequent public or private establishments where clandestine prostitution is encouraged, as also the *tables d’hôtes*, or to take lodgings where there are boarders, or day-pupils.

“They are equally prohibited to share their lodgings with a mistress, or other prostitute, or to live in furnished lodgings without a special permission.

“When in their lodgings they shall avoid everything that may give cause of complaint from those living near, or passing by.

“Those who act in opposition to these directions, who resist the officers, who give a false name or address, subject themselves to penalties proportionate to the gravity of the offence.”

“With this card she enters the dispensary, is then examined, and the following form, printed on white paper, is filled up :

“ Form No. 33, Date, — 185 ,

“ Pref. of Police; 1st Div.; 5th Office; Medical Office; Filles Isolées.

“ ——— (Here her name is entered; and in order to prevent confusion resulting from their frequently calling themselves after some favored lover, the sur-name of the woman's father is that which, even though she had been married, is always written.)

“ ——— living in ——— street, No. ———. Has been visited and found ———.

“ ——— Signature of the Physician on duty.

“ If she is found in health, her card is marked; with it she departs, leaving the last described form at the office. If, on the contrary, she is found diseased, she is then conveyed to hospital.

“ The women who are registered as *filles de maison* are examined once a week in the brothels in which they reside, and their state of health is marked in the last pages of a book kept by the mistress of the house, on the first leaf of which is written the name, &c., of the keeper of the house, number, name of the street, and the folio of the registration book, kept at the dispensary, in which are entered the names, &c., of the mistresses of brothels. Certain general instructions are printed on the third and fourth pages, to which follow thirty-eight leaves of the following form:

“ Names of the women living with ———; Names and Age; Date of reception; Date of leaving; Aged ——— years.

“ The last ten pages are divided into two columns, headed ‘visits,’ in which the physician writes his report.

“ All prostitutes are, in addition to the regular visits, subjected to special examination, whenever they pass from one class to the other, change brothels, apply for passports, are arrested, or on leaving prison, or hospital. A woman, resident in a brothel, when reported diseased, is sent to the dispensary, where she undergoes a second examination, and is, if the report be confirmed, conveyed the same day to hospital. When a woman is examined previous to passing from one brothel to another, the physician fills up a form similar to that above, but printed on *red* paper.

“ As regards the opening of a brothel, the office requires a written declaration from the landlord, to the effect that he consents to his house being let for such purpose; and it must be situated at as great a distance as possible from churches, public buildings, or offices, schools, &c.

“ As respects the mistress of a brothel, she is bound to register at the dispensary the names, &c., of the women admitted into the house; and when one of them is about to leave it, the mistress must, within twenty-four hours, inform the office of the change. The windows must be furnished with double curtains and the glass ground, or the outside shutters kept constantly closed and locked; at present, east flinted glass is used in all these houses, or at least in the windows on the ground floor. As some of the women may become diseased between the periodic medical visits, the mistress of the house is directed to bring immediately to the

dispensary any woman whom she suspects, or knows to be diseased; she is bound to inform the police of any irregularities that may occur within or without her house; and forbidden to receive minors, pupils of the colleges, or those, in uniform, belonging to the civil or military national schools. The women are not allowed to be absent from the house without sufficient cause; the doors must be constantly closed; and neither bottles nor objects indicating that any kind of drink can be obtained within, are allowed to appear in the windows; neither are they at present permitted to have any name or sign over the door, excepting a number, and that is, we believe, not such a one as should occur in the regular succession of houses in the same street, but is the figure which in the police registration book represents that brothel. Men and women have, by these precautions, been prevented from entering the brothels, presuming them to be hotels, private lodging-houses, &c., a mistake that not unfrequently occurred under the old regulations. The mistresses of the houses in the environs are obliged to send the women in covered cars to the dispensary; and for any violation of the regulations, the house can be closed for a longer or shorter period, according to the nature of the offence.

“The medical staff is composed of twelve physicians, of whom one has the title of chief physician; and three of these officers are always present at the examinations at the dispensary. There are also a number of agents of the police employed in ascertaining whether all the women attend the examinations, and conveying the diseased to hospital; in addition to which, it is their duty to visit the brothels, in order to discover whether any irregularities are committed, and to prevent clandestine prostitution as much as possible.

“If a woman desires to discontinue her dissolute course of life, and shows that she can, by some legitimate occupation, obtain a livelihood, she is kept under observation for a time varying according to the circumstances of her case, and if found to be leading a regular life, she is removed from control. If, on the other hand, she wishes to have her name taken off the police books, in order to become the mistress of any one, the office generally requires a guarantee from the person with whom she is about to live, that she shall be maintained and protected for a certain period; as it has been found that without some such security, they before long are deserted, return to their former mode of life, and, by practising clandestine prostitution, avoid the inconvenience of being under control.

“As regards the internal management of these houses, the office makes no rules, nor is there any printed tariff hung on the walls; but the cost of drink and apartments is so far fixed, that there is no fear of even a stranger being made to pay very exorbitantly, as a complaint to the first police officer that passed would ensure an immediate inquiry, and a report to the office, which might be followed by such summary punishment as closing the house for a time. In the third-class brothels are allowed to be sold all kinds of drinks; and, though at first sight this to

us appeared to be a most unwise permission, we were informed that it was tolerated in order to prevent the incessant egress for liquor that occurred when it was forbidden to make sale of drink on the premises; and wines, &c., can be had in all classes of brothels, at a little more than double their usual price.

“Whether it is advisable to have in such houses one large room wherein all the women and visitors may assemble, or a number of smaller rooms, to prevent many persons meeting in one apartment, has been the subject of much consideration; from what we, while visiting all classes of Parisian brothels, have had opportunities of ascertaining, it appears to us, that, as a general rule, the richer the brothel, the greater the amount of immorality. In a brothel of the third class, where all meet in one room, and leave it to go to sleeping-rooms, the great number present prevents any gross sensuality; in the crowd there is protection; while in a first-class house, where each party is shown into a separate apartment, in which are only two or three women, this small company soon lay aside all restraint, and the women violate the commonest forms of decency, even while in the presence of their own sex. We were unable to obtain any statistics relative to prostitution in Paris, and considered ourselves particularly fortunate in being permitted to learn and see so much of the working of this very secret department of medical-police.

“Here closes our description of prostitution as it exists, and is controlled in countries, the medico-educational and sanitary systems of which we have studied during more than two years; had our attention been exclusively confined to the subject of prostitution, the present outline of this important branch of a sanitary system, would have been filled in; but when it is remembered that this description forms but one chapter in the history of the sanitary regulations of Hanover, Saxony, Prussia, Austria, Bavaria, Belgium, and France, the impossibility of entering more fully into details will, we believe, be evident; and the first work that heads this article presents us, as we pass from what we have seen to what we have heard and read, with the past and present history of

Prostitution in the City of Algiers.

“Charged with a scientific mission to Algiers during the summer of 1851, M. Duchesne undertook the investigation of this subject, in the hope of assisting in improving this branch of hygiene. With this end in view, he proceeds to consider the necessity, origin and general history of prostitution, passes to prostitution in Algiers, notices briefly its history before 1830, the date of its conquest by the French, and since that period.

“In 1850, the gross population of Algiers numbered 54,041, the prostitutes 493; of the latter, 113 were French, while Spaniards, Mahomedans, Italians, Germans, Anglo-Spaniards, Jewesses, Mulattos, Negresses, Arabs and Moors, the last two numbering 248, formed this mixed unhappy company.

“As regards their ages, it must be remembered that in Africa a female is marriageable between the ages of twelve and fifteen, and it is not uncommon to see a young Arab at fourteen or fifteen years married to a girl of from eight to eleven, or possessing at least a slave; hence, at Cairo, a young Genoese will sell for six thousand piastres, equivalent to 62*l.* 8*s.* Further, it is almost impossible to ascertain at what age the Moors begin to prostitute themselves, for if asked how old they are, the invariable reply is, ‘I was, or was not, born when the French came.’ How trumpet-tongued this tells of the conquered yielding to the conquerors! Each of these women pays a tax of ten francs per month, and is provided with a card, on which her name, age, residence, and the date of visit are inscribed. There does not appear to be any distinction between those living in brothels and those who occupy private apartments, further than, that if a woman prefers being visited at home to being examined once a fortnight at the dispensary, she is required to pay the physician three francs for each visit. Respecting this tax, M. Duchesne observes: “We are convinced that if the system of personal and immoral taxation were abolished, a much larger number of inscriptions would be obtained, and that the proportion of clandestine prostitutes would diminish.” (p. 140.)

“For a short account of the control enforced in the provinces of Algiers, Oran and Constantina, we refer to the 183rd and following pages of the original; but as respects the frequency of syphilis, its statistics have not been accurately kept for a time sufficient to render them valuable; and we refrain from making any extracts from the section on male prostitution; suffice it, that in 1850, specific ulceration of the oral mucous membrane occurred fifteen times, of the vaginal fourteen, and an equal number of times on the anal; in the first six months of 1851, it appeared eight, five and eight times; and the author has been assured by a physician who resided many years in the East and elsewhere, that at Smyrna, Alexandria, Malta and Rome, public prostitutes propose such a choice to their visitors, as may render women liable to ulceration of any of these mucous membranes. Lastly, with a scheme of regulations for public prostitutes in Africa, our author closes a work which will be referred to by all who shall hereafter treat of this subject.

Prostitution as it is controlled in Copenhagen.

“The law has not made any provision for these women, further than by passing against them most stringent regulations, some of which we have mentioned at page 114; hence prostitutes are completely in the power of the police, who direct that certain acts shall not be committed, and recommend rules for their conduct. The state in this manner avoids even tolerating prostitution; the government only permits the police to do with it as it may please them. The police have formed a series of regulations, which are in many important respects so similar to those already described as enforced in Berlin, that we shall allude to only the leading differences between them.

“It will, perhaps, be remembered that in Berlin the women live in brothels, the keeper of which is as responsible as the women themselves for their conduct; whereas, in Copenhagen, all may live separately in private lodgings, and when even four live together, none, save each for herself, is in any degree answerable for their behaviour. The owner, though resident in the house, is no more responsible for their conduct than the proprietor of any house usually is for the prevention of noise, &c. Hence, though there may be several women living in one house, it is not considered to be a brothel; and this enables M. Braestrup, chief of police, to observe in his report, that ‘Formal concessions are not granted either to public prostitutes, or to those with whom they lodge; neither are there in Denmark brothels in the ordinary sense of the term, and as they are found in other countries.’ (p. 417.)

“In this statement there appears to us to be a palpable evasion; for, although formal concessions are avoided, full toleration is given, with however less printing than in Berlin; we can see no difference between four or more women living, in Denmark, in what they and the chief of police call a private lodging, and the same number resident in Berlin, in which they and the state call a brothel, but that in the latter there are two persons responsible for the conduct of each prostitute—that is to say, herself and the keeper of the house—and that the house can at all times be examined by the police, whereas the sanitary condition of, and crime committed in, the so-called private lodgings in Copenhagen must, we presume, remain unknown to the police until it be complained of, as it is nowhere stated that they possess the power of entry into these, any more than into other private lodgings; and, ‘the agreement between the landlord and the prostitutes is made without the interference of the police, unless there occurs at a later period some difference between the contracting parties.’ (p. 422.)

“From Dr. Behrend’s pamphlet, it appears that in 1850, the number of prostitutes in Copenhagen was 201; and in the report of the chief of police, it is stated that ‘there are at this moment 68 persons authorized to lodge each from 1 to 4 prostitutes; in all, they lodge 159; 59 prostitutes have their own dwellings.’ (p. 423.)

“There were, therefore, 198 prostitutes registered in Copenhagen in 1852. The examinations of these women, together with the treatment of the sick in the prisons, are conducted by a physician, who must also attend to all cases to which he may be called by the police.

“‘Care is taken that these women are all treated in the general hospital (*Almindelige*), and that they do not allow themselves to be treated elsewhere, unless they offer a sufficient guarantee not to propagate the disease, or unless their personal position requires certain considerations—a thing which can seldom apply to the generality of prostitutes.’” (p. 423.)

“We cannot allow this clause to pass without observing, that this regulation can only exist in a country in which the police *feel it their duty* to accommodate certain individuals. Such a regulation is, in fact,

very similar to those in force at Bruxelles and Algiers, only that in the latter cases, the women pay for each visit a stated fee, whereas, in Copenhagen, no payment is *required*; but we know the working of such regulation sufficiently well to venture, even at the risk of being accused of conjecturing relative to that which we have not seen, to assert, that as both the woman and her lovers are in the power of the police, these are rarely forgotten by those for whose 'personal position,' they make 'certain considerations.' Further, it appears to us most difficult to conceive what such a woman can give as a guarantee that she will not propagate the disease; and would it not be far better to allow men to seek women in brothels, the localities of which are soon well known, than permit all these women to walk the streets in search of lovers?

"We gladly pass from regulations, which to us appear highly objectionable, to the consideration of the very efficient means used to detect syphilis. Such, for example, as interrogating all persons placed under arrest, who are required to declare if they are then, or have been lately, diseased; which, if they conceal, they render themselves liable to punishment.

"A visit ought to be made each time a ship is about to put to sea. All non-commissioned officers, musicians, and soldiers are examined on entering and leaving the service, and also regularly every month.

"In order to prevent as much as possible the propagation of syphilis, the soldiers who are attacked are obliged to state by whom they were probably infected, in order that immediate information may be given to the police, and that the women be prevented from communicating the disease to others. To attain the same end, the authorities have the right to use a writ and rescript of the 14th March, 1788, and of the 2d July, 1790, by which the presidents of the districts are authorized to punish those who will not give early intimation of their disease, by imprisonment on bread and water after their cure." (pp. 426, 427.)

Prostitution as it existed and exists in Spain.

"M. Ramon de la Segra's note informs us that the ancient Spanish statutes made frequent mention of public prostitutes, as also of *barra-ganas* or kept mistresses; and the laws of Castile directed that a particular form of dress should be worn by them. In the chief cities of Andalusia, under the government of the Arabs, it was tolerated and limited under certain regulations; and in 1486, the *mancebias* or public brothels, in Seville and other cities, were, by the king, given to Alonso Fajardo, chief of the royal table, to whom the women paid rent and other duties. In 1559, at Granada, was fixed the amount to be paid by the women for rooms which contained certain furniture, as also for board, consisting of a certain description of food. It was forbidden to lend them bed-linen, or to receive them into the town before they had been examined by the physician charged with that duty; and he had to make oath whether the woman was then, or had been diseased.

"In 1570, by order of Philip II., the regulations in force in the

principal towns of Andalusia were extended to those of Castile, by which it was enacted, that it was of her own free will a woman became a prostitute, and that, without its being in the power of any one to prevent her, she could cease to be such although she had incurred debts. A surgeon was directed to pay a weekly visit to their houses, and report to the deputies of the consistory those who were diseased, in order that they might be removed to hospital. The keeper of a brothel could not receive into his house any who had not been previously examined, nor allow any one who was diseased to remain there, under a fine of a thousand maravedas, or eleven shillings and sixpence, with thirty days imprisonment. Each room was to contain certain furniture, and the house to be closed on holidays, during lent, ember week, and on all fast days, under a punishment of a hundred stripes to each woman who admitted men, as well as to the keeper of the house. These and other orders were to be hung up in different parts of the house, under a fine of twenty-three shillings and eight days' imprisonment.

"In 1552, in Madrid, a special hospital for venereal patients was formed by Antoine Martin, of the order of St. Jean de Dieu. Matters continued in this state until the beginning of the seventeenth century, when the regulations began to be less and less attended to, and, 'since that period, under pretence of a hypocritical disdain, the regulations of cities ceased to make mention of brothels, and confined themselves to reviving the severe catholic legislation against women of ill-fame. This unwise and culpable negligence, as respects brothels in the chief cities of Spain, has continued, and still continues, while they increase in a manner dangerous to public morality.' (p. 414.)

"We have, however, been informed, that most of the first class brothels in Seville pay a private physician, who makes examinations regularly.

"To Spain then, belongs the merit of having been the first to endeavor to control prostitution, and prevent, by medico-sanitary regulations, the spread of venereal disease. True it is that by order of the French Parliament, in 1512, a house was rented in Paris, and in it were lodged all those affected with syphilis. Again, in 1536, l'Hôpital de la Trinité, in the Rue St. Denis, was appropriated to such patients; * but even as late as 1762, the French police and administration considered any proposition for controlling prostitution, or limiting syphilis, as impracticable and utopian.†

"As respects

Prostitution in Portugal,

we have been informed, by a Portuguese physician, that neither in Lisbon, nor Madeira, are there any regulations for, or control over prostitution, though, in the former city, a Lock Hospital exists; in Madeira, a very bad form of disease is rife.

* Paris Médical, par M. le Dr. Méding, 1853, t. ii. p. 84.

† Parent-Duchatelet, t. ii. p. 49.

Prostitution as it exists in Rome.

"In Rome, prostitution is permitted, though not legally tolerated. There exists no statistics from which the number of prostitutes can be calculated, and probably the only legal reference to this class of crime consists in an enactment, that both parties shall be punished with three years' imprisonment; but this law most probably refers to cases of adultery, rather than of fornication. Public prostitutes are not to be seen in the streets. In the city, there are scarcely any public brothels; 'the result of this,' (writes a physician long resident in Rome, who kindly furnished us with these particulars,) 'is not that the Eternal City is more pure in its morals, but that prostitution is clandestine,' which, though a crime punishable with imprisonment, prevails to an immense extent; and the law is, in such cases, seldom applied, unless to gratify enmity or revenge. Respecting seduction, the law is more stringent; but, with the concurrence of the authorities, these cases are almost always compromised, and the punishment avoided by marrying, or bestowing a dowry on the woman. The amount of syphilis in Rome is always very great; and the sixty beds in San Jacomo, for this class of disease, are always filled with women, who, though criminals in the eyes of the law, are never punished by the police: indeed, after the late siege of the city, syphilis prevailed to such an extent, as to deserve the name of a pestilence; yet no accurate information can be had on this subject, as the government endeavors in every way to prevent the actual amount of crime being known; and, as a part of this system, the hospital reports are not published.

"Before closing this part of the subject, a word respecting

Prostitution in Dublin.

"From the statistical returns of the Dublin Metropolitan Police for 1851, p. 48, it appears that in 1848, there were 385 houses occupied, or frequented, by 1,343 prostitutes; in 1849, the number of such houses was 330, of the women, 1,344; in 1850, there were 272 houses, with 1,215 prostitutes; in 1851, 297 houses and 1,170 prostitutes;—hence, for every 101 males and 119 females, or out of every 220 persons, one is a prostitute.

Prostitution as it exists in Cork, Ireland.

"From an inquiry made by a gentleman who possessed the assistance necessary for making such an investigation, and has kindly furnished us with the following particulars, it appears that, in 1847, there were in this our native city about 250 prostitutes, living in 80 brothels, besides 100 clandestine prostitutes. Their ages were between 16 and 30, though one of these women has been twenty-five years leading that course of life which she began at eleven years of age. Here are to be found daughters living on prostitution in the house with, and thereby supporting, their father and mother, while the causes and consequences of prostitution are the same in this as in other cities.

“ We may perhaps assume, that those who have accompanied us so far are in some degree interested in such investigations, and will therefore continue their attention a few moments longer; but as regards those who, from dislike of such subjects, or because their sympathies are not readily awakened on behalf of these hapless fellow-mortals, have perhaps scarcely glanced at the preceding pages, yet who would be not unwilling to disabuse themselves of prejudices unworthy of women and discreditable to man, of these we have a request to make, which we feel assured they will never regret having granted; and, as reviewers seldom plead so urgently, there must be strong reasons for this request: that the article on prostitution in the 53d volume of the ‘ Westminster Review ’ be read with attention by all who may light upon this page; and if their best sympathies for suffering humanity be not extended to the class for whom we plead, then indeed the facts, statistics, and arguments we have already given, as well as the calculations we are about to enter on, will, we fear, fail to work conviction. Should this request, however, be not acceded to, the courtesy and justice of our readers will, we trust, induce them to follow us to the end, although we are about to conduct them through a calculation which falls as far short of representing the enormity of the evils that result from prostitution, or of expressing the force with which this gigantic ill calls thunder-toned for a remedy, as the horrors of the life that many prostitutes lead are beyond our imaginings.

“ The number of public prostitutes in some of the principal towns of England and Scotland, has been estimated by Talbot and Mayne (the latter one of the Commissioners of Police); to test the accuracy of whose reports, we have calculated the proportion of these numbers to those of the population of each town, as given in the census of 1851, and formed the following table, which represents the number of males, females, and population of both sexes to each prostitute: for example, opposite London, 10,000 represents the estimated number of these women, and the numbers in the succeeding columns are to be understood to mean, one prostitute to every 104 males, to every 120 females, or to 225 persons of both sexes.

[This table, with the concluding part of this paper, was published in the New Orleans Medical and Surgical Journal, for July 1854, p. 275, *et seq.*]

While this article was passing through the press, a new work on prostitution, by M. Dufour, of France, has been announced—a work in eight octavo volumes, illustrated with 20 steel engravings by eminent artists. Is not this a huge instalment, truly, considering its recent, numerous and voluminous predecessors? France must be rich in facts, cases and subjects; *Des femmes et des filles qui vivent dans cet état de dégradation*, must be numerous in that country, whose necessities require large libraries in this behalf. But the bane and the remedy

appear to go together. French prostitution is enfiladed with twenty, and syphilis with a hundred volumes. Since Venus sprung from the foam of the Ægian sea, and was stranded upon the shores of Cythera, and was inaugurated in Cyprus, never was there so formidable a battery opened upon her frail descendants, as in *la belle France*. Her scanty drapery, and even her cestus (charmed girdle) have been torn away; her swans, sparrows, doves, roses, myrtles and temples, are targets for the armies of science.

Happily, in America prostitution is (supposed to be) comparatively rare, though less so than its literature, which does not exist at all, except as the *lex non scripta*. If the foreign element—that form immigration, be subtracted from the female population of the Republic, the remainder will probably be less than in any other country in Christendom—a characteristic of the country which entitles its women, above all others, to the esteem and admiration of all who found their faith, practice, and hopes, upon moral excellence.

The French censuses taken from 1816 to 1831, show that only one prostitute in twenty-five is born out of France. In the absence of precise figures, no exact information on this point, can be given in the United States.

The number of prostitutes, known and registered as such in Paris, and subjected to control and surveillance, from 1812 to 1832—a period of twenty-one years—amount to the following numbers for each year: 15,526; 20,113; 22,866; 22,249; 26,226; 28,953; 31,042; 31,280; 32,957; 34,966; 34,831; 32,510; 31,845; 31,843; 29,948; 29,663; 31,956; 34,118; 36,337; 39,128; 42,699. This table, taken from Parent-Duchatelet,* shows an increase of inscribed prostitutes nearly three times greater in the last, than in the first mentioned year.

Were the numerical proportion of prostitutes in New Orleans, in 1855, equal to that of Paris in 1832, it would amount to about 7000.

Another fact—for such it may be assumed—indictative of the superior morality of American women, as compared to others, is inferable from the very small number of foundlings even in the large cities where they most

*Parent-Duchatelet, member of the council of health, of the Academy of Medicine, of the Legion of Honor, Physician of the Hospital de la Pitié, &c., born in Paris, 1790, died 1836. He visited the lowest dens of vice—studied infamy at its fountains—familiarized himself with whatsoever most degrades humanity, in order that he might be the better able to propose remedial measures, whereby the moral, physical and social condition of the worst men and women of Paris might be ameliorated. His enlightened benevolence, and purity of character, acquired for him the surname of The Good Parent, “(le bon Parent.)”

abound. Comparisons between nations, as between individuals are doubtlessly often invidious, unjust and untrue; but in this case the superiority of America as compared with France, there is not in all charity, room for a doubt, if the published statistics be taken for a guide. But, inasmuch as the statistics of prostitution, foundlingism and bastardy scarcely exist in an authentic form in the United States, it may turn out in this Republic as it has been recently ascertained in Great Britain, that France will not appear less moral than her neighbors wherever the arithmetical test shall be carefully applied.

The *known* numerical history of foundlingism in France is little favorable to the moral character of that country. Doubtlessly there are many countries ignorant of the extent of their own criminality.

M. Gasparin, in 1837, (Rapport au Roi) has given an elaborate table showing the number (127,567) as well as the cost (9,019,000 fr.) to the government, of abandoned foundlings in France, for 1835. In the department of the Seine (Paris) with a population of 935,108, there were 16,229 of these unfortunates, which cost for nursing and so forth, 1,560,000 fr.; a ratio which would give, annually, to New Orleans nearly 2000 abandoned foundlings, at an annual expense to the treasury of thirty or forty thousand dollars. Now for the last 20 years New Orleans will not probably afford an annual average of ten—perhaps not five—abandoned foundlings.

In the same work of M. Gasparin, above mentioned, the following summary is published by authority, (p. 45):

YEAR.	FOUNDINGS.	YEAR.	FOUNDINGS.
1819,	99,347	1827,	114,384
1820,	102,103	1828,	114,307
1821,	106,403	1829,	115,472
1822,	109,297	1830,	118,073
1823,	111,767	1831,	123,869
1824,	117,767	1832,	127,982
1825,	117,305	1833,	129,699
1826,	116,377		

Nearly two-thirds of these foundlings die in about one year after birth. They are not put upon the parish, for support, as the poor often are in England, but are *abandoned*, never more to be identified by their parents, being styled in this and other official documents—"enfants trouvés et abandonnés"—a class which threatens to absorb nearly all the charity funds of the departments, already costing the treasury ten or eleven millions of francs per annum. In view of this great and in-

creasing demoralization in France, Secretary Gasparin avows as his solemn belief that the *natural affection of family* and the *moral principles* of his compatriots have *degenerated* in modern times, particularly since the Revolution of 1789.

How great soever may be the prevalence of libertinism and illegitimacy* in France, her remedial measures are vast, liberal and complete. The same causes that render Foundling institutions necessary in France, operate in the United States, more or less, particularly in the large cities. A glance at a feature of the natural or rather social history of bastardy, will show the advantages of foundling establishments. In all countries the putative father of a bastard, is seldom disgraced in public estimation, even though he abandon both the mother and the child. It is far different with the mother. If she be poor and unable to afford her child a maintenance—if, as often happens, she be unable to get employment, thus encumbered and disgraced, the temptation to infanticide, may, and often does prove stronger than the maternal affection and the terrors of the gallows. The canals and the Mississippi river at New Orleans doubtlessly receive many dead infants which the coroner will never see. These dreadful contingencies and crimes are, to a great extent, remedied by the system of Foundling Hospitals and other charities in France, by which the disgrace of maternity may be often concealed, and the motives to infant murder removed, the infant being provided for by the public.

It is reasonable to conclude that, in such a benevolent scheme, the good preponderates over the evil. The principal objection—though more apparent than real—is based upon the theory that foundling charities afford encouragement to illicit intercourse, by the anticipating and removing some of its dreaded social, physical, and pecuniary consequences, which might deter parties from libertinism. But passion prevails over reason—the present over the future.

Prostitution originating from moral causes, is a medical subject in so far as it affects health, and tends to originate, propagate, and transmit to innocent children, diseases which are alike loathsome and difficult to cure. Experience has proved that the medico-legal control of prostitution limits and tends to extinguish these maladies. Hence, it is virtually as much a medical topic as malaria, quinine, small-pox, vaccination, contagion, and so forth.

* In the department of the Seine among 1,000 births of all species, 313 are bastards. D'Angeville, Stat. 29.

Even the physiology of prostitution offers problems not yet solved, concerning the infrequency of menstruation, alteration of voice, embonpoint, infertility, and other peculiarities incidental to the sanitary condition of that class.

A physician, as such, may altogether ignore the question whether prostitution be a social necessity. It has always existed, as it still does, and always will, either openly or clandestinely. Parent-Duchatelet does not hesitate to say that it is inseparable from, inevitable and inherent in a large agglomeration of mankind.

A physician, of enlarged views and accuracy of judgment, will seek for the causes of disease in the moral, as well as in the physical—intellectual, as well as in chemical phenomena. The organs of the abdomen, the chest and the head, suffer from maladies which do not always originate from stellar, lunar, solar, atmospheric, or terrestrial influences.—The influences of the moral, domestic and social, as well as heat, humidity, filth and malaria of the physical world, engender diseases, sap the firmest constitution, modify or defeat the curative action of drugs, puzzle the pathologist, confound the sanitarian, and disappoint the expectations of the morbid anatomist at the *post-mortem* examination.

It has been asserted—with what truth let others determine, that prostitution lessens the prevalence of masturbation. It is not for the physician to decide which of these involves the greater amount of moral turpitude; but, apart from the risk of venereal affections, it is probable that the physical and sanitary effects of prostitution are less deleterious.

The heads of families, teachers of youth, and particularly physicians, should use the utmost vigilance to prevent, detect, and arrest the progress of masturbation—the practice of which is, in many cases very injurious to the health of both males and females. It is often practised in early life, long before puberty. It is characteristic of no single class, but extends from the highest to the lowest scale of society—from the learned professions to the servile.

To ask for or expect information from adult females, concerning this practice, is altogether useless and vain, although many of their diseases, as leucorrhœa, uterine hæmorrhage, falling of the womb, cancer, functional disorders of the heart, spinal irritation, palpitation, hysteria, convulsions, haggard features, emaciation, debility, mania—many symptoms called nervous—*un triste tableau*, have been referred to masturbation as the cause; at the very least, such a practice would aggravate these affections, should they not originate them.

From the intrinsic secrecy of the case, direct evidence is rarely attainable. Sometimes apprentices, servants and others, introduce this habit into a family of children, the parents remaining in total ignorance of the same. It has been in rare instances ascertained, that a negress among the house-women of a plantation, has not only initiated the female children of her master into this vice, but little misses of the neighborhood, who casually visit her master's house, and remain for the night. The same occurrences take place more frequently in towns, and particularly at boarding-schools.

Men, more candid in their confessions in this behalf, are greater sufferers, both physically and mentally, and are probably more guilty, too, than women. A French physician, M. Réveillé-Parise, gives the following account, doubtlessly an exaggerated one, of this evil habit: "In my opinion," says he, "neither the plague, nor war, nor small-pox, nor a crowd of similar evils, have resulted more disastrously for humanity, than the habit of masturbation: it is the destroying element of civilized society," &c. (*Dict. de Méd.*, ii. 244.)

The writer of these lines is the depository of the most indubitable evidence, showing that marriage both recent and happy, does not always arrest masturbation, notwithstanding the party may repent, amend, and swear on the Bible that he will reform, yet he relapses.

Where matrimony fails, prostitution is not likely to succeed in effecting a reformation. Hard labor, or abundance of exercise in the open air, simplicity of diet, rural life, restricted sleep, the avoidance of books of an imaginative character, have been recently prescribed for several sedentary individuals in both town and country. Many years ago, a young man of fine physical development, who wrote good verses and practiced masturbation to excess, asked for medical advice—begged to be emasculated—attempted suicide—but was persuaded to try severe manual labor; he cleared six acres of heavy timbered beech and sugar tree bottom—was cured, and rose to distinction in civil life.

If these repulsive details shall prove the means of forewarning, forearmng, or saving a single individual from a deleterious course of life which causes a morbid condition of mind, verging upon insanity, producing constitutional disorders of the most varied and anomalous character, the most righteous readers of this Journal will not complain of the length, nor of the inutility of this article.

Inasmuch as the subject of self-pollution has been almost inadvertently brought into review, in connection with prostitution, it may be pro-

per to present a case, the treatment of which must be embarrassing to a practical man as well as to a chaste one, who will exclaim with King Lear—

“Fie, fie, fie! pah; pah!

“Give me an ounce of civet, good apothecary, to sweeten my imagination.”

The following letter, of recent date, is copied *verbatim*, excepting the name and the locality, which latter is beyond the limits of Louisiana:

“Dear and most respected Sir:

I must say that it is with great difficulty that I can summons sufficient courage to avail myself of the opportunity to address a few brief lines to so eminent a gentleman as yourself; but knowing, or having good reason to conclude, that you are a gentleman of the greatest philanthropy, and not inordinately fastidious, when being brought to consider upon the welfare of those who may be so unfortunate as to become afflicted with sophisticating disease, I must recede as much as possible from my usual abashment, and proceed to relate to you that I have been plunged into a horrible catastrophe by ignorantly indulging (*incognito*) in a shameful, disgusting, yet alluring habit, called masturbation. And now, dear and affectionate Dr., I cannot withstand a knowledge of your existence, avocation and abode without being constrained (through a confirmation of my impotency) to imploringly solicit of you some intelligence relative to a physician within the scope of your knowledge, whose abilities and integrity will render him competent to treat successfully this most woful malady, resulting from a want of stability to resist the action of a seductive, insatiable, and fiery propensity. Please reply immediately, as I intend proceeding, as soon as I receive your highly appreciated favor, to Philadelphia, to purchase a stock of books, and there place myself under the care of the doctor you recommend.

I am, &c., ——— ———

“P.S. Bear in mind that the doctor must reside in the aforesaid city.”

No apology is deemed necessary for devoting so large a portion of this Journal to the Medico-Legal Jurisprudence of Prostitution. It has been thought best not to divide the above article, in order that a complete exposition of its fundamental principles might be given in a single view, once for all, not without the hope that its great physiological, sanitary, and moral finalities may be the sooner effectuated, the more the subject shall be examined. The acceleration of a desirable end, though apparently distant, is praiseworthy. Although New Orleans may possibly stand in need of reformation less than other American cities, yet its chances for the speedy realization of medico-legal amelioration, are greatly enhanced by the fact that its population is still, to a great extent, Gallic,—and the French type of legislation, is in this behalf, the best model extant, as experience has proved.

PRACTICAL MEDICINE.

1. RE-VACCINATION—The *Gaz. Hebdom. de Méd. et Chir.* of May 12, 1854, quotes from a Berlin publication the results of the Re-Vaccination in the Prussian army during the year 1853, from which it appears that this operation was performed upon 44,652 men, among whom 32,642 clearly presented the vaccine cicatrix; 7,643 had scars of a dubious character and 4,367 had none whatever. Of the whole number re-vaccinated 28,329 had the regular vaccine eruption—5,933 had an irregular eruption and 7,664 none at all. Whence it appears that re-vaccination succeeded in 69 in the hundred of those who had been previously vaccinated.

During 20 years ending in 1853, it appears that the ratio of successful re-vaccinations has constantly augmented. Thus beginning with 1833, the number in each hundred, successfully vaccinated at the second operation, is for each year consecutively, as follows: 33, 27, 42, 46, 49, 50, 51, 54, 57, 58, 57, 57, 58, 60, 64, 64, 64, 61, 61, 69, 69. The first and second years (1833-4,) do not give nearly half as many successful re-vaccinations in the hundred as the two latter, (1852-3.)

During the year 1853, numerous cases of the varioloid occurred in the Prussian army, amounting to 106, together with 25 cases of chicken-pox, and 7 of small-pox.

Vaccination, how perfect soever it may seem, should be tested by re-vaccination: even in infancy, at puberty, and again at the middle age of life, from 30 to 40, this process should be repeated.

The popular error, that when the physician has performed the operation of vaccination his services are no longer necessary, is one which should be speedily corrected. Although any one may perform the primary operation, none but an individual thoroughly acquainted with the natural history of the disease, is competent to judge of its genuineness—of its characteristic phenomena—in its rise, progress, decline, characteristic cicatrix, and-so-forth.

2.—CHLOROFORM IN FEVER, TETANUS, AND DELIRIUM TREMENS.—The uses and abuses of Chloroform, outside of operative surgery, already

great, are increasing. Sometimes non-professional people buy this article, and use it for trifling ailments, without consulting a physician. They even use it clandestinely, during the physician's regular attendance, in New Orleans. On several occasions, it has happened that the editor of this Journal has found the patient unattended, in a deep sleep, with a failing pulse: the patient having applied Chloroform to his nose upon a handkerchief, where, as soon as insensibility and loss of consciousness took place, it remained, and would doubtlessly have proved fatal, but for a timely discovery and interference at the visiting hours.

*On the use of Chloroform, by DR. GORDON, Physician to the Hardwicke Fever Hospital, Dublin.**

[The fever poison sometimes acts on the brain in a remarkable manner, producing symptoms of disease both in the chest and abdomen, although those two cavities may be, in reality, free from disease. One of the most frequent characteristics of lesion of the brain is *insomnia*, causing, or followed by, delirium, subsultus, &c., and accompanied, probably, by congestion of the substance of the brain.]

There are two axioms laid down in Dr. Corrigan's late work on Fever, which should always be remembered: 1st, "the loss of sleep, if it continue, is of itself sufficient to kill;" and 2dly, "if even the shortest sleep be procured, some advantage is gained."

To procure this sleep, then, by means which will not do injury to the circulating or digestive powers, would appear to be the most rational mode of treating this complication, and the rapidity with which the symptoms of great nervous exhaustion subside, when once sleep has been procured, proves the soundness of this mode of treatment.

To the different means which have been made use of for this purpose, I would now add the internal administration of chloroform. I have used it with the happiest results when all other means have failed, and I can speak with confidence of its certain and speedy action. The following case, reported by Mr. RICHARD ABBOTT, affords a good example of its effects, and the mode of its administration:—

Patrick Dempsey, aged 25, was sent from Santry to the Hardwicke Hospital, on the 8th of December; he was then eleven days ill of fever; his body was covered with dark-colored maculæ; his pulse was 110, and very weak, his speech muttering and indistinct; he had subsultus in both upper and lower extremities. His head was shaved, he was ordered the bark mixture of the hospital, and half a pint of wine. Late in the evening he began to rave violently, and could not be induced to remain in bed; he was ordered large doses of hyoseyanus, and the back of his head was blistered; he was so violent as to require the use of a strait-waistcoat all night.

December 9.—Has not slept since admission. Pulse 132; very weak.

*The following articles are extracted from *Braithwaite's Retrospect*.

He continues constantly muttering and raving. Tongue dry and brown; eyes slightly suffused; head not very hot; respiration short, frequent, and irregular. He still requires the strait waistcoat to keep him in bed. He was now ordered twenty-five minims of chloroform in a draught, to be repeated in an hour.

After the second draught his agitation and restlessness ceased, and the waistcoat was removed. He dozed a little through the day, but only for a few minutes at a time. Towards night he again became restless and delirious; the same quantity of chloroform was again administered, and repeated in an hour, when he fell into a sound sleep, which continued for nine hours. He awoke perfectly sensible; the subsultus had ceased, and his pulse had fallen to 100. He continued to improve, and in a few days was convalescent.

In this, and other similar cases, chloroform acted by producing anæsthesia of the sensory nerves, and exerting a paralyzing influence on the muscular fibre; and this it appears to effect without depressing or deranging the nervous force, as is the case with sedatives in general, while it is altogether free from the objection of causing depression of the action of the heart, as is the case with some special sedatives. My colleague, Dr. Corrigan, has just treated a somewhat similar case by the internal administration of chloroform. I had an opportunity of daily witnessing the progress of the case; and, by his permission, I here append it, as reported by MR. EDWARD SARGINT:—

Dennis Beahan, *æt.* 20, a porter from High-street, was admitted into the Hardwicke Hospital, January 4, 1854, the fifth day of his illness.

On the sixth day he was thickly covered with bright maculæ. His tongue was loaded, but moist; his pulse 112; respiration 22; no abnormal sound in the lungs; no tenderness of abdomen. He is reported not to have slept for two nights. His eyes are red and injected, and his head is hot. His head was shaved and cold lotion applied.

Seventh day.—Pulse 116; respiration 28; slept but little.

Eighth day.—Pulse 120; very feeble; respiration 32. Ordered bark and wine.

Ninth day.—Pulse 126; very feeble; respiration 32; head hot; constantly raving, and getting out of bed; no sleep; subsultus of hands; tongue dry; great difficulty of utterance. Vesicatorium nuchæ; eight ounces of wine.

Tenth day.—Pulse 130; weak; raving continually; difficult to restrain; requiring the straight waistcoat; constant talking; no sleep; tongue brown and dry in centre; thirsty; eyes very congested; pupils dilated.

Chloroform was now administered by inhalation, without any other effect than the pulses being slightly reduced in number. The patient was in no way quieted by it. Four leeches were now applied to the temples without any good effect. At 5 p. m., he took ℥ss. chloroform by the mouth, and continued it every second hour till 11 p. m., when, as he did not sleep, and the delirium continued, he got same dose of chlo-

reform every hour through the night. At 3 a. m., he was somewhat quieter, but the same dose was continued every hour till 8 a. m.

Eleventh day, 10 a. m.—Much quieter but has not slept. Pulse 110; pupils natural size; subsultus nearly gone; tongue brown all over; sordes on teeth; bowels free; urine high-colored, sp. gr. 1.020. Another dose of chloroform in same quantity was again administered; about twenty minutes after its exhibition he fell into a quiet sleep, which lasted for two hours. Shortly after waking, he took another half drachm of chloroform, when he almost immediately fell asleep, and awoke after several hours, much refreshed and quite collected.

His return to health was further indicated by the immense quantity of nitrate of urea, which an excess of nitric acid deposited from the urine.

In the above case the chloroform was longer in producing its effects than in any instance in which I have as yet used it. We learn from it, however, that we are not to be discouraged by the apparent failure of the first dose or two in procuring sleep, for, as in the present case, although actual sleep may not be at once procured, we may expect that a state of calm and quietness will be induced, which will soon be followed by "*Nature's sweet restorer, balmy sleep.*" We learn also from this case, that the inhalation of chloroform is, to say the least, useless in procuring sleep in cases of cerebral excitement in fever. I had on one occasion before, in the Hardwicke Hospital, fully tried this mode of administering it; its inhalation was followed by general convulsive movements, very similar to an epileptic seizure, and I have not since administered it by inhalation in any similar case. Dr. Corrigan carefully tried the effect of inhalation three times in the above case; each time without any good effect.—*Dublin Hospital Gazette, Feb. 1, 1854.*

On the use of Chloroform in Tetanus.

[In the 'Medical Times and Gazette' will be found a very interesting series of reports of cases of Tetanus, forty-three in number, of which eleven recovered. The editor makes some good remarks on the use of chloroform as follows:]

The following propositions appear to be warranted respecting it:—

That in the great majority of cases, inhalation of chloroform may be practiced with safety as regards immediate consequences.

That it is always effectual in allaying spasm for the time.

That it exerts, however, no preventive influence whatever, the spasms usually returning, with even increase of severity, very shortly after its suspension.

That its continuous administration over long periods of time is not to be recommended, since the patients sink at least as fast, if not faster, than when the disease is allowed to display itself.

That it is of great benefit in certain protracted cases simply as an alleviant of the pain. In some of these it will procure rest for periods often of an hour or more after the suspension of the inhalation, and acts altogether much more favorably than in the earlier stages.

That, in certain protracted cases, it is of the greatest use in enabling the patient, while in a state of half-insensibility, to take food, who would otherwise be unable to swallow.

That, excepting for the two last-named purposes, its use does not seem to be attended by any commensurate benefit, while it may much interfere with the action of other remedies, and, very possibly, be actively injurious itself.

These conclusions must be understood to apply only to chloroform inhalation, since, from the cases published, there appears reason to believe that the results of that of ether have been more favorable. As, however, the latter agent has not been, of late years, used in London, we have no means of judging as to the proportion of cases in which it did not relieve, or whether in any it appeared injurious.

It may be worth a thought whether the employment of anæsthetic vapors *externally* in cases of tetanus might not promise some benefit. The spasms are for the most part reflex, and excited by peripheral irritants. A bath of vapor might be easily given by covering the bed with an impervious material, and exposing beneath the clothes a sponge saturated with ether. The patient's head should of course be left out, and the clothes well tucked in round the neck.

[These cases show the relative value of other modes of treatment. Two cases recovered under the use of belladonna: two cases showed the uselessness of tracheotomy: one recovered during the exhibition of sesquioxide of iron and Dover's powder. In several cases Indian hemp seemed useful. In one case nicotine controlled the spasm and repressed constitutional disturbance. On the whole, vegetable sedatives seemed most beneficial.]—*Med. Times and Gazette*, June 17, 1854.

On the use of Chloroform in Delirium Tremens, by DR. B. G. M'DOWELL, Physician to Whitworth and Hardwicke Hospital, Dublin.

[In the exhibition of Chloroform in cases of delirium tremens, the practitioner should ever remember that chloroform is a powerful sedative—it exhausts—depresses. It can, therefore, only be used in those cases which are the result of direct stimulation of the brain,—sthenic cases,—cases which are caused by some sudden, short, and excessive use of alcohol. In those cases which are caused by suddenly *leaving off* this stimulant, as in some habitual drinkers, chloroform would be highly dangerous. This is a very important distinction to make. Dr. M'Dowell's first case seemed to be of the *sthenic* character. The patient was violent, and altogether in a high state of excitement.]

Chloroform was prescribed; half a drachm in a draught, to be given every second hour.

After the fourth dose, he fell asleep. At first, his sleep was frequently interrupted, and he awoke often, but it soon became more profound, and he slept all night. Next morning we found him still sleeping, the pupils were somewhat contracted; on rousing the patient, we found that he was perfectly rational, and that no delusions of any kind remained. After getting some food, he again fell asleep, slept for

nearly twelve hours, and awoke perfectly convalescent. Discharged November 16th.

[The second case was equally successful.]

On admission, his manner was nervous and agitated. He was rational and collected in his answers, but, when left to himself, his mind wandered greatly, and he was unable to control his thoughts. The surface of his body was bathed in cold perspiration. The pulse was full and soft, there were remarkable tremors of the tongue and hands, and total loss of appetite.

Treatment.—Tepid affusions to the head. Fifteen minims of tincture of opium, in an ounce of camphor mixture, every third hour.

November 19. Slept none last night; disease more fully developed; patient raves and talks incessantly, but is easily persuaded to remain in bed; tongue dry and furred; urine scanty and high-colored; bowels confined; skin has ceased to perspire.

R. Mist. rosæ catharticæ ℥ iv. statim. Thirty minims of chloroform every second hour, after the bowels are freed.

Towards evening the patient became so exceedingly violent, that the strait-waistcoat had to be used.

November 20. Slept little during the night, towards morning he had some hours' sleep, and the medicine was omitted. He is rational and sensible, answers questions correctly, and is evidently disposed to sleep more; the pupils are somewhat contracted.

To have good beef tea during the day, and no medicine until the evening, when ℥ i. of chloroform is to be given.

November 21. Slept almost all night; with the exception of some nervousness of manner, no trace of the disease remains.

The anodyne powers of chloroform are well illustrated in the preceding cases, in both of which cases the disease was fully developed, and the nervous system in a state of high excitement. In the one case, two, and in the other, three drachms of chloroform, administered in half-drachm doses, at short intervals, were sufficient to subdue the functional excitement of the nervous system, and to induce sleep.

Sleeplessness is the principal source of danger in delirium tremens; the longer the system is deprived of sleep, the greater is the degree of nervous exhaustion, which too frequently terminates in death, preceded by convulsions. Hence, the absolute necessity of directing our treatment to this symptom especially, and the value of any remedy on which we can rely for procuring sleep.

In detailing these cases, it is far from my intention to recommend chloroform in preference to opium, in the treatment of delirium tremens. There are, no doubt, many cases which will yield as promptly to opium, as did the cases here narrated to chloroform. But I conceive it may prove of great advantage to know that chloroform is capable of con-

trolling the disease by its direct sedative and anodyne properties, for the following reasons:—

First; because there is a form of the disease in which opium is generally inadmissible; I allude to what has been termed the sthenic variety of delirium tremens, which is produced by the direct influence of stimulants, and is consequently more connected with active congestion of the brain, than the more common form of the disease, which is developed subsequent to the relinquishment of stimulants, and is consequently associated with a depressed condition of the nervous force. In the former affection, we generally rely upon purgatives, and the careful application of cold to the head; in some few cases, leeches may be applied; but if these means fail in procuring sleep, much danger is to be apprehended; the exhibition of opium is hazardous, yet death will certainly ensue, if sleep be not obtained. Under these circumstances, I conceive chloroform may be given with the greatest advantage.

Secondly; because it occasionally happens, even in the asthenic form of the disease, that opium fails altogether to procure sleep, and seems to increase, rather than to allay the excitement of the nervous system.— Under these circumstances, and especially when the tongue becomes dry, and the pupils are contracted, the use of opium cannot safely be persisted in. But in chloroform we have a remedy, which, from experience, I can confidently recommend to meet the exigencies of such a case; the following instance is adduced in illustration of this remark.

Case 3.—In the month of September, of last year (1853), I attended with Mr. Gorman, of Henry-street, a gentleman of about thirty years of age, who labored under delirium tremens for the first time. When I was summoned to see him he had been ill for three days; the opium treatment had been most fully and properly employed, yet he had not slept for forty-eight hours; all the characteristic symptoms of the disease were fully developed; the patient never for a moment ceased talking, the activity of his mind was astonishing, and the mental delusions under which he labored were of the most extraordinary and varied character. Trepidations were employed, and morphia given in full doses.

On the following day, we found that he had not slept, and that his symptoms were in no way improved. At times he was very violent, but was easily persuaded to do as he was desired.

The next day found him still perfectly sleepless. It was now four days since he had had any sleep. He was so hoarse as to be unintelligible from incessant talking and vociferating; eyes congested; pupils much contracted; tongue dry, and protruded with difficulty; urine scanty; tremors of the hands and arms, with subsultus tendinum. His strength was evidently failing, and he no longer struggled to leave the bed; the pulse had become smaller and more rapid. He was not sinking from want of stimulants or nourishment, for both had been given in sufficient quantity, but death was impending from want of sleep, and opium had been pushed to the uttermost. Under these most unpromising circum-

stances, I recommended chloroform to be given internally; a drachm was immediately given, and repeated in an hour; soon afterwards he slept for a few minutes; a third dose, also containing ʒi. was now given, and soon he slept soundly. The next day he was entirely free from every symptom of the disease.

In conclusion, I have only to remark, that an extended experience is required to determine: 1. Whether chloroform may be safely relied on in the treatment of delirium tremens; 2. The comparative advantage of a mixed treatment; *i. e.*, combining the use of opium with that of chloroform; or, 3. How far success would be rendered more certain by adopting the opium treatment in the first instance, and following it up by the administration of chloroform in full doses.—*Dublin Hospital Gazette, Feb. 15, 1854, p. 25.*

3. *On the Pathology and Treatment of Cholera*, by DR. J. ROSE CORMACK.

[One of the most excellent papers on this subject is by Dr. Cormack, Editor of the Association Medical Journal, from which we extract the following:]

1. Cholera is a fever, intimately related to those fevers which depend on malaria. 2. The intermittent or remittent type can be generally recognized in the milder, and also not unfrequently, (though less distinctly) in the severer cases. 3. The stage of collapse ought to be considered as an aggravated cold stage of the paroxysm of a pernicious fever which may spontaneously terminate in death or reaction. 4. The least dense portion of the blood has an excessive tendency to exude through the capillaries of the stomach and bowels, and pass from the body by vomit and stool. 5. The inspissated residual blood being unable to pass through the small pulmonary vessels, causes congestion of the lungs; and as speedy consequences of this condition, paralysis of the right side of the heart from over-distension, asphyxia, and other subordinate derangements of the vital actions. Death may take place from asphyxia; necræmia, *with* loss of the least dense portion of the blood by stool and vomit; necræmia, *without* such loss of the least dense portion of blood as can be discovered during life—the exudation remaining within the stomach and intestines; toxæmia from absence or deficiency of sanguineous depuration; inflammation of the lungs or other organs supervening in convalescence; debility; gastro-enteritis; two or more of the above causes combined. 7. The anatomical lesions found on dissection vary with the causes of, and circumstances attending, death.

Treatment of Cholera.—The advocacy, by many, of a uniform and empirical system of treatment of cholera, has greatly tended to obstruct the progress of rational inquiry. Several plans, and particular remedies have been found useful; and practitioners, impressed by the published account of this success, have too often contented themselves with empirically repeating the instructions of others, without carefully analyzing the facts and seeking for a rational explanation of the *modus operandi* of the therapeutical agents which they employ. The stage of the disease

has not been sufficiently noted in relation to the therapeutic means employed; and recoveries have been too largely designated cures. With one, capsicum has been the specific; with another, camphor; with another, sulphuric acid; with another, acetate of lead; with another, quinine; and so on might the list be indefinitely extended. Now, that all of these and other medicines are *par excellence* cholera remedies, I perfectly believe; and it seems to me that a more successful treatment is yet to be discovered by a judicious application of the means which we possess, than by searching for some new specific.

It seems to be of primary importance to bear in mind that we have a poison-disease to deal with, resembling, if not indeed identical with a pernicious ague; that it has a course to run, which may be modified and curtailed, but which, even when let alone, shows a disposition to terminate in recovery. We have, therefore, in the first stage, in which chills and other premonitory symptoms of discomfort may be present, to adopt measures which may be regarded as prophylactic rather than curative. It is in this stage that the pernitrate, or some other preparation of iron and the disulphate of quinine, are of signal benefit. Here, however, we must not follow a blind empiricism. We must, if the digestive system is at fault, combine with the use of these special medicines a judicious alterative system; and, should there be any tendency to copious watery evacuations—the serum of the blood—we must be prompt in our administration of those remedies which are generally termed hæmostatics; among which may be particularly mentioned quinine, sulphuric acid, nitrous acid, acetate of lead, creasote, and nitrate of silver. If the case should proceed a little farther, in spite of our endeavors, to arrest its progress or should the case from its inherent intensity, or from neglect, not present itself for medical treatment till the secondary effects of the loss of serum have become apparent, we may probably find it useless to think of the quinine, and be obliged to treat the cramps and collapse which threaten speedy dissolution. The necessity for diffusible stimulants is now apparent: and, of them all, as a general rule, camphor will be found the most useful. It can be conveniently administered in large doses by dissolving it in chloroform; and the solution combines so well with creasote, that it will often be found prudent to confine our administration of medicine to frequent doses of a mixture of camphor, chloroform and creasote. The chloroform is useful as affording facilities in dispensing; and it cannot, in the small quantities administered, do any harm. The camphor acts quickly as a diffusible stimulant; and the creasote has a powerful effect in restraining the serous discharge. Indeed, I am inclined to think that its beneficial effects are not much, if at all inferior in this respect to those of sulphuric and nitrous acids. External warmth, and the use of stimulating embrocations, are very beneficial in conjunction with the internal use of camphor. The suppression of urine is not a symptom which is to be relieved by the administration of diuretics; it is a necessary consequence of the congestive paroxysm of the disease, and its removal is likely to follow the cessation of that paroxysm.

Enormous doses of opium, of calomel, and of other powerful drugs, have been given to cholera patients; and there can be no doubt that such substances, when introduced into the system when in a state of collapse, are not likely to produce any very suddenly appalling effects. And indeed, in a vast number of cases, they seem to lie as quietly in the stomach of the cold cholera patient, and to produce as little effect as if they were deposited in a glass bottle. Should the patient, however, emerge from the state of collapse, he runs as great a risk from the poisonous doses of these medicines which have been placed in his interior, as he has just escaped from the pestilence. Large quantities may certainly pass off by the bowels, unacted upon; but it cannot be questioned that in a very great number of cases which have been treated in the way mentioned, patients have had their convalescence abruptly arrested by fatal narcotism or exhausting salivation. The use of opium and calomel, in ordinary doses, is often necessary; but the inordinate doses of these medicines formerly, and perhaps still, employed by some, cannot be too much condemned.

The importance of arresting the serous discharge, and of maintaining the warmth of the body by the application of heat externally, cannot be over estimated. In fact, the judicious carrying out of these intentions constitutes the essence of the treatment of cholera. Other therapeutic measures may be regarded as liable to considerable variety, according to circumstances, and as valuable and auxiliary, rather than as always indispensable. The prompt arrest of the serous discharge, by creasote, sulphuric acid, nitrate of silver, or other remedy, has undoubtedly saved innumerable lives; and from the chaos of contradiction, depreciation, and laudation, in which the merits of special modes of treating cholera are involved, the diligent application of warmth to the surface can be extricated as a measure of established value.

Mr. Barwell, in a little work just published, says: "It is a grand essential to keep up the temperature of the patient, since the tendency to become cold is certainly a great characteristic of this disease. Hot bottles should be placed to the feet, and inside of the thighs; and India-rubber bags filled with hot water to the loins and abdomen. The bed clothes must be ample, and should be so arranged, by means of an extra blanket wrapped round the shoulders, or passing round the chest and under the arms, that he does not bare that part to the cold in his restless jactitations, nor in rising on the elbow to vomit, as he may perhaps frequently be obliged to do." These recommendations of Mr. Barwell are sound and practical. They are, moreover, firmly based upon facts which came under his observation in St. Thomas's Hospital. In the following passage, he states very clearly, and, I think, very correctly, the relative value of internal stimulants and external warmth in cholera; and he also refers to the hospital experience already referred to, upon which his opinions are founded.

"Stimulants," says Mr. Barwell, "such as brandy, ammonia, or wine, though decidedly useful in their place, have not such effect in res-

toring the circulation, and exciting the system to greater action, as in collapse from other disease; indeed, considering the difference of its cause in this and other maladies, it is not to be expected that they would be as beneficial; for prostration usually occurs in consequence of nervous shock, and consequent loss of nervous power; therefore stimuli which act upon that system are naturally, in those cases, such as would benefit. But in this disease there is comparatively little loss of nervous power; in fact, with so great disturbance of the circulation, the retention of nervous power is marvellous. Our remedies ought not, therefore, to be directed through that system, but we should, if possible, find some means of acting on and recalling the circulation, without exciting the nervous centres; and the best mode of doing this is by external heat. This principle of combating the deadly cold collapse was not found or recognised at St. Thomas's until after several cases had been treated at the hospital, and the general inefficacy of medicines or of stimulants proved. Though a certain number under the treatment then adopted recovered, still the whole result was unsatisfactory; thus, of twenty-eight cases of perfect collapse, before external heat was used, seven only recovered—a very small proportion; but, after this was employed, sixty-one patients were treated by some mode in which this formed an essential part; and of these, twenty-seven recovered, or not very far from half the whole number.”

Did space permit, I could adduce much evidence of the same description, in addition to that which has now been quoted. In fact, I could show that the success which many have ascribed to favorite pharmaceutical nostrums, ought, with much greater probability, if not with absolute certainty, to be ascribed to the external warmth employed along with the drugs.

Having now glanced at the general principles upon which medicines ought to be employed in the treatment of cholera, I would now remark, that I have not enumerated every drug which may be usefully employed, and have selected those with the operation of which I am personally most familiar. This much, however, may be added; that they are good types of the respective classes of remedies to which they belong.

The formulæ to be adopted must of course be varied in accordance with the circumstances of each case; and it is by a ready power of modifying these formulæ, that the skill of the practitioner is displayed. The formulæ cannot be too simple. No therapeutic advantages flow from the multiplicity of ingredients; and by administering remedies for the purposes of clinical study, as well as of cure, it is obviously necessary not to give more than one medicine, or one class of medicine at the same time.

The following medicines possess in a high degree the power of arresting the serous diarrhœa, which generally precedes collapse by a good many hours, and which is the immediate cause of that collapse as well as of the cramps: creasote; turpentine; sulphuric acid; nitrous acid and nitro-sulphuric acid; nitrate of silver; quinine; gallic acid; alum; and acetate of lead.

Creasote.—Some patients refuse to take creasote, from a dislike to its odor; but if one or two doses of two or three drops can be taken every hour or every two hours, in mucilage, I prefer it to any other means of cure in serous purging. It hardly ever fails.

Turpentine may be used in place of creasote, in doses of ten minims. I have in several cases of diarrhœa found it quite successful.

Sulphuric Acid.—The use of this acid in diarrhœa is by no means new. Dr. Fuller, of St. George's Hospital, in a paper lately published, see *Retrospect*, vol. xxiii, p. 405, speaks in high terms of the efficacy of sulphuric acid in arresting diarrhœa. In bilious diarrhœa, and in certain chronic diarrhœas, he says it is of little or no avail; but in epidemic diarrhœa, in "acute autumnal diarrhœa," and in more decided choleraic diarrhœa, he has known no single instance of its failure. He gives it in doses of half a drachm mixed with water, every twenty minutes or oftener; and the effects produced are described as remarkable; heat returns to the extremities; the nausea and vomiting cease; the purging is stayed; the cramps subside; perspirations generally break out; the tongue becomes moist and slightly coated; the intestinal evacuations become healthy; and the pulse regains its normal steadiness.

Nitrous Acid and Nitro-Sulphuric Acid.—Mr. W. J. Anderson, in the "Association Journal" of November 4, 1853, recommends a combination of nitrous acid and sulphuric acid. He says: "Can a remedy be found which will readily yield up its oxygen, and supply that element to the impure blood: and at the same time, by its astringent properties, tend to check the enormous exudation which takes place from the mucous surface of the intestinal canal? In our present state of knowledge, some of the mineral acids appear to be the best adapted to this purpose; and, for certain reasons about to be explained, a combination of nitric with sulphuric acid seems to me to be preferable to any other. The acid should be administered in tolerably full doses, and repeated at intervals varying according to the nature and urgency of the case. For an adult, we may give *acidi sulphurici diluti f. ʒ ij, acidi nitrici diluti f. ʒj* in a six ounce mixture, one ounce being the dose for an adult.

Nitrate of Silver, I have not given in cholera and diarrhœa, except in the form of enema. It is valuable in this form. My experience is too limited to enable me to compare the efficiency of enemata of nitrate of silver with enemata of acetate of lead, and of alum.

Dr. Lever and Dr. Aitkin are the principal English writers who advocate the use of nitrate of silver in cholera and diarrhœa. On the continent, the authorities by whom it is recommended are Hirsch, Caustat, Boudin, Boucharlat, and Trousseau. Dr. Charles Lever, in 1832, extolled the nitrate of silver as a remedy in cholera.

Quinine. Of the power of quinine to check epidemic diarrhœa, and to thus arrest cholera in what may be regarded as its first stage, I feel well assured, from an extensive use of the remedy. It is true that I have generally combined it with sulphuric acid, and with iron, and sometimes with both; so that my facts are not available for the purpose of accurately determining the value of quinine given alone.

Gallic Acid and Alum. I have often used these remedies in the diarrhœa of phthisis, and occasionally in epidemic cholera. They are less to be relied on as means of arresting serous diarrhœa than creasote and the mineral acids. They are not superior, and hardly equal to logwood and catechu, as mere astringents; but I know that some authors regard them as especially the astringents to be used in cholera and serous diarrhœa.

Acetate of Lead may be used internally alone, or in combination with opium. It is generally a prompt astringent when used in the form of enema.

The Sulphur plan, as advocated by Mr. Grove, of Wansworth, has evidence in its favor, though I do not think that the theoretical ground—the fungus theory of cholera—upon which it is advocated, is tenable. Mr. Grove uses the following formula:—

R Sulphuris precipitati ℥j; sodæ bicarbonatis ℥j; sp. lavandulæ compositi ℥vj; aquæ q. s. ut fiat mistura ℥ijj.

A teaspoonful of this is taken every half-hour or every quarter of an hour. Mr. Grove informs me that the effect of the medicine is to restore warmth, and promptly to check the serous discharges. I believe Mr. Blackloek was the first author who recommended sulphur as a remedy for cholera. The treatment of cholera cannot be reduced to any routine formulary, but ought to be adapted to the particular condition of each patient in each stage of the disease.

The principal indications of treatment may be thus summed up:

The “rice-water” vomit and purging require to be energetically subdued by quinine, sulphuric and other acids, creasote, nitrate of silver, and such like remedies.

In actual and threatened collapse, external warmth, stimulant embrocations, and those internal stimulants which act on the capillaries, are of signal benefit.

In reaction, and during convalescence, local inflammations and congestions require to be guarded against or subdued; and rational means must be adopted to restore the secretions of the liver, kidneys, and skin, but particularly of the two former.

Lastly, though not of less importance, the character of the fever should be modified, and a repetition of the paroxysm guarded against, by change of air, or by the administration of quinine, which, in the majority of cases, from the existence of anæmia, ought to be conjoined with iron.—*Association Medical Journal*, Nov. 11, 1853.

4.—FEVER.—Be careful of your prognosis, when the nervous symptoms in fever preponderate at an early period of the attack, especially in the wealthy, who cannot bear wine so well as the poor. The more the secondary affections of fever are *anatomical*, the greater will be the use of stimulants; and conversely, the more they are *neurotic*, the less will be the use of stimulants.—(Dr. W. Stokes.) When the tongue is dry and brown, head not very hot, respiration short, frequent, and irregular;

and when added to these there is a delirium, more or less violent, give twenty-five minims of chloroform in a draught, to be repeated in an hour. This may be repeated in a few hours, if necessary, and if the pulse be watched, may be continued every second hour for some time. The *inhalation* of chloroform is useless in these cases, and may be followed by convulsive movements. (Dr. Gordon.)

5.—VENEREAL AFFECTIONS.—*Bubo*.—It is seldom necessary to open a bubo. Employ counter-irritation. Take a solution of nitrate of silver (one drachm to two drachms of water), with the addition of three drops of strong nitric acid. This is to be painted freely into the skin over the inflamed gland. This causes great pain and soreness, which is followed by rapid diminution of the gland. This treatment may be extended to other cases in which inflammatory deposits, whether purulent or fibrinous, require to be absorbed or diminished. (Mr. J. Hilton.)

Syphilis.—Treat the *infecting* chancre, the true firm *indurated* sore, with mercury. The *unindurated* sore may be safely treated *without* mercury. In treating the former, in Paris, the proto-iodide of mercury is used, but this does not answer so well in this country as mercurial frictions, blue pill, or the grey powder.

The *Phagedænic Chancre* is treated by Ricord, by first destroying the whole of the unhealthy surface with the red-hot iron, previously placing the patient under the influence of chloroform. After the sloughs have separated, the surface is covered with strips of ammoniacum and mercurial plaster, and doses of tartrate of iron are given with great success, in the less obstinate cases. The most frequent variety of sore in London is the phagedænic, with induration. In this, the actual cautery does no good; mercury must here be given, and the system supported by bark, wine, or steel. (Mr. Acton.)

AFFECTIONS OF THE SKIN, &c.—*Erysipelas*.—Early in the case, when the pulse is hard and full, and when there is no intestinal irritation, give an emetic of tartarized antimony and ipecacuanha, followed by minute doses of the antimony, till the pulse softens. Then give wine and good nutriment, especially if the tongue be brown; give from four to six ounces of wine daily with sago, beef-tea, &c., but avoid giving the wine or brandy so long as the fever is high. The above quantity ought sometimes to be doubled. As a topical remedy, mercurial ointment is as good as any, and perhaps better, but use it cautiously, or the patient will be suddenly salivated. (Dr. J. S. Hughes.)

Favus.—Apply the following ointment:—℞. Sulph. sublim. ℥ss.; hydrarg. ammonio-chlorid. ℥ss.; hydrarg. sulphureti cum sulph. ℥ss. Leviga simul, dein adde olivæ olei, ℥iv.; adipis recentis, ℥xvi.; creasoti mxx.; misc. Sometimes four or five nightly applications will perfectly clean the scalp, but it requires to be used continually, a little every night, to keep off the disease. The same ointment may be used for scabies and porrigo. (Mr. J. Startin.)

Porrigo.—Use the ung. sulph. comp. of the Pharmacopœia of the Hospital for Skin Diseases. See "*Favus*." (Mr. J. Startin.)

Scabies.—Use the ung. sulph. comp. of the Pharmacopœia of the Hospital for Skin Diseases. (Mr. J. Startin.)

Small-pox., to prevent pitting in.—Make a saturated solution of iodine in spirits of wine. Brush this tincture freely over the face once or twice daily, from the earliest period of the eruption, and continue it during the maturation of the pustules. (Dr. Crawford.) Take of carbonate of zinc three parts, oxide of zinc one part, rub these in a mortar with olive oil to a consistence. Apply this all over the face, and renew it from time to time, so as to take the place of the natural scab. (Dr. Bennett, *Dublin Hosp. Gaz.*, May 1, 1854.)

Ulcers, Cancerous.—For the prevention of smell, use the following powder made into a paste with water, brushing the parts over once a week or fortnight: Hydrarg. chloridi, ℥ ijss; hydrarg. bisulphureti ℥ ij; acidi arseniosi ℥ j; misce. (Mr. J. Startin.)

Of the Leg.—Instead of applying common adhesive plaster and bandage after Mr. Baynton's method, use strips of linen or calico, well soaked in water, just as you use the sticking plaster. Over these wet strips of linen, apply the common bandage. (Mr. C. Holthouse.)

7.—PNEUMONIA.—In sthenic pneumonia the sputa are of a rusty red color, viscid, and adherent. In asthenic, or typhoid pneumonia, the sputa are more like *prune-juice*, *i. e.* darker colored, and less viscid than in the former. A very interesting point is that, in many cases of pneumonia, the chlorides either disappear entirely or nearly from the urine, while there is a corresponding excess in the fluid poured out into the lung. The chlorides disappear from the urine up to hepatization of the lung, and as soon as resolution commences, the chlorides begin again to appear in the urine. Therefore, in cases of pleurisy and pneumonia, always *test the urine for chlorides*, as follows: Add a few drops of nitric acid to a portion of urine in a test-tube, and then a few drops of a solution of nitrate of silver. If chloride be present, a dense white precipitate of chloride of silver, which is insoluble in acids, will fall. If chloride of sodium be altogether absent, no precipitate will occur. (Dr. R. B. Todd.)

8.—GOUT AND RHEUMATISM.—Give the following: ℞. Extract. acet. colch. gr. j.; pulv. Doveri, gr. ij. Ft. pil. ter die sumend. Pulv. Doveri. gr. v., omni nocte sumend., et pulv. rhæi cum magnesiâ ℥ ij. omni mane. (Dr. Hughes.) Iodide of potassium dissolves lithate of soda very readily, but not lithic acid. Therefore give this medicine in cases of gout, except during the acute attack. Give 5 or 8 grains two or three times a day for a short time; or one grain every day in one or two doses for some weeks or even months. (Mr. T. S. Wells.) When delirium occurs in acute rheumatism it is of grave import. Examine the heart carefully,—there is very likely to be pericarditis upon which the delirium depends. Bleed generally or locally according to circumstances, give calomel and opium, with saline and colchicum. Blister the cardiac region well, both in front and back. At the same time support the patient with broth, and even wine if necessary; always remembering that many of these apparently inflammatory diseases are asthenic and not sthenic. (Dr. Watson, Dr. C. M. Durram.)

NOTICE.

MR. H. McCULLOCH, of New Orleans, having recently purchased from the succession of the late DR. HESTER, the goodwill, subscription list, and the assets of the *New Orleans Medical and Surgical Journal*, and having contracted with the undersigned, before a Notary Public, to Edit the same for Five Years, the projected *Quarterly Journal of Medicine*, though partly printed, has been cancelled, and its interests wholly merged in the first named Journal.

The patrons of the *N. O. Med. & Surg. Journal* may rest assured that its proprietor, MR. McCULLOCH, well known in New Orleans for his industry, accuracy, and integrity, will conduct its business affairs in a manner altogether satisfactory, and thereby relieve the Editor from an onerous task, enabling him to devote more time to serve all who may honor this Journal, as worthy of their attention and encouragement.

All Letters on the business of the Journal, Advertisements, and Moneys, should be addressed to MR. H. McCULLOCH, proprietor of the *N. O. Med. & Surg. Journal*, "DELTA BUILDINGS," 76 Camp street.

Communications, parcels, books for review, and exchanges should be sent, *free of expense*, to the address of the Editor,

B. DOWLER, M. D.,

OFFICE:—80 ST. CHARLES ST.

RESIDENCE:—DELORD ST., between Camp and Magazine.

Editor's Office--Notices.

MARCH, 1855.

BOOKS AND PAMPHLETS RECEIVED,

Some of which will be Reviewed in the May Number.

Puerperal Fever, as a Private Pestilence. By OLIVER WENDELL HOLMES, M. D., Parkman Professor of Anatomy and Physiology in Harvard University. Boston: Ticknor & Fields. M DCCC LV. Pp. 60.

Edinburgh Medical and Surgical Journal (Quarterly); Nos. CXCIX, CC, CCI, April to October: From Messrs. Adam & Charles Black, Publishers, Edinburgh.

Braithwaite's Retrospect, Part xxx, to January, 1855: From Messrs. Stringer & Townsend, (N. Y.) Publishers, at \$2 per Annum. Pp. 328; also, *The London Lancet*, from the same.

Annual Reports of the Commissioners and Superintendent of the Indiana Hospital for the Insane: November, 1854. Indianapolis, 1854. Pp. 74.

Books, &c.

- The Transactions of the New-Hampshire Medical Society:* Concord, 1854. Pp. 56. From G. H. Hubbard, M. D.
- The Principles and Practice of Obstetric Medicine and Surgery, in reference to the Process of Parturition:* with 64 plates and numerous wood cuts: by FRANCIS H. RAMSBOTHAM, M. D., etc., etc., etc.; a new American edition, revised by the author; with notes and additions by Wm. V. Keating, M. D., A. M.; lecturer upon obstetrics, etc., etc., etc. Philadelphia: Blanchard & Lea, 1855. Pp. 648, royal 8vo. From Mr. J. B. Steele, bookseller, 60 Camp-st.
- A New Plan of Treating Ununited Fracture:* by HENRY H. SMITH, M. D., Surgeon to St. Joseph's Hospital, Philadelphia, 1855. Pp. 20.—From the author.
- Essay on a New Method of Treating Serpent Bite and other Poisoned Wounds:* by DANIEL BRAINARD, M. D., Professor of Surgery in the Medical College at Chicago, Corresponding Member of the Societe de Chirurgie de Paris, etc., etc.; with two copper-plate engravings. Chicago, J. F. Ballantyne, 1854. Pp. 26.
- Inflammation and Ulceration of the Cervix Uteri:* by JAMES M. GREEN, M. D., Macon, Georgia. Charleston, 1853. Pp. 58.
- Autobiography of Charles Caldwell, M. D.,* with a preface, notes, and an appendix: by HARRIOT W. WARNER. Philadelphia: Lippincott, Grambo & Co., 1855. Pp. 454, 8vo. From Mr. T. L. White, bookseller, 105 Canal-st.
- What to Observe at the Bedside and after Death, in Medical Cases.* Published under the authority of the London Society for Medical Observation. Second American, from the second and enlarged London edition. Philadelphia: Blanchard & Lea. 1855. Pp. 228; royal 12mo. From Mr. T. L. White, Bookseller, 105 Canal-street.
- Table of Urinary Deposits:* by JOHN KING, M. D., Cincinnati, O. An Engraved sheet with letter press.
- Louisiana State Medical Society—*Proceedings of the—at its Fifth Annual Session. New Orleans. 1854. Pp. 22.
- Sanitary Reports of the City of Buffalo for the Year 1854.* Buffalo 1855. Pp. 57.
- Report of the Board of Managers of the State Lunatic Asylum of Missouri.* 1855. Pp. 48.
- Thoughts on Yellow Fever:* By J. S. McFARLANE, M. D. New Orleans: 1855. Pp. 8, double columns.
- Report of the Pennsylvania Hospital for the Insane, for the year 1854.* By THOS. S. KIRKBRIDE, M. D., Physician to the Institution. Philadelphia: 1855. Pp. 60.
- Report of an Expedition down the Zuni and Colorado Rivers:* by CAPT. L. SITGREAVES; Corps Topographical Engineers; accompanied by maps, views, sketches, and illustrations. Washington: 1853. 8vo.; letter press 198. From Hon. J. P. Benjamin, U. S. Senate.
- On Injection of the Bronchial Tubes, and Tubercular Cavities of the Lungs:* by HORACE GREEN, M. D., LL.D., New York: 1855. Pp. 20.

Books, &c.

Nineteenth Annual Report of the Managers of the New-York Institution for the Blind, to the Legislature of the State. New-York: 1855. Pp. 46.

The Half-Yearly Abstract of the Medical Sciences, No. xx, July to December, 1854: edited by Drs. RANKING & RADCLIFFE; published in London; re-published regularly in Philadelphia, by Lindsay & Blakiston, 25 South Sixth-street, at \$2 per annum, free of postage, if paid for in advance. The present number, which is very valuable, consists of 319 solid printed octavo pages. From the Publishers.

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University of New-York.

MEDICAL DEPARTMENT—SESSION 1855-6.

The Lectures will commence on **MONDAY, October 15th**, and be continued until **1st of March** following. The session of 1854-5 was attended by a class of 307 students; on 106 of whom the degree of Doctor of Medicine was conferred.

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4. **SURGICAL CLINIQUE WITH THE DISEASES OF THE GENITO-URINARY ORGANS**—Every Wednesday, from 3½ to 4¼ o'clock, P. M., by Prof. VAN BUREN. Besides its general Surgical advantages, this Clinique presents ample opportunity for the study of Syphilitic Diseases, Strictures, &c.

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SESSION 1855-6.

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LEWIS SHANKS, M. D.,
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THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL

FOR MAY, 1855.

ORIGINAL COMMUNICATIONS.

ART. I.—*A Glance at the "Reply" of SILAS AMES, M. D., to Experiments with Phosphorus. &c., by WM. M. BOLING, M. D., of Montgomery, Ala.*

(Continued from March number, page 616.)

In my former paper, a remark like this is found: "Here a child, seven years old, took at a single dose, one thousand eight hundred and twenty of Dr. Ames' doses for the adult." In regard to this, Dr. Ames says: "The phraseology here is somewhat peculiar; it is not said that the child took one-tenth of a grain at one dose, but 1820 of Dr. Ames' doses." Although from my experiments at the time of writing, as now, it was my impression that the quantity of the tincture given (it being Dr. Ames' diluted tincture,) contained about one-tenth of a grain, from the loss by vaporization, on mixing it with water for administration, I could not be certain as to the quantity that the subject actually got of it. I could and did know, of course, that he did not get *all* of the tenth of a grain, supposed at the time to be contained in the solution; but as to whether he got the half of it, the fourth of it, or any of it, indeed, I could not be positive. Moreover, as my recent experiments show, the

tenth of a grain was probably rather too high an estimate, as to the quantity contained in the solution, provided, even, that I could have felt sure that all of it was received by the subject. The actual quantity of the phosphorus then, was very probably a doubtful question, while as to the relative quantity, there could be no doubt; and the phraseology, therefore, was such only as the circumstances would properly admit of. In commenting on the fact, after alluding to the influence from it, that there must have been some mistake on his part, in attributing any efficacy to doses so minute as his were, when it was shown that it could be given in doses many hundred times greater, to persons in health, without appreciable effect of any kind, Dr. Ames says, that stronger instances could be found in cases in which several grains had been given, without any sensible effect. There are such cases on record, but Dr. Ames quotes, in his paper on pneumonia, authority to show, and tells us that his "own *experience* of its effects most *certainly* lead to the same conclusion," that in all such cases, "the article had undergone some change in its chemical state, which rendered it inert;" and the cases being thus disposed of by himself, there could be no necessity for me to drag them forth. This, in his paper on pneumonia, seems to be his explanation of the "eccentricity" of the action of the article as a poison. But Dr. Ames does not speak more distinctly and clearly of any action of phosphorus, than he does of certain effects observable from the doses in which he gave it, arising from "its activity, merely," and aside from any eccentricity or peculiarity of operation. It is unnecessary to re-produce here his statements on the subject. Now, holding in view Dr. Ames' opinions on this point, although it might be perfectly legitimate to explain the presence of any violent or unlooked-for effects, resulting from the use of a small dose, by the invocation of this eccentricity of the action of the remedy, or idiosyncrasy on the part of the patient, it would scarcely seem so to attempt to account in like manner for the *general* absence of *any* manifest effect under the use of comparatively such truly *enormous* doses of precisely the *same* preparation that had been supposed to produce, *generally*, a number of very appreciable effects in extraordinarily minute doses. Certain peculiarities of operation are ascribed by Dr. Ames to the "eccentricity of its action as a poison;" certain others are spoken of as its common and ordinary effects, resulting from "its activity, merely;" and the one quality cannot, of course, fairly be asked to explain away the non-occurrence of effects that ought to grow out of the other quality.

Dr. Ames, commenting on another quotation from my paper bearing on the same point, remarks: "It is evident from this, as well as another instance in which Dr. Boling has expressed a similar confidence in its harmlessness, dissolved in alcohol, that he had not studied either the medical or toxicological history of phosphorus very thoroughly."

He then again alludes to the large doses, before spoken of, in which it had been known to prove innocuous, and which, it will be remembered, he accounts for on the supposition of inertness, from chemical change, of the article used. Now for this to be parallel, or to bear upon the point at issue, as Dr. Ames would have it, and as at a superficial glance it might appear to do, it should be shown that in these very cases, or rather that with these *very same* specimens of phosphorus, that had been taken in large doses with impunity, owing to inertness from chemical change, very remarkable therapeutic results were obtained, and that without great care, even poisonous effects might be developed, and were always to be apprehended from doses many hundred times smaller. I give below the remark that more particularly elicited the above comment from Dr. Ames.

"How much further the dose (allowing to one four thousand times as large as the medicinal dose of Dr. Ames, and from which he obtained such marked effects,) might be augmented with safety, and without appreciable effect, I am at present unprepared to say; but reasons, I think, will appear as we proceed, which will render it not improbable that the quantity of alcohol, rather than any *suppositious* quantity of phosphorus, the preparations, *as prescribed and given*, may contain, should form the only necessary limit to the dose," believing it probable that a very large proportion of the phosphorus contained in the tincture was lost in the administration, and possibly that it was all so lost, or chemically changed.

The extreme and shameful degree of ignorance imputed to me by Dr. Ames, in the quotation but recently given from his paper, as also the amusing absurdity of the charge can only be appreciated by calling to mind the fact, that there is not perhaps a single one of the common text books even, on the *Materia Medica*, that speaks of the article at all, in which, instances such as are referred to by him, of several grains being taken without effect, are not mentioned; so that no tyro in medicine, who had read but one book on *Materia Medica* containing a chapter on phosphorus, could fairly be supposed ignorant in regard to them; while the sarcasm intended to be conveyed, loses its point in view of the

explanations just given,—to wit, the reason assigned by Dr. Ames for the inaction of the large doses, and the possibility of the entire loss, or chemical change of the phosphorus, in his preparations, as administered.

Alluding to the fact that other remedies are also irregular in their action, and may sometimes be given in doses, with safety, much larger than doses with which they sometimes prove mischievous, Dr. Ames relates the following case, which, he says, came within his "own knowledge, when a student of medicine;" "A child between two and three years old, got hold of a vial of calomel by accident, which she broke while playing with it, and liking its *sweetish* taste, ate up a *full* ounce of it before she was discovered. But the child showed no sensible effect from this enormous dose, except that the next day a *single* free evacuation of the bowels took place."

Now, as a matter of course, this was an exceptional case, the want of effect resulting from "eccentricity" or some other very unusual cause or causes, and therefore such want of effect, cannot be fairly adduced as parallel to the general absence of effect, in experiments with Dr. Ames' tincture of phosphorus in large doses, and of all the symptoms said by him to result from its administration in small doses, and arising from its "activity merely," aside from its "eccentricity." Dr. Ames himself, would probably not expect a similar result from calomel in more than a single instance in many thousand cases; while some of its more usual effects, it would be but reasonable to anticipate, in all of the other cases of the number. If it were known that calomel might thus be given without effect in such large doses; or could he adduce an example of a remedy, known and acknowledged by the profession to produce very prompt, appreciable and marked effects in very minute doses upon the sick, and which could be generally given to the well in immense doses comparatively, without any manifest effect; then the analogy would be such as to entitle the illustration to consideration.

The case is indeed an extraordinary one. The causes in operation to develop not only such a remarkable resistance to the action of an article, known generally to produce appreciable effects in moderate doses, when taken under anything like similar circumstances, but also in consequence of which actions, that the article usually quickens, (and at the tender age of the child mentioned by Dr. Ames, the bowels are extremely susceptible to the operation of irritants,) should have been as it were suspended, or retarded, must have been very peculiar. The child it will be remembered, had *one* loose evacuation from the bowels during the

following day; none it would seem on the day that it ate the meal of calomel, or at least after it, nor the night following it. In a period then of 24 or 36 hours, after eating a *full* ounce of calomel,—a quantity equal to about half a pound for the adult,—a child between two and three years old, and which, it can scarcely be questioned, would have otherwise—that is without the ounce of calomel—had at least two or three, had *one* free evacuation from the bowels. It is difficult to conceive what estimate should be placed upon such causes, and of course the case, as stated is not a suitable one with which to illustrate the point under consideration. It would have been interesting however to have traced the progress of such an “*infant phenomenon*,” from childhood to maturity, and to have noted the physiological anomalies that it would probably have exhibited. It would no doubt have been found that to “eat conger and fennel, and drink off candles’ ends for flap-dragons,” would have been a mere “matter of moonshine” to it. What a voracious little cormorant it must have been.

Even allowing due weight to the fact that in very rare and exceptional cases particular medicines fail to manifest their usual effects, or do so with extraordinary violence, owing to individual peculiarities, the case in its various details as related by Dr. Ames, is as he terms it, truly an extraordinary one; so much so, that were it not, that he tells us, it came *within his own knowledge*, doubts might even be entertained of the reality of its occurrence as supposed, and that there might possibly have been some mistake,—that the child broke perhaps a vial of whiting, or of some other inert powder, or that instead of really eating the full ounce of calomel, it might have wasted it. If the child was not really *seen* to eat the calomel,—and it is scarcely probable that any one whose testimony could be considered of any value as to the fact, would have witnessed the process without interfering to stop it; and moreover the discovery seems not to have been made till *after* the full ounce was eaten,—in view of the termination, many persons would hesitate to accord full faith to the supposition that it did do so. It is frequently a difficult matter to get a child to take a few grains of calomel, even in the sweetest and most tempting vehicle. Many chemists and pharmacutists tell us that calomel is tasteless. Dr. Ames says that it is *sweetish*. Every one to his taste, as a matter of course, but for myself I have never been fond of the flavor of calomel; and its *sweetness* seems of such a peculiar kind that one would suppose the grossest appetite might be surfeited with half the quantity said to have been eaten by this little

child. Curiosity might well be awakened to know whether it cried for "more."

The ingestion, by so young a child, of such a meal of calomel (and a child of the age would scarcely have eaten a much larger quantity of the most palatable solid food at once) would be likely to be very alarming to the parents, who would also probably be urgent, that means should be adopted, to secure, if possible, its evacuation from the stomach by emesis. If this was actually accomplished, and the calomel subsequently dried and weighed, of course there could remain no doubt that the child really had swallowed it; but the ease would be of less value, if possible then, as an illustration of the point for which it is adduced; and Dr. Ames does not speak of any means having been adopted with such an intent.

In considering the cause of certain discrepancies, between the results from the use of the tinctures of phosphorus in his hands and in my own, Dr. Ames makes the following quotation from my paper; modifying somewhat however, both my language and my grammar: "In connection with this experimental practice upon myself, I will again call attention to the views of Dr. Ames, in regard to the effects of the tinctures of phosphorus, and their dose. Thus he says, speaking of the saturated tincture that it "cannot be continued in the smallest quantity just mentioned"—half a drop—"for any great length of time, without inducing considerable disturbance of the stomach, shown by nausea or vomiting, burning heat and a feeling of oppression at the epigastrium." Though he admits that in the quantity of two drops, "a *single* dose, or *perhaps a few* doses may be given with impunity," he would evidently regard any lengthened use of it in such a dose, as a very grave and serious matter, and tells us of one instance in which dangerous effects resulted from the administration of three doses of two drops each, at intervals of twenty-four hours. While Doctor Ames tells us that doses of half a drop cannot be continued for any great length of time, without the most serious results, I have myself taken it in doses of five drops—being just ten times the quantity—a long time, and for eight days without omission of a single dose, without effect. While under his observation, from a cumulative action, dangerous effects resulted from three doses of two drops each, administered at intervals of twenty-four hours, being in all six drops taken in the course of three days, I have taken for eight successive days three doses of five drops each, or fifteen drops per day without effect. Indeed, unless I should discover something in its action,

which has never as yet been manifested in any of my experiments, from my own experience with the article, and with all the lights at present before me, I should not hesitate, were it not for the mere trouble of the thing, to continue it in the same manner for years."

"In the healthy subject at least, any effect of the article resulting in nausea and vomiting, could be easily appreciated, and not readily mistaken; yet not only did the subjects of my experiments take it in doses as mentioned, so immeasurably greater than the doses with which such effects are said by Dr. Ames to have been produced by it, but they took it under circumstances that were well calculated to favor the production of such an operation. Thus while they sometimes took it in the middle of the intervals between the meals, they also took it at times immediately before eating, and at others immediately after eating. On several occasions, I myself, having forgotten my dose, which I usually took just before eating, until I had partly finished my meal, have called for my vial, taken the dose, and proceeded with my meal without disrelish or any subsequent manifest effect."

In pointing out the cause of the discrepancies, says Dr. Ames, "the first step in the process, is to point out some errors in these extracts which in themselves go a good way toward effecting this desideratum." He continues—"The first error that I shall mention is, that what I said of two drop doses is applied, inadvertently of course, to the half drop doses. The latter is spoken of by me only as liable to produce considerable disturbance of the stomach, when long continued, while the former are said in effect to be unsafe in the treatment of pneumonia, if continued for any great length of time. The 'most serious consequences' therefore, should properly refer to the effects of the larger doses only."

That a proper estimate may be formed of the extract to which I may have misconstrued or misrepresented the language of Dr. Ames, it will be proper to present here the passage referred to, from his paper on Pneumonia. Speaking of doses of from half a drop to two drops of the saturated tincture, he says: "It cannot be continued in the *smallest* quantity just mentioned, for any great length of time, without inducing considerable disturbance of the stomach, shown by *nausea or vomiting, burning heat and a feeling of oppression at the epigastrium*."

Let the reader judge, are the words, "the most serious results," language too strong for such symptoms as the above, (produced it will be seen, Dr. Ames says, by the *half drop* doses,) and more especially when resulting from an article of which he had such apprehensions? If

the above are not serious symptoms or results, when following the supposed administration of so incendiary a poison as phosphorus, what results should we look upon as serious? nothing perhaps short of the death of the patient. Dr. Ames proceeds:

“Another error arises from a wrong construction of the following passage—‘Its effects are cumulative; that is to say, a dose which singly is not large enough to produce any sensible effect, may become very troublesome or dangerous, after several repetitions at intervals of *three or four hours*. This *quality* was developed in one instance, by repeating it in a dose of two drops of the strong alcoholic solution, three times at intervals of 24 hours.’ Dr. Boling construes this to mean that dangerous effects resulted from the three doses given at intervals of 24 hours; but the reader will see that the troublesome or dangerous effects refer only to the dose repeated every three or four hours, and that the cumulative quality alone is referred to in speaking of its repetition once a day.”

Now this is really, it seems to me, “about as broad as it is long.” The cumulative “*quality* was developed in one instance, &c.,” and how must it of necessity have shown itself, according to the definition of the *quality* given by Dr. Ames? “Its effects are cumulative; *that is to say* a dose which singly is not large enough to produce any sensible effect, may become *very troublesome or dangerous* after several repetitions at intervals of three or four hours,” and which *quality, so characterized* of course, was even in one instance developed by repeating it in a dose of two drops, three times, at intervals of 24 hours. It was surely not a very material error, if in my grammatical construction of the sentence, I took the *evidences* of the quality—the effects, indicating according to Dr. Ames, its operation or presence, for the quality itself. The Doctor proceeds:

“A third error, the source of which is in part explained in the two preceding paragraphs, is in the comparative estimate of the quantity of phosphorus taken by Dr. Boling and that given to my patients, to which the danger of serious consequences was ascribed, if continued any great length of time. Dr. Boling took five drops of a tincture containing less than one grain to the ounce, three times a day. Supposing it to be a full grain, each dose was about the one-hundred-and-sixtieth part of a grain. My patients took two drops of a tincture which, as we have seen, there are the best reasons to believe, contained at least six grains to the ounce.”

Proceeding on *such* data, Dr. Ames shows that his patients took much

larger doses than I did in my experiments upon myself. Now I took *five* drops *three* times a day, it will be remembered, for eight days in succession, without any effect whatever, while in the case alluded to by Dr. Ames the *cumulative quality*, characterized by the symptoms given, was developed by giving three doses of two drops each, at intervals of 24 hours; that is in all six drops, one more than my single dose, in 72 hours or three days.

"The doses taken by Dr. Boling, therefore," remarks Dr. Ames, "instead of being ten times greater, were less than half the size, or more than 20 times less than the estimate."

In regard to *one* point connected with the dose, the explanation will be given further on. Five drops of the *same* tincture at a single dose, I am inclined to think, I cannot be mistaken in supposing, must be just ten times as much as half a drop at a single dose; and five drops three times a day, in like manner for the 24 hours, just five times as much as half a drop six times a day. It will be remembered, what are the very grave effects, said by Dr. Ames to result from the half drop doses, repeated every four hours, being about three drops in the 24 hours; as also that I took, without effect, five drops three times a day for eight successive days.

Speaking further of my experiments, Dr. Ames says: "The largest quantity given to either of his subjects, in one day was 272 drops of a tincture, having *less than a grain* to the ounce, or about the third of a grain of phosphorus. The daily aggregate of the half drop doses," of a tincture let it be remembered, estimated by Dr. Ames to contain *six grains* to the ounce; "given to my patients, if repeated every four hours, is about the 45th part of a grain, so that the difference instead of being so 'immeasurably' great as supposed by Dr. Boling, is only as one-third of a grain is to the 45th of a grain; and the measure of the difference is almost exactly as fifteen is to one."

Deferring for the present, some explanations, or rather plain matter of fact statements, in regard to the strength of the tinctures, used both by Dr. Ames and myself, and proceeding for the time on the supposition that they were alike, as they really were, the daily aggregate of his half drop doses, would be three drops. My subject took in one instance 200 drops of the saturated tincture at a single dose, and on another occasion 272 drops in the course of about eight hours; the latter being just ninety times as much as the aggregate of Dr. Ames' half drop doses in the 24 hours; and without appreciable effect. But this half drop dose of the

saturated tincture here under consideration, is not, be it remembered, the medicinal dose recommended by Dr. Ames, but half a drop of the *diluted* tincture, which is only *one-tenth* as strong. When my subject then, took 200 drops of the saturated tincture at a single dose, and without appreciable effect, he took just four thousand of Dr. Ames' medicinal doses, which he says occasionally produce "some very *sensible effects* upon the head and stomach." And let it be again called to mind also, that in his reply he speaks of the "*peculiar activity* of its *physiological* manifestations in a much more minute quantity," still. It is much to be regretted I think, that the Doctor was not a little more explicit in this instance, and that he did not let it be known, *how much* more minute.

Phosphorus, according to what Dr. Ames says of it, is certainly a very "eccentric" article. *One* of the most,—though some might doubt perhaps whether it is *the* most—eccentric of all the eccentricities ascribed to it by Dr. Ames, or implied in his account of it, is that in virtue of which, the *same* tincture, prepared by the *same* apothecary, from the *same* materials, and taken out of the *same* bottle there can be no reason to doubt, should, when used in Dr. Ames' prescription, contain *six* grains of phosphorus to the ounce, and when used in my experiments contain *less than one* grain to the ounce. Not only in the instances quoted, does he speak of it under the two circumstances, as being of this relative strength, but in other places, wherever the matter is alluded to at all. There is indeed a good deal of figuring done by Dr. Ames, in which his doses are made to bear a much nearer proportion to those given by myself, than in reality they should do, all based upon this cool and quiet assumption of a difference in the strength of the tincture. He does not explain how this curious thing is brought about; but it can scarcely be questioned that by the *same process*, he might have made the disparity still greater. In my former paper, I made the following pointed statement, in regard to the tincture that I used.

"The preparation that I at *first* used, in my experiments, I obtained from the apothecary from whom I am in the habit principally of procuring medicines for my own use, and who prepared it at my request; but though I had no reason to suppose that it was not of good quality, I subsequently supplied myself with both the diluted and saturated tinctures, from the *apothecary who prepared the tinctures used by Dr. Ames.*"

I may here say, what I did not think worth while to mention at the time, and which may seem unnecessary now, that all my *published experiments were performed with the tinctures obtained from the latter source;*

though the other I have no doubt was as strong, for before ordering it, I made inquiry in regard to the process, and had it prepared, in the same manner as the apothecary at whose establishment Dr. Ames' prescriptions were put up, prepared his. The identity of the tincture used in my published experiments, with that used by Dr. Ames, would be taken for granted, it is but reasonable to suppose, from the care I took to point out the source from which I obtained it; while the motive for doing so, of course, could not be misunderstood. Dr. Ames cannot surely mean to insinuate that Mr. Theiss kept one preparation for his prescriptions, and an inferior one for dispensing and for sale to others. Indeed the interest that Dr. Ames himself, must have felt, that his favorite remedy should have the benefit of an honest and uniform preparation, if possible, in every instance of its use, would preclude the idea of such an explanation, as would also the character of the gentleman mentioned. I must confess myself, therefore puzzled to know by what strange and mysterious process the thing was accomplished. Had the doctor merely said that the tincture when used by himself, was six times as *powerful* as when used in my experiments, instead of claiming that it contained six times as much of the active principle, in the absence of a better, something of an explanation might be found in the supposition that he carried it, before using it himself through a process of "dynamization,"—by giving it say, perhaps a certain number of shakes; but his phraseology does not authorize the confident adoption of this method even, of explaining the mystery.

In my former paper, from the result of experiments performed for the purpose of ascertaining the strength of the saturated tincture, I estimated it at one grain to the ounce; in which case, Dr. Ames' medicinal dose would be, as already explained, the one sixteen-thousandth ($\frac{1}{16000}$) part of a grain; and on testing the matter still further, more recently, by a process given on a former page, and which I think it will be admitted, must of necessity be without fallacy, and accurate in its results, this estimate is ascertained to be correct,—that the quantity held in solution during the part of the year in which chiefly pneumoia is met with in our climate, does not exceed a grain to the ounce, and therefore that Dr. Ames' medicinal dose, instead of being between the one two-thousandth ($\frac{1}{2000}$) and the one three-thousandth ($\frac{1}{3000}$) part of a grain—nearer the latter than the former—which would be the size of it according to his recent estimate, as to the quantity of phosphorus contained in the saturated tincture, is in reality just what I had estimated it at, or less.

Dr. Ames, in his Reply, it may be remembered, attaches considerable importance to the fact, that my experiments were made exclusively upon the healthy subject. Between the date of the publication of my former paper and the appearance of the Reply, I administered the tincture of phosphorus in two cases of pneumonia, and in several other cases of febrile disease. Although the length to which the present paper has already reached, and the extent to which it must still further be prolonged, will prevent me from giving the details of these cases, it may suffice to say that, with all the watchfulness that I was capable of exerting, I was unable to discover the least effect, of any kind whatever, from its use. The dose administered in the several cases, varied from one to three drops of the saturated tincture every hour.

It may be asked, why administer an article from which no benefit was expected? My answer is, that though I expected no benefit from it, I did not allow its use to interfere at all (as seems to be the rule also with Dr. Ames,) with the administration of such other remedies as I deemed advisable in the cases; and that I could not possibly be brought to feel more confident (this confidence based upon many experiments upon the well, cautiously and step by step at first made,) of any medical fact, than that, in the doses mentioned, it could do no harm. With this explanation, my real motive for giving it is sufficiently obvious.

Referring to the circumstances under which my experiments were performed, upon the healthy subject, the statement is made by Dr. Ames, that I myself took the phosphorus *at my meals*. He further alludes to the probable difference in the action of a poison, taken upon a full or an empty stomach, and then goes on: "Dr. Boling, it is true, did not take his dose, literally, on a full stomach; but taking a dose *just* before eating, is, practically, or so far as concerns the irritant action of a poison, whose action is *slow* in developing itself, the same thing as taking it during or immediately *after* a meal." He also mentions the fact, that it was soon after a meal that the largest dose, taken by either of my subjects, was administered. I find, however, on referring to my experiments, that in one instance a dose of a hundred drops of the saturated tincture was given at five and a half, P. M., which must have been as near giving it upon an empty stomach, so as to also insure a considerable interval, prior to the next meal, as could well be, without the omission of a meal.

In making the experiments upon myself, my reason for taking the phosphorus before the meals was, that I might not be so likely to forget it; and though it is stated that I took it just before eating, it was not

intended to convey the idea that, designedly, I took it just at the very instant before commencing to eat. The time at which I took it, in relation to my meals, may have varied from one minute to thirty minutes; and, in a few instances, I took the dose during the meal, having forgotten to take it before. Now it will be seen, that instead of taking it upon a full, I generally took it upon a very empty stomach, though without design; and, therefore, under the most favorable circumstances for the development of the poisonous action of an article, so far as refers to the stomach itself, not *very slow* in its operation. It is in a very quiet and matter-of-course kind of way, assumed by Dr. Ames, that phosphorus is *slow* in its poisonous action; but, as regards its action on the stomach, is it probable, in view of the mode of its operation, that such is the case? On the contrary, it would seem that it is very speedy. In one instance, in which I saw some unpleasant effects produced by a dose of phosphorus, *not* administered in the form of alcoholic tincture, however, their development took place very soon; indeed, it was rejected from the stomach almost instantaneously. Lobestein supposes that it acts upon the stomach, when unfavorably, as a "caustic;" and says, referring to certain experiments, "have they not demonstrated that phosphorus, introduced into the stomach or intestines of animals, undergoes a *combustion*, and produces an unnatural heat in proportion to the quantity swallowed, dissolved and burned." Orfila believes that its poisonous action is owing to the absorption of oxygen, and its conversion thus into an acid, which, like the other mineral acids, acts as a corrosive. Experiments, too, upon various animals, performed by Lobestein, Lobel, Bouttatz, Ginlis, Brera, Mugetti, Worbe and Burgos, all lead mainly, it would seem, to the conclusion, that its poisonous action is of a corrosive character. To any inferences, however, from such experiments, of course Dr. Ames would object, upon the ground that they were performed upon healthy subjects, if for no other reason. But whatever the rationale of the poisonous action of phosphorus, the remarks of authors, generally, upon the subject, would rather lead, I think, to the impression, that it is *speedy*, rather than *slow* in its development. They are unanimous, I believe, at least, and generally very distinct in the expression of the opinion, that in its stimulant operation it is *very prompt*. Dr. Ames, it will be remembered, says that this *stimulant* action is dependant on, and therefore of course *secondary* to its poisonous action. Thus he says: "Its poisonous effect is—highly stimulant, by reason of the local inflammation it excites." In regard to its stimulant action, Lobestein says:

"its action is *very prompt, intense,*" &c. Merat & Deleus, (*Dic. de Matière Médicale*) say, that "le phosphore est un des stimulans le plus diffusible et les plus actifs; comme tel l'action en est à la fois *prompte, vive,*" &c. Galtier says: "La phosphore est un excitant très actif, dont les effets sont *prompts, instantanés,*" &c. Dr. Ames himself, further says, speaking of the impossibility of effecting a very powerful sedation with it, and in explanation of the reason why—"when the dose is enlarged for this purpose beyond a certain point, a new and opposite action is *immediately set up,*" alluding in this to its poisonous action. Instead, then, of being *slow* in its poisonous action, *as stated* by Dr. Ames, it would seem, upon his *own testimony*, even, to be very prompt; and, as a matter of course, any arguments based upon, or inferences deduced from the assumption that it is slow, are necessarily fallacious, and may be passed without further notice.

In his paper on pneumonia, Dr. Ames, after describing the effects already several times alluded to, produced by phosphorus, in doses of from half a drop of the diluted tincture to two drops of the saturated tincture; such as "very sensible effects upon the head and stomach"—"nausea or vomiting, burning heat and a feeling of oppression at the epigastrium," &c., goes on: "So far in regard to its *activity merely*. But in estimating the proper dose, several other things require to be taken into consideration, having reference to certain peculiarities in its operation. "*First, the eccentricity of its action as a poison.* Thus, while it is said on good authority to have been given at times in doses of several grains, without doing serious mischief, at other times, less than one-tenth of a grain, (six milligrammes) has been known to prove fatal." It is then added in a note. "*Cazenave.* But this author thinks that in all cases in which such large doses have been given without harm, the article had undergone some change in its chemical state which rendered it inert. My own *experience* of its effects, most certainly leads to the same conclusion."

In his reply however he says: "My limited *experience* has led me to believe that this *eccentricity* is connected more with the condition of the stomach, when the medicine is taken, than with any difference in the modes of administering it. It is more apt for instance to show its deleterious qualities in cases of abstinence, as in the experiments on myself and as is ordinarily the case, in febrile diseases, but more certainly when to abstinence, is added an *irritable state of the stomach from disease.*"

Now it would seem that the *eccentric* action, if there is really any

eccentricity about it at all, spoken of in this latter paragraph, could not be such as was spoken of in the preceding one, but such an action rather, as was referred, in the paper on pneumonia, to the "activity merely," of the article. In that case, (setting thus aside the difference referable to the previous explanation of its eccentricity,) the difference between its effects under the different circumstances spoken of—upon the sick and upon the well,—would of course be only in about the same ratio, as that of other remedies, having a prompt local irritant action; and while, in view of the entire absence of all the symptoms, poisonous or otherwise, said by Dr. Ames to result from it, in his minute doses in disease, during its administration, in very large doses comparatively in health, few who might be aware of its nullity of action thus given, would anticipate from it, the symptoms said by him to arise from it in disease, as he gave it, many would probably bear in mind, and attach some importance to the fact, that under the circumstances referred to by him,—"*an irritable stomach from disease*," &c., such symptoms, as "nausea or vomiting, burning heat and a feeling of oppression at the epigastrium," would be more likely to be present than in health, and as almost essential evidences and attendants of the state itself, even when not developed by any poisonous or medicinal agent.

As an argument in favor of the utility of the tincture of phosphorus as used by him, Dr. Ames again reminds us in his reply, of the success of his "treatment of pneumonia by this as one of the chief remedies; in which the mortality is found in sixty-eight cases occurring in the course of a little over four years, reduced to less than three per centum."

Dr. Ramsey of Georgia, without phosphorus, and relying in a great measure on two of Dr. Ames' *discarded* remedies, calomel and tartar emetic, claims to have lost but one out of 170 cases of pneumonia; a trifle more than the half of one per cent., or a degree of success, if I mistake not, about five and a half times better than that claimed by Dr. Ames.

The profession is beginning to regard, with an eye rather of distrust the subject of statistics, notwithstanding its advantages in several respects. At a first glance, it would appear that the results must of necessity be accurate, but such is by no means the case, and there are many sources of fallacy connected with it; some of them perhaps unavoidable. One for instance, might grow out of a difference in diagnosis, pronounced by different physicians; may be, from greater accuracy in one than another, perhaps from caprice, or particular views entertained;

and in consequence of which it is impossible, that where the mortality might be equal as regards some particular disease, the apparent mortality might be greater, in the records of one than the other, and possibly even greatest where it was in reality least. For example, let us suppose the prevalence of some epidemic—say pneumonia for instance—with a tendency to the development with it of some severe complications of a prominent character; say pericarditis, meningitis, enteritis, or something else. Now under such circumstances, we may well conceive, that without any intention on the part of either to do wrong a very different diagnosis might be given in the same, or similar cases, by two different physicians. One for instance, might possibly regard the pneumonia as the primary disease, and the pericarditis, say, as secondary, and a mere complication, while the other, attaching more importance to what the former regarded as a complication merely, and secondary, might set this down as the primary affection. Under such circumstances, though the mortality might be equal, the case book of the one would show a greater number of fatal cases of pneumonia than that of the other, in consequence of the exclusion from the list by the one, of cases having a fatal tendency, such as were placed upon it by the other.

On the other hand, it is not impossible, that in reference to cases of a milder and less dangerous character, the diagnosis might be such as to make the disparity appear still greater. Thus, for instance, while he who had placed upon his pneumonia list such severe and dangerous complicated cases as are above referred to, might exclude from the same a class of milder cases, such as had but little tendency to fatality, and set them down as bronchitis or catarrh; the other, who had excluded from his pneumonia list the complicated cases having a fatal tendency, might, it may be from a more searching and accurate diagnosis, or some other cause, place these milder ones upon it. Thus while, in the one case, the apparent fatality, as regards the disease in question, would be augmented in the practice of one, by including among the number, cases having a fatal tendency, and excluding others of a milder character, having no such tendency, by a reverse of this double process in the other, it might be diminished, by excluding from the list the cases in question, likely to be fatal,—placing them under some other head,—and including the milder ones, which were placed under another head by the former. Let us say what we will about the possibility of accuracy and uniformity in diagnosis, such differences do occur; and as they may occur under ordinary circumstances, it is but reasonable to suppose, that where preten-

sions are made to greater success than ordinary, in a particular disease, or the claims of some favorite remedy in its treatment are to be sustained, the advantages of this—let us say, accuracy of diagnosis—might possibly not be lost sight of. Such instances, at least, of difference in diagnosis, I repeat, are not rare. A gentleman, under somewhat peculiar circumstances, was taken sick. His disease, by the physician who was first called to see him, was pronounced a severe pneumonia. On the fifth day of his attack, when he was worse than he had at any time previously been, I was called to see him. After a most careful examination, to me the case did not appear to be one of pneumonia, though the patient had some catarrhal symptoms. A professional friend gave me the particulars of a somewhat similar case. The physician first in attendance pronounced the case to be one of pneumonia; while he himself, after a careful examination, was led to the diagnosis of bronchitis. Now here are instances in which *milder* form of disease would appear in the case book of one physician as pneumonia, and in that of another physician under a different caption; augmenting, in one instance, the apparent success, as regards this particular disease, and diminishing it in the other. I will now allude to an instance of an opposite character. A professional friend informed me that he and another medical gentleman had each a patient in the same establishment, both affected with the same disease, and pronounced by him pneumonia. The other physician, however, did not call it pneumonia. Both patients died. On the case book of the one, his case appeared on the pneumonia list, and among the fatal ones. On the case book of the other, his would be placed under some other head,—“catarrhal fever,” if I remember right, was the name given it,—and of course, his pneumonia list, in regard to success, would gain by its loss.

In the number for November, 1854, of the *New Orleans Medical and Surgical Journal*, are some remarks from the emphatic pen of the Editor, which may be quoted, as to some extent evincive of the rising feeling in the profession on this subject of statistics:

“No compliment that one can bestow upon himself—no self-advertising formula, can equal the statistical. He who cures ninety-nine in a hundred, must of course be a better doctor, than one who cures but forty-nine. Figures cannot lie. One who is his own statistician, and is addicted to ——— the practice of reporting an incredible low ratio of mortality, should not forget that quacks have in all ages used this same kind of argument, namely, figures, whereby to establish their superior skill, and the infallibility of their panaceas.”

It will be seen from the references already made to the articles of Dr. Ames, that he entertains very grave apprehensions, that serious mischiefs might arise from the use of his tinctures of phosphorus, even in the small doses in which he speaks of them, and his remarks are suggestive of great, very great caution in their administration. Moreover, his quotations from, and references to authors, in regard to the dangers of the remedy in other forms, "are numerous and emphatic." All who have written on the subject of phosphorus are ready to admit that, as is the case with many of our best remedies, it is a dangerous article, and agree in recommending caution in its administration. It is but proper that a writer, when treating particularly of an active and dangerous remedy, should be reasonably anxious to impress upon his readers the necessity of care in its use; and we find that there is no known active and dangerous article regarding which such cautions are not to be found; though it really does seem that in the present instance, Dr. Ames is strangely urgent. Indeed, both of these points—the danger and the dose—are handled by him in such a way, owing, no doubt, to exaggerated impressions in regard to them, as to be almost irresistibly suggestive of the idea, whatever the injustice that the admission of it might do him, that he was—unknowingly to himself, it should be supposed—oppressed by a heavy doubt or uncomfortable foreboding—say, rather, a latent consciousness—that his doses might possibly not be found, on a scrutinizing trial, to be so dangerous as his imagination had been worked up to depict them, and that he would prefer that the ordeal should not be made.

Speaking of the doses which he says were recommended by certain authors, he remarks: "In these doses, small as they may seem, it is spoken of in many instances as a dangerous and uncontrolable remedy, and cautions against mischief from it, are every where numerous, urgent and impressive. Dr. Chapman," he goes on, "referring to doses of the one-sixteenth of a grain, says, 'whatever may have been the degree of its utility, this appears to be *fully* balanced by the hazardous nature of the medicine, and the positive mischief which is acknowledged to result from it.'"

I quote from the article, "Phosphorus," in 2d vol. 2d edition of the Therapeutics of Dr. Chapman. Before he speaks *at all* of the dose, he says: "But whatever may have been the degree of its utility, it appears nearly balanced by the hazardous nature of the medicine, and the positive mischief which is acknowledged to have resulted from it. Even in

its moderate operation, phosphorus is described as stimulating the whole system," &c. As to the dose, he refers to a prescription of Hufeland, a seven ounce mixture containing two grains of phosphorus, regarding which the directions are, "Omni bihoris, cochleæ sumendum, aut plus, pro re nata." What he says otherwise, and all he says of the dose, that I can find, is this: "But whatever mode is selected, the *fourth* of a grain is the largest dose, and the whole amount should not exceed two grains in the 24 hours." I can find no mention made by Dr. Chapman, of such doses as the sixteenth of a grain.

Dr. Chapman, as stated, is referred to by Dr. Ames as authority, when urging the necessity of great caution in the use of phosphorus, but with what propriety as regards the method of administration pursued by himself, when we call to mind his doses, and the doses mentioned by Dr. Chapman, after the performance of a series of experiments, in which he had witnessed and become acquainted with its poisonous operation; and while in reality, himself urging the propriety of caution in its use. Now how long according to the dose and method of administration pursued by Dr. Ames, would it require for a patient to take the quantity allowed by Dr. Chapman for the 24 hours? There would be in the two grains thirty-two thousand of Dr. Ames' medicinal doses. Supposing the dose to be repeated every fourth hour—the intervals preferred by Dr. Ames—without omission, and it would take five thousand three hundred and thirty-three and one-third days, or fourteen years, seven months and thirteen and one-third days for its consumption. This, supposing the article were continuously given; but owing to the cumulative quality ascribed to it by Dr. Ames, he could not thus of course consistently venture to continue it, but would deem it necessary to omit it, much of the time, so that a longer period, say a matter of twenty or twenty-five years, would probably be required for its administration; while but little short of two years, aside from the additional time required for its suspension on account of the cumulative quality, would be required for the consumption of the quantity allowed by Dr. Chapman for a single dose; and he himself urging the necessity of caution in the use of the remedy at the time.

Dr. Ames seems strongly to suspect that great mischief may have been done with phosphorus, even in doses of from a hundredth to the twentieth of a grain, in times past, and rather insinuates that the cases were not reported. Physicians as a general rule, have not been backward in reporting to the pro-

fession, marvelous cases, or *marvelous effects* which they may have supposed to result from the remedies they have used, and it would seem scarcely probable that had any mischievous or appreciable results ever been observed from such doses, as the former at least, they would not have been reported, if for no better reason, on account of the perfect fervor of astonishment they would have been expected to create in the medical world. Indeed, in all my reading, prior to the paper of Dr. Ames, and aside from the works of the Homœopathists, I have met but one reference to doses so small as the one-hundredth part of a grain; though in the instance alluded to, and which will be more particularly mentioned hereafter, the dose is still small.

Again, in urging the dangers of the remedy, speaking at the moment however of its eccentricity, Dr. Ames remarks, "While it is said on good authority to have been given at times in doses of several grains, without doing serious mischief, at other *times* less than one-tenth of a grain, (six milligrammes) has been known to prove fatal." To this remark the following paragraph, presented to the reader on a former occasion, is appended as a note, referring, it will be seen to Cazenave, as authority for the assertion.

"*Cazenave*.—But this author thinks that in all cases in which such large doses have been given without harm, the article had undergone some change in its chemical state, which rendered it inert. Si l'on a pu dire qu'il a été administré avec inocuité, à la dose de 3, 4, 5, 6, décigrammes, on doit croire que dans ces cas il y avait décomposition, et changement dans son état chimique." Here is the sentence referred to, and in part quoted by Dr. Ames: "En effet, à la dose de 6 milligrammes, ou l'a vu *déterminer des accidens*, et l'on a pu dire" &c., as above.

Now it strikes me that there is nothing in the above sentence, to warrant the assertion of Dr. Ames, that Cazenave says, that in the dose of six milligrammes (less than the tenth of a grain) it has been known at times to prove fatal. Where the word "accident" is used in the French language, as implying death or fatality as far as I have observed, and I think that such is invariably the case, it has associated with it, the adjective "funeste" or "fatal," as for instance, "un accident fatal,"—"un accident funeste,"—or, "des accidens funestes," &c. Where it is used without such qualification, it will generally I think be found, merely to imply, grave, serious, alarming, or unlooked for symptoms, I will quote a few examples, in illustration, from different authors.

Il est probable, que donnant des boissons huileuses dans le cas où

le phosphore pris à l'intérieur, causait *des accidens*, où arrivait a les calmer." (Merat & Deleus, Supplément to Dic. De Mat. Méd. p. 555.)

The same authors, Vol. 5, page 274, of the same work, speaking of some of the peculiarities of phosphorus say that they, "exposent les expérimentateurs à *des accidens*, que du reste, n'ont rien de spécifique, et réclament les mêmes soins que les brûlures graves ordinaires." In the same volume, page 282: "La conduite tenir dans cette dernière occurrence, c'est-à-dire, en *cas des accidens* consisterait, on le sent bien à évacuer au moyen d'un vomitif," &c.

"Après l'ingestion de cette dose considérable, *les accidens* allèrent en augmentant, avec une progression effrayante; la malade devint entièrement sourd et aveugle; la respiration, s'embarassa, son pouls était misérable, sa peau se refroidit," &c. (Dic. De Méd. Vol. 26, page 569.)

"Ces préparations peuvent pourtant à elles seules, conjurer *des accidens* de la mobilité nerveuse, et quelques formes vaporeuses de l'ordre le plus élémentaire." (Traité de Thérapeutique par Trousseau et Pidoux, Tome, 2nd page 252.)

"Si le café ne neutralise pas chimiquement les agents que nous venons de citer, au moins prévient-il, et empêche-t-il leur puissance stupéfiante, et peut-il quelquefois faire cesser tous *les accidens* d'un empoisonnement." (Ib. page 470.)

The words in my former paper on phosphorus, alluding to certain effects, not of a fatal character however, said by Dr. Ames to result from his doses,—“but they”—speaking of my subjects—“took it under circumstances that were well calculated to favor the production of such an operation,” are translated by A. Dechambre, principal editor of the Gazette Hebdomadaire de Médecine et de Chirurgie,—“mais encore ils les prirent dans des circonstances bien propres à favoriser la production *des accidens*.”

Now some of these passages at least, appear to be of a precisely parallel construction with that of Cazenave, referred to by Dr. Ames; and if in the latter instance, “*des accidens*” means death, surely it cannot mean less or more, when found elsewhere, used in a similar manner. How ridiculous then in these French doctors to tell us; in one instance to administer oleaginous drinks to a corpse; in another to evacuate the stomach of a dead body, by means of an emetic; in another to treat, “*des accidens*,” (always supposing the words to mean death) as an ordinary burn; or again to tell us that the patient become deaf and blind, had embarrassed

respiration, a bad pulse, &c., when she must of necessity have been already dead.

The smallest fatal dose mentioned by Pereira, who has written so elaborately on every article of the *Materia Medica*, and whose examination of authors seems to have been so searching, and citations so numerous, is one grain and a half. After a careful examination of the authors in my library I find a reference to one solitary case mentioned by Lobestien, Lobel, in which the $\frac{1}{8}$ of a grain was believed to have proved fatal, and another, mentioned by M. Martin Solon, in which the $\frac{1}{4}$ of a grain is said to have produced a similar result. Pereira however,—as according to Dr. Ames, in his work, "*one might expect to find there, if any where*"—it is but reasonable to suppose regarded them as apocryphal, as he does not quote them.

Dr. Ames in the conclusion of his paper, says some right good things, about my not exploring his field, but preferring, "rather to look from afar off, and through an *à priori* telescope." It is not of necessity always the case, that he who may be nearest to an object, shall have the most perfect view of it. On the contrary there are circumstances under which, another at a greater distance, might possibly see it to better advantage. Such, it may be conceived possible at least, might be the case in the present instance; and that another, not having his visual organs dazzled by the prepossessions, with which Dr. Ames may have been affected, even at a greater distance, might possibly have a more distinct view of the object than himself, or one as good. Such an opinion is not a mere assumption of the writer, hastily expressed, but is supported and exemplified by actual observation in every day life. See Gilbert Blane, on this point remarks, "It is requisite for the forming of a clear, calm, and impartial judgment, that objects should be placed at a certain distance, in order that they may be seen in their relative positions and bearings, which the eye or mind of a close observer, or of a party concerned is incapable of taking in." Moreover the lights thrown upon a subject by an observer, might possibly it can be conceived, aid another to a better view of it than he himself may have had, as the vision of the bearer of the torch is often less assisted by the light he carries, than that of others who may be near. In the present instance, for example, the paper of Dr. Ames, from the fact of my having been led by it, to cypher out the size of his dose, which he himself had not done, may possibly have had some agency indirectly in shedding light to me upon the subject, which from the absence on his part of any precise investigations in this particular, might

possibly have been lacking to him; and of which moreover if he had the benefit, many will believe that the perception of the object in his mind's eye, would have been greatly modified.

It cannot be doubted however, that the impression received or opinion formed in regard to an object, may be influenced or modified, by the point from or the media through which it may be regarded, and the remarks of Dr. Ames, referred to, are suggestive of the propriety, in all cases, in estimating the degree of importance to be attached to the views entertained or given of a subject to bear these circumstances in mind. Let us therefore if possible, or as near as possible, examine the point from and the medium through which, the curative influence of phosphorus in pneumonia, was seen and examined by Dr. Ames; in doing which it will be necessary to travel somewhat from the written record.

In his article on pneumonia, Dr. Ames remarks of phosphorus, that it had already been employed in several of the phlegmasiæ, "and among the rest in pneumonia, but under precisely what circumstances, and with what success"—says he—"I have not been able to learn."

It was principally in diseases of a low grade of action, typhus and typhoid fevers, for example, and some affections of the nervous system, that phosphorus was recommended and used by members of the *regular* medical profession, in times past. But very little indeed, has been said by them of its use in pneumonia, especially so far as I have been able to learn. It *has* been used, however, very extensively, notwithstanding, in this affection; and with some, therefore, the remark of Dr. Ames, that "precisely under what circumstances, and with what degree of success" he had not been able to learn, will be read with surprise—seeing that these are very distinctly stated by those who claim to have long used it, and with great success—as great, even, as that claimed by Dr. Ames. It would seem but probable, in the absence of any intimation to the contrary, that Dr. Ames, like most other gentlemen of the profession of extensive reading and investigating habits, had made himself, to some extent, familiar with the medical literature of the Homœopathists; and he could scarcely glance at a work, at least a practical work, by any of the followers of Hahnemann, without having presented before him urgent pretensions of the *utility of phosphorus in pneumonia*, with the circumstances under which, in their opinion, it should be administered, given, as also statements of the degree of success attending its use. Among the symptoms said by them to be caused by it in the healthy, are "accelerated circulation of the blood," "increased frequency of the pulse," &c.; and the instances are numerous

in which it is directed by them in disease where there is a quick pulse, and when the circulation is accelerated. Aside, then, from any views entertained by the Homœopathists of its *modus operandi*, *should* the regular physician be influenced in his inference as to its action, from the circumstances under which it is regarded as indicated by them, it seems probable that in so far he would be led to the belief of its sedative operation. By Dr. Flieselmann, of Vienna, a Homœopathist, it would seem that it is employed almost exclusively in every stage of pneumonia; and there are perhaps few articles more used by the Homœopathists, or more confidently recommended by them in any disease, than phosphorus in this affection. Such being the case, from his own sense of justice, it will be a matter of regret, of course, to Dr. Ames, that he was not aware of their claims. It is not at all probable that these gentlemen would regard with indifference and without reclamation, even an unintentional appropriation of some of their very loudest "thunder;" and while others may think that it would be but right that even the "Devil should have his due," however small, they themselves will not, it is probable, be backward in claiming that Dr. Ames' "new medical fact" was to them an *old* medical fact. Not only is it one of their favorite remedies in pneumonia, but, seeing, as above remarked, that it is particularly specified by them in many instances that indications for its use in this, as well as other inflammatory diseases, are "a quick, and rather full pulse"—"hard pulse"—"pulse rather quick and hard," &c., therefore, it may be repeated, according to their views, interpreted in language of legitimate medicine, and the regular physician, *they* must regard it as a sedative, quoad an accelerated state of the circulation, as a morbid condition.

It may not be deemed irrelevant, on the present occasion, while thus touching, as it were, on the Homœopathists, and their pretensions and claims, to refer more particularly to a work on pneumonia, by J. T. Tessier, from which it appears that the author, actuated by motives seemingly identical with those by which Dr. Ames was influenced by a somewhat analagous process, the details of which were, as in his case, step by step adopted, arrived at very similar conclusions and results. The circumstances may with the more propriety be alluded to, as affording another instance, gratifying of course, of the corroboration of the views adopted by Dr. Ames, by an observer at a distant point; and more especially in view of the similarity in the motives, the means and the results.

Dr. Ames and M. Tessier, both dissatisfied with the results of the treatment of pneumonia, according to the more generally received and approved methods, were, as all should be, anxious to improve upon them. The feeling was certainly a laudable one, and to whomsoever may discover a more successful plan of treating this disease, than such as are at present the most approved, the entire medical profession surely will be, as they should, daily grateful. Unfortunately, however, as has always been, even in our own day many of the modifications and proposed improvements in the treatment of disease, when weighed in the balance are found wanting; and promises and hopes held forth, too often prove delusive. How frequently does it happen in practice that a series of successful cases of some particular disease will lead the physician to hope that he may have fallen on a better plan of treatment, and in how many such instances is the delusion dissipated by a series of unfortunate results, coming as it were in turn. A very distinguished gentleman of the profession, who has charge of an institution in a Southern city, in which during the winter, a great many cases of pneumonia are treated, remarked to me recently, that for several years past, during every winter except the last, a considerable number of patients with this disease had died under his care, but that during the past season, under his usual treatment, he had lost but one or two. But to return to the analogy between Dr. Ames and M. Tessier.

The remedies with which Dr. Ames had become dissatisfied were blood-letting,* tartar emetic and mercury. Those with which M. Tessier

* Strangely discordant in some instances are the opinions of medical men as to the action and efficiency of our therapeutic agents in disease; and even in regard to those whose operation and influence would seem to be the most clearly and indisputably established upon the experience and belief of the profession for centuries, now and then there are presented dissenting doctrines; doctrines opposed to the almost unanimous belief. Professor Bartlett, one of our most philosophical and accurate medical writers, after devoting special attention to the subject, and apparently with great care and impartiality examining and estimating the testimony *pro* and *con*, upon it, thus speaks, in regard to the use of the lancet in pneumonia: "I have now done with the subject of blood-letting in pneumonia; and what are the results of the investigation through which we have gone? Is it proved, or is it not proved, that the remedy has a favorable influence over the disease? Most certainly there can be but one answer to these questions. The accredited and settled belief which has prevailed since the age of Hippocrates, in the efficacy of blood-letting in the treatment of pneumonia, is not an error and a delusion; it has its foundation in nature and in truth. And this efficacy is no longer a matter of belief merely, resting on the basis, broad enough certainly, but *not always to be trusted*, of what is called general experience. It is sustained by positive proof; by all the proof, and the only proof of which in the nature of things it is susceptible. It has the sanction of clear, intelligent, philosophical demonstration. It can neither be denied nor doubted except as the reality of the material universe, or the truth of the Newtonian system, may be doubted or denied by some eccentric or crotchety dreamer," &c.

had become dissatisfied were blood-letting and tartar emetic; mercury not having been with him a remedy much if at all used, prior to his experiments in search of a better treatment. By Dr. Ames "the method employed was that of occasionally leaving out of the treatment, one or the other remedy."—"The first step in the experimental enquiry encouraging me to proceed, blood-letting and mercury come to be used only in certain circumstances."—"Tartar emetic, for the most valuable remedy of the three, was continued longer." "The treatment of pneumonia then, which was finally settled down on, somewhat more than four years ago, consists in discarding the three principal remedies in common use, and substituting others in their stead," &c. The remedies *discarded* were antimony, blood-letting, and mercury; and the remedies *substituted* were, aconite and phosphorus, while quinine, blisters and opium, which of course were of the old remedies, were continued.—With M. Tessier, after *he had discarded* blood-letting and tartar emetic, his former remedies, aconite, phosphorus, and bryony were the articles *substituted*. He says: "Après l'étude préalable des écrits de Hahnemann, et de ses disciples, j'ai lu quelques uns des recueils ou sont consignées les observations particulières de malades traités d'après le nouvelle méthode."—"Je me hasardai chez un amené à la rémission par les saignées à *substituter le phosphore* au tartre stibié que je administrais, en pareil cas."—"Je résolus alors de diminuer peu à peu le nombre des émissions sanguins au début du traitement, et de ne point attendre la rémission pour recourir à la méthode Hahnemannienne.—Je diminuai donc, une, deux, trois, quatre émissions sanguins chez les malades qui se suivirent, rapprochant toujours au début, l'administration des nouveaux remèdes. Je commençais par un dose d'aconit suivie d'une dose de bryone au but de douze ou vingt-quatre heures, et faisant suivre la byrone du phosphore. Moins je saignais, et plus les malades étant soulagés, après l'administration des doses infinitissimal."

Let it be remembered that the paper of Dr. Bartlett in which this occurs, is upon the "*Certainty of Medicines*;" that pneumonia was the disease, and blood-letting in its treatment, the first and principal remedy, selected with which to exemplify it.

Dr. Ames, on a mooted question as to the probable efficacy of his doses of phosphorus, invokes what he calls "the test of direct observation;"—"the actual experience," of himself and others who have used them; but with what consistency, in view of his virtual repudiation of such a test, in his discarding of blood-letting in pneumonia, a remedy so long and so universally sanctioned in it, by the *experience* of the profession. How can he consistently call upon others to rely upon the experience of himself and a few others, for a few years only, as a reliable "test of direct observation," in regard to statements that were, to say the least, such an aspect of improbability, in the face of his own virtual denial of the reliability of such a *test*, based upon the *experience* of the medical profession since the days of Hippocrates—for more than two thousand years?

At length, the lancet is, as by Dr. Ames, entirely discarded: "Je me decidai enfin à ne plus saigner et à recouvrir d'emblée à la médication Hahnemannienne."

Dr. Ames, alluding to aconite, says: "The best effects of this remedy in pneumonia are exerted in the first stage." M. Tessier, also, after his adoption of the "nouvelle methode," seems to have regarded the use of this remedy as most beneficial in its action, in the early stage; generally, indeed, to have commenced the treatment with it.

In regard to phosphorus, Dr. Ames tells us: "If the medicinal qualities of aconite adapt it more especially to the first stage of pneumonia, so it may be said, those of phosphorus recommend it more particularly in the second and third stages." M. Tessier's *experience* led him to a similar conclusion. He used the phosphorus, only after the aconite, and found it, in pneumonia, "utile dans les inflammations locales menacant à passer à la suppuration."

Of the cases collected in *four* years by Dr. Ames, *two* only proved fatal. With M. Tessier, "depuis plus de *deux* ans *un* seul, a succombé." *Other cases proved fatal, but somehow or other, he does not count them.*

Between Dr. Ames and M. Tessier there is also a seeming sympathetic correspondence of thought or sentiment, on other points, somewhat analogous to each other, if not identical. Speaking of his views in regard to the action of phosphorus, Dr. Ames says: "And here, in alluding to this discrepancy, I wish to say, that I am fully sensible of the *responsibility* of uttering, as a new medical fact, that which is opposed to standard authorities in medicine, and to the established opinions of the great mass of the profession."

M. Tessier, speaking of his newly adopted views, says: "Ce n'est point, en effet une légère *responsabilité* que celle que pesé sur un médecin alors qu'il va substituer dans le traitement d'une malade grave, une méthode nouvelle à celle qui a pour sanction, l'expérience universelle."

There is, however, a difference, more seeming than real, perhaps, between the doses of phosphorus recommended by M. Tessier, and by Dr. Ames; the former, giving somewhere from the decillionth to the millionth of a grain at a dose, I believe, and the latter, as has been shown, as much as the one-sixteenth thousandth of a grain at a dose: admitting, for the moment, that all the phosphorus contained in the half-drop dose of the tincture given by Dr. Ames is really received by the patient, and that it is not, (though in all probability it is,) in a great measure, or entirely lost in vapor, or chemically changed, when the

solution is mingled with the water, preparatory to its administration. Making due allowance for the latter circumstance, there is in all probability no great difference in the dose; and if it should be thought by any that, notwithstanding this, possibly there may still be some inequality in the size of the fraction, there are certainly but few who would regard the difference, practically, as of any importance, or as material whether the patient should take the millionth or the sixteenth-thousandth part of a grain, or even between the one two-thousandth ($\frac{1}{2000}$) and the one three-thousandth ($\frac{1}{3000}$) part of a grain; allowing Dr. Ames in this the full benefit of all he pretends to claim, but to which, however, as has been shown, he is not entitled, in estimating the strength of his saturated tincture at six grains to the ounce; and further, making no abatement whatever for the loss already explained by evaporation in giving it in water. Merat and Deleus, while condemning the use of the large doses—say from two to ten grains—taken by Vater, Desbois, Remer, Mentz Leroy and others, in experiments upon themselves, and administered to others—say “Woolf par exemple, qui n’employoit l’éther phosphore qui à la dose de quelques gouttes, and M. Poilroux qui donnoit le phosphore en potion dont chaque cuillerée n’en contenait que $\frac{1}{64}$ de grain” (a dose more than eighteen times as large as that of Dr. Ames,) “semblent être tombés dans un excès contraire, en ce qu’il ne permet guère d’apprécier le véritable propriétés de ce médicament, *allopathiquement considéré du moins.*”

There is also another difference, to which importance may be attached by some, though it may be looked upon as unimportant by others, between Dr. Ames and M. Tessier. While the latter makes frequent and open mention of Hahnemann, and his writings and doctrines, and speaks often of the “nouvelle methode,” and “les doses infinitesimal,” the former—“never says turkey once!”

An incident, apparently connected with the early history of the use of phosphorus, in the practice of certain physicians of our place, who are not regular homœopaths, (for by the latter it has been a favorite remedy as long as any have been among us,) is thus given by a gentleman who claims to know all about the matter, and who is himself a believer in the efficacy of the remedy; in short, a decided homœopathist. Two or three physicians attended in conjunction, several cases of pneumonia, on a plantation about three miles from town. The result in several instances being unfavorable, the proprietor stated that, should another case occur, he would send for a homœopathist; and another

case occurring in a short time, he did so. The patient recovered. The gentleman inquired of the homœopathist what were the remedies he used in such cases, and was answered, aconite, phosphorus and bryony. This information was communicated to the physicians previously in attendance, and about this time phosphorus is heard of as a remedy for pneumonia, in other hands than those of the pure Homœopaths, by whom, as stated, it had long previously been used in the place.

Many cases of pneumonia, it is but reasonable to suppose, would recover under a purely expectant treatment; and there are but few in the profession who would not be disposed to regard the instance above referred to, as of this kind. Indeed, it is but reasonable to suppose, that Dr. Ames himself must have been well prepared for such an impression from the mischievous consequences of which he had become aware, and of which he speaks in his paper on pneumonia as resulting from his earlier practice; and ready, almost, in lieu of something better that might be wanting, to adopt a somewhat expectant treatment; surely not to be startled because of a recovery without medicine. Still, under all the circumstances, it is not unreasonable to suppose, that in the adoption of the phosphorus practice, he may possibly have been influenced, to some extent, by the incident above related. Viewing, then, the question with the prepossessions thus, it may be, determined—regarding it, as it were, through “an à priori telescope”—it might be considered “no impeachment of the natural accuracy of his vision,” to admit the possibility that he may have seen wrong, and that he may have ascribed effects to the phosphorus, that were fairly attributable to his other remedies.—The homœopathic construction of the instrument, may fairly, it would seem, account for the lilliputian character of his doses; while in view of certain of the effects said by him to result from them, as a visual assistant, its powers would seem to have been such as to develop the degree of perfection implied in the couplet—

“ He hath keen optics, well I ween,
Who sees what is not to be seen.”

ART. II.—*On the Nature of Malaria, and Prevention of its Morbid Agency:* by JOHN GORRIE, M.D., of Apalachicola, Florida.

Continued from March number, page 634.

A rational plan of preventing the maladies to which we are subject, must be founded upon one of three things, viz: a knowledge of the physiological action of the cause, or of its physical and chemical properties, or of the circumstances under which it is generated. Of the mode of action of malaria, we can scarcely be said to know anything; of its chemical properties, very little; and it was, therefore, the more necessary, in an attempt to prevent its effects, to show the physical circumstances on which its production, diffusion and energy depend. With an accurate knowledge of the conditions indispensable to the existence of a malady, its prevention is so obviously within our power, that it is a wonder the simple means by which it may be effected should have been overlooked or misdirected. The consideration of these conditions and their consequences, has hitherto engaged our attention. In the remainder of this essay, I propose to describe a plan by which, aided by a proper personal discipline on the part of the individual to be benefited, malaria may be rendered innocuous.

The prevention of the morbid influence of malaria exerts too important a bearing over the happiness and prosperity of man, not to have engaged, at all times, his serious attention. Many devices, founded upon the generally received opinions of the chemical nature of malaria, or its supposed mode of action on the animal economy, have been proposed for this object; but they have been, for the most part, of simply reputed efficacy, or where real, have proved trivial and inadequate. None of them has been radical, and at the same time practicable in its operation. Though universally looking to the atmosphere as the source of the poisonous influence, no enlarged or judiciously directed effort has ever been made, like those upon water, to filter and deplete it. The means of preventing the morbid effects of malaria in common use, may be considered under four heads, viz:

First. The most prominent among these means are artificial fires. Heat being a powerful rarifier and exciter of aerial currents, and, when within a house, an efficient ventilator and diluter of gases therein contained, its salutary effect has been attributed to the substitution of pure in place of contaminated air. It must, however, be evident, from the general connection of malaria with the atmosphere, that its chief effect in this relation can only be the substitution of one impure air for another.

Something is due to the power of its carbonaceous emanations, in the form of smoke, of decomposing and forming new combinations of organic products. Carbon in every form is known to have an absorbent power for gases and vapors, and smoke consists of minute flakes of carbon, each of which acting like a sponge is capable of taking up a large quantity of any deleterious gas or vapor with which it may come in contact. It has already been explained how the beams of the sun, by expanding a combination of two vapors, may place their particles beyond the sphere of each other's attraction; so artificial heat may be equally effective in decomposing and dispersing the combination of malaria with vapor. The utility of heat, as a prophylactic against malaria, has been often practically illustrated, and its application commands, at this day, as much confidence as any other; but it is obviously better adapted to counteract the poisonous emanations producing the maladies of cold climates—such as typhus fever and cholera—than the malarial diseases of tropical and tropicoid regions.

Second. The growth of groves around dwellings has long been considered as imparting some security to their inhabitants against Malaria; and for this reason, as well as the shelter they afford from the sun, has been much encouraged. Trees are considered as disinfecting the air of Malaria by decomposing and purifying it through the large supply of oxygen they are known to furnish; but they are also radiators of heat, and it is probable that, presenting a temperature below that of the surrounding atmosphere, they attract and condense its moisture as the passing breeze sweeps through the multiplied and vast surface their leaves present. In this way Malaria is not only detained, but being deprived of its connection with aqueous vapor is rendered inert. Moreover, there is a strong attractive relation between the leaves of trees and electricity, by which the latter is condensed and absorbed from the atmosphere; and this possibly operates to change the character of Malaria.

Third. On the principle that Malaria may be chemically changed and decomposed, vapors and gases of various kinds have been used as preventives. Camphor, chlorine, acetic, sulphuric and nitric acids, &c., are powerful chemical solvents, and in the subtle state in which their particles are generally distributed, when exposed to the atmosphere, are probably disinfectors, and may possibly operate as powerful and effective agents in decomposing the still more subtle matter of Malaria. The principle on which they are used implies that it is only necessary to know the precise chemical nature of any poison, to be able to find some

counter chemical substance which would be a certain preventive of its effects.

Fourth. Gauze curtains, though used chiefly to prevent annoyance and suffering from mosquitoes, are thought to be also sifters of the atmosphere and interceptors and decomposers of Malaria. The reputed efficacy of this simple means of prevention lends a slight support to the idea that malaria is a volatile oil. Perfumers and pharmacutists, in collating the volatile oils which exist in small quantities and escape readily, are accustomed to place the flowers containing them in alternate layers within thin cotton fleece or woollen cloth wadding, by which the oil is attracted and absorbed, and from which it can be afterwards separated by distillation in quantities that, otherwise, would be unattainable.

In the same way, whatever power fibrous gauze possesses of neutralizing malaria must be attributed to the absorbent properties of the material for moisture, and, with it, the peculiar matter of Malaria.

But all these devices have been found too limited and uncertain in their operation to be looked upon as satisfactory preventives of Malaria. It was seen to be an evil widely extended, and it was deemed necessary that the remedy should be equally so; hence, under the influence of that infirmity which impels men as often to overreach as to fall behind a real point of utility, one has been sought which should effect a total subversion of the cause. To the success of a process so general, a power of altering nature, or at least a discovery of the precise nature of Malaria by an indentification of its properties with physical tests, would be indispensable. The possession of such a power would, indeed, enable us to remove the greater part of the physical evils to which we are subject; but, however gratifying to the pride of science, or beneficial to the human race, and attractive to the enthusiastic and benevolent, it might be, no claim can be laid to it, and even the hope of it is so obviously vain that the man of experience must at once consider it as visionary. Impressed, however, with this ruling idea, and overlooking in the importance of the end aimed at the vastness of the means required, nothing short of a radical subversion of Malaria at its source, has been deemed worthy of serious attention.

Regarded as an emanation from decaying organic products of marshy or moist ground, it must be conceded that the best means of preventing the effects of Malaria is to change the character of the surface and soil by removing these organic remains, draining off or covering them with water, and filling depressed grounds with pure earth. This is a remedy

equally obvious, general and effective, and, with the exception of some feeble attempts in cities to lower temperature by fountains, has hitherto constituted the only one acted upon by governments. Applied through sewerage and paving, as a part of a general system of cleanliness, it has probably been an adjuvant in lessening that premature mortality in cities, to which man in former ages and in temperate climates was so notoriously liable. But the proper enforcement of so radical a remedy is seldom practicable, and would be sometimes unadvisable; in the first instance because the extent of surface involved, particularly in rural districts, places it beyond human power; in the second, because it would render the soil useless for the purpose of cultivation. And its frequent inefficiency is attested by the recurrence of epidemic fevers in communities where it has been most urgently, and where, from the limited sphere of operation, it has admitted of being most efficiently enforced.

As *Malaria* acts through the atmosphere, it is apparent that its morbid influence might be as effectually prevented by depurating it, as by obviating its formation in the earth. But, as we have seen that its sources occupy portions of the earth's surface too large to admit of a useful interference with its generation, so the superincumbent portions of the atmospheric ocean which it contaminates are too vast to be acted upon by human power. It is then plain that no plan of preventing the influence of *Malaria* can be universal in its operation; nor even so general as to supersede care and attention on the part of the individual whom it is to benefit. But within a limited space, as a room, a dwelling house, hotel or hospital, these objections do not apply. It is quite possible to depurate such insulated portions of the general atmosphere of any malignant volatile substance they may contain; and, as will be presently shown, it is the air of such spaces, and of those alone, which does or ought to come in contact with the lungs of a person during the period of chief danger from *Malaria*. Instead, therefore, of any general scheme for subverting *Malaria*, I propose simply to place it in the power of every man to lower the temperature, below the hydrometric moisture, and depurate of miasma that portion of atmosphere actually used by him in respiration, during the period of danger from *Malaria*, by the refrigerative, condensing and disinfecting agency of ICE.*

*It is known to readers of recent Journals of physical science, that I am the inventor of a machine for manufacturing ice. This invention promises both in theory and practice to furnish ice to the warmer parts of the earth, at far less cost than it can be transported from the regions of its natural production. The data afforded by experiments conform sufficiently near with those

The purifying influence of cold on the atmosphere and its agency in subverting Malarial epidemics are generally recognized. Notwithstanding a law of definite duration, the diseases, whatever may be their forms or types, generally cease with the advent of cold weather, and more

recognized by science to authorize the conclusion that, with a machine on a large scale, a ton of ice can be made on any part of the earth for less than two dollars.

The principle employed in this invention is comprized in the physical law that air in expanding absorbs heat from substances which may be brought into contact with it, and the mechanical contrivance consists in applying this property of air to the conversion of water into ice. In carrying out the object of the invention atmospheric air is compressed in order that it may be in a condition to expand to its former bulk: this operation is accompanied by the evolution of heat which it is expedient to absorb and carry off, and this is effected by bringing it in contact with a jet of water. The compressed air, divested of its sensible heat, is then allowed to expand in such a way that, through the use of an intervening non-congealable liquid with which it is in intimate contact, it is made to withdraw the heat due to its expansion from water intended to be frozen.—The power employed in compressing the air is recovered, to a considerable extent, by the force exerted in its subsequent expansion, which force is re-applied to aid in condensing more air.

The construction of this apparatus consists essentially of a force pump, by means of which air is compressed or condensed—of a receiver or reservoir, into which the air is forced—of another force pump or engine, (connected with the former at the opposite end of a beam or crank common to both), in which the compressed air from the reservoir expands and exerts its force to aid in working the condensing pump—of a small pump to inject water into the condensing engine to absorb the heat disengaged by the air as it is compressed—of a similar pump to inject the uncongealable liquid into the expanding engine that its heat may be absorbed by the expanding air—and of a refrigeratory chamber or room, in which the water, in suitable vessels, is placed to be frozen, and which forms a reservoir for the uncongealable liquid; the latter being drawn from it to be forced into the expanding engine wheuce it is returned to the room after it has given out its heat to the expanding air.

Several causes have concurred to retard the practical application and general use of this invention. In the first place, it has been found in advance of the wants of the country. Like many other mechanical devices, it is useful only on a certain scale of magnitude; and large as would seem to be the want, in a warm country, of such an article as ice, it was found that the smallest size on which the principle could be advantageously applied, would produce many times as much ice as in the infancy of the invention, a place deemed suitable for its introduction would consume. Next, it was found that many of the physical principles involved in its operation, though apparently simple and known by name to men of science, demanded for their advantageous application, a more accurate knowledge than existed. In the course of experiment, others of importance were discovered. The necessary knowledge in regard to all of them, as is usual in mechanics, physical science could be acquired only by repeated experiments and partial failures.—In the third place, moral causes, always equally operative with physical, in advancing or retarding the process of human affairs, and which seem to be generally retarding attendants upon all attempts to unite science and lucre, have been brought into play to prevent its use. Over the operation of all these obstacles I have until recently been unable to exert a complete control.

Although some contrivance of the chief production of ice in warm countries is absolutely necessary to its universal use as a hygienic agent, yet the difficulties in applying an artificial one are not necessarily connected with, or are of but secondary importance in relation to the object of this paper. It is true, the application of ice must be limited or extended according as it is dear or cheap; but its use, in the manner proposed, is all that is essential to the grand object of preventing Malarial diseases. And, certainly, at the price at which it can be imported and stored in our Atlantic cities, and even in those along the course of many of our railroads, there is no serious obstacle presented to such a purpose; and more particularly to its application as a preventive of the most virulent form of Malarial disease—that of yellow fever.

particularly of frost. According to Dr. Drake,* they find a natural termination, if the other conditions favoring their production are deficient, at the temperature of 65° F., and in the tropics and sometimes in our own country, they cease as epidemics before this temperature is attained. It must, however, be admitted that fevers in some situations are occasionally encountered after cold weather, and even frost; but these cases may be attributed to the latency of the poison taken into the system before the change of weather, or perhaps, to a re-development of *Malaria* under a return of warmth. With these exceptions, the cessation of fever under reduction of temperature, is too frequent, sudden, and complete to be assigned to mere coincidence with natural termination, or any other cause than cold. Nor can there be any reasonable doubt of the adequacy of cold to produce such a result. As a powerful disinfectant and antiseptic, preventing all organic decomposition, it must destroy or prevent the production of matter from which we do not doubt *Malaria* is generated; and by its condensing properties must decompose that already in the air, by separating it from moisture and other volatile but condensable products.

Ice in warm countries, independent of any hygienic property, is always highly valued. Viewed at first as a luxury, it soon becomes, when used as an article of dietetic regimen, an indispensable necessary of life. Its use, better than any other substance, counteracts the lassitude of body and torpor of mind, the loss of appetite and inordinate thirst which the long continued heat of tropical countries induces. Its influences over the sentient and respiratory functions are alike highly agreeable.—Used as a beverage it gives tone to the weak stomach, and restores the functions of the nervous and muscular system when languid, to their pristine vigor. From the bracing effects of air, naturally cold, on the body and mind; its influence in promoting the exercise of both, and imparting general vivacity and health, there can be no doubt that ice employed to cool the air of respiration could exert similar pleasing and salutary effects. If to these properties of ice we can impart to it a power of decomposing *Malaria*, it would scarcely be easy to conceive of a substance more truly valuable to man.

Ice has three properties which eminently fit it for the purpose of refrigerative ventilation, viz.: it maintains a permanently low temperature, it is an energetic absorber of heat, and in the process of solution it renders a large portion of heat latent. As a means of producing change of

* Diseases of the Valley of the Mississippi. p. 712.

temperature it is rendered by these properties quite as effective a motive power in causing an ingress of air, and hence a ventilation in a limited space, as a body heated as much above as it may be below the surrounding medium. Every particle introduced into a room of higher temperature than itself, acts as a cause of ventilation and refrigeration, more or less efficient, according as the difference of temperature between it and the general atmosphere is greater or less, or the position it occupies conforms with an advantageous use of the laws of nature brought into requisition. The absorption of heat from the air with which it comes in contact, condenses it and tends to form a vacuum which must be filled from the surrounding atmosphere. If placed in an elevated part of a room the increase of specific gravity which the absorption of heat, and consequent condensation imparts to the air, must cause it to descend; and if provision be made for the escape of this air at its lowest point of descent, a constant circulation of it may be maintained. From these considerations of the properties of ice, and their operation upon air, it is evident that to produce a practically refrigerative ventilation in a house or room, it is only necessary to bring the two substances, first in contact with each other; and second, so that the ice, after withdrawing heat from the air, may diffuse and dismiss it from its influence, and thus cause it to give place to a fresh portion.

Lowering the temperature of such a quantity of air as may be required for respiration, is an important property of ice, but it is not the only nor the most valuable one for the purpose in view. A more important one is that by which it condenses the vapor, which enters so largely into the composition of the atmosphere at high temperatures, and by which Malaria is held in suspension, is transmitted from place to place, and upon which, it is probable, all its poisonous energy depends.

Nor is it unessential to remark, that the hygienic influence of ice upon air is not limited to a reduction of its temperature, or the condensation of the hygrometric moisture it may contain. As the process of congelation is known to be to free water from impurities, both in solution and admixture, by precipitating them below, or forcing them above its surface, so the solution of ice exerts a purifying influence upon air by absorbing gases more obnoxious to human health, than any deleterious matters contained in water in its ordinary state. In this way, ammonia, sulphureted hydrogen and carbureted hydrogen are known to be dissolved; and as the putrefaction of animal and vegetable substances is considered to be an oxidation of their elementary constituents, it is

probable that ice, in the process of melting, may, by attracting oxygen, divest them of their offensive and deleterious character. If *Malaria* be of the nature of a volatile, what can be so effective a separator of it from air or vapor, as ice? As a powerful antiseptic, the properties of ice are well known. Animal and vegetable matters may be preserved in it, without a particle of change, for unlimited periods of time. In this respect, its properties far exceed those of common salt, nitre, corrosive sublimate, arsenious acid, alcohol, creasote, the essential oils, and all other well-known preservers, or retarders of organic substance from decay.

Though no connection has been traced with certainty between electricity and *Malaria*, it is scarcely possible to suppose that these powerful agencies exert no influence over each other, or that the former in one state does not render the latter more active, or in another tend to decompose it, and at the same time increase or diminish the susceptibilities of the human body to its effects. Electricians are well aware that ice, at low temperature, is a good non-conductor of electricity, and, consequently, a good electric. Achard, of Berlin, observed that a cylinder of ice, excited by friction, was capable of electrifying a prime conductor, so as to attract, repel, give shocks, and present all the phenomena of common electrical machines; and Faraday has since found that its insulating action is still more decided towards galvanic electricity.* †

Ice, at every temperature, may be considered a reservoir of electricity; and, from its fusion from the contact of a warm current of air, must, like the friction from the air of the bellows on the electrophorus, (mentioned in the note,) excite, and at the same time carry off a large quantity of this energetic fluid. As there is an undoubted affinity, and, possibly, an identity between electricity and vitality, is it unreasonable to suppose that ice, in the act of solution, may set free a large amount of vicarious or absolute vitalizing force? Experiment shows that ice, applied to the surface of the human body as a purifying substitute for water, imparts a peculiar, pleasant and healthful glow to it, increases greatly both the inclination and capacity to physical and mental action,

* Adams' Essay on Electricity. P. 44. Edinburgh Philosophical Journal.

† Before I heard of Achard's or Faraday's experiments, I had paid much attention to this subject, and made many experiments, partly with a physical and partly with a physiological object. In one, made in New York in the winter of 1852-3, I found that a disk of ice, at a temperature of a few degrees above the zero of Fahrenheit, excited by a cat's skin, and also by blowing on it strongly with a bellows, formed a good electrophorus. With a plate of eighteen inches in diameter, and an insulated metallic sole of fifteen inches in diameter, I obtained sparks an inch or more in length, and charged a quart Leyden jar to the point of spontaneous discharge, by transferring to it the electricity collected from about twenty contacts of the sole with the excited ice. The physiological observations are referred to in the text.

and infuses a general energy far beyond the power of water, or any other known substance. Is it, then, visionary to assume, that the solution of a certain quantity of ice by the air used in respiration, would impart an electrical and vivifying influence to it, which would aid the human system in resisting the causes of Malarial disease?

The mechanical contrivance by which I propose to take advantage of these properties of ice, so as to effect a refrigerating and depurating ventilation, is so simple, not to say natural, that those who fancy complexity is always necessary to the attainment of important results, will probably look upon it with contempt, or at least pass it by with indifference. Such men being alike incapable of making or appreciating an improvement, I do not expect to either amuse or instruct; but I have full faith that the reflecting—those who are accustomed to observe that the greatest effects in every department of science and art are generally made by the simplest means—will give it a patient examination. My whole process consists in, first, suspending an ornamental mantle vase, urn or basin, in which the ice is placed, by chains, like a lamp or chandelier, from the centre of, and close to the ceiling of a room. Next, over this vessel an opening is made in the ceiling from which a pipe is extended, between the ceiling and the floor above, to the chimney of the house. It is made to enter the chimney for an important auxiliary purpose, which will be presently explained; and through it and the pipe, instead of the doors and windows, all the air, as far as possible, required for respiration or the combustion of lights, ought to be received. As a free and unimpeded current of air must be allowed to pass through the room, so as to maintain thorough ventilation, and prevent the pernicious effects of the *foetid* exhalations arising from the human body and combustion, there must be, third, a similar but shorter pipe, with a self-acting adjustable valve, communicating with the interior of the room, on a level with its floor and the open air.*

* It is very desirable and almost indispensable to this or any plan for effecting a cooling ventilation in warm climates, that the houses should be constructed with a proper regard to insulation.—Without due attention to this important consideration, it would be impossible to effect the object but at a cost which would render it generally impracticable.

This is a subject which seems to have engaged very little forethought, on the part of either the builders or the inhabitants of houses, in warm countries. Neither appear to know or to care for health or comfort in dwellings. All may be aware of the general truism, that pure as well as fresh air is essential to the preservation of health, in every climate; but no one seems to attach so much importance to it, as to think that its maintenance should enter as a principle into the construction of his house, and should form a part of his domestic government. A little experience, indeed, assures every one that the healthy exercise of the functions of life cannot be maintained, in any place of residence, without some contrivance for procuring currents of fresh air, so as to make up

In such an arrangement, the external and fresh air is attracted through the first mentioned pipe to the upper part of the room, in consequence of the partial vacuum formed around the ice; and thence, after

the deficiency of oxygen, which is so rapidly consumed by respiration and combustion. In temperate climates, and more particularly in healthy situations, the requisite abundance and purity of fresh air is easily obtained by natural, or at least very simple processes of ventilation.—Merely opening the apertures of houses, such as windows and doors, is sufficient for every valuable purpose; but in the tropics and tropical regions, such natural ventilation is the grand source of human evil. Over these regions, throughout the whole or the greater part of the year, as well as over large portions of the temperate zones, during the summer and autumnal seasons, the atmosphere is contaminated with Malaria. Compelled by the heat of the weather and the natural wants of respiration, to admit air in the greatest abundance, but cut off from enjoying it in a pure state, the inhabitants of these regions can secure health only by artificial processes for purifying it, and at the same time ventilating their dwellings.

In cold climates, the great desideratum in the construction of houses is to fit them for the exclusion of the external cold, so that it may be practicable to raise the internal temperature by as small a consumption of fuel as possible. The principles applicable to the insulation of cold being nearly the same as to those of warm bodies, the construction of houses for the attainment of either object should be also nearly the same. With an equal attention to these principles, and an equal willingness to incur the necessary expense and labor in moderating the temperature and lessening the moisture of the air admitted within the dwellings, with some attention to the other and obvious hygienic rules, it would be as practicable to obtain comfort and security in warm as in cold countries.

But these principles are not regarded, at least in the Southern and rural portions of the United States. The houses are either of a slight, temporary hut or tent like character, made of scantling or boards; or, if more solid, are rudely made of logs, with interstices open to the passing breeze. Both kinds seem more adapted to a semi-barbarous, nomadic agricultural population, than to a people advanced in civilization, skilled in the arts, and permanent in their habits.—They are generally so defective in construction for even the purposes of simple comfort, that while they offer but a slight obstruction to the external temperature, they insulate or retain that once received or generated within them.

It is obvious that for the purpose of excluding external heat, or the preservation of internal cold, the construction of houses should be on a very different principle. In place of the thin screen from the sun's rays of pine boards an inch thick, which commonly form the walls of a house in the Southern States, there should be thick walls of a good insulating material like stone or bricks; or, what would be better, double walls, like an ice-house. In place of numerous openings for the admission of winds, it should have no more windows and doors than are sufficient for the admission of light and its occupants; and as windows are the great channels through which heat is transmitted into or out of a house, there would be a great gain, in an economical point of view, by making the sashes double, as is practiced in Sweden and Russia for the purpose of excluding cold. The roof should be well insulated, and all the walls shielded from the direct rays of the sun by verandahs or piazzas. In such a building it would be found as practicable and cheap to cool it by ice, as to warm it by fuel. This would be the case, supposing that the depression of temperature in the one was required to be equal to the elevation in the other, and that the cost of ice was double or treble that of fuel: in the former, the whole of the material may be applied directly in attaining its object; in the latter, a large portion is inevitably lost from the necessity of letting the products of combustion escape up the chimney.

Independent of the advantages of thick walls and a diminished number of openings in a house to an economical use of ice as a refrigerator and ventilator, there are some facts which seem to prove that they may act as direct barriers to Malaria. Dr. Rush informs us, that in the epidemic yellow fever of 1793, (works vol. III. p. 83) a number of families, who shut up their front and back doors and windows, escaped the disease; and Asalini, in his essay on the plague, says that the inhabitants of the citadel at Cairo have always escaped from the most malignant attacks of that disease. At New Orleans, it is a popular opinion that the inmates of the parish jail enjoy

being cooled, it is dismissed in a diffused shower, like a cataract, to the floor, to be discharged by the lower pipe. The mathematical proportions in the area of the pipes, and the mechanical construction of the apparatus, must conform to certain well known natural laws and artificial rules, which need not be described on the present occasion. In regard to them it is sufficient to say, that the quantities of air received and discharged will be proportioned to the area of the pipes, the elevation of the room above the surface of the earth, and the difference of temperature within it and that of the external atmosphere. •

The pipe for the admission of air is made to enter the chimney, for various reasons. First, because experience has shown that the contamination of the atmosphere from Malaria diminishes rapidly upwards; and that, at a certain height from the earth, and all over its surface, it is perfectly pure. If such a height were accurately ascertained, and means were devised for drawing all the air required for respiration from its superincumbent strata, it is obvious man might, in every climate, secure to himself an exemption from the effects of Malaria. This is impracticable; but it is fully within his power to draw air, for the purpose of respiration, from an elevation in the atmosphere in which, to a greater or less, and, ordinarily, a considerable extent, it is free from the impurities which abound in it at the surface of the earth, by making the chimney the channel of communication between the external air and that of the room in which he dwells.

But the chief advantage of such an arrangement is derived from the chemical affinities of the soot which, under ordinary circumstances, lines chimneys. Carbon, it has been already intimated, is a powerful absorber of the vaporous and gaseous products of animal and vegetable decomposition at common temperatures, which it yields again when heated; and hence, it is powerfully anti-putrescent. Soot consists, in far the greater part, of carbon, in a state of minute division; and is, therefore, in a favorable state for purifying a current of air drawn through or around it, by absorbing any gases or miasmatic vapors it may contain. Its affinity for volatile oils is shown by their constant presence in wood soot, and by its power of destroying the peculiar odor of many of them by

an entire immunity from Malarial diseases; and, though the opinion is not literally true, they are found to have an extraordinary degree of exemption from them. The security of people under all these circumstances, must be owing to the insulation that the thick walls and small number of apertures in the buildings afford, or to the exclusion of the poisonous miasm by the high walls with which they are surrounded.

contact.* Incapable of change itself, at natural temperature, it is only necessary, when saturated with gases, vapors or volatile oils, to subject it temporarily to moderate heat, by occasionally making a fire in the chimney or hearth, to drive off these volatile matters, and to re-fit it for a renewal of its absorbent properties. In this way, the ordinary flues of a house may be made powerfully auxiliary to our proposed mode of producing a disinfecting ventilation; and, conjoined with the action of ice, may decompose and absorb the miasmatic and infectious impurities existing in the air during the prevalence of Asiatic cholera and typhus fever, as well as those of ague, yellow fever, and the diseases that are looked upon as more emphatically the effects of *Malaria*.

No scheme for any object, however landable, can be of value, if, from any cause, it is impracticable; and one for the present purpose will be of very limited usefulness, if the ability to apply it does not come within the means of men generally. The want of a pure ventilation, where existing at all, extends to all, and the supply should be concurrent. The pecuniary ability of the majority of men being small, the material used for such an object should be cheap, easily procurable, and easily replaced when exhausted; and as their judgments in relation to such matters are weak, the principle on which it is applied should be self-acting. Other articles than ice, as solutions of frigorific salts, might be found to act as depurative and refrigerative ventilators; but I have chosen ice, from a conviction, carefully founded on experiment, that besides its superior chemical properties, it is, whether furnished by nature or art, by far the cheapest and most available material. It is, however, not the less necessary, before proceeding any farther, to make some calculation of the cost of its application.

Proposing to prevent the occurrence of diseases arising from the respiration of impure air, by the substitution of artificially pure air, the first enquiry into the subject should be; what is the quantity of air required to be substituted, or in other words, what is the quantity of air consumed in respiration by a human being? Observation shows that this quantity differs very materially under different circumstances. The mature and the robust require more than the young, aged, and infirm; the

* Considering this subject within the true scope and range of Malarial etiology, and indeed the only one left to our investigation, I suggest to others, and propose to myself, on the occurrence of a Malarial epidemic, to apply this property of carbon to an experimental examination of the atmosphere, with a view to discover whether or no essential oils form part of its composition at such times.

male than the female. "Under all circumstances more is used by every individual during the day than at night—in health than in sickness—in a high than a low temperature—during muscular exercise than at rest—after a meal than when hungry—when the attention is drawn to the function of respiration than when unconscious of its performance.* In the mere process of respiration a full grown man takes into his chest, on an average, from three hundred to four hundred cubic inches of air per minute; and of the oxygen contained in this quantity about half is exchanged for carbonic acid, and rendered wholly unfit for use again.—Taking the larger amount, and considering the whole as vitiated, he will employ in respiration ($400 \times 60 \div 1728$) nearly fourteen cubic feet of air an hour, or twelve and a half cubic yards, or twenty-five pounds of air per day. But air is required for and is vitiated by the moisture he exhales with the carbonic acid from the lungs, and with all the exertions and exuviae from the rest of his body. When it becomes saturated with moisture and unfit for absorbing more, it becomes unfit for respiration. On this account, it is found that the quantity of air absolutely necessary for respiration must be mixed with six or eight times its volume of pure air to maintain an adult human being in comfort; so that to fulfil all the purposes of ventilation, he will require a daily change of air equal to ($12\frac{1}{2}$ cubic yards \times 8) one hundred cubic yards, or about two hundred pounds.

Now to fulfil the objects set forth in this essay, so as to secure an adult human being from liability to take Malarial disease, one hundred cubic yards, or two hundred pounds of air, must be daily disinfected or rendered respirably pure. This purity is to be obtained, as already explained, partly by exposing the air to the chemical affinities of carbon, but chiefly by divesting it of certain portions of its temperature and hygrometric moisture by ice. A consideration of the quantity of soot which may be necessary for the former purpose is not, perhaps, of much consequence, and, if necessary, requires experimental examination; but the measure of cold for the latter is important, and fortunately may be easily ascertained, by calculation from well-known data.

It has been shown that a temperature of 60° F. in that portion of the atmosphere in which we dwell, is necessary to the generation of Malaria; and it is a probable ground of inference that at this temperature,

* Griswold. Use and Abuse of Air. P. 66.

the affinity between it and air is greatly weakened or totally ceases.* It must be regarded also, as a favorable coincidence, and perhaps a natural evidence that poison in the atmosphere is incompatible with this temperature, that it is the one in which man enjoys athletic strength, and at the same time, maintains the heat of his body without any extraordinary exertion of his system; and is, therefore, the one most favorable to mental and physical activity.

Now supposing that the mean temperature of the infected air is that of the equator, or about 80° F., and it is desirable to cool it twenty degrees or to 60° F., and supposing also, that ice is the refrigerating material used, it is necessary to ascertain the quantity of the latter which the quantity of air used in respiration, at the former temperature, is capable of thawing. According to the best authorities on the properties of heat, the quantity of caloric which ice in the process of melting absorbs and renders latent is 140° F.; and, therefore, if its specific heat and that of atmospheric air were equal, to cool 200 pounds of air 20° F. would require $(200 \times 20^{\circ} \div 140) 28\frac{8}{14}$ pounds of ice. But the specific heat of air is only the one-fourth (2669) that of ice (or rather water), so that instead of $28\frac{8}{14}$ pounds, only $(28\frac{8}{14} \div 4) 7\frac{2}{14}$ pounds of ice would be the absolute quantity consumed in cooling and disinfecting the quantity of air necessary for the healthy respiration of one person during twenty-four hours.

But even this small quantity of ice may be greater than necessary for the purpose. It is well known that there are portions of the day in which the atmosphere of the most noxious regions appears to be harmless,

* It is proper to notice that moderate, and more particularly temporary reductions of atmospheric temperature, seem to have very little power in moderating the malignancy of prevailing Malarious diseases, or in lessening the liability of individuals to be affected by them. Although intermittent fever, and indeed, every acknowledged form of Malarial disease, require a higher temperature than 60° or 65° F. for their generation, it does not follow that they must cease at these temperatures; and in fact, they rarely abate till the thermometer has fallen and remained for some time below these points. It is even possible that mere reduction of temperature by bringing the elements of Malaria into closer proximity to each other, may increase its virulence. And it may be that though a temperature of 60° or 65° F. is necessary for the combination of the volatile constituent of Malaria with moisture, yet simple reduction from a higher to this temperature may enable the latter to hold enough of the former in solution to maintain its malignancy. To check an epidemic by reduction of temperature, the proper principle of action would be not simply to carry the reduction to 60° F., but to some ten or twenty degrees below the dewpoint; for it is by the condensation and separation of moisture from the air, rather than the privation of heat that Malaria is decomposed. My plan supposes that much moisture is absorbed from the air in its passage through the chimney; and, that carried into full and complete operation, it commences with the first occurrence of uncomfortable solar heat, and of course with the first generation of the noxious matter, so as to prevent any extraordinary accumulation of it in the apartment by adhesion to solid substances, &c.

and when it would be supererogatory to attempt to purify it. Though the poison from decomposing organic matters is formed during the day more abundantly than in the night; yet observation shows that if it is not wholly inert in the former, it is vastly more active in the latter period. The obvious cause of this difference of energy is the difference of temperature between the two periods of time, and the mode in which it operates is to be found in the different affinities exerted between the constituents of Malaria and the air under changes of temperature. During the day the direct rays of the sun warm the surface of the earth so intensely that the superincumbent air is heated and expanded probably to a degree that decomposes the constituents of Malaria—leaving them free to be acted upon by that law of uniform diffusion and dilution which belongs to all simple aerial bodies. In the night, on the contrary, the earth, no longer receiving the sun's rays, becomes colder than the air, and condenses and precipitates the vapor towards its surface; and in its descent the law of combination with volatile and condensable matters renews its action, and Malaria is formed again in a concentrated state. It is obvious that if such a change takes place, Malaria must have a more ready access to the lungs, or other organs through which its influence is propagated, in the night than in the day.

This tendency of Malaria to return to the surface of the earth and to manifest a more general and intense malignity at night is both a popular and scientific belief. In many Malarious regions the ground floors of houses though used during the day for a variety of domestic purposes, are never occupied as sleeping chambers by those who can avoid them; and, on the other hand, sleeping apartments near the top of a lofty house are sought as affording an immunity from that degree of Malarial intensity which produces the intermittent and remittent fevers, and some share of security during the earlier part of a yellow fever epidemic.

With the physical change in volatile atmospheric matters from temperature, there may be a physiological cause concurring to increase the liability to take Malarial disease during the night. Besides the increased energy of the poison, it is probable that its action is aided by the greater susceptibility of the human system to injurious impressions during sleep. Little has been effected by physiological science towards elucidating the different degrees of influence exercised by respiration on the animal economy during the two radical states of its existence—wakefulness and sleep; yet, seeing that the relative quantities of carbonic acid and nitrogen in the expired air vary at different hours, it is reasonable to suppose

that the function of respiration is not the same during the waking and sleeping periods of life. We know that in the sleeping state all the principle phenomena characteristic of life are diminished. Sleep announces itself by a diminution in the activity of the senses, by an inability to make voluntary movement, by a less frequent but generally more lengthened respiration, and by an evident languor; and observation shows that the whole nervous system is enfeebled, the circulation is less rapid, calorification is lessened, and direct but temporary debility is induced. In this state of diminished energy of the whole system, it is probable that the functions of respiration and nutrition lose some of their vital and healthy power of discrimination, and offer less resistance to the impressions produced by noxious external influences, while the functions of excretion are less able to eliminate a poison once received.

But, in whatever way the effect is produced it is indisputable that the chief danger from *Malaria* exists in the night; and even that its pernicious influence is but slightly or not at all exerted during the day. It has been long recognized as a fact of which endeavors have been made to take advantage in almost every country subject to *Malaria*. It was in the night that "the angel of the Lord went out and smote in the camp of the Assyrians one hundred four score and five thousand men."—The great plague of London was found peculiarly obnoxious at night and on one particular night four thousand persons were seized with it. Dr. Rush tells us in the history of the epidemic fever of 1793, that a great proportion of those who were affected were attacked in the night. In all the recent epidemics of Asiatic cholera, a large proportion of the cases have been observed to occur between midnight and the rising of the sun; and in some places the fact has caused "so great an alarm that many persons have refused to go to bed, lest they should be attacked unawares in their sleep." Many of the people of Charleston look with a horror, that must be regarded equally superstitious and well founded, upon the night air of the Malarial districts adjacent to their city; and, though they have no apprehensions of evil from visiting them in the day, view a necessity for passing a night in them as equivalent to a sentence of death. It is a many times told tale of travelers, that the inhabitants of Rome have such a dread of the evening dew, from its supposed saturation with the *Malaria* of the neighboring Pontine marshes, that as soon as it is perceived they shut themselves up in their houses, and practice every precaution in their power to prevent its admission into them. Lind, and other British naval surgeons, tell us of numerous instances

where seamen have left their vessels and rambled throughout the day about the shores of the most pestiferous districts—hunting, shooting, washing linen, wooding and watering—without suffering the slightest indisposition. And it is a common practice to employ the crews of vessels anchored in the bays and harbors of the West Indies, in unloading and loading and transferring to and from the shore the cargoes; and all this, if done during the day, is not attended with danger of taking Malarious disease. But where the same men, in similar situations, have imprudently remained, and more particularly slept on shore during the night, it has often occurred that all have been attacked with fever. In our Southern cities, when Malaria is prevailing in its most concentrated and malignant state, as when the yellow fever is raging, people from the adjacent country do not hesitate to visit them and transact business or pursue pleasure during the day; but sad experience has taught them that they must carefully avoid doing so during the night.

From all the considerations heretofore presented it is certainly probable that if a man had it in his power, and could be sufficiently wise and decided to confine himself at the first appearance, and during the continuance of this annual source of pestilence, to an atmosphere artificially cooled and depurated in the manner described, he might exempt himself from the influence of this afflicting scourge of humanity. But such a privation from the customary habits of life, or a pursuit of most of the occupations by which we are constrained to get our daily bread, is not indispensable to this security. The recognized difference between the influence of Malaria in the day and in the night, shows that nothing more is absolutely necessary to maintain immunity, even in the most deadly regions, than confinement during the night, and particularly during sleep, to an atmosphere rendered respirably pure.

If these views are correct, it is then obvious that instead of $7\frac{2}{14}$ pounds of ice being required to effect an adequate purification of air, one-half the quantity, or $3\frac{8}{14}$ pounds, might be sufficient.

But in estimating the quantity of ice required to depress and maintain the air of a room at a proper temperature, it is necessary to consider that some heat is absorbed through windows, walls, ceiling and floors. Regarding the room intended for the sleeping apartment of one person as of a thousand cubic feet capacity, insulated in the manner represented (in the note) with one window, it will be reasonable to add to the above quantity of ice about an equal quantity, or, so as to make the amount $7\frac{1}{14}$ pounds for the night, or $14\frac{4}{14}$ for twenty-four hours. Supposing,

however, that to meet these sources of absorption, and all the purposes of a cooling and disinfecting ventilation, twice the quantity of ice assigned as necessary for one person should be required, the object would still be attained at as small an expense as an equal temperature could be artificially raised, and would be within the ability of every prudent and industrious person.

Although we know nothing certain of the elementary character of *Malaria*, yet it is generally believed, and has been abundantly proved, that the circumstances essential to its existence are organic decomposition, moisture and heat. The study which has been applied to these circumstances, assures us that their influence is equal and mutual; certainly, no one, nor, is it probable, any two of them are capable of producing Malarial disease; but the co-existence of all is undoubtedly adequate to generate all its forms, and in both their sporadic and epidemic characters. Such being the operation of nature, in regard to the production of *Malaria*, the prevention of its effects is more clearly within human power than any other diseases, because the relationship between them and the local causes is more generally understood, and more easily interrupted. The obvious means within this power are a subversion or prevention of one or more of these local causes.

The peculiar preventive device I have proposed, though local and limited, acts upon two of these causes in such a way, that, with proper care and caution, it will be found adequate to attain its object. The solution of ice, it has been shown, is attended with a number of chemical actions on air, none of which is injurious to human health, and all are concurrent towards one grand effect—the decomposition of *Malaria*: mixed with warmer air, it reduces its temperature, and in the same process causes it to deposit its combined vapor in the form of water; and, at the same time, it absorbs other volatile matters and extraneous gases contained in the air. Receiving, as part of the device, the air intended to be acted upon through a long conduit like a chimney, lined with carbon dust in the form of soot, must also tend (from the affinity of carbon for vapors and organic oils,) to decompose *Malaria*, and operate powerfully as an auxiliary to the ice.

Before concluding this subject, I deem it proper to remark, that in addition to this or any other comprehensive plan of preventing the morbid effects of *Malaria*, it is proper that persons liable to their influence should subject themselves to a suitable personal discipline. It would, no doubt, be always the most secure preventive against a pre-

vailing epidemic, for unacclimated persons to remove beyond its influence; but this, in most cases, is attended with an expense and sacrifice of business, which it would be of vast advantage, and which it is precisely the object of this essay to render unnecessary. If this is impracticable or undesirable, and my mode of prevention should be deemed effective, it is still absolutely necessary that the exciting cause of disease should be avoided. In paludal localities, or in cities, during the prevalence of malignant epidemics, the liability to disease is often increased by mental, moral and physical causes, that ought to be avoided. The depressing or exciting passions, intemperance in the indulgence of diet and drink, exposure to the sun, dews or rain, or any thing that depresses the physical and mental powers, are sufficient to predispose the system to be attacked by a prevailing disease. The injurious influence of night is found to extend to the early part of day; and, therefore, a principle of personal discipline should be to avoid exposure to the external atmosphere, before the sun, in his diurnal progress, has dispersed its vapors, as well as after his setting has allowed them to condense and accumulate near the surface of the earth.

Such are the means by which I propose to preserve the health and promote the comfort of the inhabitants of Malarial and warm climates. But, besides these objects, there are other advantages to be derived from the generation of a cool and dry air within buildings, more attractive, perhaps, to the sordid, than health, from their subserving purposes of gain and economy. The temperature which it is proposed to maintain, would be eminently favorable to the production of that mental and physical energy, which is alike necessary to the ingenuity, labor, perseverance and nice economy, without which success is unattainable in the mechanic and manufacturing arts, and thus admit of their introduction into warm countries. Equally applicable to ships as to buildings, it might enable the hardy mariner, who contributes so much to our wealth and pleasure by transporting from shore to shore the rich productions of the mephitic groves and swamps of the equator, to pursue his occupation free from the apprehension of the most dreaded of evils—a premature death from loathsome disease. As animals or fruits, when divested of life, may be preserved entire with all their juices, in a low temperature, this principle of producing and maintaining cold might be made instrumental in preserving organic matter an indefinite length of time, and thus become accessory to the extension of commerce. By teaching the importance of constructing buildings in a form adapted to our com-

fort, of solid materials, and thoroughly insulating them, it would promote the introduction of a better style of architecture and its collateral arts, than at present exists.

Nor are the security of the physical economy and the increase of the material comforts of man, the only advantageous effects that would arise from moderating atmospheric temperature in the houses of warm countries. The moral, intellectual and general scientific benefits arising from it, would be at least equal to the physical and commercial. There is no phenomenon in nature more intimately connected with a ready understanding and correct investigation of almost every scientific process, or with the performance of many of the most useful arts, than the maintenance of a pleasant atmospheric temperature. Among other kinds of knowledge, it may be made, by giving immunity from Malarial disease, eminently auxiliary towards enlarging the boundaries of geographical discovery; and, in connection with it, administer, by extending the channels of commerce, to both the delight and benefit of mankind. Protected by such a means of maintaining health and comfort, we might dwell with safety, and pursue any business of life, in the harbors or cities of Vera Cruz, Havana, Sierra Leone or Batavia, or explore at our leisure the æstuaries and geographical characters, or collect and examine the natural productions of New Guinea, the Crinoco, the Niger and the Amazon, or the sources of the Congo and the La Plata.

A PALACHICOLA, *January 1, 1855.*

ART. III.—*A Lecture on the Nature, Diagnosis and Treatment of Hip-disease: by* LOUIS BAUER, M. D., Member of the Royal College of Surgeons, of England; Corresponding Fellow of the Medical Society of London; Member of the Pathological Society of New-York; late Surgeon of the Royal Orthopædic Institution of Manchester, G. B., and Surgeon of the Brooklyn Orthopædic Institution. Delivered to the Medical Society of the State of New Jersey.

Mr. President and Gentlemen:—The magnanimous invitation you have tendered me, both a personal stranger to you and a foreigner to this great empire, that I may be enabled to state my opinions on one of the most important subjects of surgery, is the best evidence of your high conception of medical science. You have adopted Abernethy's notion,

that medical science is a republic, one and indivisible; that you recognize within the boundary of our noble profession neither Aliens nor Know-Nothings, but citizens who deserve well for science; you disavow all national exclusion in matters of science. On the contrary, you acknowledge the necessity of scientific intercourse of nations, and the rights of individuals. This position, Mr. President and gentlemen, enables your society, as a body scientific, to rise far above the imputation of profane eliquism. With my sincere thanks for your polite invitation, Sir, I combine the sentiments of profound respect for your professional magnanimity.

The rapid advance of surgery during this century, has of necessity brought also the study of articular diseases within the compass of scientific research; for, perusing the literature of this subject, we find that all civilized nations have paid their proportionate tribute towards the rational understanding of those affections, and that the most eminent surgeons have nobly engaged in elucidating those hitherto obscure subjects. With the aid of the microscope and the chemical vial, we have penetrated the minute alterations of the concerned tissues from disease, and we know its effects as to its physical and chemical composition. Pathological anatomy and comparative pathology have taught us the course these diseases commonly take; and experiments have extended our knowledge as to the susceptibility of certain articular structures to morbid affections. In fine, most important facts have been brought to light, and a method of investigation has been adopted, which must lead to still more valuable discoveries. But what has as yet been the practical result of all this scientific valor? Neither has our diagnosis become more certain, nor our treatment more effectual! Our knowledge is decidedly augmented; our practical deficiency, however, remains in *statu quo ante*. We may pitifully look at former periods, which confounded the lesions of the synovial membrane, the articular cartilage and the diaphyses of the bones; but can we, in the face of most deplorable facts, boast of the infallibility of our diagnosis? And as to the general result of the adopted mode of treating articular diseases, is the deformed and emaciated condition of our late patients, pronounced cured, not the most lamentable testimonial of our practical incapacity, not to speak of a large fraction of fatally terminating instances. In referring to incipient affections of the joints, are we able to check always their progress, or to control their termination? Do they not mostly take their course, in spite of leeching, cupping, blistering, and

the whole train of cauterics, combined with the most rigid and constitutional treatment? But in fixing our attention on advanced cases, we find that ordinary means forsake us, and that we are unfortunately driven but too often to the very *ultima ratio chirurgorum* in removing with the diseased parts the entire member, thus mutilating the poor sufferer for ever. Where is the surgeon who would not deem himself lucky and successful, when benevolent nature allows his patient to escape from his ardent scientific treatment with ankylosis and deformity? These observations on articular affections in general, appertain peculiarly to hip-joint disease. We dare not conceal the fact, that errors in diagnosis are committed every day, and even by men whose scientific attainment and devotion, place them, in professional estimation, far above mediocrity. Neither talent, knowledge, nor acuteness of observation is safe from diagnostic deceptions. It is, therefore, evident that the present basis on which our diagnosis rests, is either inefficient or untenable. Hence the imperious necessity of a better and more reliable diagnostic ground. Sir Benjamin Brodie's opinion on the difficulty and uncertainty of diagnosis Hip-disease, pronounced some ten years ago, is in full force at this very day. And as to treatment of morbus coxarius, our therapeutical nullity is amply exemplified by the prolonged sufferings of our patients, the crippled and enfeebled state of the cured, and the fearful ratio of victims that fall a prey to this disease. An instance reveals itself to my memory, that may serve as an example of the blessings afforded by the present method preferred for the treatment of this malady.

On the 26th of June last, I was invited to see the child of one of our opulent merchants at his country seat in the vicinity of New-York city. There I found a little girl of about seven years, who had been suffering from the ravages of hip-joint disease for the last three years.—She was in the most emaciated condition imaginable, and I presume that her weight did not exceed twenty pounds. Her skin was dry, hot, wrinkled and in every respect like parchment. Pulse uncountable, small and feeble, movements of the heart scarcely perceptible; respiration heaving and frequent; digestion almost annihilated; neither appetite, nor egestion of fæces, tongue coated with diphtheritic exudation; abdomen tender and tympanitic; prostration of vital powers; the cadaverous countenance bore the expression of horror, especially when dressing commenced. The affected extremity was drawn up, adducted and inverted; the pelvis in an oblique position; the environs of the joint were discolored and perforated

by a number of sinous openings, through which a considerable quantity of foetid discharge issued. Corresponding with the large trochanter, there was an extensive ulcer, through which a piece of dead and apparently moveable bone projected. In this condition the little sufferer lingered eleven days longer, when death terminated the distressing scene.

Taking in consideration the external circumstances of the case just related, we may form a fair estimate as to patients belonging to the humbler sphere of life. In the former we find the little sufferer surrounded by affluence, care and affection; the most skillful surgeons coöperate with tender nurses to save a darling child from premature dissolution. What may we anticipate when want, filth, and gross neglect compete with each other in accelerating the decay of the afflicted!

Certainly we may read in the Hospital Records of the *American Medical Monthly* that the expectative method hitherto in common use, had effected within the walls of Ward's Island Hospital almost miraculous cures. My own experience, however, limited as it may be, does not permit me to credit such extravagant statements, even if the source were more reliable, and the *end in view less suspicious*. In a practical point of view, it would be more than useless to relate all those doctrines which have been broached upon hip-disease by earlier writers, and which abound in the works of Ford, Sabatier, Camper, Portal, Dessault, Albers, Fisher, Boyer, Larrey, Rust and others. They have at present but historical interest. By the way, may be here remarked, that these doctrines were almost exclusively derived from clinical analysis of the symptoms attending hip-disease, and by no means based on such facts which I should consider reliable and stubborn. With the advancement of pathological anatomy, more especially through the efforts of Sir Benjamin Brodie, our information about the morbid condition of affected hip-joints gains better ground. This author demonstrates and is well supported by anatomical evidence, that the disease in question may originate in either the synovial membrane, or cancellated structure, and may consecutively involve the whole articular apparatus, leaving, however, undivided whether the articular cartilage be also susceptible to primary disease of any description, owing to its being without bloodvessels and nerves. Since then, bloodvessels in the strictly cartilaginous texture have been observed by Toyuboe and Kollicker, and the latter believes even, that it is likewise endowed with nervous fibres, more especially in younger individuals. At any rate the subsequent experiments and observations of Redfern, Ecker, Goodsir, Koelliker, Virchow, Ejnrlt, leave no doubt as to primary affec-

tions of articular cartilage, the susceptibility of which being, however, comparatively slight. Atrophy, softening, defibrination, hypertrophy and calcarization, form the principal ailments of articular cartilage.—There are, however, appearances indicative of structural alterations analogous to inflammation of the cartilage; as the closing of cartilaginous wounds by fibrous substance, the granular disintegration of its peculiar cells, the generating of fatty matter, and so-forth show.

Within these three structures hip-disease is said to generate exclusively. This is, however, an error which I shall try to correct.

There are numerous pathological specimens appertaining to hip-disease, in which the articular apparatus evidently has suffered least, while the periosteum of the ilium and femur is extensively destroyed, the bone being in a carious condition. The pathological museums in London abound with specimens of this description. On the other hand, specimens exist in abundance in which almost the entire joint, with its integral tissues are destroyed, and yet the periosteum of the adjacent bones is perfectly intact. This is a distinct difference of pathological appearance, for which there must be a cause to account for. It cannot be said that the burrowing and the extending of the pus, below the fascia lata, up and downwards has eroded the osseous surfaces, for if this was the case we would find caries of the ilium and the shaft of the femur more frequently than it actually is, as there is rarely an instance of hip-disease without such diffuse abscess. And in looking more closely into the circumstances of such cases, we notice often that the patient had enjoyed apparently good health before his hip-complaint set in; that the disease began with violent pains, extending over an unusually large space, and that it was mostly attributable to a fall or some other local injury. Hence I conjectured that in these cases the periosteum might have originally suffered, but subsequently involved the articular apparatus. This supposition I had lately the opportunity to confirm in a case, the items of which I had the honor of laying before the New-York Pathological Society. It concerned a little girl, seven years old. Four months previous to my seeing her, she had sustained a heavy fall on her left hip upon a stone. Before that accident she was in the enjoyment of perfect health, the appearances of which had not then vanished, after the most severe suffering of so long a period. Immediately after the fall, she felt pain on the injured place which rapidly increased both in extent and intensity. A short time had elapsed when the affected member assumed an inverted, abducted and semiflexed position in both the hip and knee-

joints. Inflammatory fever, pains, and sleeplessness, reduced her bodily strength and weight. Two months after her fall she experienced occasional chills when she grew easier, her night's rest being but transiently disturbed by sudden and shooting pains. By-the-by the extremity became inverted and adducted, and along the inside of the thigh a diffuse swelling made its appearance. At this juncture I saw the patient. The history and development of the case rendered it probable that periostitis had been the original disease, but the position of the limb, the immobility and tenderness of the joint, the retraction of the adductor muscles and the obliquity of the pelvis, left little doubt in my mind that the joint had been gradually brought within the compass of the disease, and the capsule most probably already broken. One circumstance was hereby decisive in leading to that diagnosis, namely, the rapid rupture of the capsular ligament, which does by no means give way so quickly, when the disease proceeds from within. I advised the free opening of the subfascial abscess on the outside of the extremity, preferring the latter place for the purpose of securing a fair inspection of the joint. Finally I determined to remove that portion of bone which should prove to be in a carious state. On dividing the fascia, a large quantity of a thin and brownish matter escaped. The neck of the femur was found to be carious anteriorly and superiorly. The anterior wall of the capsular ligament presented an opening into which I could easily introduce my finger, previously oiled, thus entering the articular cavity. The part of the femur below the neck being perfectly sound, I resected the latter with the *caput femoris*. This specimen carefully examined shows clearly that my diagnostic conjecture had been correct in every point. For there was superficial caries of the neck to the extent of about two inches, while corresponding to the perforation of the capsule, the cartilage had just commenced to be detached. The discoloration of the cartilage did not extend over two lines in each dimension. Around the head there was a moderate injection of the bloodvessels. In every other respect the cartilage, synovial membrane, and cancellated tissue were intact. In comparing the degree of disease within and without the capsular ligament, it became evident to me that the affection had proceeded in the reverse direction, or in other words, that original periostitis had complicated itself by extending into the capsule merely by its anatomical connection.

This is not the place to argue the objection raised against my operation by some prominent members of that distinguished scientific body; but I shall take another opportunity of doing so hereafter. Perhaps

the statement will be heard with interest, that the little patient is doing well, and is fast progressing towards recovery. As to the relative frequency of either species of hip-disease, namely—

Arthromeningitis,	}	Coxæ.
Arthrositis,		
Artrochondritis, and		
Periostitis,		

I have not yet collected sufficient matter to justify me in bringing forth any reliable figure. Perhaps we may never gain any conclusive statistics on the subject, the occurrence of which seems to depend on so many heterogeneous influences and accidents. However, in as far as I have been able to trace the source of such affections as befall the hip-joint, and the inferences I have drawn therefrom, I may be permitted to offer for your consideration. But I request you, Mr. President and gentlemen, to take them as mere conjectures, subject to correction; because, besides some facts, my opinion has, in a great measure, been formed on analogy.

It appears that arthromeningitis (synovitis) and periostitis coxæ, co-exist and occur, with other rheumatic affections, in the equinoctial seasons, in certain localities, as, for instance, near the sea-coast, in valleys abundantly supplied with streams, and sporadically after cold, while perspiring.

Arthrositis coxæ seems to be prevalent in large cities, in over-crowded habitations, in valleys characterized by swamps and stagnant waters.—The evaporations of newly cultivated land, vegetable and insipid food, may be also accused of exercising some deleterious influences upon the disease under consideration. At any rate, it is certain that mountainous ground is most unfavorable to this species of hip-disease, while it occurs very frequently in places on which intermittent fever is endemic, as, for instance in some parts of Ohio. There is no doubt that both diseases co-exist; but not rarely intermittent fever precedes tuberculis of the osseous system; and it would be of great scientific interest to learn whether fever-eakes, especially hypertrophy and fatty degeneration of the spleen, exercise any causal influence upon the origination of the tubercular form of hip-disease. I may here state, that I have made, or have been present at the post-mortem examination of six corpses, whose earthly careers had been terminated by tuberculosis of the osseous system, and consecutive caries, (two of tubercular hip-disease,) in each of whom enlarged spleen was found. In one case, referring to a boy of eleven

years, who had died of tubercular caries of the spine—the specimen of which I had the honor of exhibiting to the class of Professor Willard Parker—the spleen weighed eight pounds and some ounces, and was in a greatly disintegrated condition. All these patients had been previously suffering from ague.

In approaching the subject of diagnosis, I deem it imperatively necessary to give some explanation of what I consider to be hip-joint disease, in the strict sense of the term. This term implies an affection of the hip-joint, in which all the structures of the articular apparatus are more or less involved. Where there is but one tissue occupied by the disease, I give its appropriate name; and this verbal discrimination is by no means without practical value.

[To be continued.]

V

ART. IV.—*Chloroform in Puerperal Convulsions—Delivery by means of Instruments:* by JOSEPH B. COTTMAN, M. D., Parish of St. James, Louisiana.

On Wednesday, the 31st of January, 1855, I was called early in the morning to visit a negro woman in labor with her first child, on the estate of Col. John S. Preston: arrived about 8 o'clock; found the woman in convulsions, which had continued since daybreak, at intervals of every fifteen or twenty minutes. She was a short thick-set woman, very fat, about 18 years of age. The nurse reported that she had had some slight pains since midnight, at which time she was called to her. On examination *per vaginam*, found the neck of the womb dilated to the size of a dollar, the parts soft and yielding, presentation natural, respiration stertorous, pulse strong, full and bounding; took about a quart of blood from the arm; the pulse sank; the convulsions continued with equal frequency; the bowels had already been relieved by an injection. Ordered a combination of tincture of valerian, tincture of ergot, and tincture of assafœtida, to be taken every half-hour; cold applications to the head. Saw her again in the afternoon; convulsions have continued since the morning, but not so frequent; gave her ether by inhalation, with no effect. Left her, and procured some chloroform. Saw her again about 10 o'clock at night; convulsions continue equally as frequent, but do not last so long; gave her chloroform by inhalation, which immediately arrested the convulsions. Sat by her until midnight,

and whenever I saw any premonitory symptoms of a return of the convulsions, gave the chloroform. The quantity given in all was not more than a quarter of an ounce. In one hour from the first exhibition of the chloroform, her speech returned, and she asked frequently for water; her respiration became natural, and she fell off into a sound, and apparently refreshing sleep. At midnight, finding no return of convulsions, and no labor-pains, I left her, with directions to give her, from time to time, some brandy and water.

February 1st; Saw her early in the morning; had rested well through the night; no pains; no return of convulsions; respiration natural; intelligence good; asks for whatever she wants; pulse feeble. The child's head can be distinctly felt within the labia; continue the brandy and water.

I determined to deliver her with the forceps; returned home for the instruments, but lost much time in seeing other patients, and did not see her again until 3 o'clock in the afternoon, when I delivered her safely with the forceps of a monster of a child. Left her soon after, with directions to the nurse to give her the brandy and water frequently, and to keep her perfectly quiet until I should see her again, at night. Saw her again at 9 o'clock at night; found her in a very comfortable situation, in truth, doing as well as could be expected; pulse which had sunk had returned; skin good; respiration natural; no hæmorrhage; slight discharge from the parts of a sanious fluid. Gave directions to keep up the brandy through the night.

February 2d. Early this morning was called to see my patient, and found her in a sinking condition, after having spent, as the nurse said, a good night. I found her lying on her back, eyes turned up, pupils dilated, pulse scarcely perceptible, respiration stertorous, skin warm, no hæmorrhage; very little discharge through the night; had a free operation on the bowels in the night. I strongly suspected that the nurse had drunk the brandy and gone to sleep, and consequently had neglected her; continue the brandy and water as before. Saw her again about 2 o'clock, P. M.; found her in the same condition as in the morning. She died at 6 o'clock the same evening.

ART. V.—*Case of Double Womb; both impregnated: by* DR. I. T. KANNON, of Mississippi.

On the 28th of December, 1853, I was called in consultation by Drs. Horton and Montgomery, in the case of Mrs. ———, who had been delivered of a child early that morning. There were still all the external appearances of another child remaining, the womb being entirely empty, the placenta having come away. Owing to great prostration consequent upon the birth of the child, and the womb being empty, it was very natural to suppose that laceration had occurred, and the fœtus had escaped into the cavity of the abdomen.

On making an examination, I passed my hand into the empty womb, and found its walls solid, not the least appearance of a rupture having occurred. The probability of a septum with a double womb, suggested itself to my mind, and further examination revealed the correctness of this opinion. Looking upward and forward, near the edge of the symphysis pubis, I found the os uteri of another womb, with the head of the fœtus resting directly on the pubis. There was no effort making on the part of the womb to expel the fœtus, and the usual remedies failing to bring on the pains, an attempt to turn and deliver was tried; but the peculiar position occupied by this second womb, rendered it very difficult. As by this effort it was discovered that the fœtus was dead, we resorted to the crotchet and brought away a child weighing six or seven pounds.

This lady was the mother of five children, but never previously had both wombs been impregnated at the same time. The septum was horizontal, with the os tinæ of the lower and posterior womb in the natural position, the anterior considerably elevated.

ART. VI.—*Critical Remarks upon Double Uteri and Superfetation: by* B. DOWLER, M.D.

The *Gaz. Hebdom. de Méd.*, for March 17th, 1854, quotes a German journal in which M. Horl reports the case of a woman whom he delivered of twins, and who had a double-womb. In this case, the translation of which is copied from *Ranking's Abstract*, the French—"utérus bicorne"—is rendered "double uterus." Although the latter is not a literal translation of the former, it is probably more conformable to

anatomy, as there is no proof that the duplex human uterus is usually two-horned (*uterus bicornis*) as among certain quadrupeds.

“A delicate woman, *æt.* 30, was subject, during the first three months of her second pregnancy, to periodically recurring attacks of uterine hæmorrhage, which, although easily checked for the time, returned more violently in the seventh month. At this period, her abdomen was found very much distended at both sides, but level in the central region, from the umbilicus to the symphysis pubis. Percussion yielded a tympanitic sound in this hollow, and a dull sound on either side. An inch and a half above the pubis the uterus could be felt distinctly, through the parietes, dividing into two parts, of which the one on the right was largest. Both were convex on their internal, and somewhat concave on their external surfaces; and each resembled in appearance the normal gravid uterus at the full time. In each the foetal heart could be distinctly heard on auscultation, and the form of a child easily felt by external tactile examination. On internal exploration, the vagina was found quite normal; and at its roof the cervix was felt, short and broad, and having two ora uteri, through both of which the presenting parts of the children could be distinguished. Over the right os uteri was situated one of the placenta, and a portion of the other projected from the left os. On account of the violent uterine hæmorrhage, Hohl induced premature labor, and delivered the children by turning. The right placenta was spontaneously detached, but that on the left side adhered so firmly that it had to be artificially separated. The twins, which weighed three pounds, died almost immediately after their birth. Subsequent examinations of the uterus, with the sound and the finger, confirmed the correctness of the original diagnosis.”

Uterine duplication is the normal type for many mammals, chiefly among the Rodents, (*rodentia*, *rongeurs*) though its anatomical configuration is varied, being generally cornuated—each horn having, as naturalists affirm, double ovaries, two fallopian tubes, and two uterine orifices, which open into a single vagina. In the *Marsupialia*, the uterine system is singularly complex. Whether the double-uterus of the human female has for each division double ovaries and tubes, is, probably, little if at all known to practical anatomists.

The possibility of superfœtation or the fecundation of a woman already pregnant, has sometimes been denied, but upon insufficient grounds, as well authenticated examples prove its occasional occurrence. It is most reasonable to suppose, what, indeed, the facts seem to indicate, namely, that the second conception takes place soon after the first, probably before the ovum of the first descends into the uterine cavity.

Uterine duplication is suggestive of moral, physiological and medico-legal possibilities, of high concernment to society, touching criminality, divorce, inheritance and paternity.

May not this anatomical conformation be more common than is generally supposed? And may it not serve, in some cases of twins, to account for a plural paternity, as that of a white and that of a black child born at the same time of the same mother—instances of which have occurred, particularly in the mixed populations of the Southern States of this Republic, among both white and colored mothers, who have borne twins—the one a white, and the other a mulatto; or, the one a black, and the other a mulatto. In these cases, the confessions of the parties usually corroborate the physiological evidence.

The occasional persistence of menstruation during pregnancy, might, perhaps, be best accounted for by the existence of the duplex womb, in cases where one only had been impregnated. Yet the infrequency of known examples of double-womb, is incomparably greater than examples of intercurrent menstruation during gestation.

The following cases of superfœtation, very interesting in themselves, are added as possible examples of double uteri: In the *June* number (1854) of the *Southern Medical and Surgical*, edited by Prof. Dugas, of Augusta, Georgia, (page 348) is the subjoined history of a

Case of Twins of different colors: reported by A. F. ATTAWAY, M. D., of Madison county, Georgia.

“Mrs. C——, a white woman, the mother of three children, gave birth to twins on the 16th of January, an interval of an hour intervening between the births.

“The first born was very dark, and had every appearance of being of African paternity. Not being willing to suggest such a thing, I tried to explain the matter, by attributing the color to cyanosis. At the expiration of one hour, the second child was born, and had very light colored hair, fair skin and blue eyes, which made the contrast very striking.

“The condition of the mother and children was such that they required medical treatment for several weeks, during which time I marked the great difference between the children with peculiar interest.

“After the recovery of the woman and her children, seeing the African characteristics more and more developed, I asked the mother to give me a correct relation of the circumstances connected with her conception, &c.

“After some hesitation she gave me the following history of her case:—She said that five days after the cessation of her last menstruation,

she had sexual intercourse with the white man, whom she considered the father of the white child. Three days thereafter, making eight days after menstruation, she cohabited with a negro man, who she said was the father of her other child. She assured me that this was the only coitus she had with the negro man for more than one month after she menstruated. If this be true, she conceived at that time.

“The precise period of her other conception is less definite in consequence of the fact that she had connection with the father of her white child, at different times during the month following her last menstruation.”

In the *British and Foreign Medico-Chirurgical Review* for October, 1854, Robert Barnes, M. D., of London, in his Report on Midwifery, quotes from M. Thielmann (*Med. Zeitung*) the following case:

“A peasant girl, *at.* 25, had borne, at 20 and 23, girls. In July, 1852, she became pregnant a third time; menstruation appeared twice after conception. On the 26th of March, 1853, the first pains appeared, and next morning she was delivered of a girl, small but living; the after-birth came away normally. The lochia ceased in a few hours.—The secretion of milk was so scanty that the child could not be supported by it. Eight days after the delivery, the woman returned to her household duties; but she felt in her left side the movements of a second child. On the 18th of May—that is, fifty-two days after the birth of the first child—pains came on, and the birth of a second living girl followed.—From this time the secretion of milk went on so freely, that both children derived sufficient nourishment. M. Thielmann says this case was officially certified.”

The natural history and physiological anatomy of fecundation and conception, present many formidable barriers to the doctrine of superfoetation in the normal or single womb, and the more so as the double conceptions differ in point of time.

A few additional references may prove acceptable to the reader, by calling attention to a topic which has been little investigated.

In his magnificent work on *Obstetrics*, (1855,) Prof. Ramsbotham says:—“It would appear impossible for a woman to conceive of a second ovum, while the uterus already contains one. Many instances are on record of a woman conceiving in utero, while she retained an *extra-uterine* foetus in the abdomen. But some cases are reported, which, on a superficial view, would lead to the belief that such an occurrence might happen. Thus Maton has put on record, that a lady was delivered on November 12, 1807, of a boy, which was strong and hearty, and on the 2d of February, 1808, (not quite *three calendar months* from the preceding date,) of another boy, also at full time. And Desgranges has

reported another of a very extraordinary kind: A woman brought forth a living child at seven months, on January 20, 1780; but no milk was secreted, no lochia flowed, and the size of the abdomen was scarcely at all diminished. Dr. Desgranges was called in, in consultation, and he declared his opinion that there was another child in utero. Three weeks after her delivery, she felt the movements of the fœtus; and on July 6, 1780, (five months and sixteen days after the first birth,) she was again delivered of another living girl. The milk now appeared, and she was able to nurse the child. M. Cassan, in his thesis on 'double uterus and superfœtation,' adduces a case that occurred to Mad. Boivin: on the 15th of March, 1810, a woman *æt.* 40, gave birth to a female infant, weighing about four pounds. As the abdomen still remained bulky, Mad. Boivin introduced her hand into the uterus, but found nothing within it. The examination, however, led her to suspect that there was another fœtus, either extra-uterine, or contained in a second cavity of the womb. On the 12th of May, following, she was delivered of a second girl, not weighing more than three pounds, feeble, and scarcely able to support respiration. The mother assured Mad. Boivin that she had had no connection with her husband, except on the 15th and 20th of July, 1809, and the 16th of the following September. The only satisfactory way of accounting for these cases, is on the supposition that each of these women possessed a double uterus.—Professor Lobstein tells us, that he actually delivered a woman of two children, one a month after the other, and was able to convince himself that this was owing to her having *two uteri*, to each of which there was a distinct vagina. 'There is in the pathological collection at Vienna, a preparation of double uteruses, each of which bears evident marks of having been pregnant.' A case of double uterus, in which one chamber has been impregnated, is preserved in the London Hospital Museum," &c. (469-70) Other references are given by Prof. R. See, also, Velpeau's Midwifery, Beck's Med. Juris., &c.

The supposition that a double-womb is constituted by a simple septum, the organ being otherwise a unit, is attended with great difficulties in regard to the physiological anatomy of parturition. If there be but one true body and fundus, its expulsive force, both longitudinal and circular, must act equally on both fœtuses simultaneously; or, if only one side should be impregnated, parturition would probably rupture the septum, if it should prove much weaker than the ordinary normal walls of the uterus. Such an occurrence might have been expected in the case

of the lady mentioned in the preceding article, who had borne five children before a double impregnation happened—an occurrence of grave import, that might place life in jeopardy. But labor coming on in a duplex womb, one might act while the other remained torpid, or both might act at once, according to the period of ovulation, conception, gestation, and the oneness of each organ.

ART. VII.— *A Case of Tetanus produced from Vaccination; Cured by large doses of Opium.*: by JOSEPH B. COTTMAN, M. D.

On Tuesday, 31st of July, 1839, Isabella, a negress on the Orange Grove plantation, reported herself to the overseer as sick. She was sent to the hospital. Upon examination he found that she complained of a difficulty of deglutition, a rigidity of the muscles of the face, a sensation of stricture at the top of the sternum, with a rigidity of all the extensor muscles of the extremities. He ordered her a warm bath, and afterwards to be well rubbed; then an emetic of lobelia which vomited her slightly; the following day he applied a few dry cups along the course of the spine—all, however, had little or no effect, for the following day she had spasms, opisthotonos, a perfect rigidity of the extensor muscles; the mouth could not be opened by exerting the strongest force. He gave an enema composed of two ounces of the tincture lobelia, two ounces of the tincture opium, with a little sweet oil and milk; this had the effect of freeing the bowels and relaxing the system generally.—When I was called to see her, I found her very much prostrated from the effects of the enema, but sensible. She answered questions rationally. Ordered the spine to be freely cupped, and four grains of opium at night, a continuation of the enemata, but in smaller quantities; the following day to take a brisk purge of jalap. Two days afterwards I found her somewhat improved, but still a twitching of the muscles of the extremities, rigidity of the muscles of the jaws, inability to protrude the tongue; senses natural; says she feels no pain, yet complained of her arm being sore where she was vaccinated, which was cauterized freely with nitrate of silver the first day she came in the hospital, and afterwards a lye poultice to be applied, which formed an eschar the size of a silver dollar.

August 14th. It is now two weeks since the commencement of the attack; the wound on the arm is suppurating finely, and healing: the

twitching of the menses has almost entirely ceased for some days past. She is able to open her mouth easily; sits up; appetite good; sleeps well; the pills of opium to be continued, together with the enemata, both in less quantity.

This patient has taken up to this time about two ounces of solid opium, and two pounds four ounces of laudanum in the enemata. She continued to convalesce rapidly; in a few weeks from the time of the attack she was able to walk about, and came to my house, a distance of two miles from the plantation.

September 25th. She is now and has been for a month past engaged at work on the plantation as formerly.



ART. VIII.—*Post-mortem Examination in a case of Blue Disease, (Cyanosis, or Morbus Cæruleus:)* by B. DOWLER, M. D.

Madam R—, attached to one of the theatres, a widow, whose husband died of cholera about three months since, requested, through her family physician, Dr. Tiffin, that I should make a post-mortem examination of her child, a boy aged twenty months, who died suddenly and unexpectedly at six o'clock on the morning of the 27th of March, 1855. Mrs. R. stated that the deceased was her favorite child, for whose health she had sacrificed much of her living; that her physicians had not agreed as to the nature of the child's malady; that she was anxious to know, if possible, what caused its death; and, for this reason deemed a post-mortem examination necessary.

Apart from the great physiological and pathological significance of this case, the good sense, rational judgment and praiseworthy example on the part of the mother of the child, in the midst of bereavement, deserve commendation and publicity, giving to the case an enhanced interest, of deep import to society, worthy of all regard, moral, social and medical. Were reason popular, and stern duty paramount, the dead body would seldom be regarded with superstition and consigned to the vile worms, by inconsiderate friends, without a scientific examination, by which errors of diagnosis and treatment among the sick might be dissipated—by which family maladies and constitutional predispositions might be ascertained in their passive or nascent state, and their full development be anticipated, prevented or modified—by which the dead would contribute to the well being of the living—by which the

art of healing would cease to be the art of guessing—by which Death would prove to be a great and useful teacher, instead of being a repulsive, unmeaning and worthless corpse.

Without further remarks, an outline of the case of Blue Disease under consideration, will be presented to the reader, the previous history of which is very imperfect.

Dr. Tiffin regarded the disease as a congestive one, affecting the liver and other abdominal viscera; the regularity of the pulse and of the heart's movements, did not, to him, appear consistent with heart-disease; besides, excepting occasional diarrhoea, the child's health was generally good. It was lively and playful. But at all times its skin was more or less blue. During its illness this color was more intense, particularly in its extremities, nails, neck, face, lips, mouth, tongue and gums. On pressing the skin with the hand, the blueness gave place to a yellowish white color, corresponding in figure to the impressing hand and fingers.

No marked cardiac, or pulmonary symptoms, as faintings, palpitations, difficulty of breathing, purulent sputa, nor fever had been observed in the case, though the mother had noticed an occasional cough with mucous expectoration. The temperature of the skin was cooler than natural. Madam R. had taken the child out of the city, in a carriage, the evening before its death. The child continued well until towards morning, when it began to cry and breathe with difficulty, and with a shrill noise; the purplish discoloration increased, and the child expired suddenly.

The following is a synopsis of the post-mortem examination, made seven hours after death, in the presence of Dr. Tiffin: Moderately rigid; muscles contractile after having been supplied by extensions and flexions; body warm; purplish discoloration of the lips, neck and abdominal parietes; hair white and scanty; pupils moderately dilated; cornea a little glossy; limbs somewhat emaciated; lymphatic glands of the neck, groins, &c., not enlarged; abdomen tumid; muscles natural, though somewhat attenuated. Omenta well developed with adipose tissue, but their venous and capillary tissue, and that of the whole abdominal system much congested with very dark fluid blood, which did not seem disposed to coagulate. The internal lymphatic glands enlarged; no serosity in the peritoneal cavity, nor in the bowels, nor in the pleural cavities. The mesenteric glands enormously enlarged, 10 to 20 times, formed an irregular agglomeration without coalescing, each gland being nearly

globose, or quite round, some of which were nearly as large as a crab-apple. These glands, highly injected, both centrally and externally, were hard, of a dark red color, without any tuberculous softening, and resembled, in consistence, the cortical substance of a normal kidney.—This mass contributed much to give the abdomen its marked prominence or convexity. The liver was enlarged ($\frac{1}{4}$ to $\frac{1}{2}$), was purplish externally; its parenchyma without any marked change—the gall bladder contained a few drops of turbid serosity and mucous; the spleen, of good consistence, was enlarged about one-third; pancreas enlarged; bladder empty; left kidney normal; the right congested, enlarged ($\frac{1}{3}$ to $\frac{1}{2}$), contained a rough, grayish, and somewhat yellowish stone, resembling the form of the kernel of a peach stone, though smaller, less compressed, and less elongated, measuring the fourth of an inch in its longest diameters.—The stomach, collapsed, contained a little mucosity and chymous matter, had its mucous coat somewhat softened; the intestines were throughout of a dark venous color externally and internally, contained no fecal matter nor gas, but a little blood, mucus, with chylous paste slightly yellow; the submucous tissue, in many places, infiltrated with blood, or hæmorrhagic effusions; the solitary glands, from the pylorus to the rectum, enlarged and prominent. The jejunum had suffered two firm intussusceptions; one enclosed from 4 to 5 inches of the bowel—both being from above downward, that is, the proximal passed into the distal portion of the intestine.

The apex of the right lung, strongly adherent to the costal pleura, consisted of a firm, irregular conglomeration of immature, indurated tubercles, though rather inclining to a fleshy or glandular consistence. In the upper lobe, a cavity, one-fourth of the size of a hen's egg, filled with bloody pus, and lined with a hard, cheesy substance, was found.

The left lung was normal, though blueish. The cavas were large and contained a thin, dark, uncoagulated blood; the right auricle large and distended with blood; the left very small; an exterior view of the great systemic aorta presented an enormous dilatation from its connection with the heart throughout its great arch, becoming less in its descent through the abdomen, yet not assuming its normal size before its division into the common iliaes.

March 31st. The heart preserved in spirits was carefully examined. The systemic aorta at its origin, three inches in circumference, but was something more at the arch. The right auricle very large, thick, fleshy internally, resembling the columnæ carneæ, being eight or ten times

larger than the left auricle. The thickness, size and capacity of the right ventricle exceeded the left three or four times. The size of the pulmonary artery was not ascertained, as it had been accidentally cut away close to its origin. The foramen ovale was closed. The tricuspid valve thickened. The septum between the ventricles, though thick and strong at the apex of the heart, gradually became thinner until it totally disappeared—the two cavities forming one of great size just below the semilunar valves, corresponding in size to the aorta, which was implanted directly over this, the common outlet of both ventricles. The ventriculo-aortic opening of the right side, though much larger than that of the left, was a good deal obstructed by an enlarged, fleshy vertical column which constituted one of the columnæ carneæ of the tricuspid valve. It must have divided or broken the current in its passage into the aorta, and consequently produced a regurgitant action in the right ventricle. The mitral valve like the columnæ carneæ of the left ventricle, was attenuated, small and rudimentary. The pulmonary veins were small at their entrances into the auricle. The semilunar valves were three-fourths of an inch long, and one-third of an inch deep in the middle of their pockets. There were no indications of aortitis, the aortic valves being natural, though large. The great size and the central perpendicular position of the fleshy column of the tricuspid valve, already mentioned, must have caused a valve-like obstruction to the current during the systolic action of the right ventricle. May not this obstruction be regarded as the cause of the great venous vascularity and engorgement of the abdominal cavity and its organs, as the mucous and sub-mucous membranes, the liver, kidney, mesenteric glands, the spleen, and so on?

The cyanotic symptoms were doubtlessly greatly augmented in intensity by the coincident pulmonary, venous, and mesenteric alterations, as well as by other pathological changes, described in the anatomical history.

In this case, the aorta from its peculiar mode of implantation and size, belonged more properly to the right than to the left ventricle, its anatomical structure being normal, its great magnitude excepted. It could not be called aneurismal, pouch-like, or irregularly dilated in any part of its course in the thorax and abdomen, but declined in size from the heart to the common iliacs in a gradual and symmetrical manner.

The deficiency of the inter-ventriculo septum, from which resulted the inter-mixture of the venous and arterial blood, was probably conge-

nital, and served during foetal life in place of the foramen ovale and ductus arteriosus, the patency of which latter may be doubted in such a case. Herein is witnessed a great physiological experiment, contrived by the hand of nature with a precision, duration, and significance which artificial processes, such as vivisections, cannot reach, showing that extra-uterine life may be perpetuated under most unfavorable circumstances environed by black carbonated blood, much of which must have gone the rounds of the circulation with but comparatively little arterialization.

This case is important, as showing the compatibility and contemporaneity of Blue-disease and tuberculization. Drs. Jones and Sieverking, of England, in their new work on Pathological Anatomy, in an article on Cyanosis, say: "An important point is, an observation that Rokitansky concludes his remarks on the subject with; to the effect that cyanosis is incompatible with tuberculosis, against which he states that it offers a complete protection. We do not deny that this is the prevailing rule, yet it is not as absolute as the author quoted asserts. In the Report of the Pathological Society for 1848, (p. 200,) we find a case presented by Dr. Peacock, which refutes the universality of the law. There the post-mortem examination of the individual, a man aged 20, established the following facts: 'The right lung,' (as in the child above mentioned,) 'was extensively permeated by tubercle, and towards the apex exhibited several small cavities; the left lung contained much solid tubercle; the heart was hypertrophic; the pulmonary artery exhibited a complete diaphragm, formed by adhesion of the valves, leaving only a small triangular aperture; the foramen ovale was very widely patent. There had been cyanosis during life, but not in a very marked degree.'"

Were it desirable to prolong this article, the writer could present cases, facts and speculations upon the subject of cyanosis, both in the living and dead body. With respect to the latter, although an individual may be personally, morally, socially and legally defunct—definitively dead, yet certain vital actions may continue for hours; among which, may be named the capillary circulation, venous congestion, muscular contractility, animal heat and cyanosis; the pale or the yellow corpse becoming for a time blue, and even black. The shrivelled features may become plump, tumid, discolored and injected from causes wholly different from decomposition, gravitation, or other known physical alteration, being entirely due to persistent physiological laws.

ART. IX.—OPERATION FOR RECTO-VAGINAL FISTULA. *Cure.*

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

Mr. Editor:—The perusal of the case of Mrs. Watkins, in the March publication of your journal, suggested to me the idea of sending you the following, which, should you judge it of sufficient importance, you are at liberty to publish.

In the month of June last, Catharine, a mulatto girl, on Mr. Fay's plantation, was delivered of a large, healthy child. For some time previous, she had been in bad health, suffering from bronchitis, and was much emaciated. The bronchitis subsided, however, under the use of the ordinary remedies; but the woman did not recover her strength, and I was under the impression that her exhausted frame would sink under marasmus and slow consumption. On strict and minute inquiry, she confessed that wind and fœcal matter frequently passed '*in front,*' and upon examination, I found in the posterior parietes of the vagina the opening of a fistula, which communicated with the rectum. This aperture was at three inches from the lower end of the vagina, but opened about half an inch higher in the intestine. A crow quill was passed freely through the sinus. The patient informed me that she felt it growing gradually larger. The irritation caused by the passage of gas and fœcal matter through this unnatural channel, caused her so much pain, that she at last was forced to overcome her reluctance to discover the cause of her sufferings.

I first attempted to produce adhesion, by applying different caustics to the edges of the wound, and by introducing lint into the rectum, so as to exercise compression, and at the same time keep the parts undisturbed; but these means failed.

I thought of the suture; but the thickness of the parts, the difficulty of reaching them, the situation of the wound, and the great loss of substance effected by this time, caused me to abandon the idea. I resolved to operate in the following manner: I had a wood canula made with a deep groove on one side, which I introduced into the rectum.—A grooved catheter was then passed into the sinus, until the end rested on the canula in the intestine. The bistoury was directed on the catheter, until the point came in contact with the conductor in the rectum.—The whole vagino-rectal septum and perineum were divided at one sweep. The operation lasted hardly one minute. The hæmorrhage was slight. Chloroform was not given: the patient's lungs being affected, I would

not risk the rapid, though temporary congestion of blood, which this agent, in its action, calls forth in the brain and respiratory organs. My attention was afterwards directed to the dressing of the parts. A long and thick wick of *charpie*, well smeared with resinous cerate, was introduced into the rectum, at some distance above the womb. Half of the lint was left in the rectum, but the other half was turned over the V opening of the division, and inserted and kept in place by the natural contraction of the superior folds of the vagina. The cleansing and dressing were repeated twice a day. Granulation commenced at the bottom of the opening, and proceeded until the whole of the parts were properly re-formed, without any stricture or loss of power in the sphincter muscles. Eight weeks after the operation, the parts had healed, the patient could retain her fæces as in the natural condition, and had gained flesh. She is still on the plantation, and doing well.

This case has at least one interesting feature, if no other: it is its origin. The rupture of the rectum and vagina was certainly caused by the ignorance of the 'granny,' in neglecting the first duty you are called upon to fulfil in a case of labor, viz: of seeing that the rectum and bladder be carefully emptied, in order to give the head full scope in its evolutions.

I am, Mr. Editor, very respectfully,

Your obedient servant,

A. MAGUIRE.

PARISH OF ST. MARY, LA., *April 9, 1855.*

ART. X.—*Cancer in the Sacral, Rectal, Vaginal, Vesical, Uterine, Ovarian, Hepatic and Pulmonary Regions.—Death.—Post-mortem Examination; with Remarks: by B. DOWLER, M. D.*

Madame O——, born in Alabama, long a widow, aged 38, mother of two living children who were born from 15 to 17 years ago, resident in New Orleans as a teacher in a celebrated young ladies' institution of learning—of delicate frame, small, emaciated, of lively spirits, and of a well cultivated intellect—had been for some months in bad health, and at the close of the summer of 1854, when I saw her, she was no longer able to perform her professional duties. Dr. Egan had seen her first, and for nearly half a year aided me in the case with his sound and discriminating advice.

Madam O.'s most urgent symptom was hæmorrhage at short intervals, and which Dr. Egan justly considered not uterine but vaginal; the justness of this opinion will more fully appear in the sequel.

Anterior to this period the nature of Madam O.'s malady remained unascertained. I found masses and bars of scirrhus which nearly blocked up the calibres of both the rectum and vagina, commencing just with the sphincters of these organs. The cervix uteri could be reached only at an isolated point, where a firm nodulated body as large as a pea was felt. The os uteri could not be touched owing to a sharp unyielding scirrhus ridge, the base of which rested on the rectal aspect of the vagina. This ridge seemed as hard as bone, its apex reached to the neck of the bladder, and rendered the touching of the os uteri impracticable. This ridge was a double one, as the rectum was obstructed by a similar one, the base of which rested against the vaginal aspect of that intestine, its summit extending towards the sacral bone, in an opposite direction.

It is not intended to give in detail the remedial measures which were adopted in this case for the first two months, further than to indicate its general features, namely: low diet, the horizontal position, alterative doses of the preparations of iodine, occasional opiates, and many varieties of medicated vaginal injections.

These measures arrested the hæmorrhages and gave the patient and her friends strong, but illusory expectations of recovery. Another examination at the end of this period, however, proved that the scirrhus deposits had greatly increased, particularly in the vagina which had become surrounded with knobby masses. The period of fusion and ulceration was now deemed near at hand.

All the subsequent treatment, therefore, was based on the palliative platform—than which nothing could be more satisfactory in a case necessarily fatal, as will more fully appear in another place.

As the malady progressed, the appetite, usually good, became capricious, and sometimes craving. Digestion was comparatively easy. At the close of life, vomiting and nausea were her most distressing symptoms.

The vomiting, retching, and nausea did not seem to be due to indigestion, nor to inflammation of the stomach; nor was the food often rejected, nor appetite destroyed. These symptoms were probably owing to the increase of the ulceration and to the absorption or rather resorption of the Cancerous ichor or morbid matter lying for a long time in

the vagina, now nearly closed at the os externum, as she was too weak to bear vaginal injections, which gave her pain. Her tongue, always free from fur, became very smooth: the papillæ had disappeared; the mouth and tongue, always moist, had little, if any, saliva. During the last week of her life she had hiccough.

Madame O. had suffered some anomalous affections of the inferior limbs, during the latter years of her life, as dull achings, weakness in the joints, numbness without palsy, and without cramps until the last stage of her disease.

Lencorrhœo was seldom troublesome. The fœces for many years had been compressed into thin ribbon-like plates and passed with great pain, though rarely without saline purgatives, as Epsom salts.

The difficulty of defecation disappeared during the last two months of her life, owing to the erosion, fusion and discharge of the large scirrhus bars just within the sphincter ani, which would scarcely allow the tip of a small finger to pass, five months before death. Enemata always gave great pain, and had to be abandoned, owing to the scirrhus stricture of the rectum by which it was almost occluded. The obstruction was so great that a recto-vaginal fistula was expected before these bands ulcerated. Hence, during the last two months preceding death, defecation became more regular, though accompanied with pain; the stools passed without having been compressed into bands, as had been usual previously, yet coated with gelatinous, serous and purulent matter. The facility with which defecation took place during the period mentioned, gave the patient strong confidence in the opinion that the disease was subsiding, though she knew that this was not the opinion of her medical attendants.

No difficulty was experienced in retaining and voiding urine until two or three months before death. During this period she suffered most severely in both these functions; the latter invariably required the erect position. Mucosity, blood and black granular matter passed with the urine, which, towards the close of life, became offensive in odor.

By the horizontal position, a great variety of vaginal injections, restricted diet, and various other remedial means, the hæmorrhages were arrested; but in a few weeks they were replaced by a slighty turbid serosity, a burning ichor, which lasted until death, and which she very justly and constantly termed "the white hæmorrhage."

She experienced no local or general spasms or convulsions—symptoms vaguely attributed to reflex action. She had a few days before death,

slight cramps in the legs, and twitchings in the fore-arms and fingers.

Madam O.'s pain was paroxysmal or intermittent, which, however was not amenable to quinine, stimulants, siampisms, nor any remedy but opiates.

The paroxysm occurred daily or oftener towards the close of life.

The psychological history of Madam O——, is important as showing that her intellect, naturally active, was unimpaired under the daily use of morphia amounting to 12 or 13 grains, which did not produce stupefaction of mind; nor did this quantity in the least retard the action of bowels. She usually conversed or read during the day; slept for the most part naturally during the night. Her dreams were not oppressive, but related chiefly travelings, gorgeous scenery, &c. The dose of the morphia was at first, but one or two grains in the day; but the paroxysms of pain recurred once or oftener daily, and were almost invariably subdued by this medicine, which, however, had to be gradually increased to 5 or 6 grains for each dose, until finally 13 grains were necessary in 24 hours. The leading principle in the use of this remedy was, to use the smallest possible dose consistent with the end in view, namely, the annulment of pain. Compared with all other cases in my practice in which opiates were not used or were used in small and occasional doses, her's was an easy descent to the tomb. Active, careful and augmenting doses of this medicine are imperatively required to change a long agony to euthanasia.

Post-mortem Examination.—February 5th, 1855., 2½ p. m.; Dr. Egan assisting. Body, naturally small and delicate, now indescribably emaciated and anæmic; skin wrinkled and sallow; nose pinched; angles of the mouth depressed; features sharp and sad; warm; moderate *rigor mortis*; abdomen concave; muscles atrophied; sanious, offensive liquid issuing from the os externum and anus; the adipose tissue attenuated; the mammae, together with the superficial lymphatic glands, natural, except an enlarged inguinal gland on the left side (simple hypertrophy) and one on the right side, which was larger than an almond, and was scirrhus.

The left lung natural though devoid of air, being collapsed so as not to fill one-fourth of the cavity; the right lung, greatly collapsed, was strongly adherent at its apex to the costal pleura; one-third of its superior lobe was an irregular aggregation of tuberculo-scirrhus tumors, mostly inclining to true scirrhus, being hard, white, nodulated with puckering of the pulmonary substance.

A well-defined globular mass of scirrhus, half imbedded in the convex surface of the liver, projected from its surface, being hard, white, bloodless; on following its radicles implanted in the liver, their color approximated that of the organ itself. Gall-bladder distended with bile. Heart, spleen, kidneys, pancreas, mesenteric glands, and the deep-seated lymphatics, &c., healthy. The blood-vessels of the omentum, mesentery, bowels, and other organs, enlarged but empty; all the tissues being almost entirely bloodless. Urinary bladder distended; its vaginal aspect being thickened to one-quarter of an inch—rough, nodulated, having inclosed between its coats a hard, scirrhus plate, having on each side a vascular zone, the vessels being empty.

The vagina contracted in all its diameters; scarcely two inches long, being almost altogether closed with aggregations of hard, knotty tumors in a state of ulceration, upon their outer surfaces, having fissures, jagged ridgy and angular. The ulceration had not penetrated far into the neck of the womb, being least on the left side. The recto-vaginal septum was thick, lumpy and hard. The scirrhus masses nearly occluded both cavities. Ulceration had eaten away parts of the still thick, puckered bands and folds of the intestine.

The ulcerated surfaces, when thoroughly washed, were of a mahogany color, presenting a tufted surface, with shallow, minute holes; a line or two beyond the ulcerated surface appeared the white, dense scirrhus.

Behind and on both sides of the rectum, chiefly on the right, a large mass of scirrhus, implanted over the greater portion of the inner or concave surface of the sacrum, moulded itself to the configuration of that bone—it was thickest on the right side along the sacro-lumbar junction, reaching to the very extremity of the coccyx. A large portion of this scirrhus deposit was cut away, together with a part of the rectum, base of the bladder, the vagina and the womb, with its appendages for future examination.

A re-survey of this anatomical inquest, will show that the cancerous degeneration was chiefly confined to the right half of the body—in the lung, liver, groin, uterus, vagina, rectum—and more than all in the sacral concavity upon the right side of the mesial line.

April 18th, 1855.—*Dissection of the Uterine Specimen.*—Left fallopian tube, in its distal or fimbriated half, open—in its proximal, closed, being a small, hard cord. Left ovary reddish within, having no corpus luteum; right fallopian tube patulous at its fimbriated, but closed at its

uterine extremity. The right ovary, also, without corpora lutea, being, as already mentioned, an irregular mass of white, scirrhus tumors, connected with similar degenerations enclosed in the duplications of the broad ligament, intermingled with fibrous striæ and granules.

The cavity of the womb, virtually obliterated, although a vertical, uniform linear space was noticed sufficient to admit half an inch of the point of a fine lancet, from the fundus downward.

The fundus and body of the womb approached scirrhusity in firmness and non-vascularity, being of a pale white color. Vertical sections from the fundus to the os tincæ, displayed a striking phenomenon—an idea of which may be thus conveyed: suppose the scirrhus to be foreign to the womb, lying around the os tincæ—suppose this mass to insert itself like an inverted Δ , entering the neck of the womb from without, midway between its outer and inner surfaces, passing upward like a symmetrical wedge, ending in an acute angle, about $1\frac{1}{2}$ inches from its entrance. This wedge, half an inch broad at its insertion, ended in a fine edge, so to speak, by which the tissue proper of the cervix was equally divided, giving the os tincæ a thick, bulging, irregular appearance. The double inclined planes of this scirrhus wedge, were well defined in two ways: 1st, By a white color; and 2, By a greater density than the tissue proper of the organ.

Of the whole mass of scirrhus in the uterine, vaginal, rectal and sacral regions, (amounting, perhaps to one pound,) this wedge alone had a well defined and regular form. Around the os and cervix uteri, at the base of the bladder, backward, toward the right side, and downward to the extremity of the sacrum, the lumps and plates of scirrhus varied in thickness, from the $\frac{1}{4}$ th of an inch to nearly two inches—being everywhere combined and blended with the mucous membranes and cellular tissue, lying in contiguity, it gave them its own characteristics, as density, non-vascularity, nodular puckerings, and so on. In one of the scirrhus masses on the left side of the os and cervix uteri, an artery, of unusual size, plugged with a cylinder of dense fibrinous, almost leather-like substance, such as may be found in old aneurisms, was noticed; its ramifications, however, were lost abruptly, on reaching the white, scirrhus, bloodless, turnip-colored substance of the mass.

The ancient notion of cancer, (*carcinoma*, or the crab,) is not altogether inappropriate as descriptive of the conformation of this morbid mass. The main body of the cancer, or crab, lay around the rectum and vagina—one of its prolongations being inserted into the cervix uteri, as already mentioned. Imbedded in, and blended with the cervix, 2 or

3 lines from the ulcerated margin, was found a series of isolated nodules, encircling the cervix, having nearly the color of gum arabic, being semi-transparent, somewhat rounded and firm, and each about the size of a pea, containing in its centre one drop of clear, thick, almost gelatinous juice, which, in the microscope, appeared to consist of a thin intermedium, and innumerable granules or cells. The scirrhus, which enclosed about four inches of the rectum, lay chiefly behind, and to the right side of the intestine, and reached downward and laterally to the extremity of the os coecum, though only a part of it was cut away from its numerous implantations. The portion examined might weigh half a pound. Its configuration resembled, in some degree, a vertical longitudinal section of the sacrum, having been moulded to the inner surface of that bone, being at its middle opposite the acetabulum, and still higher up the rectum, over an inch in thickness, though varying in an irregular, abrupt manner. Upon the right side, near the rectum, many arteries entered the outer margins of the mass, not being patulous in the scirrhus itself, but totally disappeared in its texture proper. Upon the exterior of the scirrhus, chiefly next the sacrum, were found indurated fatty masses, from the most minute size up to that of a pea or hazelnut, being rounded, flattened, thready, and mixed with fibrous striae.

The hæmorrhagic period succeeded the failure of menstruation. The blood was fresh, arterial-looking, and unlike the menses, having been inodorous; it probably proceeded from a slight ulcerative lesion or rupture of the branches of the large anomalous artery, above mentioned, on the side of the cervix uteri. The vaginal discharges had always been scentless until a few weeks before death.

REMARKS.

The coagula, which formed occasionally very soon after the fluid blood passed away, were most extraordinary, and never had been equalled in firmness and apparent organization among the fibrinous concretions which I have often met with in the heart and elsewhere, in the many hundred dead bodies which I have examined. Specimens of these coagula, on being shown to several pathologists, were regarded as organized fleshy bodies. They had a brownish color, like tanned leather.

This lady had never complained of pulmonary symptoms—indeed, she regarded her lungs as quite sound; nor was any dullness noticed from a hurried percussio, made at my first visit.

The long persistence of scirrhus degeneration, anterior to fusion and erosion, without great constitutional disturbances—without the so-

called cancerous cachexy, is an important pathological point, which I have seen unexpectedly verified in persons who had perished from acute intercurrent diseases, as fevers. The scirrhus degeneration of the lung is remarkable, inasmuch as such an occurrence has been questioned, or regarded as extremely rare, by some pathologists of note.

It is probable, from circumstances related by the patient, that the scirrhus degeneration commenced twenty years ago; that the original site of its implantation was in the concavity of the sacral bone; that this matted mass next invaded the recto-vaginal tubes; and that, finally, it involved the neck and mouth of the womb, together with the neck of the bladder, the right ovary, the apex of the right lung, and the lymphatic glands of the right groin. With regard to the rectum, it must be recollected that pain, constipation and difficulty in defecation, began early in life: she declared that "something was always wrong there." The birth of her second and last child, in the city of Mobile, about sixteen years ago, was tedious, difficult, and excessively painful. That the womb was organically affected only a year or two since, is probable from the persistence of regular menstruation, and from the absence of offensive vaginal discharges. Drs. Jones and Sieverking, however, say, on the contrary, as a general rule, that "uterine cancer is ordinarily a primary affection; carcinoma of other organs may be developed simultaneously or consecutively; but, except as a result of the fusion of the former, and its consequent introduction into the system, it is not often the case."—*Path. Anat.*

Madame O., about two months before her death, in anticipation of that event, had, as I afterwards learned, charged her accomplished and most steadfast friend, Mrs. M., to request me to make a *post-mortem* examination of her body. This request, made known immediately after her death, did not proceed from a freak of fancy or eccentricity of character, but from a sense of duty, and an elevated intellect, unbiassed by prejudice, false delicacy and intense selfishness. Doubtless, there are but too many who would prefer to see a whole continent invaded by an epidemic, rather than permit their dead bodies to become useful to the living. Pride, which invades the realms of death, and stands sentinel over the insensate corpse, will not yield a little hour to utility, but awaits the worms and decomposition without perturbation or fastidiousness.

Some of the greatest philosophers (among whom was Sir Humphrey Davy,) have indulged in the absurd fear of post-mortem examination. —

Sir H. Davy, who died at Geneva, in 1829, had, during his long illness, displayed the enthusiasm of a student, the calmness of a philosopher, and the resignation of a Christian; yet "he had a dread of a *post-mortem* examination, founded on an idea which occurred to his active mind, that it was possible for sensation to remain in the animal fibre after the loss of irritability, and power of giving proof to others of its existence: consequently," says his brother, Dr. John Davy, "such an investigation not having been made, his disease, as to its exact kind and the immediate cause of his death, must ever remain doubtful."—*Works*, i, 425, *London*. 1839: by Dr. John Davy.

PROGRESS OF MEDICINE.—MISCELLANEA.

ART. I.—*Fœtal Circulation.*

Professor E. R. Peaslee, A.M., M.D., in an able monograph of 26 pages, on the Fœtal Circulation, sums up the results of his researches and conclusions, thus:—

The View of the Fœtal Circulation required by the present state of Physiological Science.—1st. The human fœtus, during the last half of fœtal existence, has a *reptile* circulation—the mammal circulation commencing at birth; and the structure and the function of each particular part of its circulatory apparatus are in subservience to this fundamental fact. The characteristics of a reptile circulation are—i, The circulation of a *mixed* blood (and of the same degree of impurity) through both the aorta to the tissues, and through the pulmonary artery to the lungs; and ii, The transmission of far less blood to the aerating apparatus than is sent through the aorta.

2. The foramen ovale with its valve is the only simple mechanism which could answer the requirements of the case, viz: a temporary reptile circulation with a capability of instantaneous change to a permanent mammal circulation, the foramen becoming permanently closed about eight days after birth.

3. The ductus arteriosus is merely a "waste pipe," conducting into the nearest portion of the aorta that part of the blood sent into the trunk of the pulmonary artery, which the collapsed lungs of the fœtus are unable to receive. After birth the latter admit all the blood, and the ductus is, therefore useless. It does not enter the descending aorta to avoid sending its blood to the head and upper extremities.

4. Though the lungs are more solid in the fœtus than after birth, they are probably permeated by about two-thirds of the blood entering the trunk of the pulmonary artery, and this is returned as *venous* blood to the left auricle.

5. The blood arriving in the right auricle from the two *venæ cavæ*, is completely intermixed by the diastole and systole of this cavity; and the same mixed blood is therefore transmitted through the foramen ovale into the left auricle. Or if by any possibility more placental blood enters that cavity, the venous blood, returned by the pulmonary veins, most probably counterbalances that advantage.

6. The Eustachian valve cannot prevent the admixture of the blood from the *venæ cavæ*, nor direct that from the inferior cava at once through the foramen ovale; it merely prevents regurgitation from the auricle into the inferior vena cava, at the same time incidentally preventing the current from the superior cava from impinging so forcibly upon that of the inferior. Hence, the valve of the foramen ovale replaces it to some extent, in respect to its principal function; and, therefore, it becomes atrophied in proportion as the latter is developed.

7. No artery in the body of the fœtus contains *arterial* blood. The aorta and pulmonary artery, and all their branches, contain a *mixed* blood, about five parts, at least, venous to one part placental. The precise proportions, however, are unimportant, the blood being of a *highly venous* character, and as impure in the aorta as in the pulmonary artery. Only the umbilical vein and the ductus venosus contain pure, aerated placental blood.

8. The umbilical arteries contain the same mixed blood as the aorta, and possibly return one-sixth of the blood received by that vessel; but this amount, aerated in the placenta and returned by the umbilical veins, suffices to maintain the low standard of aëration in the fœtus.

9. The head and upper extremities of the fœtus do not receive a purer blood than the lower parts of the body. They, as well as the digestive and urinary apparatus, are earlier developed, in accordance with a general law of development.

10. The fœtal liver is a *depurating* organ only so far as it secretes bile, and, therefore, to a slight extent, though it does not thus convert venous into arterial blood. Its large development, from the placental blood abundantly distributed to it, has relation to its function as a *blood-making* and not as a *bile-secreting* organ; and this blood becomes *venous* in the capillaries and the hepatic veins, as all analogy proves.

11. The trunk of the vena portæ is, in the fœtus, both the *nutrient artery* of the liver, and also corresponds to the vena portæ of the adult—its formative branches containing venous blood from which the bile in the meconium is probably secreted.

12. Anatomy, the history of development, and comparative physiology, combine to sustain the preceding propositions.

ART. II.—*An Experimental Inquiry concerning some points in the Vital Process of Assimilation and Nutrition:** by PROF. N. S. DAVIS, M. D. (North-West. Med. and Surg. Jour.)

There have been numerous analyses of venous blood in comparison with arterial, but without any reference to the particular parts from which the venous blood had been returned. The one exception to which allusion was just made, was by Simon, who procured the blood from the renal veins, the hepatic veins, and the aorta of a horse, and subjected each specimen to careful analysis, particularly in reference to the quantity of *fibrine* contained. The results are given on page 139 of his work on the Chemistry of Man, and are as follows, viz:—

	Renal Vein.	Hepatic Vein.	Aorta.
Water,	778,000	725,000	790,000
Albumen,	90,230	130,000	90,300
Fibrine,	000	2,500	8,200
Whole solid matter,	222,000	275,000	210,00

This presents truly a striking result; the blood returning from two of the largest secreting organs in the body, is found to contain far less *fibrine* and *water*, and more *albumen*, than that from the aorta. But there are two circumstances which render this analysis unsatisfactory. First, the horse from which the blood was taken was not healthy, and was in a *starved* condition. Second, the quantity of blood obtained by Simon from the renal veins, was insufficient to determine accurately the proportion of fibrine, being only 50 grs. To obviate these objections, and at the same time add another important element to the comparison, I procured a large, healthy and active dog, and procured some blood from the renal vein, the iliac vein, and the iliac artery, in the following manner: the dog was stunned by a blow on his head, the abdomen quickly laid open, and a ligature passed around the renal vein near its entrance into the ascending cava, when, on puncturing the vein, 590 grains of blood flowed readily into a clean cupping-glass. A ligature was next passed around the iliac vein, and, on puncturing it, 771 grains of blood were collected in another cup. The iliac artery, being now easy of access, was punctured, and the blood flowed in a full, pulsating stream, and was received into a third cup, to the amount of 1816 grains. All these specimens of blood were most carefully analyzed, following the method recommended and so long practised by Andral and Gavarret, with the exception of the mode of separating the fibrine. For accomplishing this most accurately, I fully agree with Mr. Bence Jones, preferring to allow the blood to coagulate perfectly, and then enclose the whole clot in a clean, fine linen cloth, and wash it with distilled water, until the red

* A portion of this paper Prof. D. quotes from one which he read at Charleston, in May, 1851, before the American Medical Association.

corpuscles are entirely removed. The results, in a tabular form, are as follows, viz:—

	Blood from the Iliac Artery, in 1000 parts.	Blood from the Iliac Vein, in 1000 parts.	Blood from the Renal Vein, in 1000 parts.
Water, - - -	812,20	811,40	802,40
Solid Matter, - - -	187,80	188,60	196,60
Of which were fibrine,	2,20	2,50	1,70
Extractive Matter and Albumen, - - -	98,10	89,50	98,50
Red Corpuscles, - - -	82,50	92,70	92,20
Salts, - - -	5,00	3,90	4,20

From this extract, which, together with the whole paper, was published in the *North Western Medical and Surgical Journal*, for September, 1851, it will be seen that the blood of the renal vein *was analyzed by me five years since*; that the blood so analyzed *did contain fibrine*, though in a much smaller proportion than the arterial blood of the same animal; and, finally, that the albumen in the blood of the renal vein, instead of being increased in proportion to the diminution of fibrine, is not positively increased at all.

Its slight *apparent* increase being less than it should be from the diminished proportion of water. It is not at all strange that M. Bernard should be ignorant of the paper containing the foregoing analysis; but the *representatives* of our profession abroad should be well informed in regard to the state of medical investigations at home; and especially should this be the case with such as take it upon themselves to publish books after they return home. The largest portion of the paper read by me to the American Medical Association, in 1851, was occupied with the results of a somewhat extensive series of experiments, in relation to the influence of diet and drinks on the functions of respiration and calorification. Some of these results I find are not in consonance with the views of M. Bernard.

For instance, he alleges that the temperature of the body is the highest at the *end* of the active period of digestion—that is, about six hours after taking food. I made numerous experiments in reference to this point, and have always found the highest temperature *during* the most active period of digestion, instead of at the end of that process. In the paper alluded to, the results of some of these experiments are given as follows, viz: “The observations were repeated six times each day, at 7½ o’clock, A. M., half an hour before taking food; at 10½ A. M., two and a half hours after breakfast; at 12½ P. M., immediately before dinner; at 3½ P. M., two and a half hours after dinner; at 5½ P. M., half an hour before tea; and at 8 P. M., two hours after tea. The following is the average result of 16 days’ observations under the influence of an ordinary mixed diet, viz:—

	7½ o’clock, A. M.	10½ A. M.	12½ P. M.	3½ P. M.	5½ P. M.	8 P. M.
Av. Tem’pture,	94° F.	96° F.	95° 2 F.	96° 7 F.	95° 2 F.	96° F.
Highest,	94° 5 F.	97° 3 F.	98° F.	97° 2 F.	95° 5 F.	96° F.
Lowest,	93° 7 F.	96° 6 F.	94° F.	96° 3 F.	95° F.	96° F.

From this table, the inference is plain that the temperature of the body is uniformly from one to two degrees higher during the active stage of digestion—that is, about *two* hours after eating, than after the digestive process is fully completed.

Similar experiments were made in reference to the temperature of the system under the influence of a diet exclusively carbonaceous, and under that of a diet wholly nitrogenous; the results of which are given in the paper, and correspond closely with the above, so far as relates to the influence of digestion. I am aware that the paper to which I have alluded, as having been read to the American Medical Association, in 1851, attracted very little attention; and after its publication, I do not know that it was either copied or commented on by any of the medical journals in the country. And though the experiments, observations and analyses detailed in it, cost me much tedious labor and time, yet I might have persuaded myself that the results were really of no importance, had it not been that similar experiments and observations have been made and published in European journals, from time to time since, and these have invariably been copied and sent the round of American journals, as though they possessed much interest.

This fact seemed to justify me in again calling attention to my own work. It has been frequently alleged as a defect in American medical literature, that it contained so little matter of original experimental character.

And probably the best mode of supplying the defect, would be to render full and prompt credit to such individuals as do attempt investigations of this character; for there are few things more discouraging, than to spend much time and labor on experimental inquiries, and see the results entirely neglected or overlooked, until some European savant takes up the same subject, perhaps spends less than half the time, and announces results very similar, which are caught up with avidity, and copied into every journal in the Union.

ART. III.—*Gunshot Wounds of the Heart.*

[The following cases taken from the exchanges of the *New Orleans Medical and Surgical Journal*, are of extraordinary interest; the first from *The American Medical Monthly* for April, 1855; the second from *The Edinburgh Quarterly Medical and Surgical Journal* for April, 1854.]

1. *Case of Penetrating Gun-shot Wound of the Heart—Life protracted for eleven days—Bullet found buried and encysted in the substance of the Heart: by J. M. CARNOCHAN, M. D., Surgeon-in-Chief to the State Hospital, Professor of Surgery in the New-York Medical College, &c.*

On the 27th of February, 1855, I was called in consultation to see William Poole, a young man aged 33 years, of unusually athletic form

and muscular development, who had been wounded two days previously in an affray with fire-arms. He had received a bullet wound in the outer aspect of the right thigh, two inches above the upper border of the patella. The wound, however, which created alarm among his friends, was situated upon the anterior wall of the thorax, about three-quarters of an inch to the left of the mesial line, and about half an inch below a line drawn across the chest, from one nipple to the other. A bullet probe could be passed slantingly from right to left, along the track of the wound, for about an inch. At this depth, the probe was arrested, and it was not thought expedient to use force in making further exploration. Poole received his wounds during a deliberate onslaught made on him by some five or six persons, armed with Colt's revolvers. The first ball took effect on the right thigh, and brought him to the ground. While thus prostrate, another assailant placed the muzzle of a pistol close to his chest, and discharged its contents. He immediately jumped up, and reeling towards a door rested, as if stunned, against it for support, during some minutes. He then fell, exclaimed that he was dying, and remained senseless, cold, almost pulseless, and apparently moribund, for about four hours. From this condition he rallied, and became so free from the usual symptoms of severe injury, that his medical adviser, Dr. Putnam, considered that the ball had really not penetrated into the thoracic cavity, and my opinion was sought to corroborate or dispel this favorable view of the case.

Professor Carnochan relates, at length, the diurnal symptoms, treatment and progress of this case, until its fatal termination, on the 7th of March; also, the *autopsy*, seven hours after death. "The body" was in a perfect state of preservation and showed a powerful and well developed organization.

The surface of the body presented three orifices of gun-shot wounds; two on the external side of the right thigh, a short distance above the patella, by which, apparently, a ball had made its entrance and exit, respectively; and one on the anterior aspect of the chest, three-quarters of an inch to the left of the median line, and about half an inch below a line drawn across the chest, from one nipple to the other. The examination revealed that all the organs of the body were in a healthy condition.

The sternum and cartilages of the ribs having been partially elevated, a bullet probe could be passed without difficulty, slanting from right to left, through the wall of the thorax, at the place of junction of the cartilages of the fifth and sixth ribs with the margin of the sternum.

The sternum being completely elevated, the pericardium was seen to be much distended, and on its surface, in continuation with the external wound, was observed a rough spot, which proved to be an opening into the cavity of the pericardium, thinly closed by the exudation of plastic material.

The right and left cavities of the pleura were free from effusion, and the lung on each side was in a sound condition. The pericardium was found filled with serous fluid, tinged with blood, and was so distended that it encroached very much upon the lungs on both sides. Upon opening the sac of the pericardium and removing the large quantity of serous fluid, the external surface of the heart and the serous lining of the pericardium were both found to be entirely covered with plastic exudation, presenting all over signs of high inflammatory action. A cursory examination of the heart in position did not disclose the presence of any foreign body. It was afterwards taken out, and upon a careful examination, a bullet one inch in circumference, was found enveloped in a delicate cyst, and embedded to the depth of a quarter of an inch, in the muscular tissue of the septum, between the right and left ventricles, about midway between the apex of the heart and the base of the ventricles. Its locality was only indicated by the sense of touch, for as the wound had entirely cicatrized, there was no outward visible sign of its presence. Obviously, the cause of death was inflammation of the pericardium and heart, and its results.

This case is one to be added to the few already on authentic record, showing that penetrating wounds of the heart are not always immediately mortal.

It has, moreover, peculiar features which will render it remarkable in the annals of surgical pathology.

Several cases are mentioned in which patients have survived one or more days the effects of penetrating and non-penetrating wounds of the heart, inflicted by cutting instruments, and also of non-penetrating wounds inflicted by *gun-shot*.

But the peculiarity of this case is, that although the wound was a penetrating gun-shot wound, leaving the ball deeply buried in the tissue of the heart, the patient survived for a period of time so long as to encourage the hope of recovery.

This position of the ball discriminates the case from that mentioned by the French surgeon, Latour, where the ball had not penetrated deeply into the heart, but rested on its surface, partially encroaching upon the muscular wall of the heart, and enfolded partly by the pericardium.

The autopsy of this case also revealed, that the wound was not only closed and cicatrized, but that a cyst was in process of formation around the ball.

By this case, also, it is established, that hæmorrhage is not necessarily a consequence of a gun-shot wound of the heart; for the serum found in the pericardium was merely tinged with blood, and there was no coagulum. The absence of hæmorrhage may be accounted for by the conical shape of the ball, and by its direction, two circumstances which favored its passage between the muscular fibres of the superficial layer of the heart, without severing them, and caused

it to rest slantingly behind the anterior coronary artery, without wounding it.

[The following are among the contributions to surgery—derived from the late campaign in Burmah. We extract them from the first number of a new half-yearly journal—*The Indian Annals of Medical Science.*]

2. Case of Lodgement of a *Musket-ball in the left Ventricle of the Heart.*

A private in H. M.'s 80th Regiment was wounded at the storming of the Great Pagoda. The ball entered a little above the anterior fold of the left axilla, taking an oblique direction to the cavity of the chest, from which, on his admission to the Shore Field Hospital, and for several days after, blood issued. Dyspnoea and emphysema had followed the receipt of the injury. The left side was dull on percussion, and the respiratory murmur slight, and only under the clavicle. The dyspnoea recurred with increasing severity from time to time, and was accompanied with hæmoptysis. The action of the heart was weak, but natural; its systole and diastole regular and equal. Hectic set in, the patient became emaciated, and died about ten weeks after the infliction of the wound.

On dissection, "the passage of the ball through the pectoral muscle could not be observed, but its course appeared to have been between the third and fourth ribs, passing downwards to the left side, forming a cavity which extended from the first to the seventh rib, from the spine to the cartilages of the ribs anteriorly; this contained a pint of pus. Left lung impervious to air throughout; a small portion of the cloth of the jacket was lying loose at the orifice of a canal situated about the middle of the lung in its convex aspect, which canal, passing forwards and inwards, stopped short close to the union of the pulmonary veins, where all further traces of the passage of the ball were lost. On raising the heart, however, a hard and firmly impacted substance was felt at the apex—this, on examination, proved to be the ball, in the left ventricle at its most inferior part, crossed and recrossed by the *cordæ tendinæ* and *columnæ carneæ*, which firmly secured it in its position.—The heart was perfect in every respect, and the only conclusion to which we could arrive is that the ball must have perforated the pulmonary vein, and thus passed into the left auricle, and ultimately into the ventricle.

A case very similar to the above is detailed in the seventh volume of the "*Transactions of the Provincial Medical and Surgical Association.*" In this case the foreign body was a piece of wood, and was found after death (which occurred five weeks after the receipt of the injury,) loose in the cavity of the right ventricle. The piece of wood appears to have penetrated the walls of the heart, but, as in the case we have detailed, no trace of its passage into the viscus could be found on careful examination.

We think the entrance of the ball through the pulmonary vein, and left auricle less probable than its having penetrated the muscular walls of the heart. In any view the case is very remarkable.

ART. IV.—*Chronic Heart Disease.*

[The following paper from the Association Med. Jour., Sept., 1854, taken from Braithwaite's Retrospect, written by Dr. A. Harvey, Physician to the Aberdeen Royal Infirmary, possesses the merit of brevity if not originality, being a lucid recapitulation from various authorities, as delivered by the author to his classes]:—

I. DISEASES OF VALVES AND ORIFICES.—1. The valves and orifices of the left side of the heart are at least twenty times more liable to disease, than the valves and orifices of the right side.

Disease of the semilunar pulmonary valves is so rare as to be a matter of pathological interest merely. Disease of the tricuspid valves is more frequently, although still seldom met with; and still seldomer without co-existing disease of either the aortic or mitral valves.

2. In early life, and in women, the mitral valve and corresponding auriculo ventricular orifice are oftenest diseased; in advanced life and in men, the aortic. In many cases, at all ages and in both sexes, the two sets of valves and openings are simultaneously diseased.

3. The diseased states of the valves and orifices are mostly the result of (a) antecedent endocarditis of rheumatic origin, and consist of concretions forming on the valves; or (b) mere perversion of nutrition, independent of inflammation, and occurring in connection with atheromatous, and other kinds of deposit, in the aorta and arteries generally.

4. The effects of these changes on the functions of the valves and orifices, are two-fold: *i.* By contracting and narrowing the orifice, to impede or obstruct the passage of the blood (*valvular obstruction, obstructive diseases of valves*). *ii.* By thickening, corrugating and shortening the valves, to make the orifice more or less permanently patent, and allow regurgitation or reflux of blood (*valvular insufficiency, valvular patency, regurgitant disease of valves*).

5. Valvular obstruction and valvular insufficiency often exist separately, but are oftener co-existent in the same valve. Each of them may obtain in very different degrees, and the two may be very variously combined.

6. In a very few cases, the cause of patency is preternatural widening of the orifice without disease, but with no adequate extension by growth, or the valves; and, in a few others, it is owing to disease with shortening of the chordæ tendineæ, the valves themselves being sound.

II.—DISEASES OF WALLS AND CAVITIES (*Hypertrophy and Dilatation*).—1. Hypertrophy and dilatation may be either partial or general, oftenest partial. Dilatation, unaccompanied by hypertrophy, is rare.

2. Hypertrophy and dilatation are very generally *secondary* morbid states. They may be produced by various causes: *i.* In a very few cases, by unknown causes, or causes unconnected with obvious change of structure elsewhere (*primary*).

ii. By adhesion of the pericardium to the heart, consequent on plastic exudation from pericarditis.

iii. By preternatural widening, with or without change of structure, of the ascending aorta.

iv. By diseased states of the valves. This is the most common cause.

3. The *rationale* of their production may be thus stated: *i.* Disease of the valves, widening of the aorta, adhesion of the pericardium, create a demand for increased action of the heart; and this increased action, in a constitution otherwise healthy, leads to increased nutrition of the organ. The walls of the heart are thereby gradually increased in bulk and in contractile power (*simple hypertrophy*).

ii. When, along with this increased action, there is (*a*) a full quantity of blood in the body, the cavities of the heart are gradually dilated at the same time that their walls are thickened (*hypertrophy with dilatation, eccentric hypertrophy*). But (*b*) when the quantity of circulating blood is much diminished, the tonic contraction of the thickened fibres of the heart, which are no longer distended, leads to a diminution in the size of its cavities (*concentric hypertrophy*). This is rarely met with, except either as a congenital affection, or, in connection with the *rigor mortis*, as a temporary *post-mortem* condition.

iii. Dilatation without hypertrophy, consequent on obstructive disease of the valves, occurs chiefly (*a*) in a weakly habit of body, when the excitement of the heart, by the preternatural amount of its stimulus, is feeble; but (*b*) it may be produced, in a habit otherwise healthy, by mere morbid debility of the muscular fibres of the heart, as after some cases of rheumatism affecting them.

iv. In the great majority of cases, it is the left ventricle that is the first and chiefly affected, and hypertrophy and dilatation combined (*eccentric hypertrophy*) that obtains, and in connexion with pre-existing mitral or aortic disease. With mitral disease, there is commonly rounding of the apex, and often simple hypertrophy merely; with aortic disease, there is elongation of the apex, and commonly *eccentric* hypertrophy.

Pure dilatation may affect either ventricle, oftenest, perhaps, the right, and that chiefly in females; and in such cases seems to occur in connection rather with disease of lung (*e.g.*, habitual asthma) than with diseased valves.

III.—DIAGNOSIS OF CHRONIC HEART DISEASE. *Diagnosis of Disease of Valves and Orifices.*—Practically, in at least nineteen out of twenty cases, the questions to be determined are whether it be the mitral or aortic valve that is diseased, or both; and whether the disease be of the nature of valvular obstruction, or of valvular insufficiency, or both.

The diagnosis is mainly founded on these two things: first, a bruit or murmur heard, called, from its blowing character, *bellows murmur*, or, from its seat being within the heart, *endocardial murmur*; and, secondly, the state and character of the pulse at the wrist.

1. *Bruit or Murmur*.—Its import varies according (*a*) to the time when it is heard, (*b*) the place where it is best heard, and (*c*) the direction in which it is conveyed furthest. The second and third may be taken together, and included under one head.

(*a*) *The Time when it is Heard*.—This may be either with (or rather in place of) the first natural sound (systolic bruit); blood then passing from the left ventricle into either the aorta before, or the auricle behind; or with the second natural sound (diastolic bruit); blood then passing into the ventricle, from either the aorta before, or the auricle behind.

(*b*) *The Place and Direction in which it is Best Heard and Conveyed Furthest*.—This may be either over the base of the heart, and thence upwards along the aorta and carotids, or at and in the direction of the apex. Accordingly—1. Bruit, systolic and loudest at base, indicates obstructive disease of the aortic valves or orifice. 2. Bruit, systolic and loudest at apex, indicates mitral insufficiency (patency) with regurgitation. 3. Bruit, diastolic and loudest at base, indicates aortic insufficiency with regurgitation. 4. Bruit, diastolic and loudest at apex, indicates mitral obstruction. Rarely if ever heard.

Various combinations of these murmurs are sometimes heard, constituting double bruits. Their import is to be gathered from that of the single murmurs. Thus, *e. g.*, there may be a systolic bruit, equally loud at the base and at the apex, indicating at once aortic obstruction and mitral insufficiency; or there may be a systolic and a diastolic bruit, both loudest at the base, and indicating aortic obstruction and aortic regurgitation.

2. *The Pulse*.—*i.* Pulse at wrist irregular, intermittent, unequal, and soft, small or weak, indicates mitral disease, obstructive or regurgitant, particularly the latter, on which this condition of the pulse is specially dependent. *ii.* Pulse regular, full or hard, indicates aortic disease; if jerking and resilient, aortic insufficiency (with hypertrophy).

Diagnosis of Disease of Walls and Cavities.—1. Of Hypertrophy. *i.* The action of the heart preternaturally, sometimes excessively strong, with corresponding impulse felt on laying the hand or the ear over the præcordial region. *ii.* The sounds of the heart's action, and especially the first sound, less audible than natural—obscured and muffled.

2. Of Dilatation. *i.* The action of the heart weak, somewhat undulatory, with indistinct impulse. *ii.* The sounds of the heart's action, and especially the first, preternaturally clear, short or abrupt.

3. Of Hypertrophy and Dilatation. With various modifications, as the one or the other condition predominates, of the abnormal impulse, and the abnormal sounds of the heart's action. *i.* The area of the natural dullness on percussion increased, often with squareness of outline

of this area. *ii.* The apex of the heart seen and felt to beat lower down than natural, often as low as the seventh or eighth ribs. *iii.* The præcordial region in some cases obviously bulging and prominent.

IV.—PROGRESS AND RESULTS OF CHRONIC HEART DISEASE. PROGNOSIS.—1. It is important to discriminate between (*a*) the affection of the heart itself, and (*b*) the secondary affections—*i. e.*, affections of other parts and organs resulting from it.

(*a.*) As time advances, there occurs a gradual aggravation of the heart affection; an increase of the valvular obstruction, or of the valvular insufficiency, or of both; and likewise an increase of the hypertrophy, or of the dilatation, or of both.

(*b.*) Sooner or later, secondary affections, of various kinds, extrinsic to the heart, supervene, *e. g.* 1. Dyspnœa; bronchitis; pneumonia; pulmonary hæmorrhage, with or without pulmonary apoplexy. 2. Enlargement of the liver, or of the liver and spleen; hæmatemesis. 3. Cephalalgia; tinnitus; vertigo; syncope; epilepsy; epistaxis; cerebral hæmorrhage; apoplexy; palsy. 4. Dropsical effusions; œdema of face, or of face and hands—of lungs; hydrothorax; ascites; anasarca or general dropsy. 5. Various combinations and successions of the foregoing.

2. Both the aggravation of the heart disease and the supervention of some secondary affection, take place more surely and more quickly in young and full-blooded individuals, than in those already old, or thin, or emaciated.

3. As to the relation that obtains between the secondary affections and the diseased heart—1. The connecting link between them is the altered (abnormal) condition of the general circulation produced by the state of the heart. This condition may be one or other, or a combination of three kinds: (*a*) impeded venous current behind (pulmonic and systemic), as in mitral disease, whether obstructive or regurgitant; or (*b*) undue impulsion of the arterial current in advance, as in hypertrophy; or (*c*) too feeble impulsion, as in purely passive dilatation of the ventricle. 2. Nevertheless, the diseased state of the heart, and these effects of it on the general circulation, are commonly only *permanent predisposing* causes, not the direct or exciting causes of those secondary affections—the immediate causes of them (often in a great measure *avoidable*) being intemperance, over-exertion and fatigue, mental emotion, and especially exposure to cold, or cold and wet. 3. Some of the secondary affections are themselves the occasion, or concurrent and co-operating causes, of certain other secondary affections, *e. g.* bronchitis—of œdema of the lungs, or of general dropsy; enlargement of the liver, or of the liver and spleen—of ascites, hæmatemesis, epistaxis, apoplexy; or consolidation of lung and enlargement of the liver combined, with bronchitis—of general dropsy.

4. The worst kind of heart affection is that in which there is at once obstructive and regurgitant disease of the mitral valve, because the

most effectual in deranging the general circulation, and the most fruitful, therefore, of secondary affections. The least unfavorable is a diseased state of the aortic valve only, with moderate hypertrophy of the left ventricle; the hypertrophy, though reckoned a diseased state, being, in fact, a provision of nature for overcoming the resistance offered to the exit of the blood.

Insufficiency of the mitral valve, however, with much hypertrophy of the left ventricle, is very hazardous; the hypertrophy in this case enhancing the evil naturally attendant on the patency of the valve.

5. When death ensues, the fatal event may take place suddenly, in the way of syncope, and be directly referable to the diseased state of the heart; or it may take place more gradually through the medium of some secondary affection, and in very various modes—in the way of asthenia, or in the way of coma, or in the way of asphyxia—according to circumstances.

6. The prognosis in any case must mainly be based upon, and may in general readily be determined, from—1. The kind and extent of heart affection. 2. The tendency that may appear towards secondary affection; and the kind and extent of this. 3. The age and bodily habit of the patient. 4. The degree to which the habits and circumstances of the patient can be controlled and regulated, and the probable efficacy of the remedies applied, with the view in respect of them all (*a*) of retarding the progress of the heart affection—(*b*) of warding off secondary affections—and (*c*) of removing those when they supervene: these several objects furnishing at the same time the main indications of treatment.

ART. V.—*The State of the Heart in Fever: by DR. STOKES.*

I have sometimes observed that students were under a misapprehension about the doctrines which we have long held in the hospital, with respect to the condition of the heart as a guide for the use of wine. They have come to the erroneous opinion that we are only to give wine where we find the want of the first sound of the heart, and that we are not to give wine where the heart is acting well. This is a mistaken view of the matter. What we have established as to the state of the heart in connection with the effect of stimulants, is simply this: we have ascertained that the efficacy of stimulants is often directly as the debility of the heart. It has been also ascertained, that the power of bearing stimulants, their effect upon the nervous system, their good effects on the general condition, are directly as the weakness of the heart. We may lay down as a rule, that there are three conditions of the heart to be looked at by the practical man in the treatment of fever. In one, we have an excited heart—a violently excited heart, all through the case; and this heart may be excited and violent, although the symptoms be those of extreme adynamia, although the surface be cold, the breath

cold, and the pulse so feeble that it cannot be discovered. Nay, the heart may act with great force for several days, and yet there be no pulse at the wrist. This is one case. In the next case, we find exactly an opposite condition, in which the systolic force of the heart is diminished. This is shown by loss of impulse of the heart, by diminution of the first sound, and, in certain cases, by extinction of the first sound of the heart while the second remains. This is a case which calls for wine, and in which you should give it: it is a case in which, in the vast majority of instances, wine will agree with the patient. There is a third set of cases, in which the heart does not seem to be implicated at all in the course of the disease, in which, notwithstanding the existence of the most extraordinary group of symptoms affecting various organs, the heart, in the middle of the storm, seems to be in a state of calm and quiet. If we compare these three sets of cases, with a view to prognosis, we may arrange them in this way. The case of excited heart all through, with feeble pulse and with adynamia, is unquestionably the worst case.—There is no worse symptom in fever than an excited heart. It is especially a bad symptom, when, with that excitement, we find a feeble pulse. The next will be the case of sinking of the heart; and the most favorable case is that in which, as I said before, the heart seems to escape disease. But you are not to suppose, that because you have an excited heart you are not to give wine if the symptoms of the patient require it: and you are not to suppose that, because the heart is not affected at all, you are to withhold wine, if the general symptoms of the patient require it. You are not to found your exhibition of wine or stimulants upon any one thing; you are to take the general state of the patient into consideration. What we have done is to discover an intelligible, practical rule, which will guide you in the use of wine in certain, I think in many cases; but you are not to suppose that because this man has a clear first sound at his heart, therefore you are not to give wine. You are not to suppose that because the heart is safe, you can do without wine.—*Ranking's Abstract.*

ART. VI.—*Epidemic Changes in the Local Affections of Fever:* by
DR. STOKES.

In their seat, if not in their nature, these affections are observed to vary in different countries. On the continent—at least in France, and in a large portion of Germany—the frequency, and, probably, the preponderance of the secondary disease of the intestines, is a matter that must be admitted. So remarkable, indeed, is the predominance of the tumefaction and ulceration of the mucous glands of the intestine in France, that Andral, in the first edition of the *Clinique Médicale*, described fevers under the general head of diseases of the digestive system; and yet Andral was no blind follower of Broussais. In

Ireland, however, we do not find this remarkable preponderance of the secondary diseases of the digestive system ; but, when I state this to you, I wish you to understand and adopt this principle, that all statements as to the anatomical characters of fever, as it prevails here or elsewhere, are to be accepted only so far as they apply to the prevailing epidemic. And, although it is true, that on comparing our typhus with the French typhoid fever, this difference becomes apparent, that the existence of follicular disease of the intestine is almost the rule, and its absence the exception in the latter affection, while, in the Irish typhus, this condition of the intestine is rare; you must, however, bear in mind, that in Ireland, and in our own time, we have had a great epidemic of what was certainly typhus fever, in which the condition of the intestine accurately represented that which is found to prevail on the continent.
—*Ibid*

ART. VII.—*The Great Importance of Nourishment in Fever: by*
DR. STOKES.

I wish also strongly to impress on you the great importance of the use of other forms of nourishment in this disease; for we must not only keep up the nervous energy of the system by wine, but we must support nature by food. There is no mistake more fatal in fever, than the withholding of food. I was early taught the importance of the use of careful nourishment in fever, by my friend and colleague, Dr. Graves. I remember once, Dr. Graves, when speaking of the necessity of the use of nourishment in fever, made use of these words:—
“If you are at a loss for an epitaph to be placed on my tomb, here is one for you—*He fed fevers.*” In addition to the prejudices with which the inflammatory doctrine imbued so many minds, with respect to the use of food in fever, there was a new set of arguments raised against it, in consequence of the experiments of an American physician. I allude to the case observed by Dr. Beaumont, and so often quoted since. In this remarkable case, various medicinal substances and articles of food were introduced through an external fistula into the stomach, their effects being noted, as also the conditions of temperature, vascularity, &c. A set of results were subsequently published in connection with the action of the stomach upon food. One of the results stated to have been thus obtained, was that the existence of the state of fever altogether suspended the process of digestion. Here was a statement which had the appearance of being the result of strict observation. It influenced a number of young men; but did it influence those who had once been in charge of a fever hospital? Not at all; because those men knew very well that, no matter what Beaumont might say about the stomach not digesting when the patient had fever, in thousands of cases patients in fever digested remarkably well, required food, and derived benefit

from it. In a large number of cases of typhus fever, the stomach has an excellent power of digestion; and, I believe, if we were bold enough, we would find that many articles of food usually forbidden to fever patients, might be given to them with safety.

[“The feeding of fever,” here spoken of, may be very necessary in Irish typhus, but is not well adapted to the already well-fed whites and blacks, in the acute fevers of the Southern States of this Republic. The novelty, brevity, simplicity and alliteration of this maxim, constitute its chief merits; but, nevertheless, these do not compensate for its mischiefs out of Ireland. Fortunately, however, very few patients can tolerate this full feeding in the active state of fever, as they are devoid of appetite for food, which, if taken, is soon vomited, in most cases, or it aggravates the fever, even in cases wherein diffusible stimulants are beneficial.

A few years ago, when the opposite extreme, starvation, was fashionable, some celebrated pathologists attributed danger, relapse and death, to two teaspoonfuls of chicken-water, and the like meagre diets.]—
Ed. N. O. Med. and Surg. Jour.

ART. VIII.—*The Non-inflammatory Nature of the Ordinary Bronchial Complication of Typhus:* by DR. STOKES.

You will commonly hear it said, that this or that patient in typhus has got bronchitis; and, if we were to be guided by physical signs alone, such statement would seem to be correct. But I wish you to believe that the essence of this affection is not bronchitis, but rather a special condition of the air-passages, secondary to typhus fever, the result either of typhus deposit, or of the vascularity with turgescence, of which I spoke in a former lecture. If bronchitis—that is to say, if inflammatory action supervenes—it must be considered either as reactive or specific. I am anxious to impress this upon you, because there are still many practitioners who hold that the physical signs of bronchitis are sufficient to establish the existence of inflammation. Now, I do not know any characteristic difference between the physical signs which may occur in ordinary idiopathic bronchitis, and those which present themselves in typhus, when the air-tubes are engaged. In both you have sonorous, sibilant, mucous and crepitating râles; and yet the two diseases are pathologically distinct. Observe, that whatever will diminish the calibre of the tubes, whether it be deposit, typhoid congestion or true inflammation, will give rôle; whatever causes secretion, whether it be true inflammation, or something the very opposite of inflammation, will give you rôle. We have,

as I said before, in typhus, the physical signs which are observed in true bronchitis; but beware how, in any given case of fever, you conclude from their presence that the patient has true bronchitis. In certain cases there may be reactive irritation; but never forget that the typhoid disease alone, without any inflammation whatever, is competent to produce all the signs of bronchitis. Why do I urge this so much on you? Because I wish to avail myself of every opportunity of removing from your minds the erroneous doctrines of inflammation which have been so long in vogue. We are greatly influenced by names; and though I do not suppose that there are many who would treat a case of the bronchial affection in typhus with the same reducing measures which they would employ in the idiopathic disease, yet I am sure that the idea of these signs proceeding from inflammation makes many of us, who have not yet unlearned our early teachings, timid in the use of stimulants.

We find that this bronchial disease runs a course exactly analogous to that of the other secondary affections of typhus. It comes on insidiously, or, as I said before, silently; it gradually advances to its maximum, and sometimes increases to that degree that the patient dies by asphyxia. This is often the case, when the disease has not been recognized at an early period. It is in almost all cases preceded by the symptoms of typhus for several days. I think in the best marked cases it first shows itself about the fourth or fifth day of the disease; but it may supervene at any period of the case. It subsides spontaneously. You will have abundant opportunities of observing the following curious circumstance in the subsidence of this disease, either when the affection runs its natural course, or when it has been necessary to treat it specially. In the true idiopathic bronchitis, when the patient is placed under treatment, we observe the disappearance of the râles to be gradual; they are less intense and less complicated day by day; and this goes on probably for a week or ten days, or it may be a fortnight, before the last shade of râle disappears. In the typhus affection, on the contrary, you will often observe that the most extensive, intense, and complicated râles disappear as if by enchantment, leaving the respiratory murmur perfectly pure. This sudden disappearance of the physical signs, is only an argument among many to show their non-inflammatory origin. Nothing can be more remarkable than this; it seems analogous to the sudden disappearance of the eruption of scarlatina from the skin. You may often see this eruption lasting for three or four days, and then suddenly disappearing, leaving the skin white and pure. Consider the case of the lung in the same way, and in place of the scarlatina eruption, take the secondary bronchial disease or eruption, if you will, and you can understand the occurrence of a similar change. Mind, I do not say this happens in all cases; and I suppose that for its occurrence it is necessary that there shall have been little, if any, re-active irritation. And, as I said before, we shall see it in cases not only where the disease has been little, if at all, interfered with by treatment, but in others in which we have used such remedies as dry-cupping, counter-irritation, and various

stimulant medicines. Here the practitioner is often surprised at the rapid and complete success of his treatment, and may take credit to himself for bringing about a change which was, to a great degree, at all events, induced by the operation of the law of periodicity.

In the next place, we find that the best treatment in such cases is the stimulant. The mere circumstance of a patient having or presenting the most intense signs of bronchitis in typhus fever, does not by any means warrant us in bleeding him, in reducing him, in exhibiting tartar emetic, or in withholding wine. Nothing of the kind; the best treatment for such cases is the free use of wine, of ammonia, turpentine, bark, and such measures.

Another argument is drawn from this interesting fact, that in a large number of cases of softened heart in typhus, we find a combination with the bronchial disease; and it is quite fair to conclude, that the conditions of the lung and heart in these cases are similar. The practical conclusion, then, to be drawn, is that the physical signs of bronchitis in a case of maculated typhus fever should not make you conclude that the patient had bronchial inflammation; and, therefore, you should not treat the case as such.

There is a remarkable case of fever which is not at all uncommon in this country, in which we have an alternating disease, as it were, between the abdominal and the pulmonary organs in typhus fever. This is a very bad form of fever—one of the worst. We find that to-day, we shall say, the chest is greatly loaded, that you get no good respiratory murmur; there are most intense râles, and all the symptoms of extensive disease of the lung. At this time, the belly is soft; it is not tender on pressure; and there is no diarrhœa. Things go on for two or three days, when we find the belly to be swollen, tympanitic, tender on pressure; there is diarrhœa; and on applying the stethoscope to the chest, we find it comparatively free, and the râles either gone, or almost altogether gone.—*Ibid.*

ART. IX.—*The Relative Frequency of Chest and Head Symptoms in Fever:* by DR. STOKES.

With respect to the question of the comparative frequency and importance of the pulmonary, as contrasted with the nervous symptoms in fever, as it affects the lower and upper classes in this country, my decided impression is, that taking the experience of the last twenty-five years, the secondary bronchial, or, to speak more generally, the pulmonary complications are much more frequent and dangerous in hospital, than in private practice. It is not easy to explain why this should be so, but certainly we find a greater preponderance of nervous symptoms in the typhus fever, as it affects the upper classes of society, than in cases of the disease as we meet it in hospital; while in hospital prac-

tice the nervous symptoms, though we cannot say that they are absent, seldom require any very special interference on the part of the practitioner. No doubt we meet with coma and subsultus tendinum occasionally, but that predominance of nervous symptoms in the early periods of typhus, which is so common in the upper classes of society, is but rarely seen in our wards; and if you will only reflect on the simple fact, that we so rarely have occasion to shave the head among our hospital patients, you will fully see the truth of what we say. Remember, too, how many cases we have had, in which, while all the symptoms of typhus—such as prostration, weakness of the heart, eruptions of maculæ and petechiæ, and well-marked secondary diseases of the mucous membrane of the intestines—were present, while the patients' minds remained quite unclouded, and while no symptoms occurred calling for any special measures directed to the head. The typhus of Ireland, then, is not characterized, as Dr. Lombard has described it, by a preponderance of cephalic symptoms, at least when it occurs in that class from which he supposes the best specimens of the disease to be drawn. He is as incorrect in his statements about the predominance of cephalic symptoms, as when he says that the absence of follicular ulcerations of the intestines is a distinctive mark of Irish typhus.

In the typhus fever of the upper classes in this country, the nervous symptoms are generally much more aggravated and developed at an earlier period; and it may be that this preponderance of the nervous symptoms, this tendency to affections of the brain in one class, even though these affections be principally neurotic, is a cause of the comparative exemption of such cases from the secondary bronchial disease.—You will find, as you advance in the study of general pathology, plenty of examples in which diseases of structure or of deposition are suspended or replaced by purely nervous affections. Explain it as we will, the general proposition appears true, that the nervous symptoms, comparatively speaking, are but slightly developed in the fever of the lower classes, while those indicative of nervous disease are much more prominent; and conversely, that in the upper ranks symptoms indicative of irritation of the mucous surfaces are less developed, while the nervous symptoms are severe; and this, perhaps, may throw some light upon the doctrine which has been long held by many, that the mortality of fever is greater in proportion as we ascend in the scale of society.—*Ibid.*

ART. X.—*Quickness of Pulse after Fever:* by DR. STOKES.

There is a case in the small fever ward to which I would wish to direct your attention. Although the patient has convalesced after a long fever, and is now gaining flesh and strength, we have found that the pulse continues rapid. Now, this is a circumstance which must

always excite suspicion. In this patient, the signs of abdominal and pulmonary lesion have disappeared, as well as the characteristic expression of what may be termed the condition or state of fever—yet, we find that his pulse does not correspond with the signs of improvement in all the other functions. It was suggested by Lænnec, that the rapidity of pulse in patients after fever, might depend on softening of the heart; but we shall see, by and by, that the true typhous softening of the heart, so far from inducing rapidity of pulse during convalescence, has much more frequently the effect of making it slow—not only slow as considered with reference to the condition of health, but actually falling below the ordinary standard.

These cases of quickness of pulse are of two kinds. In one class, the pulse has never lost the rapidity it attained during the fever; or, it has, perhaps, come down fifteen or twenty beats in the minute, and its rate then remains stationary. In the other cases, the pulse, which had become quiet, rises to 100 or 120, or even higher, and remains at that rate for days together, without our being able to detect any cause for this increased rapidity. This, I think, is the worst case of the two; at least, it appears more often to indicate a new pathological change.

The local diseases which have been found most frequently to attend this condition, are of two kinds: one of them is tuberculosis—the deposition of tubercles in the lungs and other parts; the other is the existence of a secondary reactive inflammation in the mucous glands of the intestines. To this subject Dr. Cheyne long ago drew attention, in speaking of imperfect convalescence in typhus fever; and he gives several cases in the Report of the Hardwicke and Whitworth Hospitals, in which patients had recovered well from typhus fever; had, to all appearance, regained a certain degree of strength; had regained their appetite; but they showed no disposition to leave their beds; the pulse gradually got quicker and quicker; the belly swelled; diarrhœa came on; and the patients died with symptoms of disease of the intestinal canal. Upon dissection, extensive ulceration of the mucous glands was found in the intestine. These are the two most common of the local diseases which you should suspect, when you have a patient who has gone through a long fever with the pulse continuing or becoming very quick.

But now suppose that you examine such a patient with great care. You percuss his chest; you examine the state of his respiration in every way, and you cannot satisfy yourself that there is any disease in his lung. Well, you may make up your mind, from the absence of all these signs, that the patient is not becoming tuberculous, at all events.—When you proceed to examine the abdomen, you will find, perhaps, that he has a good appetite; that his thirst is gone; that the belly is hollow and soft, and there is no tumefaction of it; that there is no tenderness on pressure anywhere; no throbbing of the abdominal aorta; no tendency to diarrhœa—in fact, no one symptom of disease of the mucous membrane of the intestine. And yet you have a pulse with this unpleasant degree of quickness.—*Ibid.*

ART. XI.—*Simaba Cedron in Intermittent Fever.*

Dr. S. S. Purple, of the *New-York Journal of Medicine*, arrives at the following conclusions in regard to the remedial effects of this article:

That it possesses decided anti-periodic properties, and is, therefore, applicable in the treatment of periodic diseases.

That it is less likely than quinine to produce the aggregate of encephalic or menophatic phenomena, induced by over doses.

That it may, in large doses, repeated often, produce griping of the bowels, and even diarrhœa; but that these conditions are easily controlled by appropriate medicaments.

That, as a remedy in intermittent fever, it possesses properties, in many respects, equal to quinine, and in most cases is equally adapted to the curat.ion of this disease.

That, in the treatment of yellow fever, it does not appear to possess any particular advantage over quinine, but, nevertheless, is equally well adapted to fulfil the indications which call for the use of this latter remedy.

That it possesses marked tonic properties, and deserves a prominent place in this classification of the *Materia Medica*.

That in chronic dysentery, diarrhœa, dyspepsia, and all states of the stomach accompanied with impaired or difficult digestion, its use will be found to be attended with benefit.

That, should a demand arise for its use in medicine, it is believed that it will be found not difficult to obtain a supply, in quantities sufficient to afford it at much less price than quinine.—*New-York Journal of Medicine*.

ART. XII.—*Phthisis.*

Night Sweats of.—Give the following draught at bedtime: R. Acid. gallic. gr. viij.; morphiæ acet. gr. $\frac{1}{2}$; alcohol q. s. (a few drops); syr. toltan. ʒ ss.; aquæ ʒj. The night-pill of the Brompton Hospital is as follows: R. Acid. gallic. gr. v.; morphiæ hydrochlor. gr. $\frac{1}{2}$; mist. acac'æ. q. s. ft. pil. ij. (Mr. Hutchinson.) Give gallic or acetic acid. Dip the night dress in sea-water, or salt and water, and dry it before using. But the best remedy is four grains of oxide of zinc at bedtime, combined with a little henbane or hemlock.

Cough of.—Mix one part of chloroform with three parts of spirits of wine, and let the patient inhale when necessary, but with caution, and under medical direction. The inhalation of camphorated spirit is often sufficient, or even the vapor of hot water, or infusion of hops. Sometimes frequent deglutition as the swallowing a little oil, will relieve the cough. Sometimes four minims of tincture of aconite is a good palliative.

Profuse Expectoration of—To check this give creasote, pyro-acetic spirit, infusion of pitch, or balsam of tolu; but by far the best remedy is petroleum or Barbadoes tar, which often moderates the cough and expectoration remarkably. (*Braithwaite's Retros.*)

ART. XIII.—*Observations of Morbid Changes in the Mucous Membrane of the Stomach:* by Dr. HANDFIELD JONES, Assistant Physician to St. Mary's Hospital.

(Medico-Chir. Trans.)

The first part of this communication comprises a description of the minute glandular structure of the mucous membrane of the stomach, in which the author corroborates the account given by Kölliker. On first commencing his researches into this subject, he was not aware that lenticular or solitary glands had been seen in the mucous membrane of this viscus. The author not imagining they could be normal structures, had at first viewed them as simply nuclear deposits, supposing they were of new formation. Kölliker had observed that these lenticular glands did not constantly occur in the stomach of adults, even though they might be possibly always present in those of children. In very many cases he had met with no traces of them; in others they were seen to be extremely numerous, covering the whole surface of the stomach; yet the thought could hardly be excluded, that the morbid conditions of the part, which were always present, had not had something to do with their formation. The author thought it difficult to fix any exact limit to the healthy development of these glands. He considers the gastric tissue in its most normal and efficient state when there were but few of these glands or nuclear masses, and when those that existed did not encroach materially upon the tubular or gastric glands of the stomach. He thought great individual varieties might exist; that they were naturally larger and more numerous in some individuals than in others. He ventures to think that these solitary glands and their groups in the intestines (Peyer's patches) had really no use, and fulfilled no function in the human body, but existed in a rudimentary state in obedience to the law of unity of type. They might be regarded as portions of undeveloped embryo substance, existing in inverse ratio to the surrounding specially organized tissues, and with this view their simple nuclear structure, so common in embryonic parts, was very accordant. Dr. Jones's opinion is, that the epithelial contents of the tubular glands are thrown off during digestion, and form an important constituent of the gastric juice, probably the so-called pepsin. The evidence of this rests on examination of the stomachs of animals killed while digestion was proceeding, and of a man who died suddenly soon after a meal. The following deviations from the typically healthy condition of the stomach are mentioned as examples of morbid changes:—

1. *Nuclear masses*.—It is doubtful what degree of development of these is to be considered as surpassing the physiological limit; but observation proved that they became both hypertrophied and atrophied, and the latter seemed to take place by a kind of liquefying, so that a cavity was formed containing a clear fluid and some nuclear corpuscles.

2. *Diffused nuclear formation*.—The effect of this is, that the tubes become more or less atrophied and obscured by interstitial deposit.

3. *Intertubular fibroid formation*.—The tubes become atrophied by the presence of a fibroid or granular deposit, in which some altered vestiges of the tubes might be brought into view by acetic acid.

4. *The tubes appear to decay spontaneously*, but not from the presence of new fibroid tissue.

5. *Black pigmentary deposit*, occasionally within the tubes, more often between them; sometimes yellow pigment is found; both may be regarded as altered hæmaturia.

6. *Cystic formation*: produced in one of three ways: First, a nuclear mass liquefied and left a cavity; secondly, white atrophy of the tubercular glands was going on, and a portion of one became distended; thirdly, a cyst was produced as a large vesicle, a true new formation.

7. *Mammillation*, usually affecting the pyloric region.

8. *Gathering up of the lower parts of the tubes*, so as to form a group of convolutions like the acme of a conglomerate gland.

9. *Unhealthy condition of the epithelium of the tubes*, occasionally exhibiting the characters of a fatty degeneration.

10. *Self-digestion* was of frequent occurrence, and invariably confined to the splenic region: the mucous membrane was more or less deeply colored, thinned, smooth, and semi-translucent. In extreme cases the nerves and vessels were seen altered, as when treated by strong acetic acid.

11. *Small dark-red circumscribed spots*, manifestly the result of hæmorrhage; ulceration often takes place in these.

12. *The tenacious adhesive mucus of gastric catarrh*.—Its microscopical characters are very clearly described.

The author has observed torulæ in the mucus of the stomach of a diabetic patient. The paper is accompanied by a table of 100 cases of post-mortem examinations, in which the morbid changes in the mucous membrane of the stomach are fully and minutely described, together with an analysis of these cases, in which the influence of age and sex, habit of life, &c., are considered, as well as the frequency of the several morbid changes already enumerated. Eight drawings, executed by the author, illustrate very intelligibly the diseased conditions which the microscope had revealed.

In further elucidation of the subject of this paper, Dr. Jones says, that all that his inquiries went to show was, that degenerative changes might be going on in the stomach concomitant with other diseases in which there was a degenerated state of the blood, as anæmia, and diseases of a like character. This degeneration, too, might be progressing

without attention being specially called to the stomach, the prominent symptoms of dyspepsia being absent. This was explained by the circumstance, that when the stomach was in a tolerably healthy state, sensations of dyspepsia might exist; but when there was degeneration, such as he had described, the sensibility was destroyed, and no symptoms of indigestion presented themselves. His observations, then, had made no advance in the treatment of dyspepsia. As our knowledge advanced by after-labor in the course which he had commenced, we might, perhaps, be enabled to detect the early symptoms of this or similar diseases, and be able to guard our patients against habits which would produce them, and which, when once established, were as fatal as Bright's disease, or other affections coming under that category. These inquiries would call upon us to watch carefully the symptoms of failing health in our patients, and so, perhaps, prevent the occurrence of incurable disease. He had no suggestions as to treatment, except that of support generally. With respect to the connection of the disease with delirium tremens, he had seen two cases of this affection in which the degeneration existed; there was nothing particular in them, and the delirium tremens was associated with granular disease of the kidney, or some other disease.—In reply to other questions, Dr. Jones says that he had found the changes to which he had referred, connected usually with depressing diseases, such as phthisis, anæmia, &c. He had had no opportunity of knowing, as the patient had usually died from acute diseases in the hospital, whether they had suffered from dyspepsia in former years, or whether they had been treated by drastic purgatives and other active treatment. It was probable, however, that they had suffered from dyspepsia. The change he had described differed altogether from that which was observed as the result of inflammation; the tissue was remarkably pale, and there were no traces of injected vessels.—*Ranking's Abs.*

ART. XIV.—*Remarks on the Influence of Fear in producing Functional Derangements:* by JOHN B. COWAN, M.D.

The powerful influence exercised by mental emotions on the condition of the human frame, has long been recognized. Under their influence, the flow of saliva may be checked—that of urine may be increased—tears may be produced in inordinate quantities—diarrhœa, or copious perspiration, may be brought on. But although these facts have been clearly ascertained, it is difficult, if not impossible, to trace the definite connection betwixt the physical organization and the mental manifestations, in virtue of which these effects follow.

The prevalence of a fatal and wide-spread epidemic affords, however, an admirable opportunity of observing the influence of one mental emotion—Fear—in producing, or assisting to produce, certain morbid states

of the system. Many writers, both on metaphysics and on medicine, have alluded to the symptoms of bodily and mental derangement caused by fear.

Burton, in his *Anatomy of Melancholy*, says: "Many lamentable effects this fear causeth in man, as to be red, pale, tremble, sweat; it makes sudden cold and heat to come over the body, palpitation of the heart, syneope, &c. It causeth oftentimes sudden madness, and almost all manner of diseases." And again, after narrating the effects of terror which followed the massacre at Lyons, in 1572, he adduces the instance of "Themison, the physician, who fell into a hydrophobia by seeing one sick of that disease." In another part of his work, he makes the following very apposite observation: "Men, if they see but another man tremble, giddy, or sick of some fearful disease, their apprehension and fear is so strong in this kind, that they will have the same disease;" and quoting from Dr. Cotta, narrates two stories, "the one of a parson's wife in Northamptonshire, *anno* 1607, that, coming to a physician, and told by him that she was troubled with the *sciatica*, as he conjectured, (a disease she was free from,) the same night after her return, upon his words, fell into a greivous fit of a *sciatica*: and such another example he hath of another good wife, that was so troubled with the eramp; after the same manner she came by it, because her physician did but name it." These, however, are rather instances of the force of imagination acting upon weak minds, than of diseases caused by fear.

Dr. Darwin, in his great work on the *Laws of Organic Life*, treats of the diseases of association, under which he classes those produced by fear. His theories on this, as on other points, deserve attentive consideration, as the results of the studies of an acute observer and original thinker. He accounts for the increased flow of pale urine in hysteric diseases, by supposing that "the motions of the absorbent vessels of the neck of the bladder become inverted by their consent with those of the skin, which are become torpid by their reverse sympathy with the painful ideas of fear." The same effect may follow from anxiety, where there is little fear; as an instance of which, the frequency with which young men about to be examined for a degree pass urine, is cited. His theory of *Diarrhœa a timore* may be quoted entire: "The absorbent vessels of the intestines invert their motions by direct consent with the skin; hence many liquid stools, as well as much pale urine, are liable to accompany continued fear, along with coolness of the skin. The immediate cause of this is the decreased sensorial power of association, which intervenes between the actions of the absorbents of the cold skin, and those of the intestinal absorbents; the motions of the latter become on that account weakened, and at length retrograde.—The remote cause is the torpor of the vessels of the skin, eatenated (in plain English, linked,) with the pain of fear. The capillaries of the skin consent more generally by direct sympathy with those of the lower intestines, and of the bladder; but by reverse sympathy, more generally with those of the stomach and upper intestines. As appears in fevers,

where the hot skin accompanies indigestion of the stomach; and in diarrhœas, attended with cold extremities. The remote cause is the torpor of the skin, owing to its reverse sympathy with the painful sensual motions, or ideas of fear, which are now actuated with great energy, so as to deprive the second link of associated motions of their due share of sensorial power. It is also probable, that the pain of fear itself may contribute to exhaust the sensorial power, even when it produces no muscular action.*

Dr. Holland, in his *Medical Notes and Reflections*, devotes a chapter to the effects of mental attention on bodily organs. He shows that direct effects follow from consciousness being, by a distinct voluntary effort, directed towards organs or parts of the body. Of the force of this statement, every one must be easily convinced. Among other instances, the state and action of the bowels is alluded to as thus influenced. The attention being concentrated on them, sensations previously unnoticed are experienced, and their action excited and quickened. But this, after all, amounts to a species of fear. If not actually commencing, as it is most likely to do, from apprehension or dread, caused, perhaps, by some reference to that part of the system, the consciousness, unless kept concentrated by fear, is not likely to continue long directed towards it; or the consciousness will degenerate into fear. Feuchtersleben, in his work on *Medical Psychology*, says: "Fear causes especially enuresis, diarrhœa, seminal discharges, erysipelas, and eruptions about the lips; facilitates the reception of contagion and miasma, disturbs crises, and aggravates every disorder."† After enumerating instances of actual organic lesions produced by the evidence of this emotion, and instancing its well-known effect in causing jaundice, he adds, "Here we ought to go further, and pass on to the psychical causes which act on the nervous principle; but the quality of these by no means explains their mode of action. Fear and horror act, moreover, variously—either exciting or paralysing, according to the greatness of the danger, and according to the individuality of the persons affected by them."

There cannot be the slightest doubt that the presence of Asiatic cholera causes in a community, and in individuals, a dread and a terror, which is not exhibited in anything like the same extent during the prevalence of other epidemics, scarcely less fatal. The reasons why cholera should excite such powerful emotions, are sufficiently obvious. As yet, it may be regarded, in this country at least, as a disease of comparatively modern origin. Its exciting causes appear as inscrutable, as its removal seems beyond the reach of sanitary measures, or the best applied efforts of medical art. Its very suddenness is appalling, so that we have all the elements to keep alive and foster fear. Fear seems to produce, during an epidemic of cholera, no well-marked effects upon those who are under its influence. The one of these, as might be anticipated, is a species of hysteria, so characteristic that it might be designated by the terms *choleraic hysteria*; the other is actual diarrhœa or vomiting.

* *Zoonomia*. Part II., p. 523.

† *Sydenham Society Edition*, p. 186.

The hysterical symptoms are most frequently met with in females; but the writer has seen one case in the male, which appears to him interesting. On the 26th of December, last year, shortly after cholera made its appearance in Glasgow, he was called late at night to see a young man of moderately robust make, and whose employment was that of a groom. His habits were remarkably temperate, and in every respect he was a steady and good servant. He was found walking up and down his room in a very excited state, and occasionally applying his hands to the abdomen, and seemingly disposed to vomit. On inquiry, it was ascertained that he had no symptoms of diarrhoea or vomiting; but he declared that he felt that if he lay down in bed he would immediately purge and be sick. Persuasion was utterly useless, and so was abuse or ridicule. His master lent assistance to strip him by force; he was compelled to lie in bed, and a strong opiate was administered, under the effects of which he was soon in a profound sleep. He awoke quite well on the following morning, but still laboring under mental agitation, and declared that he had felt convinced the previous night he was dying of cholera. This man seems afterwards to have quite overcome his fear as regards cholera, having watched for a considerable time by the death-bed of a fellow-servant who had been attacked by that disease.

The following instance was related to me by a medical friend:—

One evening, lately, I visited two young ladies, between 20 and 30 years of age, in whose house a relative had died of cholera the previous week. Since then, they had been affected with the most overpowering fear of the disease. They would not eat for fear of inducing vomiting, and felt persuaded that they were both about to take cholera. They felt sick, and had an uneasy sensation over the epigastrium; and though neither of them had previously had any hysterical or nervous affections, they now frequently fainted, felt alternately cold and hot, and had occasional shiverings. They refused to lie down for fear they should become sick, and scarcely slept at all at night. They had taken no nourishment for four days, except little bits of biscuit, and a mouthful of cold water with some aromatic substance in it; and they could hardly be persuaded to swallow a little wine and water. They had not vomited at all, and they had resisted the inclination and succeeded in preventing any passage from their bowels for six days, under the delusion that it was the safest way to prevent diarrhoea. They were constantly moving about to assure themselves that they were still unattacked; and as night drew on, they felt perfectly miserable at the thought of requiring to retire to bed. Altogether, I never witnessed such a lamentable example of the effects of fear. I persuaded them to take some tea and toast, and a little negus, on going to bed, and ordered a laxative pill to both, assuring them that the nourishment would strengthen them, and that they might expect to be better in the morning. They slept pretty well, and in the morning felt more composed, principally, I presume, from the assurance I had given them, that they would be better. The pills operated mildly, and had the effect of relieving some of the uneasy sensations. They still

disliked the idea of taking solid food, but gave in when I insisted on their doing so, as well as taking some wine several times. Having once overcome the fear of taking food, they soon regained their strength.

Dr. Stephen has detailed to me a most interesting case—that of a man whose dread of thunder was such, that during a thunder-storm he had invariably an attack of diarrhœa. During the epidemic of cholera, in 1849, this person resided in Hamilton; and when the disease appeared in that town, he kept himself closely secluded in his house, never venturing out. Towards the close of the epidemic, on a day during which he had heard there had been no fresh case of cholera, he went out, and Dr. S. had some conversation with him. He was attacked by cholera, and died on the following morning; and his was the last fatal case but one which occurred. It seems apparent that this man felt himself secure so long as he lived secluded; and probably it was to that feeling of security being destroyed, and agitation, perhaps, induced by conversing on what to him was an all-absorbing topic, that the seizure was in some degree to be attributed. Similar examples might be multiplied, but those narrated are sufficient to indicate the character of a class of cases, which all medical men will recognize as having been of frequent occurrence during the last few months.

To such an extent has this fear of cholera existed, that it has led to the commission or omission of acts discreditible in the highest degree to those concerned. We learn from the public press, that one unfortunate man was left to die on a public quay; and this is by no means a singular case of desertion of duty. Husbands have been known to desert their wives, parents their children, and children their parents; while the relatives of those who die of the complaint, hasten to bury them within a few hours of their decease. No wonder the disease should spread in every locality, while such a panic continues to prevail.

A fear of the existence of the disease has often been produced by the too indiscriminate employment of opium and other astringent medicines. A loose evacuation called for brandy and laudanum. These were taken, the stomach became deranged, vomiting perhaps occurred, and even cramps, real or imaginary. The medical practitioner was hurriedly summoned to a case of cholera, and from the excitement and vague statements of the patient and attendants, would find it extremely difficult to discover the real state of matters. Or again, a person has been constipated for some days, takes before retiring to rest some laxative medicines, which, beginning to operate, causes alarm. Opium is resorted to, and the antagonistic action set up, soon produces general disorder of the system, increased by mental perturbation not easily allayed.

Although, however, fear undoubtedly produces such derangements as those briefly touched upon, as well as others to which no allusion has been made, it is obviously impossible to trace either its direct influence, or to assign to it its due share in causing these effects. But believing, as all practitioners must do, that the violence of this mental emotion

predisposes to, if it does not actually produce cholera, it is a point, we apprehend, well worthy of consideration, whether no means exist by which it can to some extent be controlled. Every effort should undoubtedly be tried to put an end to cholera being made a constant theme of conversation during its prevalence, and to discourage the practice which has become so common, and is so fraught with injury and danger, of making the public press the vehicle for discussing its various phases and modes of treatment. Any one who has glanced at the "*Times*" newspaper for many months past, must have been struck with the innumerable infallible modes of curing cholera, which have been promulgated through its columns, equally unworthy of regard, whether emanating from "Eastern Travelers" or "Hospital Physicians." The last and most notable instance of this, was the publicity given to a plan of treatment, by no means new, which, it was stated, had been adopted with success in some dozen cases, and which received the approving fiat of the great organ of public opinion.

But while some slight degree of good may result from attempting to check the tendency of the public mind to dwell upon such an alarming subject, the true source of all this pusillanimous dread lies much deeper, and cannot, we fear, be reached. It is the result of an educational system conducted upon erroneous principles, and the errors and defects of which, more especially as regards the female sex, have frequently been pointed out, and by none more forcibly than by Barlow, who has in a very striking manner shown the fruits of misdirected early training in producing insanity. To the same identical causes, the want of self-control, of moral courage, and, in the case of females of the higher classes, of interesting occupation and active exercise of the mental powers, may be traced the existence and frequency of a fear during epidemics, which is demoralizing in its effect upon a community, and is actually fraught with danger, not only to those who indulge in or foster it, but to the public at large, by its undoubted tendency to increase the prevalence and fatality of the epidemic. It is earnestly to be hoped that any future outbreak of cholera may be distinguished by the entire absence of such cases as those shortly related, and may not be aggravated by the violence of such a depressing mental emotion as fear.—*Glasgow Med. Jour.*, Oct., 1854.

ART. XV.—*Death in Typhoid Fever from Intussusceptio.*

Prof. Geo. B. Wood, President of the College of Physicians, of Philadelphia, relates briefly, in the transactions of the society, the following case of Typhoid Fever which had recently terminated fatally in the hospital, from intussusceptio of the jejunum. The patient was a woman, and, though very ill, began to evince signs of amendment, when she was attacked with vomiting, which nothing could permanently arrest. Anodyne enemata and a blister to the epigastrium, had some effect in quieting

the stomach, but only for a short time. Medicine and food were rejected, and she died at the end of about a week from the supervention of the vomiting, utterly prostrate, having been cold and almost pulseless for some days before death. The cause of the symptoms and the result, were made plain upon *post-mortem* examination. Numerous ulcers existed in the ileum, but they were all in a fair way to recovery, and some had nearly healed. But in the jejunum, at no great distance from the duodenum, were two invaginations, one involving about two inches of the bowel, and the other from three to four inches; and, in the latter, some force was required to pull out the invaginated portion, which was remarkably white, as if deprived of blood. Inflammation had not taken place, perhaps in consequence of the low state of excitability of the parts. The probability is, that the intussusception occurred about the same time with the vomiting, and was the cause of it. The patient died of exhaustion, as nourishment could not reach a sufficient extent of bowel to permit an amount of absorption adequate to the support of life.

ART. XVI.—*Treatment of Cholera.*

1. *On the Injection of the Cellular Tissue with Water, as tried in the year 1848, in the Treatment of Cholera:* by A. BUCHANAN, M. D.

(IN A LETTER TO THE EDITOR.)

DEAR SIR—As you have inquired of me more than once, as to the trials made in the year 1848, of injecting water into the cellular tissues as a remedy for cholera in the stage of collapse, I send you a statement of what I recollect respecting it; for the results, unfortunately, were too little important in my estimation even for insertion in my note-book, and I certainly never expected to have been asked to place them on a more permanent record.

The proposed remedy seems to me to rest on sound pathological views, as the principal symptoms of the collapse in cholera are directly referable to the want of water in the constitution of the blood, and other fluids and solids of the body; and no mode of treatment can be of any avail, that has not for its object to restore the lost liquids. The injection of water into the blood-vessels, by which that object is most directly accomplished, having been found after many trials, not to answer the expectations entertained of it, the only remedies which my experience of the disease in 1832-3 led me to think of any service, were the free use of drinks, the plentiful injection of warm milk into the bowels, and fomentations to the external surface. During the interval between the first epidemic and that of 1848, it occurred to me that the absorbent power of the cellular tissue, which has been found so active with respect to poisonous substances, might possibly be made available for introducing liquids into the system in cholera. Accordingly, this was

the very first remedy of which I made a full trial on the opening of the Cholera Hospital in 1848. It was before I was myself encumbered with any official duty in the wards, and had therefore full time for such an investigation, while, through the kindness of the attending physician, I had every possible facility for carrying it out. The object I had in view was to distend the cells of the cellular substance with water, just as we see them distended with air in emphysema, or with serous fluid in anasarca; and I hoped to be able to effect this by a process much the same as that by which we see butchers blow up veal. I found it, however, to be altogether impracticable. I succeeded, indeed, in injecting water under the skin; but, in every instance, I found that I had injected it, not into the cells of the cellular membrane, but into a cavity formed by disruption; that is, by tearing the cellular tissue, and separating it from the skin and subjacent parts. I tried it, with the same unsuccessful result, both on the limbs and the trunk of the body. I recollect, in particular, of producing on each side, over the ribs, a swelling of the size of a child's head, but flatter. It looked like an abscess, and was probably exactly of the nature of a thrombus, or an ecchymosis, the result of violence. Although compressed by means of a bandage, it did not show any tendency to disappear, either by being absorbed into the blood-vessels, or diffused into the adjacent cellular tissue. I therefore abandoned the attempt, being satisfied, first, that I could not inject water into the cellular tissue so as to produce an equable distension of the cells; and second, that the injection in the way in which I employed it, was of no use as a remedy for cholera.

Yours very truly,

A. BUCHANAN.

Glasgow Med. Jour., Oct. 1854.

2. *Injection of the Peritoneal Cavity with Water in Cholera: (Braithwaite's Retros.)*

On the supposition that the collapse of cholera is due to the loss of fluid which has been occasioned by the vomiting and purging—a position which is by no means tenable—Dr. B. W. Richardson argues that good might follow the injection of water into the peritoneum or cellular tissue *in extreme cases*. The idea is not without ingenuity. Dr. Richardson writes:

“The most important step that has as yet been made on the human subject towards supplying the exhausted system with fluids in cholera, is that of throwing a considerable quantity of fluid directly into the circulation through the veins; and it cannot be denied that, in practice, this plan has been attended with some amount of success. In contemplating the subject of transfusing watery matter into the system, the question occurred to me, whether any other means existed of introducing fluid matters into animal bodies in very large quantities, and in a manner which should secure their absorption. This thought led me to look back upon the pathological characters and treatment of those diseases in

which serum is largely thrown out into serous cavities or cellular tissue. I recollected that, in cases of this class, (dropsical cases,) not only were several gallons of fluid often thrown out of the circulation into the system without immediate danger, but that patients thus situated could tolerate a degree of purgation which would absolutely destroy healthy individuals. This immunity must arise solely from the fact, that the effused serum lies as a reservoir, from which the circulation is fed while the purging is going on. The idea furnished an important suggestion; and I consequently commenced a series of experiments on animals, which, as they are as yet in an imperfect state, I shall only describe in general terms, reserving the particulars for my next report.

“The experiments up to this time have mainly consisted in exhausting dogs and cats by starvation and violent purgation, with large doses of elaterium, and afterwards injected either their peritoneal cavities or their cellular tissue with large quantities of distilled water at the blood temperature. The results, in general terms, are as follows:

“I find that either into the peritoneal cavity, or into the cellular tissue, a quantity of water, varying from a tenth to a fifth part of the weight of the animal, may be injected with little risk. The effect of this is to induce a sleepy condition, which lasts from twenty to thirty hours, long before which time all trace of the injection is lost. If more than this is thrown in, the sleep, or rather torpor, (for the animal only remains quiet, and rouses when spoken to,) ends in death. This was the case in one dog weighing ten pounds, into whose cellular tissue I injected three pounds of water. In animals that have been much exhausted, more fluid may be injected, and the absorption is greatly quickened. In animals which have died from the experiment, and have been examined immediately after death, I have never found any trace of the injected liquid; but the blood has always been exceedingly fluid, and has coagulated slowly, if at all. I should notice also, that in two cases, in which a cat and a dog were operated on while under the influence of an anæsthetic agent, they died; the one in four and the other in forty-eight hours, without ever rallying fairly from the effects of the narcotic. I should mention, too, that I have met with one or two casualties in the course of these experiments, which have arisen purely from their novelty, and cannot be considered as bearing on the merits of the system.—Thus, in two young cats of the same age, I injected a sixth part of their respective weights of water into the cellular tissue of one, and into the peritoneum of the other; the result was, that the one whose peritoneum was injected, after remaining apparently well until three or four hours previous to death, died about twenty-four hours after the operation. On opening the body, all trace of fluid was absent; but I found that, from having made too large an incision into the peritoneum, and from the animal having reclined on the belly, about two inches of intestines had slipped into the opening, and had become strangulated and deeply congested. In another instance, where I inserted pins into two openings, erysipelatous inflammation came on in the surrounding skin; but not in another part of the body, where the wound was left untouched. In a

third casualty which occurred to me, I learned a very useful lesson. Into the peritoneum of a dog three pounds of water were thrown, and one pound into the cellular tissue of the thigh. The animal seemed pretty well for a short time after the operation, and the fluid was absorbed in the course of three or four hours, although it constituted a fifth part of the weight of the whole body. In a little time, the creature gradually became exhausted, and died in eleven hours. On opening the body, I found that all the fluid injected had disappeared; but in the peritoneum there lay about a pound of semi-coagulated blood, which had escaped from an artery that had been wounded by the trocar.—The blood in the veins and heart of this dog was like water itself, and showed no tendency to coagulate. The heart was flaccid and collapsed. In this case, the rapid absorption of the injected fluid arose doubtless from the hæmorrhage, and from the too rapid and copious flow of fluid into the exhausted circulation. For, while the blood lost its solid constituents on the one hand, it gained too large an amount of water on the other. This animal had been reduced somewhat by purgation. Previous to the operation, he weighed twenty-six pounds; immediately after it, thirty pounds; and, at his death, a little more than twenty-four pounds. He must consequently have made use of all the injected fluid, and have lost somewhat more than one pound additionally, during the interval between the operation and death. Setting aside, therefore, these casualties, I infer from my experiments as they now stand, that into the peritoneum or the cellular tissue of a patient in a proper state of collapse from cholera, water to the extent of at least a tenth, or even a fifth part of the whole weight of the body, might be injected with safety, and that the absorption would be almost immediate.

“I am not at this moment able to say which of the two kinds of operation is best, but I am strongly inclined in favor of injecting the peritoneum; for, although the absorption of fluid from the cellular structure seems most rapid, the difficulty of introducing water in full quantities into the peritoneal cavity is much less, and the pain attendant is considerably milder. In either case, too much care could not be taken to throw the fluid slowly, to have it at a temperature not less than 62° Fahrenheit, and, if the peritoneum is the part injected, to be mindful not to distend the cavity to the extent of interfering with the action of the diaphragm.

“As yet I have no favorable opportunity of trying the suggestion I have here thrown out, in cholera; but that it might be put into application in desperate cases, and might prove advantageous, I have no doubts whatever. The advantages of it would be:

“*First.* That an abundant store of fluid would be supplied, which the exhausted circulating system would rapidly and effectually take up.

“*Second.* That, as this imbibition would take place through the capillary system, the fluid would enter the veins freely charged with the solid constituents of the blood, and would pass to the lungs in a condition suitable for respiration.

"*Third.* That the process would in no way interfere with the administration by the mouth, of astringent solutions, or such other medicines or liquids as the practitioner might consider indicated.

"But although I am thus sanguine on this subject, I wish it to be understood that the first trial of this process should commence in a case where the patient is in the last stage of collapse, where other remedies have failed, and where death is imminent. This, it is true, would scarcely be putting the new remedial measures to a fair test; but my ideas of the value of life are so great that I would not, even in so important a disease as cholera, recommend the employment of a new remedy wantonly, or without that due care for the welfare of the sick man which every honest practitioner of medicine intuitively feels."

3. M. L. KNAPP, M.D., on Cholera. *

Discarding all the hypothetical causes of Cholera that may have been offered, such as contagion, infection, epidemic influence, an imaginary entity, possessing the quality of portability, astral influence, malaria, or miasmata, (whether vegetable fungi or animalculæ,) telluric emanations, supernatural agency, called the displeasure of Providence, or the vengeance of God; I look only to known, natural causes of disease, or extreme impressions of the natural vital stimuli, for the cause of Cholera, and a rational explanation of all the phenomena it presents.

Deviations from a healthful standard, in the impression of the vital stimulus of food, or alimentation, constitute one class of causes of disease.

Deviations from a healthful standard, in the impression of atmospheric air, constitute another class of causes of disease.

Deviations from a healthful standard, in the impression of heat, light and electricity, always united, so far as science reveals, constitute a third class of causes of disease: these second and third classes are called meteoric causes.

The question, then, naturally propounds itself here, which of these causes is most probably the remote cause of Cholera?

Looking at the phenomena as presented in the spread of Cholera, the two meteoric classes of causes named, covering high solar heat or insolation, vitiated air, vicissitudes, or sudden changes from heat to cold, and *vice versa*, humidity, rains, gales, tempests, thunder-storms, and the barometrical phenomena noticed, appear in immediate connection with its outbreaks, progress or aggravation, and are not only generally considered, but are universally held and conceded to be its *exciting* causes. Hence it follows, that some error in the vital stimulus of alimentation is

* Discovery of the Cause, Nature, Cure and Prevention of Epidemic Cholera; by M. L. Knapp, M.D., late Professor of Materia Medica, and President of the College of Physicians and Surgeons of the University of Iowa; late Professor of Obstetrics and Diseases of Women and Children, in Rush Medical College; Author of "Lectures on the Science of Life Insurance," etc., etc. New-York: Holman, Gray & Co. 1855. Pp. 27.

the *remote* cause of Cholera: there is no other remaining source in the world from whence to derive it.

Forced to this conclusion by sound logic, I would ask, in the next place, does the error in the vital stimulus of food, which produces Cholera, most probably come of a profusion, a poisoning, or a scarcity? All the world will answer the latter; for the poor and destitute are its special victims, while the rich, having means to purchase plenty and variety, though prices be high, and so to fulfill the omnivorous law, usually escape; and under a general poisoning of food, rich and poor would indiscriminately fall. Furthermore, the population of a country holds in the ratio of its productions: famine is always followed by pestilence.—Hence, I am forced again, by universal observation and the axioms of political economy, to conclude the error arises from an abridgment; and as the vegetable kingdom is most obnoxious to the meteoric influences causing blights, I infer the difficulty lies in an abridgment of the vegetable productions and stores, or a scarcity in kind, leaving the poorer classes on a cheap, stale, refuse, animal, coarse *one kind of diet*, without succulent vegetables.

The antiscorbutic remedies which the writer now sees were effectual on account of their antiscorbutic virtues, were the common salt emetic (chloride of sodium), which he was continually in the practice of administering as a first remedy, and soda powders throughout the attack. The soda powders were indifferently composed of the bicarbonate of potash or soda, and citric or tartaric acid. The salts of potash and soda, and the vegetable acids named, stand at the head of the list of antiscorbutic remedies; and the known efficacy of common salt in arresting hæmorrhage, also further explains its value in cholera. These views also throw light on the success that has attended the saline treatment.

The preventive relied on was punch, (lemonade dashed with brandy,) with five or ten grains of quinine to the quart; and when diarrhœa was present, a grain of morphine was added. This combination was relied on from having observed scorbutus complicated with vernal agnes, and midsummer bilious fevers, in the malarious districts of Illinois, for nearly twenty years, especially after cold winters; and from the further observed fact, that cholera delighted in the same localities, particularly along the Michigan and Illinois canal. The writer could go on and specify numerous instances of the prophylactic effects of the remedy in families where, one or two members being struck down with cholera, the balance were put under the daily use of the mixture and antiscorbutic diet, and escaped attack. Though nearly prostrated by a scorbutic taint, and not, as generally expressed, by that cloak for our ignorance, the senseless phrase "epidemic influence," their strength and spirits would revive, their tongues become clean, their choleric cease, their appetites return, and an array of evidences, as strong as Holy Writ, proclaim their salvation from cholera by means of the antiscorbutic preventive.

The analysis of twenty cases of cholera, observed at Pittsburgh, Pa.,

on the 25th and 26th of September, 1854, chiefly in the Mercy Hospital, contains the crowning testimony that removes all doubt of the correctness of the writer's views, reduces the matter to certainty, theory to knowledge, and incorporates this discovery into the pages of medical literature, among the established truths of medical science.

The analysis of twenty cases of cholera, then, discloses the fact, that every case was a case of scurvy, not a solitary exception, in or out of the hospital, comprising *all seen*. This is a remarkable announcement; nevertheless, remarkable as it may seem, every word of it is truth. Had not the physical evidences of scurvy been present in *every* case, I should have marveled. The appearances which I have described will be found in all cases of true cholera, and will henceforth be noticed by all practitioners, in all parts of the world. Why they have been so long overlooked, why they should have escaped my observation heretofore, *generally*, and were noticed in 1849 and '50, a complication of diseases only should have been inferred, is a matter of as much astonishment to me as it can possibly be to others; but such are the facts. Why I have now been made the humble instrument of explaining the matter, is doubtless due to circumstances rather than to extraordinary penetration, or superior professional attainments. It has been a hard and difficult task to divest my mind of the false notion of some specific, poisonous influence, *overlaying* scurvy, even since I have been fully aware of the scorbutic diathesis *underlaying* cholera. It may be difficult for others, even yet, to see clearly; but if, as appears by our analysis, every case of cholera occurs in a scorbutic subject, or in other words, that cholera is a messenger of death riding *always* on the time-honored steed, scorbutus, it matters but little what be the theory as to the office or entity of the messenger—if we destroy the steed, the rider will get on but poorly. This we know how to do. But I can see no occasion now to search for further cause of cholera, than the causes producing scurvy, no phenomena in cholera other than what harmonize with the known laws of scurvy; and nothing at the bed-side after the hæmorrhagic action is arrested, but the physical evidences of scurvy, neither do the books describe any anatomical lesions contradictory to this view.

In conclusion, if I have not explained everything pertaining to the subject in this brief monograph, I have given the key that will explain everything, when the laws of scurvy shall be perfectly understood; and it is a consoling reflection, that nations, cities and families can hereafter enjoy protection or immunity from the scourge of epidemic cholera, by simply conforming to the natural laws in regard to diet. The proofs presented, that cholera is a modified form of scorbutus, are as strong as physical proofs can be, or as strong as inductive reasoning can present; as strong as that the sun is the centre of the solar system, and that the earth and the other planets revolve around it: all the phenomena admit of explanation by the theory—the theory tested by practice proves effectual. *Why* scurvy is thus modified, (it always was a Proteus,) has constituted no part of these researches. This will be a subject for

further reflection with the writer. The laws of scurvy have yet to be investigated by modern observations, researches and statistics. The scorbutic diathesis may yet be found to hold a more special relation to *all the zymotic diseases* than has heretofore been suspected. Medical meteorology, or the constitution of the seasons, (abridgment of the crops and fruits by blights,) holds a direct relation with the state of the public hygiene; and it is to this *tangible* cause of disease, and not an imaginary *malaria*, that we are to turn in search of the laws governing epidemics. With a rational pathology, treatment, and prevention made known, cholera is divested of all its terrors. Sanitary regulations can now be instituted that shall meet its invasions at the very threshold, in ship, camp or city; and the public mind be so indoctrinated by suitable publications on the subject, that every family may know in what constitutes its safety: in fine, the true philosophy so disseminated that the wayfaring man, though a fool, need not err therein.

4. *Note on the non-absorption of Medicines in Cholera.*—[Ed. N. O. Med. Jour.]

In No. 48, *Gaz. Hebdom. de M^d.*, will be found M. Duchaussoy's experiments with enormous doses of medicines, chiefly the sulphate of strychnine, proving the non-absorption of these substances in the advanced stages of cholera—a fact that must have struck every practical man. Camphor, brandy, opium, mercury, quinine, together with a host of cholera specifics, which have been, or may hereafter be proclaimed to a credulous world, not being absorbed, but passing off with the stools, or lying as a foreign body in the stomach and bowels, cannot, in many cases, produce any or but little medicinal effect, how good soever these remedies may really be under more auspicious circumstances.

Case 1. M. D says, that in a well marked case of cholera, five milligrammes of the sulphate of strychnine, given in five hours, afforded no sign of having been absorbed; in the mean while, one gramme of belladonna, rubbed into the skin of the forehead, produced no dilatation of the pupils; the following day, one of the pupils became dilated. Recovery.

Case 2. A centigramme of the sulphate of strychnine, by the rectum—no sign of absorption—belladonna acted on the pupils. Cure.

Case 3. Three centigrammes of strychnine, by the mouth; no effect.—injection of belladonna into the bladder without affecting the pupils.

Case 4. Six hours before death, four centigrammes, sulph. styrch. and one ext. belladon., introduced into the sub-cutaneous cellular tissue—no physiological action.

Case 5. Same remedies in the same manner; no physiological action. Recovery.

Case 6. Nine centigrammes, sulph. strychn. by the mouth during comatose re-action, without any sign of absorption.

The physician is not, however, to take for granted, even in the worst cases of Cholera that absorption has altogether ceased, and that poisonous doses may be given without danger, as it will often happen that the power of the absorbents will be only crippled, not destroyed—enfeebled, not lost. Patients who, *not long before* death, had taken large doses of brandy and laudanum have had the stupor of the latter, and also the scent of the former in their breath, well marked, as the Editor of this Journal can affirm from experience.

ART. XVII.—*Cancer and Tuberculization.*

[The following example of the concurrent development of Cancer and Tubercle, taken from Ranking's Abstract, reported by Mr. Sibley, Registrar of Middlesex Hospital, in the Transactions of the Medico-Chirurgical Society, for 1854.]

This was the case of a woman, *et.* 48, admitted into the Middlesex hospital, with a sloughing cancerous sore in the left breast; there was a hard tumor on the inner side of the size of an orange, and several small nodules of cancer at its edges. In the course of five days after her admission nearly the whole remaining portion of the tumor sloughed away, leaving a clean-looking surface, which immediately began to cicatrize. Subsequently, pulmonary symptoms became developed, profuse expectoration followed, and she sank and died three months after her admission. On making a section of the structure of the left breast, it was seen to be an extremely dense form of infiltrating scirrhus, traces of breast tissue, such as ducts, being very apparent. In the thorax, large masses of tuberculous lung tissue were observed. Tubercular cavities existed in the apices of both lungs; a part of the lower lobe of the right lung was in a state of gray hepatisation, and the bronchial tubes were thickened and dilated. In the left pleura were numerous crude tubercles. On examining the dates of this case, positive proof was obtained that a cancerous tumor was increasing in the breast simultaneously with the increase of tubercular disease of the lungs, and that for a period of at least six weeks. The author thinks that a single instance of the concurrent existence of these diseases was sufficient to destroy the doctrine of the absolute incompatibility of tubercle and cancer with each other.

ART. XVIII.—*Puerperal Fever: by O. W. HOLMES, M. D., Prof. of Anat. & Phys. in Harvard University.*

In a pamphlet of sixty pages, Prof. Holmes advocates the contagiousness of this malady, and draws the following conclusions in regard

to the proper conduct of the obstetrician in view of this assumption:

1. A physician holding himself in readiness to attend cases of midwifery, should never take any active part in the post-mortem examination of cases of puerperal fever.

2. If a physician is present at such autopsies, he should use thorough ablution, change every article of dress, and allow twenty-four hours or more to elapse before attending to any case of midwifery. It may be well to extend the same caution to cases of simple peritonitis.

3. Similar precautions should be taken after the autopsy or surgical treatment of cases of erysipelas, if the physician is obliged to unite such offices with his obstetrical duties, which is in the highest degree inexpedient.

4. On the occurrence of a single case of puerperal fever in his practice, the physician is bound to consider the next female he attends in labor, unless some weeks, at least, have elapsed, as in danger of being infected by him, and it is his duty to take every precaution to diminish her risk of disease and death.

5. If within a short period two cases of puerperal fever happen close to each other, in the practice of the same physician, the disease not existing or prevailing in the neighborhood, he would do wisely to relinquish his obstetrical practice for at least one month, and endeavor to free himself by every available means from any noxious influence he may carry about with him.

6. The occurrence of three or more closely connected cases, in the practice of one individual, no others existing in the neighborhood, and no other sufficient cause being alleged for the coincidence, is *primâ facie* evidence that he is the vehicle of contagion.

7. It is the duty of the physician to take every precaution that the disease shall not be introduced by nurses or other assistants, by making proper inquiries concerning them, and giving timely warning of every suspected source of danger.

8. Whatever indulgence may be granted to those who have heretofore been the ignorant causes of so much misery, the time has come when the existence of a *private pestilence* in the sphere of a single physician should be looked upon not as a misfortune but a crime; and in the knowledge of such occurrences, the duties of the practitioner to his profession, should give way to his paramount obligations to society.

ART. XIX.—Cases of *Gastrotonny*.

1. *Case of Gastrotonny for the removal of a Leaden Bar—Recovery: by*
T. B. NEAL, M. D., Columbus City, Iowa.

Editor Medical Examiner:

DEAR SIR—I transmit, for insertion in your valuable journal, the following remarkable and perhaps unique case.

The subject of this notice, L. Bates, *æt.* 27, resides at Wapello, twelve miles from this city. During the three days preceding Christmas last, he had been drinking freely of common whiskey; and on that day, while intoxicated, attempted, on a wager, to swallow a bar of lead.—The bar was ten inches long, one-half by three-quarters of an inch thick, and weighed one pound.

Thrusting it far down the œsophagus, it slipped from his grasp, and immediately entered his stomach. Dr. Bell was sent for at once, but as Bates had formerly been a juggler, the doctor, thinking that he was at some of his tricks, refused to go. Bates, not much concerned at the non-attendance of the physician, worked for three days after the accident, in a pork-house, with but little inconvenience. During the night of the third day, however, he was seized with great pain in the stomach, accompanied with shooting pains along the spine, extending from the lumbar region to the sacrum, and thence to the hips. The next day he walked to Columbus, a distance of six miles, and sent for Dr. Robertson, the oldest physician in this county, to attend upon him. Dr. R. requested me to see the case with him. We found him on the fourth day, comparatively easy. His tongue was white, breath very foul, and bowels constipated. Upon careful examination, the œsophagus was found perfectly free and unobstructed. We administered to him morphia in small doses, and attempted to act upon his bowels and neutralize the poisonous effects of the lead by large doses of sulphate of magnesia. Under this treatment, although the bowels were but slightly disturbed, he was rendered astonishingly comfortable, and could walk about a little. On the 3d of January, the tenth day after his accident, the severe gastric pain again returned, accompanied with vomiting, and other symptoms of gastritis.

The operation of gastrotomy was now resolved upon. Dr. Bell, of Wapello, performed the operation by making an incision through the walls of the abdomen, from the umbilicus to the false ribs, four inches in length and two inches to the left of the median line. The peritoneum being divided, Dr. Bell introduced his hand, and pushing back the protruding intestines, found that the bar of lead was nearly perpendicular, the upper end inclining a little to the left. The bar was pushed up, until the lower end came opposite the abdominal opening. It was then seized, and an incision made in the walls of the stomach, just large enough to admit of its extraction by means of forceps. The contraction of the muscular coat of the stomach caused the incision in the organ to close perfectly and without trouble. The external wound was stitched, and a compress applied.

The operation was performed between three and four o'clock, P. M.; the day was cloudy, and towards sunset grew quite cold. The patient was entirely under the influence of chloroform until about two minutes before the last stitch was taken, when he revived somewhat, and expressed himself as feeling better than he had done before. When the chloroform was first administered to him, he vomited freely, hence, when

the opening was made into the stomach nothing escaped therefrom, that viscus containing nothing but the leaden bar.

For the ensuing three days the system of the patient was kept under the influence of opium, and nothing but mucilaginous drinks, in small quantities, allowed as diet. He recovered as well as a patient does of uncomplicated gastritis.—*Medical Examiner*, April, 1855.

2. *Gastrotomy performed in a case of Cancerous Stricture of the Œsophagus: by Dr. E. FENGER, Surgeon-in-Chief to the Friedrich's Hospital, Copenhagen.*

[The operation of gastrotomy, as performed by M. Sedillot, of Strasburg, has been lately repeated by the Danish Professor, Dr. E. Fenger. As in the first cases, the operation failed to preserve the life of the patient, and the profession must decide as to the fact of its propriety.]

CASE.—A man, *æt.* 55, of good constitution, was admitted into the Friedrich's Hospital, Copenhagen, January 10, 1853. Habitually in good health, he had experienced, especially after eating rapidly, attacks of vomiting, attended with a slight flow of blood; but for the last two or three years the blood had ceased to appear upon the voided matter. From this time he had suffered less, feeling only during his work occasional sharp pains in the epigastric region, nausea accompanied by an acid taste, and rarely attacks of sickness. He had once drunk freely of spirits, and nine months ago had been treated for delirium tremens; his limbs were still tremulous. A fortnight before his admission, the patient began to feel a fixed pain in deglutition, which he referred to the pit of the stomach, where there seemed to exist an obstacle difficult to be overcome. For the last five days he has been able to swallow only fluid aliment. The abdomen was distended superiorly, and sonorous upon percussion, except at the upper part, where there was evident dullness. A sound, introduced into the œsophagus, was stopped at about eight inches distance from the dental arch by some obstacle which could not be removed, and the attempt excited pain and the desire to vomit.

M. Fenger, after repeated trials at dilatation, performed, at the desire of the patient, who was made acquainted with the dangers of the proceeding, the following operation, March 23:—An enema having been previously administered, the patient was rendered insensible by the action of chloroform. An incision was then made, commencing at the lower border of the ensiform cartilage, and extending downwards, outwards, and to the left, by the border of the costal cartilages, to the outer border of the rectus muscle. The skin, aponeurosis, and muscles were divided; the epigastric artery was cut through and tied. The peritoneum was next divided, and the left lobe of the liver was felt. The index and middle fingers of the operator, introduced through the wound, touched the diaphragm, the spleen, and, lastly, the stomach. Its anterior surface was seized and dragged to the wound, where it was secured by needles and ligature. The front of the stomach was then opened with care, and the

mucous membrane fixed to the exterior; half a cup of mucilaginous liquid was introduced by means of a glass tube, the wound simply covered, and the patient put to bed. During the day he had some attacks of cholera, but he slept well. On the morning of the 25th he had a sensation of hunger; pulse 104. Some greenish fluid flowed from the stomach. Towards midday the countenance altered, and he died 58 hours after the operation.

Autopsy.—There were no traces of peritonitis; near the spleen there was a small quantity of thick brown fluid. The mucous membrane of the stomach was natural. A cancerous tumor occupied the lower part of the œsophagus. There was no disease in other organs.—*Ranking's Abstract.*

ART. XX.—*Yellow Fever in Charleston.*

In the March number of this Journal the numerical history of the epidemic yellow fever in Charleston, in 1854, has been already given. The following tableau from the pen of T. Y. Simons, M. D., is copied from the *Charleston Medical Journal* for March, 1855, showing the nativities of the individuals who fell victims to Yellow Fever in the two preceding epidemics, namely, 1849 and 1852, from which it will appear that, of 426 only 7 had been born in that city. Now upon the doctrines of chance, it may be rationally supposed that many children are born in Charleston of unacclimated parents, soon after reaching the city, among both emigrants and families from the rural districts, not to name returned natives who had been long absent, in high latitudes, and that, therefore, some of them might perish; yet it is difficult to account for the small number of the deaths in this class. It has been most industriously, perniciously and falsely asserted that, in yellow fever cities and epidemics the mortality is not owing to a want of acclimation. The contrary is true. For example, of 1,800 certified deaths from yellow fever in New Orleans in 1841, *five* only were born in the city; one aged three weeks—three aged two years, and one, a disputed case. In 1843, among 692 deaths from yellow fever, only two were born in the city; but both of these nativities were denied in the public prints.—[*Ed. New Orleans Med. and Sur. Jour.*]

In 1849, there were from yellow fever 123 deaths: Ireland, 65; Germany, 34; England, 3; Scotland, 3; New-York, 6; Boston, 1; Georgia, 1; South Carolina, 3; Charleston, S. C., 2; Italy, 1; Portugal, 2; United States, 1; New Brunswick, 1; Total, 123. Thus there were 99 from Ireland and Germany; 12 from all other nations of Europe; 1 from New Brunswick; 9 from the United States, exclusive of Carolina;

3 from South Carolina, of which two were natives of Charleston, and equivocal cases. In 1852, there were 302 deaths: Ireland, 150; Germany, 70; Scotland, 6; England, 13; Prussia, 3; Wales, 4; France, 5; Sweden, 2; Norway, 2; Switzerland, 2; Natives of Charleston, 5; State of South Carolina, 15; United States, except South Carolina, 35: Total, 303.

From the same Journal the following table, by J. L. Dawson, M. D., City Registrar and Chairman of the Board of Health, showing the progress of Yellow Fever in 1854, is taken:

Deaths in each week:

1st.	From 13th to 14th August,	-	-	4
2d.	" 20 to 26 "	-	-	20
3d.	" 27 to 2 September,	-	-	26
4th.	" 3 to 9 "	-	-	75
5th.	" 10 to 16 "	-	-	129
6th.	" 17 to 23 "	-	-	118
7th.	" 25 to 30 "	-	-	73
8th.	" 1 to 7 October	-	-	54
9th.	" 8 to 14 "	-	-	49
10th.	" 15 to 21 "	-	-	32
11th.	" 22 to 28 "	-	-	24
12th.	" 29 to 4 November,	-	-	9
13th.	" 5 to 11 "	-	-	6
14th.	" 12 to 18 "	-	-	3
15th.	" 19 to 25 "	-	-	5
	Total,	-	-	627

The same gentleman, in his letter to Dr. Happoldt, (now editor of the *Charleston Medical Journal*,) says:

The first death that occurred was on the 14th of August—the last, on the 22d of November, 1854.

It is worthy of remark, that the three first cases occurred at a distance of at least a mile apart—one being in Tradd-street, one in Calhoun-street, and the other in Pineknay-street, about midway between the two. All efforts have failed to prove that the two first had any communication one with the other, or had visited any vessel from an infected port.

A strenuous effort has been made through the columns of your journal to prove that the fever was imported from Cuba. In a report which I made to the Board of Health in the commencement of the epidemic, I gave a full account of all the cases that had been brought from abroad; and, to make the history complete, I have added, in this report, an account of all the localities where deaths occurred during the first three weeks of the epidemic. From these data every

one will be able to form an opinion for himself, whether or not the disease spread from infected vessels, or originated from causes existing in our midst.

ART. XXI.—*Case of Sloughing of Caput Coli*; by R. LANING, M. D.

On Friday afternoon, January 6, 1854, I was called to Groton City to visit a female child of Andrew Perry, aged four months. Found it laboring under hæmorrhage from the bowels, attended with pain in the abdomen, irritation, restlessness, and extreme debility—pulse hardly perceptible. The history of the case as related by the parents, was as follows:

On Saturday previous, the child was taken with pain in the bowels and vomiting. Various domestic remedies were given, also castor oil, with little or no effect. On Sunday evening, blood passed the bowels freely, and the vomiting continued. Monday morning an eclectic was called, and the child had been under his care till Friday morning, when he gave it up, thought it had disease of the brain, and could not live. What his treatment had been I could not ascertain, except ordering rhubarb in the place of the castor oil.

I prescribed anodynes, tannin, and mucilages in such quantities as the stomach would bear—also anodyne fomentations to the abdomen. Saturday, the hæmorrhage and vomiting had ceased, and pus passed the bowels. Continued the anodynes. Remained about the same till Wednesday, when the restlessness increased. In the evening, there appeared at the anus a portion of the intestine, having the vermiform process attached. Pulling on it gave pain, and it was thought best to let it alone. On Friday the portion of intestine came away, being about four inches in length, the vermiform process being about two inches long. The child was extremely weak. Pus and feces passed the bowels. From this time it improved. The treatment was anodynes and tonics. The convalescence was slow, but the recovery perfect.—*Trans. Med. Asso. of Southern Central N. York.* 1854.

ART. XXII.—*Ethnology.—Medicine among the Indians.*

[Abstract from Mr. Geo. Catlin's Work on American Indians: 2 vols. royal 8vo, London, 1842, Edit. 4th; many hundred illustrations.]

Mr. Catlin, the Red-man's admirer, after spending 8 years among 48 tribes of Indians, numbering 400,000, and after having made 310 oil paintings and 3,000 full figures of these people, whom he calls "noble, lofty, moral, religious, decent, modest, unassuming, inoffensive, hospitable,

friendly,"* admits, nevertheless, though somewhat against his theory, that they are inferior to the whites in muscular development in the chest, arms, &c., the legs and feet only excepted. They look effeminate, are not corpulent, have lighter bones, thinner skulls, and narrower shoulders. Their teeth are good but yellow. They have no beard generally—when they have, they eradicate it—they reckon it as vulgar; 18 out of 20 are entirely without it by nature—of the few who have it, 19 out of 20 pluck it out at the age of puberty, when its growth is successfully arrested; occasionally one may be seen who has omitted to destroy it at that time, which subjects him to repeated pains in extracting it. (ii, 225-7.)

Crosses among the whites or blacks cause a proportionate beard, which is allowed to grow, or is plucked out with much tact and great pain.

Mr. Catlin doubts whether the process of flattening the head practised by the Flat-head or Chinooks, is painful. He thinks the half-breeds are deteriorated, but is not very positive, though he affirms that the Indian and the Negro produce the finest and most powerful men that he has seen. It is rare for an Indian woman to have more than four or five children, being generally contented with two. He has never seen a woman who had 10 or 12, as in civilized life. He thinks parturition easier among Indian women, because they work more and undergo greater hardships than whites.

In 1838, the Mandan tribe was extinguished by small-pox, together with 25,000 of the adjacent tribes. Mr. C. thinks it probable that *all* the Mandans perished—a fact which proves that their "medicine-men" are not so good as those of the whites. These Mandans never would accept the offers of vaccination tendered from time to time by the United States' Agents, as the redmen had no faith in the white medicine-men, but preferred the practice of their own medicine-men. The latter used powows, magic, soerey, and medicine-bags made out of skins, and died themselves along with their patients.

Mr. Catlin who was present during the prevalence of cholera among the Indians, maintains that the dissipated tribes who lived on the frontiers who used salt and vegetables perished of cholera, while others using meat only without salt escaped.

* I fearlessly assert to the world, (and I defy contradiction,) that the North American Indian is everywhere, in his native state, a highly moral and religious being, endued by his Maker, with intuitive knowledge. Morality and virtue, I venture to say, the civilized world need not undertake to teach him." (ii, 243.) Mr. C. estimates the number of Indians in North America, at its invasion by the whites, at 16,000,000,—of which 12,000,000 have perished from whiskey, small pox, and the bayonet.

ART. XXIII.—*The Mortality of War.*

A writer in the *North British Review* for May, 1849, says: "During the last four years of the Peninsular war, 24,930 privates died of disease; only 8,899 died of wounds, or were killed in battle. The average deaths in four battles,—Talavera, Salamanca, Vittoria, and Waterloo—were 3.9 per cent. of officers, and 2.11 of privates."

In the war now raging in the Crimea, it appears that in a few months, an incomparably greater proportion of the British troops have perished, chiefly from disease, than during the four years referred to above. Those who fell in battle during the late war between Mexico and the United States, constituted but a small proportion of the actual mortality. It is not by the arms of war, but by the inglorious diarrhœa, dysentery, fever, hunger, cold, and inclement skies, that civilized warriors, deprived of their accustomed physical comforts, perish by thousands.

In a recent number of the *London Lancet*, the following deplorable accounts have recently been published, and of which a part of this article is a summary. The British military system is bad,—the chief French hospital, the Mejidich, at Pera, is now a complete school of surgery; the English hospital at Scutari is a pest-house filled with 5000 or 6000 starved soldiers, dying of dysentery, scurvy, rheumatic fever, and other diseases, the result of the exposure to climate, and deficient commissariat arrangements; while as to operative surgery, the appalling fact has been stated that, from the same causes, out of 60 secondary amputations, only one recovered. The average mortality from sporadic causes, compared with that of the French, is 1 in 88 English and 1 in 360 of the French.

At the Battle of Waterloo there were 387 English operations in all, and 166 deaths; at Vittoria and the Pyrennes, 584 operations, and 287 deaths.

Pyæmia or purulent infection, from what cause not known, is much more frequent after simple amputations, than after the very worst kind of comminuted fracture, if the patient is not fed.

"It is curious that in all our continental revolutionary wars since Waterloo, as well as in the late battles in India, the tendency of all the best surgeons, whether in Sardinia, in Germany, Schleswig-Holstein, Paris, or Chillian-Wallah, (India,) France or Holland—has been to amputate less and less; and towards the end of all such campaigns, scarcely to amputate at all, from the experience gained by accident after such battles, of wounded soldiers not brought in time to be operated on, and yet recovering perfectly without amputation."

There is good reason to fear that the English and American Governments when war breaks out suddenly, send with their armies a large proportion of incompetent medical men as being good enough for the soldiers, while the French Government, on the contrary selects its medical staff with as much discrimination as it does its general officers.

Let the bleaching bones of thousands of soldiers who perished in hospitals in Mexico during the late war, bear testimony on this point. Who does not know that many of the medical men received in the armies of Generals Taylor and Scott, were incompetent to treat the sick and wounded?

The horrors of war upon the battle-field are sufficiently great; blackened visages; headless trunks; bodies severed by shot, crushed by the warhorses' hoofs, by the wheels of the artillery;—the shattered limbs of the living, and the groans of the dying. But these horrors are less disastrous, less fatal to an army, than those presented by the hospitals. The former are casualties inherent to war; the latter, for the most part, are preventable, resulting from exposures, fatigues, bad or insufficient diet, the want of clothes, tents, cleanliness, nursing, medicine, medical skill, the kindnesses, sympathies, and moral influences.

The medical and military sciences in the Orient form a strong contrast to the sciences of healing and killing in the Occident. Great Britain, a few years ago, declared war against one-third of the human species in the East, that is, against China, in the matter of opium-eating, opium-smuggling, opium-destroying, opium-interdiction, and opium-reparation; whereupon, the irate British Lion, with 19,000 people, including soldiers, seamen, marines, sepoy, and camp-followers, fell upon China, and in fifteen battles, from July 5, 1840, to July 21, 1842, did "*tare to pieces*" about one score thousand Orientals, with a loss of only 69 men, all casualties reckoned.

"Our whole contest with China," says Mr. Treasurer Martin, "resembled the war which might have been expected between the Brobdignags and the Lilliputians."

ART. XXIV.—*Oriental Medicine*.—Ed. N. O. Med. Journal.

The wave of Chinese immigration upon the golden shores of California, must ere long bring with it Oriental medicine. It may be well to know in advance, how one-third of the great world understand and practice physic: therefore, the latest and most reliable authorities will be

put under contribution, as the precursors of the cloudless morning of medical science, which is breaking in the East, and approaching occidental realms.

In Canton, wherein 2,000 doctors practice physic, the following, says Mr. Martin, is the standing prescription, with its *modus operandi*: the *Ginseng and Foo liquid*: to regulate the breath and blood of the "Yin and Yang," let a dose of the ginseng and foo (a medicinal herb) be taken, prepared with boiling water. The commentary on this is as follows: the former part of the body when produced is called the prior heaven; the latter the subsequent heaven. The constitution of the first depends upon the kidneys, which are the gift of the father and mother; the constitution of the second depends upon the stomach, which is renovated by water and grain. The prior heaven is the substance in nature preëminent for repose, and therefore the child enwrapped within the womb depends upon its mother's quietness for nourishment, and then in its living breath, the divine concealment and secret springs of life will be tranquil. The subsequent heaven's breath is the *use* of the primary principles in nature, which is carried out in motion; therefore after the nourishment of figure, water and grain are administered to it; and in the production of the body, the divine impulse is set in motion and begins to circulate; heaven and man unite their virtues. The two substrata, that is motion and rest, are in mutual operation, whence the "latter heaven's breath," there is life, and when there is life there is no repose; but if the former heaven's breath obtains the latter, renovation commences; where there is renovation, there is no exhaustion. If in motion or at rest, the kidneys are injured by want of care, the former for heaven's breath will be empty; if eating and drinking be immoderate, the stomach will be injured and, the latter heaven's breath will be empty. Now to supply this deficiency, there is nothing equal to the draught made from the two ingredients, ginseng and foo.

HONG KONG, 22° 16' 27"—8 miles long, 2 to 4 broad, separated from China by an inlet of the sea, half a mile to 3 miles wide, consisting of a broken precipitous ridge with peaks 2000 feet high, having been ceded to England, in 1841, and having proved excessively fatal, both professional and non-professional men seem to agree that, a pestiferous gas of "deadly activity" is generated by the decomposing coarse granite and sand-sandstone causing "la maladic du granite." Mr. Martin, quotes Dr. Heyne's Account of India, going to show that swamps and vegetable matter do not cause endemic fevers, since certain hilly parts quite free

from trees have most destructive epidemics, which are therefore, owing to rocks, particularly to granite, and to ferruginous magnetic hornblende ! It is the hill-fevers remote from swamps which Dr. H. and others regard as the most malignant, being apt to run into typhus ! The hill-fever on these coasts exists almost exclusively among the hills of the granite formation, or where iron-stone is found in large quantities.—(*China, by R. M. Martin, Colonial Treasurer, ii. 277-8.*)

At Hong Kong, Mr. M. shows that, in 1845 during 182 days there fell 118 inches, nearly ten feet of rain, that for the half-year, the heat is from 80° to 90°, and 80° even at Christmas. He says each man goes into the hospital on an average five times a year; 2 in 7 die yearly; of every 100 men 15 or 16 only are fit for duty; the Royal artillery, the finest in the world, 135 men, lost 51 by death and 45 by invaliding in two years. At Chuck-Chaw, composed almost entirely of rotten granite, the 18th Royal Irish lost from March 1st to December, 1845, nearly 23 per cent. The prevailing disease is a fever combining the character of the African and West Indian fevers. There is no extent of marsh on this island; but decomposed *mineral* substances yield an aëriform poison.

[The following notes are taken from Mr. S. W. Williams' Chinese Empire, 2 vols., 1848.]

The plague is unknown any where in China. Crowding can hardly produce it or even Yellow Fever.

CANTON, 23° 7' 10'' is in nearly the same latitude of Havana, has one million of inhabitants; July and August, 80° to 88°; January and February 50° to 60°; the highest recorded 94°, in July; the lowest 29° in January; a fall of snow nearly two inches deep occurred at Canton, in February, 1835, which remained on the ground three hours; but it was such an unusual event that the citizens hardly knew what was its proper name, some calling it *falling cotton*, and every one endeavoring to preserve it as a febrifuge. Canton lies on the low banks of Pear River.

Few large cities are more healthy than Canton, no epidemics nor malaria prevail there, notwithstanding much of the town is built upon piles. In the close streets and creeks of Canton, reeking with offal of every description, the heat is aggravated by radiation from the walls, and by vile smells urged forth by the sun. The canals are filled by the tide, and present a revolting mass of reeking filth when the retiring waters expose the bottoms.

PEKING, 39° 54' 13'' N. contains two millions of souls. The climate

is salubrious; epidemics rare; the plague unknown; heat 75° to 90° usually—in summer 95° to 105° .

Medical Practice.—Blood-letting is rarely attempted, but leeches and cupping are employed to remove the blood from a particular spot.—Blood-letting is disapproved in fevers, “for,” says the Chinese reasoner, “a fever is like a pot boiling; it is requisite to reduce the fire and not diminish the liquid in the vessel, if we wish to cure the patient. The Chinese have no idea of the distinction between venous and arterial blood, nor between muscles and nerves. Mercury, in its common preparations of calomel, or red precipitate, is exhibited by Chinese physicians.

In all departments of learning, the Chinese are unscientific, and, while they have collected a few facts, invented many arts, and brought a few to a high degree of excellence, they have never pursued a single subject in a way calculated to lead them to a right understanding of it, and a proper classification of the information they possessed relating to it.

ART. XXV.—*The Devil's Notion of Medical Science: from GOETHE'S FAUST.*

Mephistopheles.—I tell you, a fellow that speculates is like a brute driven in a circle on a barren heath, by an evil spirit, whilst fair, green meadows lie everywhere around him.—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. [Not a bad satire on many morbid anatomists.]—Set to at metaphysics—conceive profoundly what is not made for human brains. A fine word will generally 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.

Medical Student.—But there must be some meaning connected with the word.

Meph.—Right! only we must not be too anxious about that; for it is precisely where meaning fails, that a word comes in most opportunely. Disputes may be admirably carried on with words; a system may be built 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 through the great and little world, and let things go on in the end—as it pleases God.

[Certain characters, when among the witches, spoke thus]:

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, and am delighted with these; for, from devils I can certainly draw conclusions as to good spirits.

Skeptic.—Here I am quite at home.

Ci-divant Geniùs of the Age.—With proper people, one becomes somebody.

Mephistopheles' reply to Faust's lamentations on the misfortunes of Margaret, and the cruel death that awaited her, was—"She is not the first!"

When Faust expressed doubts as to testifying to the death of Martha's husband, without a full knowledge of the fact, Mephistopheles said—"Oh, holy man! There's for you now! 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 in it? And, looking fairly at the real nature of things, did you—you must confess you did not—did you know as much of these matters as of Mr. Schewerdtlein's death?"

POST-MORTEM EXAMINATIONS.

Mephistopheles.—I have never had any fancy for the dead. I like plump, fresh cheeks the best. I am not at home to a corpse.

Faust.—Instead of animated nature, for which God made man, thou hast naught around thee but beast's skeletons and dead men's bones, in smoke and mould.

The Practice of Physic.—[Dr. Faust, after having got a deep knowledge of medicine, complained.] *Faust.*—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 traffic in words.

Wagner.—How hard it is to compass the means by which one mounts to the fountain-head [of knowledge]; and before he has got half way, a poor devil must probably die!

Faust.—Must I go on reading in a thousand books, that men have everywhere been miserable—that now and then there has been a happy one? Thou hollow skull! what mean'st thou by that grin? but that thy brain, like mine, was once bewildered—sought the bright day; and, with an ardent longing after truth, went miserably astray in the twilight?

Dr. Faust, in speaking of his and his father's practice in fevers, and of the honors which the people were paying him for his success in the same, says: "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 murderer praised!

Wagner.—How can you make yourself uneasy on that account? Is it not enough for a good man to practice conscientiously and scrupulously the art that has been entrusted to him?

A word, not from Dr. Faust, but from Molière, may be added on this subject, of which Wagner speaks: "A dead man is a dead man, and this is a fact of no consequence; but the neglect of a formality, carries with it a notable prejudice against the whole body of the medical faculty."—*L'Amour Médecin*.

Dean Swift asserts that a doctor who has prognosticated a fatal result, would rather that the patient should die than that the prediction should fail.

ART. XXVI.—*Of what Disease did the Czar of Russia die?*

The editor of the *Lancet*, in his number for *March* 17 last, has indulged in some speculations on this subject, but he has left the enigma entirely unsolved.

"Reports," he remarks, "from the palace tell the world that he died of inflammation of the lungs, of apoplexy of the lungs, of *atrophy* of the lungs! But we defy the pathologist most skilled in the progress and termination of disease, to extract out of palace reports or medical (?) bulletins any consistent or intelligible history of the malady which has destroyed the Czar."

After quoting the newspaper statements, and the official bulletin from his medical (?) attendants, the writer in the *Lancet* observes:

"We are told by his three physicians, that the Emperor had at first only the influenza, but that since the 22d of February, 'fever manifested itself, with slight attacks of gout.' On the 28th of February, there was 'pain in the right lung, and the fever was much greater;' on the 1st of March, he is simply 'unwell,' the fever has 'somewhat diminished, and expectoration takes place without difficulty.' No hint of danger

yet; and then, at eleven at night of the same day, 'fever had increased, and expectoration from the affected part of the right lung had become more difficult.' And then, early next morning, he was dead.

"Did he die of inflammation and apoplexy of the lungs? If he did, we take upon ourselves to say, that the course of the pneumonia was unusually rapid, and marked by circumstances inexplicable by the known laws of those affections. Dating the onset of the inflammation, from the 28th February, when pain in the right lung was observed, to the fatal conclusion, on the morning of the 2d March, it lasted but forty-eight hours at the utmost. Or shall we surmise that the physicians had overlooked the earlier accession of one of the most severe of diseases? But, on the 1st March, the *second* day of the supposed pneumonia, the *fever* had *diminished*, and the *expectoration was easier*, and yet the pneumonia, if pneumonia there were, *must have been progressing*. These statements are perfectly irreconcilable. With a pneumonia rapidly advancing to a fatal end—a pneumonia of only forty-eight hours' duration, the fever does not abate, and the expectoration does not become easier on the second day. In the early stages of inflammation of the lungs, the expectoration is always difficult and remarkably viscid. To reason, medically, concerning a disease which has a remarkably definite course, it is then probable that on the second day of this rapid and suspicious illness, there was no pneumonia at all. But then it is said that, at a period at which we must presume to have been about twelve hours before death, 'atrophy of the lungs was *feared*;' and, some hours later, Dr. Mandt communicated to the Emperor 'that atrophy was *possible*.' Now, what particular idea the public may entertain of atrophy of the lungs, we know not; but every medical tyro knows that atrophy is a chronic change of almost imperceptible advances, and that it forms no part of the progress of an acute pneumonia. If we are not at liberty to accuse the physicians of the Emperor of the grossest ignorance, we must conclude that the word 'atrophy' is altogether a mistake, taken up by unprofessional persons. Was it *apoplexy*, then, that destroyed the Czar? Apoplexy and inflammation of the lungs are not absolutely incompatible. But there is something reported which is absolutely incompatible with either or both of these conditions, as related in this incomprehensible history. Apoplexy and inflammation of the lungs preclude the idea of the Emperor, in his last moments, blessing his wife, children and grandchildren, *separately, with a FIRM VOICE*.

"We are then reduced to this alternative; either the symptoms reported are fabulous or imperfectly related, or pneumonia and apoplexy of the lungs did *not cause death*.

"Where there is so much of palpable contradiction, so strange a suppression of all public mention of illness until the day preceding death, such scanty information given, and the absence of *post-mortem* examination, it would be idle to speculate upon what was the precise cause of death."

If the statement we have seen made be true, that the Czar entrusted

his health to the infinitesimal gentry, the editor of the *Lancet* might have saved himself of the hopeless task of endeavoring to glean a ray of truth from the impenetrable mists of ignorance and extravagance which envelop the imaginings of homœopathists.—*Medical News and Library*, April, 1855.

It was Rochefoucauld who said, a long time ago—"The world will find out that part of your character which concerns it: that which especially concerns yourself, it will leave you to discover." How true soever this maxim may be in regard to the affairs of life in general, it is utterly false in the medical profession. At the bedside the non-professional public cannot find out who is the quack; that is to say, it cannot determine the nature, progress, seat and tendency of the disease, nor the appropriateness of the treatment. The prescription may have retarded or accelerated the recovery—hastened or arrested the march of death.—The ignorant and the wise, alike patronize the charlatan, or the learned and skillful. The quack is preferred, not because he is believed to be ignorant; the learned physician is repudiated, not because he is supposed to be learned, but for the reason that he is supposed to be incompetent. So it has been, now is, and will ever be. Complaint and blame are alike useless. The strongest instinct of humanity, a love of life, impels the sick to seek for relief from those from whom they believe it can be obtained.

There is little doubt that the late Emperor of Russia trusted in homœopathy. From a late conversation with Dr. J. B. Cottman, of Louisiana, it appears that Dr. C.'s brother, recently resident in St. Petersburg, where he was one of the late Czar's physicians, entertained this opinion of the Czar's homœopathical predilections, though Dr. C. was not present at the time of the Emperor's last illness. All accounts of the diagnosis of the Czar's malady, show the incompetency of his physicians—which incompetency is the more culpable, because the physical diagnosis of diseases of the chest has arrived, in modern times, to a wonderful degree of certainty, quite unknown to preceding generations. As the nature of the disease had been mistaken, it is reasonable to suppose that the treatment must have belonged to the same tragical category. "The Atrophy of the lungs" spoken of, is probably a verbal error in the dispatches.

The post-mortem examination of all corpses ought to be made obligatory upon the attending physician, as an indispensable part of professional duty. Morality, family affection, truth, science, religious teachings, the well-being of society and legal enactments, should enjoin this

paramount duty, as, in formal medico-legal inquests. Is it right—is it true to give a mortuary certificate of death from disease of the liver, when the kidney, spleen, lungs, heart, brain, or some other organ, is the seat of the malady? Did the physician feel certain that the truth or falsehood of his *præ-mortem* diagnosis of the seat and symptoms of a disease, would be tested by a *post-mortem* examination, would he not take care to inform himself, to give a sound judgment, and to treat the disease with scientific discrimination? Would he not feel a ten-fold greater responsibility, if not for the patient, yet for his own reputation? If ignorant, would he not ask for aid from the skillful? As the matter now stands, the quack, like the pirate, knows that the dead tell no tales—that the grave covers up all blunders, and that he can commit homicides *ad infinitum* with impunity. The dying patient, like the dying Czar, says to his doctor—“*It is no fault of yours!*” Czar! it may be so! It may not! If the public want a reliable standard by which to distinguish between the charlatan and the man of science, let it not look to the grandeur of his equipage, the suavity of his manners, his boastings and his infallible pretensions, but to the opened corpse. In that case, the Czar Alexander might have known whether the doctors of the Czar Nicholas deserved honor or disgrace—the mines of Siberia, or Imperial gratitude. Czars and fellow-citizens! be wise, even in death! *Walc!*—*Ed. N. O. Med. and Surg. Jour.*

ART. XXVII.—*The Death of Hume.*

The nature of Mr. Hume's disease, though slow in its progress to a fatal termination, remains involved in mystery equally with that of the late Czar. J. H. Burton, advocate, Edinburgh, the able biographer of Hume, in a recent work in two volumes, has brought forth new documents not heretofore published, concerning the amiable yet too sceptical philosopher, who so tranquilly departed this life in Edinburgh, his native city, 89 years ago. From this work the following extracts are taken.

Hume's physicians did not agree as to the nature of his malady; and when he died, his brother refused to permit a post-mortem examination. A word in this place concerning Hume's character, may be allowed. Whatever errors may have pervaded his metaphysical and religious speculations, his exposition of the evils of superstition, bigotry and intolerance, has been beneficial. “Fallacies,” says Mr. Burton, his biographer, “may be the brilliant insects of a day, but truth is eternal;

and when the searcher in false philosophy, groping amid the darkness of man's imperfect reason, produces falsehoods, they are speedily forgotten; but if he develop great truths, they live to bless his species for ever. There are few who will now deny that mankind have learned many valuable truths of David Hume. The wide influence of his mind over thought and action, during the last hundred years, is experienced in the mere naming of the systems of which he was the author or suggester." (ii. 518.)

Inasmuch as Hume, rather than Bentham,* was the author of the modern doctrine of utilitarianism, it is reasonable to suppose, that if permission had been asked of Hume to allow a post-mortem examination, he would have enjoined it upon his friends, the dead-body being good for nothing else.

But it is time to return to the subject of his malady, concerning which, not long before death, he thus wrote to the Rev. Dr. Hugh Blair: "You have frequently heard me complain of my physical friends, that they allowed me to die in the midst of them, without so much as giving a Greek name to my disorder: a consolation which was the least I had a reason to expect of them. Dr. Black, hearing this complaint, told me that I should be satisfied in that particular, and that my disorder was a hæmorrhage—a word which was easy to decompose. But Sir John Pringle says, that I have no hæmorrhage, but a spincture† in the colon, which it will be easy to cure. This disorder, as it both contained two Greek appellations and was remediable, I was much inclined to prefer; when, behold! Dr. Gustard tells me that he sees no symptoms of the former disorder; and as to the latter, he never met with it, and scarcely ever heard of it. He assures me that my case is the most common of all Bath cases," [Hume was then at Bath, for the waters,] "to-wit, a bilious *complaint, which the waters scarcely ever fail of curing: and he never had a patient of whose recovery he had better hopes." (ii. 504.) Dr. Black, in writing to Adam Smith, says, Hume's "disorder is a hæmorrhage in the upper part of the intestine!"—(ii. 488.)

About two weeks before his death, Hume wrote—"Dr. Black tells me plainly, like a man of sense, that I shall die soon, which was no disagreeable news to me. He says, I shall die of weakness and inanition, and perhaps give little or no warning." (ii. 508.) "As the physicians

* Bentham, at the age of 84, bequeathed his body to Dr. Southwood Smith for dissection.

† This is probably a typographical error.—*Ed. N. O. Med. and Surg. Jour.*

of London and Edinburgh were divided about the seat of his disorder, those of the city where he died? [Edinburgh] "proposed that his body should be opened; but that his brother would not permit." (ii. 517.)

ART. XXVIII.—*The Ci-devant Genius of Physic in New Orleans, forty years ago.*

Draw near, Young Physic! Approach, ye non-committal expectationists, ye steaming Thompsonians, and ye infinitesimal potentates of homœopathic attenuations! Draw near, ye venerable gray-beards, once companions of Jabêz W. Heustis, M. D., Surgeon U. S. Army, "who being dead, yet speaketh" in his book on the Topography and Diseases of Louisiana, published near 40 years ago, when, as he affirms, "it often took from 60 to 80 days to perform a voyage from New Orleans to Natchez—when the middle of the streets of New Orleans was not paved—when remnants of the Albania, Tunica, Chittamaches and Atacapas tribes of Indians resided in the vicinity of the city;" and when doctors differed in the treatment of Yellow Fever, as they do now. Read, and judge ye!

Of the Cure of Yellow Fever.—Respecting the cure of this disease, I scarcely ever found any difficulty in removing it, provided the patient applied in time. During the forming stage of the complaint, before much commotion has been excited in the system, and where irritability of the stomach does not exist, I have always found the best and most certain effect from the exhibition of an emetico-cathartic, consisting of tartarized antimony, or ipeacuanha, calomel and jalap, in suitable proportions. By this remedy, assisted with plentiful diluting drinks, the morbid biliary colluvies in the primæ viæ are evacuated, the disease is cut short in its progress, and the system speedily restored to a healthy condition. When the patient is affected with rigors, pain in the back and loins, a small, weak pulse, anxious respiration, and a cold, constricted skin, I exhibit a dose of nitrons æther and spirits of ammonia, and assist their operations by warm drinks. When this stage has passed, and the pulse rises and becomes strong, attended with an increase of heat, I have immediate recourse to bleeding. In the early period of the first and third forms of this disease, the arterial action was sufficiently strong and quick to authorize the employment of this remedy, which I always found productive of the best effect. The pulse thereby becomes softer; the pain of the head abates; the eye and the countenance are rendered placid; the general distress is relieved; the skin, from being hot and constricted, becomes soft and moist; and a free perspiration often ensues. The persons affected with this disease being frequently those of strong and robust constitutions, I have generally made use of liberal evacuations

of blood, to the extent of sixteen or twenty ounces; and when resorted to in the early period of the disease, I never had occasion to repent of its employment. The operation should be repeated in the course of twelve or eighteen hours, if the symptoms seem to require it. When, however, the remedy has been neglected till the third or fourth day, the period at which it might be employed with safety and advantage is past, and the operation then becomes doubtful and hazardous.

When the irritability of the stomach does not prohibit its exhibition, I have found the use of nitrous æther, assisted in its operation by the plentiful use of warm elder-flower, or other herb-teas, a serviceable remedy.

It frequently happens, however, that the irritability of the stomach is so great as to reject the blandest substances, as well as the most cordial anodynes. In these cases, flannel clothes, wet with hot brandy, and impregnated with laudanum and camphor, and applied to the region of the stomach, are found serviceable. But the most effectual remedy is an injection of a hundred drops of laudanum with some mucilaginous liquid.

Cooling acidulous drinks, as lemon-juice and water, and sucking the juice of ripe fruits, are highly refreshing and serviceable in this disease.

But I have found it especially necessary to attend to the state of the bowels. Such was the disposition to the generation and accumulation of bile, that, unless removed by frequent purging, the powers of life became oppressed, and nature rapidly declined. The patient would complain of great weight, anxiety, and oppression about the epigastrium; colic and dysenteric pains would ensue, with ineffectual, bloody and slimy discharges. A cathartic, which it was frequently necessary to give in large doses, on account of the constipated state of the bowels, speedily removed these symptoms, and always insured a respite to the disease. The evacuations procured in this manner were very copious, of a dark green or black color, and extremely offensive. When the patient was too much debilitated to bear the operation of a brisk cathartic, injections of Glauber's salts, with molasses and water, were substituted with the greatest benefit. These injections frequently become necessary to assist the operation of the cathartics taken by the mouth; they might be employed two or three times a day with advantage; they were speedy, mild, and gentle in their operation, and free from the debilitating effects which sometimes followed the exhibition of cathartics. The debility produced by purgative medicines, however, was only momentary, and the ease and composure which they afterwards afforded to the body, satisfied the patient of their necessity and usefulness. According to the strength of the person, and the degree of constipation, I used calomel and jalap, Glauber's salts, or cream of tartar; the latter, in the quantity of one or two table-spoonsful, with just enough water to admit of its being swallowed, was mild, and generally effectual in its operation, and in affording relief. This medicine, when given in a state of solution, is apt to run off by the other excretions, without affecting the bowels: in proportion as they were kept free from the accumulation of biliary matter, so was the yellow color of the skin prevented or removed.

The efficacy of calomel, as a remedy in this disease, has been much extolled. Influenced by the torrent of fashion, I was once an advocate for the liberal exhibition of this medicine; but experience, at length, taught me never to prescribe it with the view of producing salivation, though I have seen this effect sometimes occasioned by it, when given in the ordinary doses, as a febrifuge, in combination with other medicines: but, even in those cases, I have known the disease to continue, and in some instances prove fatal. When the fever was slight, the skin soft and moist, and the pulse yielding and free from tension, as the mouth became gently affected, the symptoms subsided. But this is no indication of its utility; it only evinced a mitigation of the disease, which permitted the mercury to show its specific effects; for, when high inflammatory symptoms prevailed, or when the circulation was languid, and the skin cool and dry, it was either impossible to produce a salivation, or, when effected, it afforded no relief; and the patient died, notwithstanding the appearance of this promising and wished-for symptom; or, a slow and difficult recovery confined him for a long time to a sick bed.

The sad consequences of the practice were particularly exemplified at New Orleans, in the year 1812. Three companies of the 1st regiment of Artillery were then stationed at the barracks in that city, of whom a great portion died with the yellow fever, and from the effects of mercury. Agreeable to custom, they were treated upon the mercurial plan.

This *Sampson* of the *Materia Medica* was not prescribed by the weight and measurement of grains—that would have been feeble and insignificant, and unworthy the characteristic liberality and boldness of its great advocate and supporter—it was given to the patient in a cup; and he was directed to eat it by the spoonful, like so much sugar. A fatal surfeit! few survived to tell the mournful story.

As to *cold bathing*, I have frequently used it in bilious fever, and was never sensible of any injurious effects from its employment; at the same time, I cannot say much in favor of its efficacy. When the skin was hot, dry and parched—the circulation rapid, and the pulse strong, with a throbbing of the temporal arteries—cold bathing, (which I employed by affusion) to the extent of from ten to twenty buckets of water in succession, gave, at least, a temporary coolness and refreshment.—After the affusion, the patient was conveyed to bed, was covered with blankets, and took a dose of nitrous ether in some warm tea. For a short time he continued cool and composed: the pulse then rose by degrees; the heat increased; and the symptoms seemed to be established in nearly their former violence, and so continued till a perspiration broke out, and after continuing some time, procured an imperfect remission.

Analogous to cold bathing, I have frequently experienced very good effects from a copious draught of cold water, taken in the height of the hot stage: it cools the body, relaxes the surface, and expedites and facilitates the appearance of a free perspiration.

For the purpose of moderating the distressing heat with which the patient is affected, and which is particularly felt in the head, palms of

the hands and soles of the feet, in place of cold affusions, I have applied to these parts linen cloths wet with cold vinegar and water: they relieve the heat and pain in the head, and abate, in some degree, the temperature of the body generally. Sponging the body with cold vinegar, nitre and water, is both refreshing and conducive to cleanliness, so necessary to be observed in this disease.

The patient's room should be well ventilated; all excrementitious matters speedily removed; the floor occasionally scrubbed, and frequently sprinkled with vinegar.

I have employed the nitrous acid in this disease with good effect: when properly diluted, and softened with sugar, it forms a pleasant drink, and has a tendency to correct the alkaline disposition of the fluids.

In the advanced stage of the more mild remitting form of bilious fever, when the remissions were pretty distinct, I have employed the bark with advantage. It is taken at the period of the greatest remission, in as large quantity as the stomach will bear. The ensuing exacerbation frequently runs high, and is aggravated in degree, in consequence of the exhibition; but we have the pleasure to find the paroxysm terminate in a complete intermission, at which time a few doses of the bark prevent the recurrence of the disease, and establish a state of convalescence.

The convalescence, however, is slow: the patient is liable to frequent relapses upon the least fatigue, or exposure to the sun. Against these occurrences nothing can secure him but the strictest temperance and care, and the daily use of Peruvian bark, until his strength shall be established, which is rarely effected before the cold weather commences. In general, convalescence is much slower in Louisiana than in the Northern States, owing to the relaxed and debilitated condition of the system, previous to the attack of the disease. During this period, I direct a tea-spoonful of bark to be taken in the morning, upon the patient's rising out of bed, with a glass of wine. Provided the invalid is faithful in the observance of this practice, he is pretty secure against the danger of relapse.

In every form of this disease, the pulp of ripe fruits is highly serviceable. These substances are refreshing, deobstruent, gently aperient, correctors of the acrimony of the bile and of the alkaline condition of the fluids; and not unfrequently produce a diuretic and diaphoretic operation.

ART. XXIX.—*Philosophers and Physicians against Philosophy and Physic.*

In a work in two volumes, on the Correspondence of Scientific Men, (Oxford, 1841) will be found the following extracts from the letters of Sir Isaac Newton: "I have long since determined to concern myself no further about the promotion of Philosophy." (Dec. 5th, 1674. Yet he afterwards published his *Principia*.) He complains in a letter to Collins, of having been treated rudely in the Royal Society, and wrote that he desired "to be put out from being any longer a Fellow of that society."

"I see I have made myself a slave to philosophy. I will resolutely bid adieu to it eternally, excepting what I do for my private satisfaction, or leave to come after me; for I see a man must either resolve to put out nothing new, or become a slave to defend it," (Nov. 18th, 1676). "For I see not what there is desirable in public esteem, were I able to acquire and maintain it. It would perhaps increase my acquaintance, the thing which I chiefly study to decline."

It is remarkable that while Sir Humphrey Davy enjoyed honors, emoluments, &c., consequent upon his scientific knowledge, he should call knowledge a curse. "Our *real knowledge* is but, to be sure, that we know nothing; and I can but doubt if this be a curse or blessing.—Those who hope trust and believe, are surely happier far than those who doubt."

Dr. Radcliffe was, probably, the most eccentric, and at the same time the most successful physician of his day. Although he practiced medicine, he had as great a contempt for physic as he had for physicians, avowing it as his opinion, that the whole art might be written on a sheet of paper. Yet it may be doubted whether a more luminous lesson was ever given than his declaration that, when a young practitioner, he possessed twenty remedies for every disease, and before the end of his career he found twenty diseases for which he had not one remedy.—(*Physic and Physicians*, vol. 1).

An eminent physician renounced the practice, which he had exercised thirty years, saying, "I am weary of guessing."

Hood, though no doctor, said: "If any man labors in the *dark* like a *mole*, it is a *physician*. He has continually to divine the *color* of a *cat in a bag*. He is called in to examine a *suspected trunk*, without the policeman's privilege of *search*. He is expected to pass judgment on a *physical tragedy* going on in the *house of life*, without the critic's chance of seeing the *performance*. In fact, every malady is an *enigma*, and when the doctor gives you *over*, he gives it up."

Molière, who nearly two centuries ago amused as well as satirized the world, particularly the medical part of it, in his last comedy, in the acting of which he was seized with his death-sickness, namely, the *Malade Imaginaire*, puts the following words in the mouth of Beraldo: "I don't know a more pleasant piece of mummery, or any thing more ridiculous, than for one man to undertake to cure another."

Swift says of his doctors—

"They rather choose that I should die,
Than their predictions prove a lie."

ART. XXX.—*Extracts from a Lecture on the Rights and Liabilities of the Physician and Surgeon—being part of a Course of Lectures on Medical Jurisprudence, delivered in the Medical School of Dartmouth College, in the Boylston Medical School, at Boston, and in the New-York Medical College, by JOEL PARKER, LL.D., Professor of Medical Jurisprudence.*

In some occupations in the service of the public, where an individual holds himself out as ready to perform a service to whoever may apply, the law requires him to render that service to whoever comes in a proper manner, and is prepared to pay, so long as he thus holds himself out and is provided with the means. Such is the case with common carriers, stage-coach proprietors and railroad corporations.

This principle, I think, has never been extended to the learned professions.

It is not to divines. It could not well be to lawyers; and I am not aware of any intimation that a physician may not refuse an application for his services in the first instance.

But the question occurs, what obligation does he assume upon answering this call, and proceeding to take charge of the case? I have found no case where this question has been litigated and settled.

Upon this principle, there seems to be no good reason for holding, from the mere fact that the physician has been called and prescribed, that there is a binding contract on his part to attend the patient until the termination of the disease. There is no contract upon the part of the patient to continue the employment until that period. He is at liberty to dismiss the physician at any time, and this without rendering a reason. He may so far recover as not to desire the farther attendance of his physician, whenever he chooses. And so it seems that the physician may in like manner dismiss the patient when he elects so to do, with this qualification, arising from the nature of the case—that he gives reasonable notice of his intention to abandon the employment, so that the patient may have a proper opportunity to employ another person in his stead.

By accepting the call and undertaking the care of the case, the physician undertakes generally for the performance of a service in a reasonable manner, but with no stipulation for the continuance of his attendance for any particular period. The contract of the patient is to pay for such service as shall be rendered.

It has been decided that an attorney, having engaged in a cause, if desirous of giving up the conduct of it, must give reasonable notice of his intention so to do.

And I perceive no reason why the same principle may not hold good in relation to physicians.

There seems to be a sufficient reason why the physician, after commencing an attendance generally, should not desert the patient without notice, if the case requires farther treatment.

This responsibility on the part of medical men, will not be a source

of much difficulty.

For malpractice, a physician may, in certain cases, be exposed to a criminal prosecution. There are several cases of this character against quacks and pretenders, but they have usually arisen from an attempt to commit some unlawful act, or from gross ignorance or negligence; and courts have been ready to excuse those who have erred with good intentions.

The danger of the educated physician does not lie here.

A physician is liable, also, to a civil action, for not exercising sufficient skill and care in the performance of his duty to his patients.

This is a principle fraught with danger to all medical men, and one requiring their profound consideration—not that they are subjected to a greater responsibility than that imposed upon other professions and occupations, nor that they are more likely to be heedless in the performance of their duties, but because of the peculiar means which the patient has above all other litigants, of misrepresenting his case, and of moving the sympathies of the jurors who pass upon it.

The rules which regulate this liability are deduced from the general principle of the law, that where any person undertakes for a reward to perform a work for another, which requires skill for its accomplishment, he holds himself out as possessing the requisite knowledge and skill, and engages that he will exert and apply that skill in the performance of the work which he undertakes. And if he does not possess the skill which is requisite, and the employer, by reason of the want of it, suffers an injury; or, if possessing it, he does not use it, either from wilfulness or negligence, and damage is thereby sustained by the employer, the party who undertakes the performance of the service is liable to a suit for the recovery of the damages sustained by his incapacity, wilfulness or negligence. Thus the blacksmith who undertakes to shoe a horse, and ignorantly or negligently lames him; and the mechanic who, undertaking to repair a piece of machinery, fails, from incapacity or carelessness, to perform what he agrees to do, becomes answerable for the injury occasioned by his breach of duty. And so the attorney at law, who, from want of a competent knowledge of his profession, or from neglect in applying his professional skill to the case committed to him, is liable at the suit of the client, for the damage thereby sustained.

In like manner, and upon the same principle, a person who holds himself out as a physician, and assumes the care of a patient, undertakes for the necessary skill to treat the disease in a proper manner, and to prescribe the medicines suited to counteract and cure, if, perchance, it may be cured, or to alleviate the pain of it, so far as that may reasonably be done; and if he fail in the performance of the duty thus devolved upon him by his employment, whether the failure be the result of ignorance of his profession, or want of due care in his prescriptions, if injury to the system of the patient, delay in the cure, or unnecessary pain can be shown to have resulted from his want of skill or care, the patient may have an action to recover his damages. The rule applies in full force in surgical practice, and the great danger of the medical man lies here:

Actions are rare except in cases of surgery; and in cases of that character the plaintiff generally comes into court with some visible deformity, which, however it may have been occasioned, whether by his negligence, restlessness or wilfulness, excites sympathy; and being attributed to the mismanagement of the defendant, he is in great danger, even when he has been in no fault.

Several cases of this character have fallen under my own observation.

The inquiry naturally arises—How may the medical man best avoid the danger of loss—loss of money, and perhaps, to some extent, loss of reputation, through such prosecutions?

It has been suggested, that before acting in the case, he should require a bond or writing, exonerating him from all responsibility, or, perhaps, containing a direct stipulation not to prosecute a case of failure to effect a cure. Possibly such a document may have a tendency to restrain the party who has executed it from instituting a suit. But it admits of very grave doubt whether it could have any legal operation to exempt the physician from any responsibility.

Theoretically, he is liable only for want of skill or negligence.—Practically, he is in danger from an unreasonable claim, and from the sympathy of the jury. The document, to be of any avail as a legal bar to a right of action, would be in effect a contract that the physician should not be liable, even if he lacked skill, or was guilty of negligence. But it has been held that it is against the policy of the law to permit a party to contract for exemption from the legal consequences of his own fraud; and gross negligence has been held equivalent to fraud.

Whether the principle would be extended farther, and held to embrace such a contract with a physician, remains to be seen.

Such a precaution may do no harm, but from the nature of the case, it must be impracticable, in some instances, to procure such writing from the patient himself.

Another precaution, in cases where danger of a suit may be apprehended, is to secure sufficient evidence of the state and progress of the case from time to time, by an examination by other physicians, and by intelligent men not of the profession, and, therefore, not liable to a suspicion that their testimony is influenced by an *esprit du corps*.

Another, when the patient is refractory or unreasonable, is to decline farther attendance, assigning the reason and giving reasonable time to procure another medical attendant. But whenever this course is resorted to, there should be plenary evidence respecting the state in which the case was left.

Other measures will suggest themselves as adapted more or less to each case as it arises. Perhaps none of them can give a perfect assurance of exemption from unreasonable prosecutions.

But something may yet be hoped from an intelligent public opinion, which shall rebuke this sponging of the profession to indemnify the patient, in many instances, for the consequences of his own folly or negligence.—*Trans. New Hampshire Med. Soc.*

ART. XXXI.—*Anno 1855—Horoscopic and Periscopeic.*

Prognostication has always been regarded as within the pale of medicine; indeed, the word prognosis signifieth the foreknowing the progress and result of a thing, particularly of a malady, as its synonymes, prænotio, prænuntiatio, præscientio, prædietetio, and the like, indicate.

The most reliable prophetic horoscope is that of Virgil, being non-committal;

But oh! commit not thy prophetic mind
To flitting leaves, the sport of every wind,
Lest they dispense in air our empty fate:
Write not; but, what the powers ordain, relate.

[*Dryden.*]

Charlatany rages beyond the limits of terrestrial medicine, having reached realms spiritual, at least in literary matters. Judging from the writings which purport to have come recently from the spiritual world, science, literature, poetry and physie are at the lowest possible ebb in the spirit land. Indeed, Sidney Smith's estimation of American literature in 1820, in the Edinburgh Review, will apply to spiritual literature of 1855. "Literature," says Sidney, "the Americans have none.—They have hitherto given no indications of genius, and made no approaches to the heroic, in their morality or character. In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? What does the world yet owe to American physieians or surgeons?"

By a rapid, yet easy descent, a periscopeic view of the terrestrial regions is attainable, and is worthy of regard. The first four months of the year 1855, though favorable in a sanitary point of view, have been remarkable for the general prevalence of drouth, and for the absenee of the periodical freshets in the rivers of the valley of the Mississippi. It is supposed that during the present century the lower Mississippi has never been so depressed during the spring season, as at present. Thousands of square miles, annually inundated in lower Louisiana, in the spring and summer, present now an expansion of desiccation, baked and fissured by the sun and the drying winds. Theorists who have attributed epidemics to the desiccation of soils recently wet, or recently inundated, now have data whereupon to prognosticate; while those who rely on moisture and mud, will find the shallow lakes and lagoons, and that depressed class of the swamps usually more or less covered with water, all in readiness for the implantation of their doctrines. Complete desiccation in some, and incomplete in other districts, wherein vast areas of mud, moisture and decomposition abound, will afford the dialectician

facts, if not logic, good for either favorable or unfavorable prediction—
if, like *Hudibras*,

He had been long towards mathematics,
Optics, philosophy and statics,
Magic, horoscopy, astrology,
And was old dog at physiology.

Among the periscopic signs of the times, must be reckoned the diminished number of medical students at the colleges and universities of both Europe and America—from which it is inferable that the disinterested love, or the pecuniary rewards of the medical profession are declining. The supply exceeds the demand, or the demand is supplied outside of the regular profession. A slight periscopic glance will show that whatever decadency may appear in the numerical proportion of the regular, is more than counterbalanced by the increase of the irregular practitioners of medicine. Witness the increasing number of eclectics, botanics, steamers, homœopaths, hydropaths, table-turners, spiritual knockers, mesmerists, and other sectarists without number.

A powerful party has arisen in the United States, who, by a singular misnomer, call themselves by the flattering title of Reformers; and who, with several other sects in medicine, have established colleges, periodical journals, dogmas and panaceas, characterized by the minimum of medical science, the maximum of arrogant pretension, together with a short and easy road to the title of physician—a title which never can be merited without years devoted to the study of the structure, functions, remedies and diseases of man.

The fundamental, cohesive principle, which binds together the crafty allies opposed to legitimate medicine, is that of denouncing whatsoever has been sanctioned in all ages, tested by universal experience, and recommended by learned, skilful and conscientious physicians, of the past and the present eras. To the profoundest investigations, the so-called reformers oppose sciolism; to the experience of ages, blind empiricism; to the well-established methods of cure, deceptions, false and dangerous practices.

The word Eclectic—a dogmatic word—a word which repudiates system, yet pretends to choose its system out of all systems, which is itself a system, and undertakes to pass an exclusive, definitive and final judgment on every other. Although all parties in medicine profess to choose what is true, and reject what is false in the various systems, yet in this procedure it is self-evident that it is the philosopher, not the ignorant and incompetent pretender, who is entitled to give a valid

judgment in the premises. No sect in medicine can appropriate to itself exclusively the term eclectic, without directly, or by implication, claiming infallibility; or, in other words, claiming to be competent to determine the truth or falsehood of all theories and modes of practice. Eclectic doctors, eclectic apothecaries, eclectic surgeons, eclectic colleges, eclectic journals, and the like, with which the land is overrun, are misnomers.

The Periscope of 1855 includes in its orbit an increasing number of female doctors. Mrs. Dr. —, and Miss Dr. —, are announced in the journals as practitioners of medicine. Genius has no gender. A good prescription is neither male nor female. In fact, *medicina* (Lat.) *médecine* (Fr.) are feminine, not to name other tongues equally favorable—*salutem in Æsculapio!*—*Ed. N. O. Med. and Surg. Jour.*

ART. XXXII.—*Scientific Correspondence.*

TO DR. B. DOWLER: *Dear Sir:*—I have just received your parcels, and hasten to express my obligations to you for them. I was aware, from various intimations in the *Boston Journal*, and particularly from the flattering notice of Dr. Cartwright, that there was some similarity between your views and mine, and have felt very desirous to know to what extent they coincided; so much so, that I had written a note to append to the last article of mine,* inquiring of the doctor where some account of them might be obtained. But I felt rather ashamed to publish my ignorance to the world, and did not insert it. You may well imagine, therefore, that it was with no small degree of pleasure, as well surprise, that I opened the package from the post-office and found it from you, and that it contained the very thing I wanted.

I rejoice to find that though we agree in many things, and are co-workers in the same cause, we have arrived at the same results by different methods, so that there is no likelihood of any dispute arising between us. To you clearly belongs the merit of being the first to prove by experiment as well as reasoning, the utter baselessness of Sir Charles Bell's theory, and the doctrines founded on it. Nor is this the sum total. By your superior method of experimenting, together with your more philosophic mode of interpreting the results, you are able to revise

* Dr. Haskell here alludes to the *Boston Medical and Surgical Journal*, in which he has published several papers containing some of the best reasoned doctrines relating to the Nervous System, that modern times can boast of.—*Ed. N. O. MED. JOUR.*

and correct the conclusions of re-discoverers, when they re-discover more than the truth,—when, as Hamlet didn't say, "there are more facts in their philosophy than are wot of in nature," as in the experiments on the *post-mortem* contraction of the muscles. These experiments have an interest to me, independent of the considerations you have drawn from them. More than two years ago I advanced the proposition, (though anonymously) that the sole office of the arterial blood was to stimulate the nerves, and intended to prove it by arguments founded on the foetal circulation—on that of the liver—on fainting by loss of blood, on restoration therefrom by inducing the horizontal position—on asphyxia from carbonic acid—on bloodvessels accompanying nerves—on the volume of the blood sent to the brain—on its not being necessary to any of the processes of nutrition or secretion, except as the nerves are necessary, &c., &c. But the French experiments which seemed to prove the necessity of the presence of the arterial blood to the contractility of the muscles, rather puzzled me; but your's set me right.

As for myself, I claim to have discovered a principle which, when applied to the nervous system, will give the exact function of every part of it. And the proof will consist in its giving a reason for every variation in anatomical structure and position, in its furnishing a satisfactory explanation of all well conducted experiments, and pathological facts connected with that system. A principle which corresponds with mental philosophy as it is, and which will connect it with physiology as it should be. The system of Sir Charles Bell lay in my way, as it is in the way of all true philosophy, and I attacked it. And although the current of public opinion now sets against us, as it set against Fulton, along with the current of the Hudson, *par ignobile fratrum*, when he launched his first boat on that river. I tremble before it as I should before the voice of many waters, and no more. He reached Albany in spite of the opposition of both currents. And so have we.—The system of Bell stands *doubly* refuted. And the critics who hesitate to give their assent to the truths you have developed, or to the practical value of them, now occupy the position of those who stood on the deck of that boat, after being carried in her from New York to her place of destination, and questioned whether she could do it over again; and if she could, doubted whether any benefit would result from it. As to the verdict of posterity, I have not a moment's misgiving. And I tell my wife, when she says to me, "why don't you charge your visits and collect your bills, instead of spending your time the way you do?" that Sir Charles Bell's

widow got a pension, and perhaps she will fare as well, if she will not interrupt me! What I want most is, time and leisure to develop them. And this brings me to the consideration of the great encouragement that is held out to men in this country, who would occupy themselves in the advancement of knowledge. Had you been in France or England, their journals would now have been boasting of your discoveries, and you would have had some substantial tokens from Government that your labors are appreciated. But in this country, every one seems to labor under the idea that we must always exist in a state of colonial dependence on Great Britain and France for our science and literature.

No one seems to contemplate the possibility of an original thought springing up here, worthy of attention. Everything must be imported to give it value. A physician, to get a reputation, must edit an English book, or translate a French one. Our rich young medical graduates, having picked up their A, B, C's in our schools, go to Paris, walk the Hospitals until they get well impregnated with their odor, and visit the saloons until they become tinted with the Parisian society, and then come back accomplished physicians. I despise that Know-Nothingism which persecutes a man because he is a Catholic, or oppresses him because he is a foreigner; but I would worship a Know-Somethingism which would hold out an equal chance to a native as a foreigner, to be heard when he advances a new idea in science. Some five or six years ago, I brought forward what I believe is the true pathology and method of treatment in hydrophobia. No notice being taken of it; two years ago I reconsidered it, and enlarged somewhat on it. Cases occurred immediately afterwards, within 35 miles of me, and the same old life-trifling plan was pursued, without any deviation. But Marshall Hall recommended tracheotomy, and the foolish thing was done at Buffalo. One at Charleston, near Boston, was smothered. So that the only commutation of his sentence, that medical science holds out to the hydrophobic patient, at the present day, is a choice of deaths! He may be strangled or have his throat cut. It reminds me of the old-fashioned court-martials, when the culprit is condemned to be hung, and his sentence is commuted into being shot. But I am growing tedious. I would only add, that we not only need an international copy-right law for American authors, but I had almost said a Maine law, to keep certain foreign works at a respectable distance. And it should be considered a disgrace for a nation of 25 millions to send its students abroad for a medical education.

Yours, &c.,

BENJ. HASKELL.

ART. XXXIII.—*Melanges. On the Nature of the Syphilitic Virus.*

M. Castano, physician-major in the army of the East, regards syphilis as resulting from the introduction into the economy of a *vegetable fungiform parasite*, the germ of which is developed in the tissues. The cure of the venereal disease consists in destroying this new body, and in the elimination of the spores and sporeules which it reproduces. Caustics, and especially the metallic anti-syphilitics, act by destroying this vegetable parasite, and rendering its spores incapable of germination. Hygienic means, as baths and plentiful lotions, may sometimes, but rarely, suffice to eliminate these germs of the malady, by eliminating organic matters in which they have their source. (*Rev. de Thér. April 1, 1855*).

Credulity is great.—The Humboldtian tale of inoculation with the poison of a serpent as a sure preventive of Yellow Fever, is passing the rounds of the French medical press. M. Murin-Lauzer has an account of this “inoculation préservative de la fièvre jaune,” in his Journal, dated on the *First of April, 1855*, but not, however, as *un poisson d’Avril*. Shade of Molière!

Health of the Army of the East.—From the correspondence of the *Gaz. Hebdom de Méd.*, of the 23d of March, it appears that on the 5th of the same month, in the French army of the Crimea, scurvy was prevalent, owing to the moral and physical sufferings to which the poor soldiers were exposed—nostalgia or home-sickness, being the most prominent predisposing cause of the disease.

The pathetic lines of the old poetical banker, Rogers, in which he describes the home-sickness of the Swiss soldiers, apply equally to the gallant French on the desolate shores of the Crimea:

The intrepid Swiss, who guards a foreign shore,
Condemned to climb his mountain cliffs no more,
If chance he hears the song so sweetly wild,
Which on those cliffs his infant hours beguiled,
Melts at the long-lost scenes that round him rise,
And sinks a martyr to repentant sighs.

Well did Byron say of Napoleon I, in 1814, that “the world was worth neither the trouble taken in its conquest, nor the regret felt at its loss.” The star of Napoleon which culminated over fields where three millions bit the dust, suddenly fell upon a desolate rock in distant ocean!

The great mass of the soldiers engaged in war, gain little more than Byron has enumerated in two lines, namely:

Guns, bayonets, swords, pikes, lashes, wounds, devotion,
A constant chance of death—but no promotion.

One more suggestive word will complete this enumeration—the word HOSPITAL!

A WORD TO READERS.

The patrons of this Journal will perceive that, while its pages are somewhat diversified with a few topics of a literary and miscellaneous character, in which calomel, quinine, castor-oil, lancets, hard cases and tedious details of formulæ, form no component part, there will be found a compensating advantage in the greater number of pages than usual, which will enable those who relish nothing which is not perfumed with drugs, to get their usual allowance, without those articles not so imbued. Besides, the readers of this Journal are not all medicine-men—and it is intended hereafter more fully to consult their tastes in publishing articles, none of which, however, will be such as are devoid of interest to the faithful of the true church of *Æsculapius*.—*Ed. N. O. Med. and Surg. Jour.*

TABLEAU OF THE YELLOW FEVER OF 1853.

With Topographical, Chronological and Historical Sketches of the Epidemics of New Orleans since their origin in 1796, illustrative of the Quarantine Question.

BY BENNET DOWLER, M. D.

New Orleans: 1854. 66 large octavo pages. Price—50 Cents per copy; three copies for \$1; 100 copies for \$25.

Orders should be directed to H. McCulloch, Delta Office, 76 Camp-street, New Orleans.

The editor of this journal considers this pamphlet the best that has been written on the subject. It contains much valuable matter in a small compass, and adds to the many laurels already won by the indefatigable author in this and other branches of investigation. His last chapter, while it has all the vigor and truthfulness of the poet, introduces us to the bedside of the dying and the dead—goes beyond the usual boundaries of observation, and brings back a wreath to crown the altar of Science, from a region too sterile for the poet, and even for the less energetic philosopher.—[*Phil. Med. and Surg. Jour.*]

In the sixty-six pages, of which this pamphlet consists, Dr. Dowler has contrived to condense more matter of an important and instructive character, bearing directly upon the etiology and character of Yellow Fever, than would suffice, if fully developed and examined in all its relations, to fill a goodly sized volume of several hundred pages; and that, too, without any undue extension of subject or prolixity of style.—[*The American Journal of the Medical Sciences.*]

Editor's Office—Notices.

MAY, 1855.

Owing to the extraordinary labors incidental to the preparation and proof-reading of two numbers of this Journal—more than 300 pages—in a little more than one month, instead of four, it has been impossible for the Editor carefully to peruse the new works on hand, without which reviews are valueless or pernicious.

The London Lancet, and the Western Journal of Medicine and Surgery, not received. Parisian Journals received to 1st of April.

A Communication has been received from WM. C. ASHE, M. D., of Demopolis, Ala.

BOOKS AND PAMPHLETS RECEIVED.

- A Practical Treatise on the Diseases peculiar to Women.* Illustrated by Cases derived from Hospital and Private Practice: by SAMUEL ASHWELL, M. D., Obstetric Physician and Lecturer to Guy's Hospital, London. Third American, from the Third and Revised London Edition. In one octavo volume. Pp. 528. Philadelphia: Blanchard & Lea. 1855. From Mr. J. B. Steele, Bookseller, 60 Camp street.
- On the Chemical Analysis of the Tennessee Collection of Urinary Calculi :* by E. B. HASKINS, M. D.; Clarksville. 1855. Pp. 24, with 2 plates.
- Smithsonian Report, 1854:* from Prof. HENRY, through Mr. Russel, of Fifeshire, Scotland. Svo. Pp. 269. Washington: Beverly Tucker.
- Blood-letting in Mental Disorders ;* By PLINY EARLE, M. D., &c., &c. New-York: S. S. & W. Wood. 1854. Pp. 126. Svo. From Mr. T. L. White, Bookseller, 105 Canal-street.
- An Essay to prove the Contagious character of Malignant Cholera ;* by B. M. BYRNE, M. D., Surgeon U. S. Army. Pp. 160. Svo. Philadelphia: Childs & Peterson. 1855.
- Medicine a Science; or, Disease a Unit :* By H. BACKUS, Selma, Ala.—1855. Pp. 46. 12mo. From the Author.

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EDITED BY

BENNET DOWLER, M. D.

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NEW ORLEANS, April 1st, 1855.

University of New-York.

MEDICAL DEPARTMENT—SESSION 1855-6.

THE Lectures will commence on **MONDAY, October 15th**, and be continued until 1st of *March* following. The session of 1854-5 was attended by a class of 307 students; on 106 of whom the degree of Doctor of Medicine was conferred.

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SESSION 1855-6.

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VOL. XI.

JULY, 1854.

No. 1.

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HERSHEL S. PORTER, D. D.	"	Comparative Anatomy and Geology.
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VOL. XI. SEPTEMBER, 1854. No. 2.

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THE
NEW ORLEANS
MEDICAL AND SURGICAL
JOURNAL.

Vol. XI.

JANUARY, 1855.

No. 4.

EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia; Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresponding Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c. &c.

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1855.

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THE
NEW ORLEANS
MEDICAL AND SURGICAL
JOURNAL.

VOL. XI.

MARCH, 1855.

No. 5.

EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia; Fellow and Honorary
Vice-President of the Medico-Surgical College of the same City; Fellow of the
Medical Society of Virginia; Corresponding Member of the Society of Statistical
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P. M. D. G. H. C. M. D. C.

THE
NEW ORLEANS
MEDICAL AND SURGICAL
JOURNAL.

VOL. XI.

MAY, 1855.

No. 6.

EDITED BY
BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences in Philadelphia, Fellow and Honorary President of the Medical-Chirurgical College of the same City; Fellow of the Medical Society of Virginia; Corresponding Member of the Society of Statistical Medicine of New York; Fellow and Fourth of the Royal Society of Northern Antiquaries of Copenhagen, &c., &c.

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MR. H. McCULLOCH, of New Orleans, having recently purchased from the succession of the late DR. HESTER, the goodwill, subscription list, and the assets of the *New Orleans Medical and Surgical Journal*, and having contracted with the undersigned, before a Notary Public, to Edit the same for Five Years, the projected *Quarterly Journal of Medicine*, though partly printed, has been cancelled, and its interests wholly merged in the first named Journal.

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Although the subscription list of this Journal is large and increasing, it should be borne in mind that prompt payment is indispensable, inasmuch as the cost of printing is high in New Orleans, while the difficulty of collecting small sums distributed over vast territorial expansions of the Southern States, is great as well as expensive. Justice, expediency and the mutual interests of all the parties concerned in this Journal, demand payment in advance, through the medium of the post-office, or through city agents or merchants.

A large edition of this Journal is printed; so that new subscribers may get the back numbers from the beginning of the volume, which commences with July; notices of discontinuance should be given two months before the end of the subscription year which closes with the issue of the May number. Provided no discontinuances can be made except at the option of the proprietor unless all arrearages shall be settled.

Postmasters will please to return, without unnecessary delay, all numbers of this Journal not called for in a reasonable time, as the law directs. The same request is made of all to whom specimen numbers may be sent, should they decline subscribing for the work.

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RESIDENCE:—DELORD ST., between Camp and Magazine.

NEW ORLEANS, *May 1st*, 1855.

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