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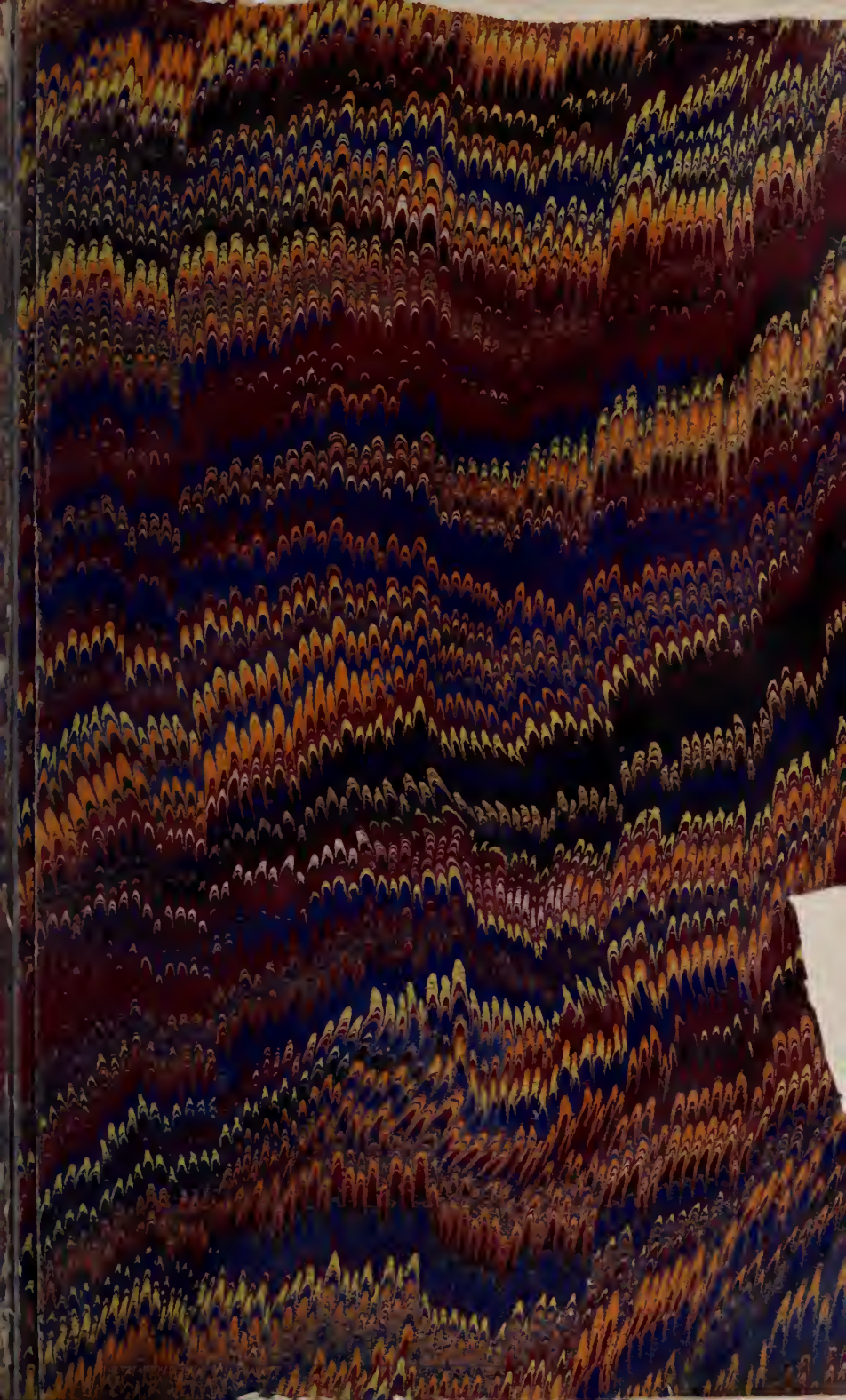
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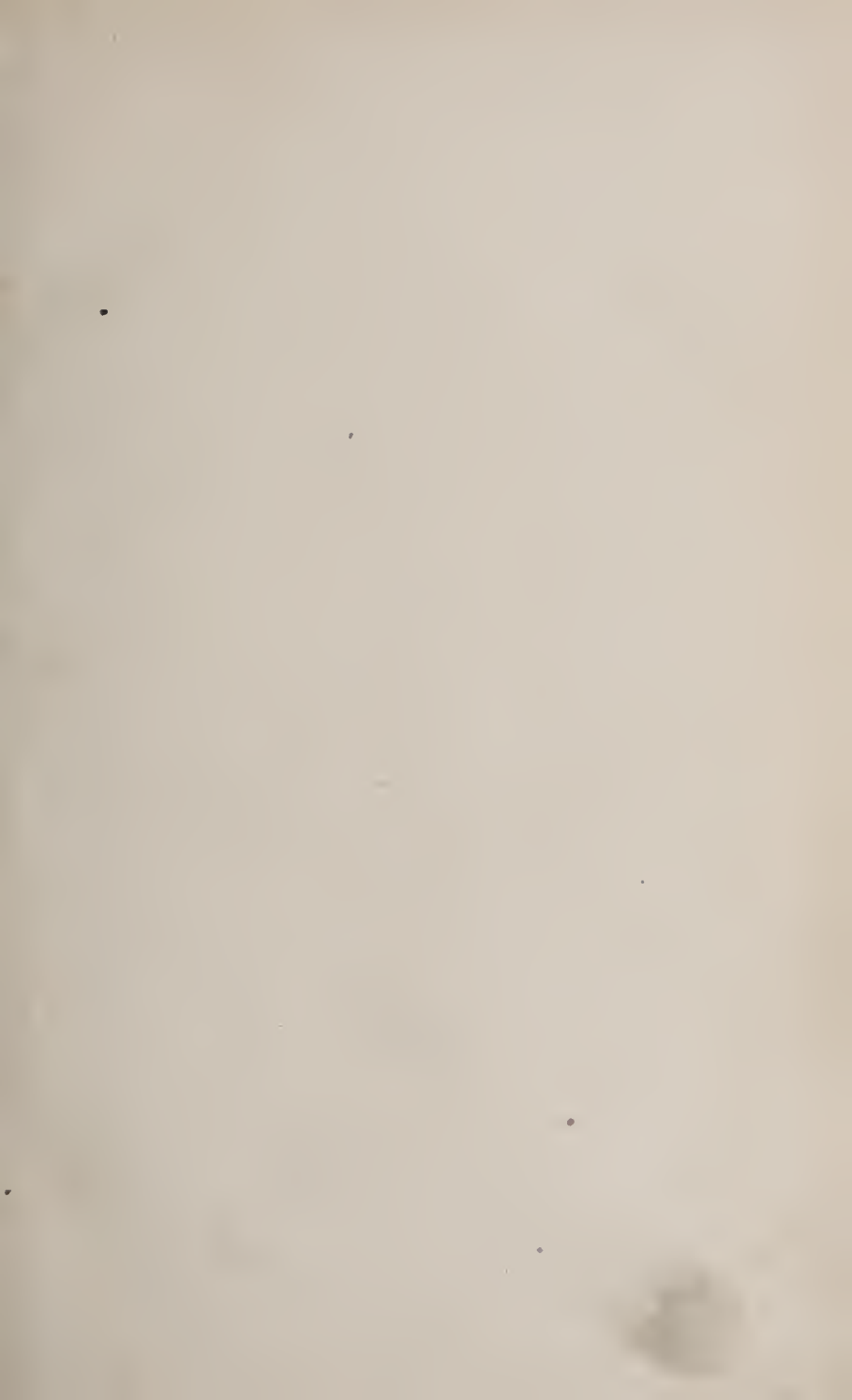


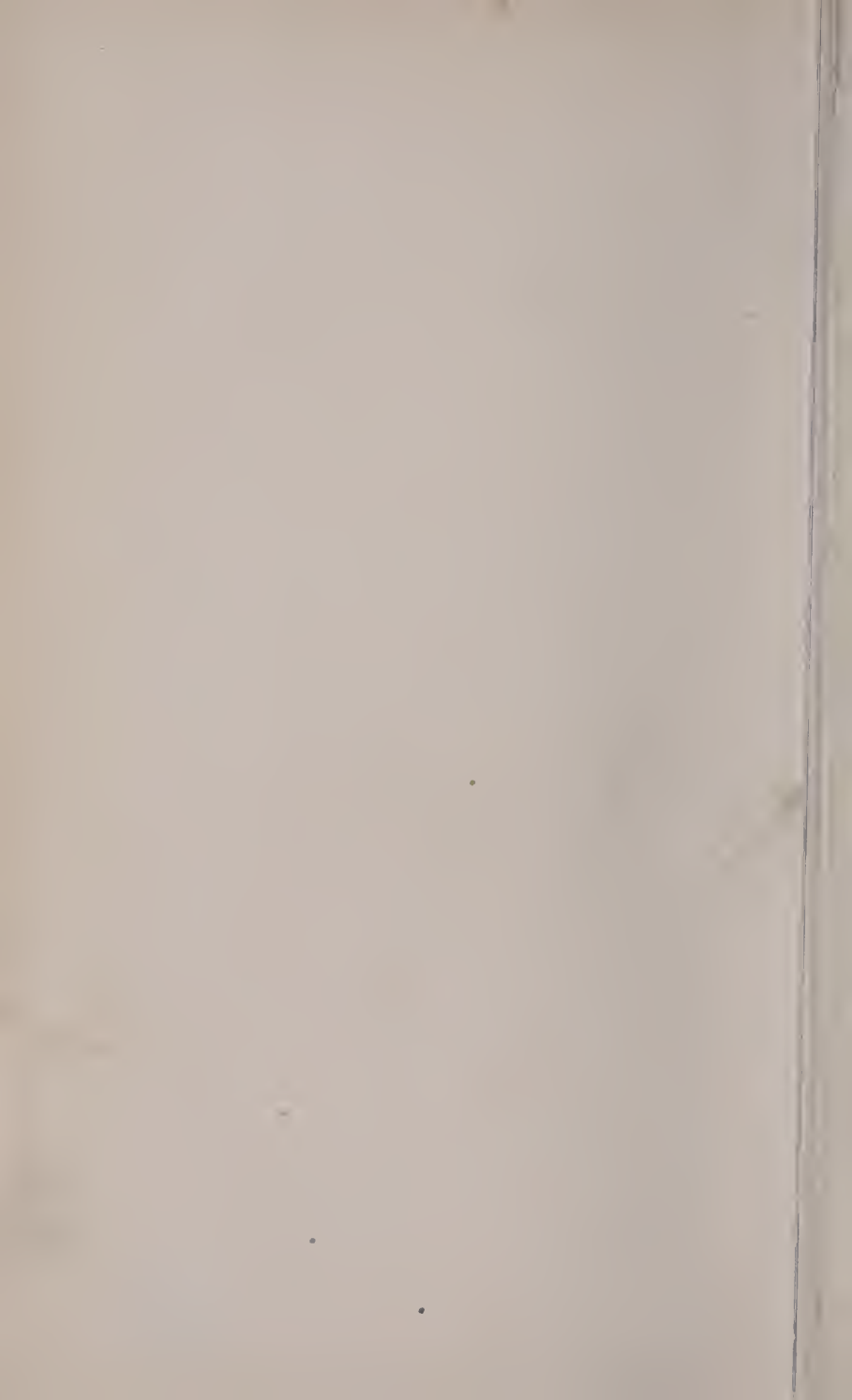
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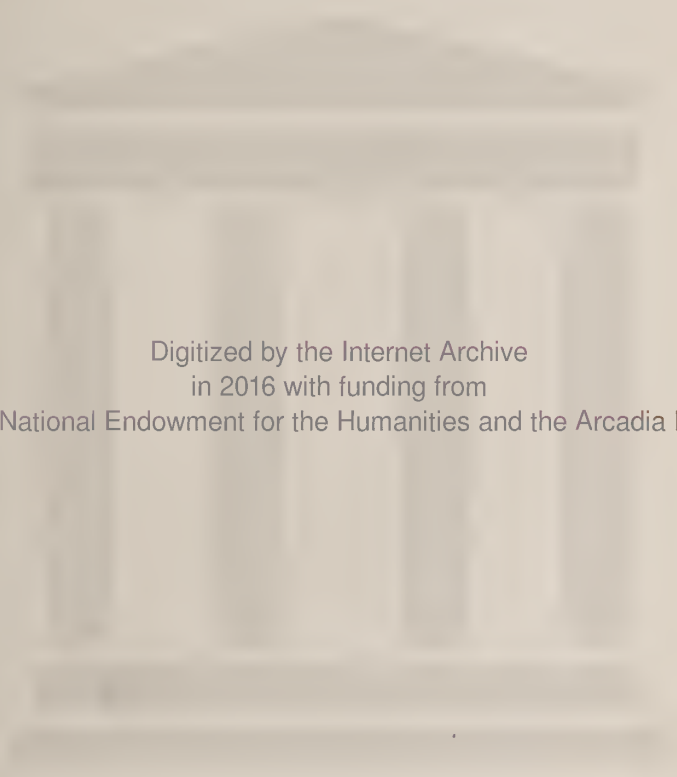
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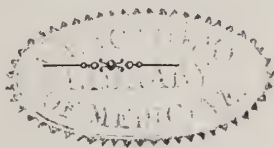
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NEW ORLEANS  
MEDICAL AND SURGICAL  
JOURNAL.

(PUBLISHED EVERY ALTERNATE MONTH.)



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VOL. XVI.

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NEW ORLEANS:

PRINTED AT THE BULLETIN BOOK AND JOB OFFICE.

1859.



# INDEX TO VOL. XVI.

	PAGE.		PAGE
<b>A.</b>			
Abscess of the Brain.....	577	Brain, Abscess of.....	577
Absorption and Assimilation of Metallic Oxides, influence of fatty bodies on .....	390	Brodie, Sir B., on Lithotrity.....	285
Absorption, Cutaneous.....	873, 875	Brown-Séguard, on Tactile Sensibility .....	703
Académie de Médecine, Award of Prizes.....	299	Brown, Periodic and Eruptive Fevers.....	120
Acids, influence in producing Salivation .....	333	Buchanan, Vesico-Vaginal Fistula	109
Æsthetics of a Suicide.....	588	Bullet in the Human Body for Forty Years.....	143
Æther Quinic, Inhalation of.....	730	Burns, Treatment by Warm Bath..	528
Ailanthus Grandulosa, Vermifuge	559	<b>C.</b>	
Albinism.....	741	Caloric, Materiality of .....	903
Alcoholic Fermentation.....	871	Case of Scrofula.....	41
Albuminuria in Croup.....	713	Cataract, Remarks on.....	1
Ammonia, Hydrochlorate of.....	410	Cartwright, on the Influence of Floral Dynamics, etc.....	175
Anæsthesia and Anæsthetics.....	272	Cartwright, on Diphtheria.....	378, 797
Anatomy, Gray's, Review of.....	886	"    Frambœsia and Vesico-Recto-Vaginal Fistula.....	513
Anatomy .....	451	Caustics, Treatment of Phthisis by	264
Anæsthetic Discovery .....	281	Cellular Pathology.....	873
Asphyxia, Apparent Drowning 134,	203	Chaillé, on Board of Health.....	370
Anæsthesia, hysterical .....	754	"    Administration of Charity Hospital.....	373
"    galvanic.....	269	Chaillé, on Administration of Insane Asylum.....	317
Amphiuma Tridactylum .....	13	Chaillé, on Longevity.....	417
Amphiumidæ, Natural History of..	14	"    on Suspension of the Pulse	388
American Medical Association.....	591	Charleston, Yellow Fever of.....	596
Aneurism by Anastomosis, Ligature of Carotid Artery.....	672	Chepmell, on Irrigation in Lacerated Wounds.....	256
Aneurism of Common Iliac Artery	732	Chest, Wounds of.....	259
Animals, Croup of.....	529	Cherry, on Ligature of Carotid and other Cases.....	672
Animal Heat.....	87	Chinese Poisons.....	145
Antiphlogistic Treatment.....	720, 876	Clinical Lecture, Tonics, etc.....	116
Atmosphere, Researches on.....	565	Chloroform, Effects on the Heart... 103	
Atropine in Epilepsy.....	530	"    Modus Operandi of... 573	
<b>B.</b>			
Barbot, translations by, Treatment of Phthisis Pulmonalis.....	264	Chloroform in Operative Surg. 129	413
Barbot, Therapeutics.....	76	Chloroform, Suicide by.....	589
Bennet, Prof.....	451	Cholera, case of.....	66
Bed-Sores, Treatment of.....	581	Chronic Inflammation of Joints, etc	263
Bigelow, on Rational Medicine, rev. of.....	899	Cinchona, Inhalation of.....	403
Bismuth, Sub-Nitrate, in Diseases of Prostate Gland, Gonorrhœa... 324		Cochran, on Labor with Occlusion of the Vagina.....	486
Bismuth, Sub-Nitrate, Impurities	107	Cod Liver Oil.....	330
Black Vomit.....	628	Cold Plague, or Diphtheria.....	378
Blood, Action of the Sympathetic Nerve on the Circulation of.....	395	Commencement, University of La.	452
Blood ... ..	573	Cold, Effects of.....	713
Board of Health, Synopsis of Rep.	370	Conservative Surgery, Chloroform Question.....	129
Board of Health, Administration of Charity Hospital.....	473	Consumption.....	268
Board of Health, Administration of the Insane Asylum.....	374	Contagion .....	348
Boeck, on Syphilization.....	705	Convention of N. Y. Quarantine... 520	
Bone, Formation of.....	568	Contributions to Midwifery, rev. of	746
Boys de Loury, letter from .....	440	Coxe, on Yellow Fever.....	60, 153
		"    on Asphyxia.....	203
		Craniographers, Hints to.....	285

	PAGE.		PAGE.
Croup of Animals.....	529	Endermio Medication, Preparations	
Croup, Albuminuria in.....	723	of Iron.....	559
Croup.....	716	Enteric Juice, etc.....	571
Cure of Inguinal Hernia.....	37	Epilepsy, Atropine in.....	530
Cutaneo-Hysterical Anæsthesia, 754		Ergot.....	582
D.			
Death from Vomited Matters in the		Eruption Produced by Dermanys-	
Trachea.....	567	sus Avium.....	532
Delirium Ebriosorum.....	643	Eruptive Fevers.....	120
Denson, on Tumor of the Uterus, 663		Eyelids, Extracting Foreign Bodies	533
Dermanyssus Avium, Parasite.....	532	F.	
Development of Infants.....	687	Fæcal Retention, etc.....	43
Diabetes, Pathology of.....	632	Fracture of the Skull.....	200
Diarrhœa, Raw Meat in.....	541	Frambœsia, Vesico-Recto-Vaginal	
Diet, Influence of.....	723	Fistula.....	513
Diphtheria.....	797, 847, 842	Fatty Bodies, Influence on Absorp-	
Diphtheria.....	545, 556	tion of Metallic Oxydes.....	390
"  Note on.....	556	Fermentation, Alcoholic.....	871
Dowell, on Cause of Scrofula.....	41	Fever.....	207
"  Anatomy, etc., of Spleen..	337	Fever, Periodic and Eruptive.....	120
Dowler, Bennet, on Natural Histo-		Fever, Typhoid and Typhus.....	825
ry of the Amphiumidæ.....	13	Fever, Yellow, 60, 153, 207, 220,	
Dowler, Bennet, on Pathological		"  "  .....	506, 596, 602
Anatomy of the Large Intestine,		Fistula, Vesico-Vaginal.....	109, 881
etc.....	43, 220	Fœtus, Presentations of.....	582
Dowler, Bennet, on Illustrations of		Foreign Bodies, Extraction from	
Fever.....	207	Eyelids.....	533
Dowler, Bennet, on Longevity.....	424	Foreign Bodies in the Trachea....	737
"  "  Med. Literature..	441	Formation of Bone, etc.....	568
"  "  Quarantine Conv.	520	Formation of Clots During Life... 850	
"  "  Purpura, Scurvy	609	G.	
"  "  Arachnidans and		Gaines, Cod Liver Oil.....	330
their bites.....	786	Galvanic Current, Anæsthesia.....	269
Dowler, Bennet, on Typhoid and		Galvanic Current, Therapeutic.....	141
Typhus Fever.....	825	Gangrene, Hospital.....	261
Dowler, Bennet, rev. of Gray's		Gases Injected into the Tissues....	871
Anatomy.....	886	Generation, Spontaneous.....	690
Dowler, Bennet, rev. of Contribu-		Glycogeny of the Liver.....	751
tions of Drs. Noeggereth and Ja-		Goodman, Pathology of Diabetes..	632
cobi.....	746	Goff, Laryngotomy.....	335
Dowler, Bennet, rev. of Bigelow's		Gonorrhœa, Use of Iodo-Hydrarg.	
Rational Medicine.....	899	Potass, etc.....	324
Dowler, Bennet, on Hydrophobia..	498	Gordon, Hydrophobia.....	489
"  "  Molar Pregnancy	668	Gratuitous Medical Services.....	604-5
"  "  Albinism.....	741	Gull, Tonics and their Administra-	
"  "  Anæsthetics.....	272	tion.....	116
"  "  Drowning.....	134	H.	
Dowler, M. M., Y. Fever, 220, 305	457	Hæmorrhages.....	623
Dysmenorrhœa, Sanguinaria Cana-		Heart, effects of Chloroform on....	103
densis in.....	407	Heat, Animal.....	87
Dowler, J. R., on Tænia Solium....	472	Heddens, removal of Needle from	
Drowning.....	134	the Body.....	336
Drowning, Resuscitation from.....	203	Helmick, Iodo-Hydrarg. of Potash	324
Duffel, on Acute Pemphigus.....	515	Hernia, radical cure of.....	878, 879
Dublin Lying-in Hospital.....	8, 654	Hernia, radical cure of Inguinal... 292	
E.			
Ecraseur.....	252	474, 37	
Effects of Substances on the Heart	299	Hernia, theory of its production... 257	
Elastic Tubes for Tents.....	751	Hints to Craniographers.....	285
Electrical Anæsthesia.....	95	Hospital, Dublin Lying-in.....	8, 654
Electricity, Iodo-Therapy, etc.....	399	Hospital, Gangrene.....	261
Electro-Galvanic Anæsthesia.....	269	Hydrochlorate of Ammonia.....	410
		Hydrophobia.....	489, 489

PAGE.	PAGE
Hygiene.....	603
Hypertrophy of Neck of Womb.....	534
I.	
Iliac Artery, Ligature of.....	732
Infants, development of.....	687
Inflammation.....	761
Inhalation of Cinchona, etc.....	403
Inhalation of Quinic Ether.....	730
Injury with remarkable symptoms.....	676
Intestinal Wounds.....	205
Intestines.....	43
Inundations, influences of.....	506
Iron preparations.....	557
Irrigation in Lacerated Wounds...	255
Joints and Ulcers, treatment of...	263
Journalistic Musings.....	149
L.	
Labor with Occlusion of Vagina... ..	486
Lacerated Wounds.....	255
Laryngotomy.....	335
Lencocythemia.....	713
Letter to Warren Stone, M. D.....	436
“ from Jesse Peebles, M. D... ..	439
“ “ Dr. Boys de Loury.....	440
Ligature of the Common Carotid Artery.....	672
Ligature of Common Iliac Artery..	732
Ligation of Iliac Arteries and Veins	863
Liniceps.....	396
Literature, Medical.....	441
“ “ in New Orleans.....	595
Liver—a Sugar-making Organ.....	751
Lithotomy, new operation.....	300
Lithotripsy.....	235
Longevity.....	427, 417
M.	
Malum Egyptiæcum.....	797
Materiality Caloric.....	903
Maupin M.....	261
McElrath, Surgical Memoranda.....	205 195
McPheeters on Dublin Lying-in Hospital.....	8, 654
Meigs, J. Aitkin, M. D.,.....	285
Medical Literature.....	441, 595
Menses, Suppression of,.....	653
Mercury in Syphilis.....	710
Mercury, Perchloride.....	675
Meteorological Tables, 150, 456, 608, 760	760
“ “.....	908
Microscope.....	904
Midwifery and Diseases of Women and Children.....	746
Military Life,.....	872, 904
Mitchell on Experiments.....	299
Molar Pregnancy.....	667
Monographs.....	904
Mortality of Soldiers.....	872 904
Mortality Statistics, ...151, 453, 608	760
“ “.....	908
Mortality in England in 1856.....	603
Muscle, new.....	572
N.	
Natural History of the Amphiuma..	13
Nature and Prevention of Periodic and Eruptive Fevers.....	120
Nature of Miasms and Contagion..	348
Needle in the Body for Twenty Years.....	336
Nicotiana.....	589
Noeggerath on Midwifery, etc.....	746
Notice to Subscribers.....	607
Nott on Cataract.....	1
O.	
Obituaries.....	754
Occlusion of the Vagina.....	486
Operations for Cataract.....	1
Operative Surgery, Chloroform in..	413
Ophthalmoscope.....	606
Osseous System.....	568
Overflows preventive of Yellow Fever.....	506
Owens, on Fracture of the Skull... ..	200
P.	
Parotiditis.....	649
Pathological Anatomy of Large Intestine.....	43
Peak, on Acids in Salivation.....	333
“ “ Delirium Ebriosorum... ..	649
Pemphigus Acute.....	515
Peritonitis, Quinine in.....	867
Petechiæ.....	627
Pharmacopœia.....	727
Pharmacopœia of United States... ..	600
Phthisis Pulmonalis, treatment by Caustics.....	246
Placenta Prævia.....	584
Pneumonia, Typhoid.....	651
Poisoning with Perchloride of Mercury.....	675
Poisons, Chinese.....	145
Practical Surgery and Medicine... ..	123
Presentation of the Fœtus.....	582
Preservative Liquid.....	396
Propylamin.....	405
Pulmonary Artery, Occlusion and Death from.....	861
Pulmonary Tuberculosis.....	67
Purpura.....	610
Q.	
Quarantine Convention of N. York, ..	520
Quinic Æther.....	730
Quinine in Peritonitis.....	867
Quinney, on Molar Pregnancy.....	667
R.	
Radical Cure of Inguinal Hernia... ..	37
Radical Cure of Hernia.....	878, 879
Raw Meat in Diarrhœa.....	541
Remak, on Therapeutic Action of the Galvanic Current.....	141

PAGE.	PAGE.		
Remarks on Cataract.....	1	Syphilis, Mercury in.....	710
Report of Board of Health.....	370	"    Secondary.....	712, 753
"    "    Administrators of		Sympathetic Nerve, Action of, on	395
Charity Hospital.....	373	T	
Report of the Administrators of the		Tactile Sensibility.....	703
Insane Asylum.....	374	Tadpole, Vitality of.....	753
Researches on the Atmosphere....	565	Tænia Solium.....	472
Richardson, Notes on Surgical		Tannin, .....	396
Cases.....	679	Tarantulated Patient.....	784
Richardson, on case of Cholera....	66	Tarantula, Natural History.....	786
Richardson, on Vesico-Vaginal Fis-		Temperature of Patients.....	93
tula.....	881	Therapeutics, French.....	865
Roser, on Theory of the Production		Therapeutics.....	76
of Hernia.....	257	Therapeutical Memoranda.....	821
Ronalt, on Caustics in Phthisis		Tonics and their Administration..	116
Pulmonalis.....	267	Tracheotomy.....	737
Riddell, on Nature of Miasm and		Tumor of the Uterus, etc.,.....	663
Contagion.....	348	Tubes instead of Tents.....	751
Russel, on Parotiditis.....	649	Tuberculosis, Pulmonary.....	67
S.		Typhoid Fever.....	777
Sanguinaria Canadensis, Use in		Typhoid and Typhus Fever.....	825
Dysmenorrhœa.....	407	Typhoid Pneumonia.....	651
Scrofula, Treatment, Cure.....	41	U.	
Scurvy.....	616	Ulcer of the Stomach.....	533
Scott, on the Treatment of Joints		Uni. of La., Commencement of.....	452
and Ulcers.....	263	Use of Speculum.....	582
Sensibility, Tactile.....	703	Uterus, Tumor of.....	663
Skey, on Conservative Surgery		V.	
and the Chloroform Question....	129	Vagina, Occlusion of.....	486
Skin, Change of.....	741	Vaginal Fistula.....	513, 881
Skull, Fracture of.....	200	Vena Porta.....	572
Slemons, on Typhoid Pneumonia..	651	Vermifuge properties of Ailanthus..	559
Smith, Typhoid Fever.....	777	Vertigo.....	560
Spleen, Anatomy, Pathology .....	390	Virchow on Pathology.....	873
Spontaneous Generation.....	690	Voluntary Suspension of the Pulse..	388
Statistics, Vital.....	873	Vomited Matters in the Windpipe..	567
Statistics, Mortality.....	603, 151	W.	
Statistical Researches on the De-		Warm Bath in Burns.....	528
velopment of Infants.....	687	Wunderslich, C. A., Temperature	
Stone, Letter from Paris.....	436	of Patients.....	93
Stomach, Treatment of Ulcer of... 533		Wurtz, Henry, Chemical Exami-	
Stone, on Inflammation.....	761	nation of a Bullet, etc.....	143
"    Trachetomy.....	737	Wounds, Intestinal.....	205
"    Pulmonary Tuberculosis... 67		Wounds, Irrigation in.....	255
"    Letter to.....	436	"    of the Chest.....	259
"    Ligature of the Common Il-		Womb, Lengthening of the Neck..	534
"    iac Artery.....	732	Winnemore, S. E., Suppression of	
Strube, Therapeutical Memoranda	821	Menses.....	653
Subscribers, Notice to.....	302, 607	Whitfield, Geo., Remarkable Case	
Sub-Nitrate of Bismuth, Impurity	107	of Injury.....	676
Suicide by Chloroform.....	586	Women and Children, Diseases of..	746
"    æsthetics of.....	588	Wounded in Italy.....	752
Treatment.....	337	Wounds, Lacerated.....	255
Suppression of the Pulse.....	388	Y.	
Suppression of the Menses, Treat-		Yellow Fever.....	60, 153, 220, 305, 457
ed by Cauterization.....	653	"    Overflows a Preventive of	506
Surgical Cases in Charity Hospital	679	Yellow Fever in Hospital of Lis-	
Surgery and Medicine...200, 195,	123	bon .....	602
Surgical Memoranda.....	195	Yellow Fever of 1859.....	751
Syphilization.....	705	"    of Charleston in 1858	596
Syphilis, Secondary, Inoculation in	870		

THE  
NEW ORLEANS  
MEDICAL AND SURGICAL JOURNAL.

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JANUARY, 1859.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Remarks on Operations for Cataract:* By J. C. NOTT, M. D.,  
Mobile.

THE operations for cataract may be classed under two main divisions which cover the ground of the few remarks I propose to make, viz.: those with the *knife*, and those with the *needle*. In the former, an opening is made in the cornea, and the cataract, whether lenticular or capsular is *extracted*. In the latter a needle is introduced, and the cataract is displaced, or broken up and left for absorption, as the case may be.

The current of opinion has been, for years past, running so strongly in favor of the operation of *extraction*, that few have had the courage to raise their voices against it, and an operator at the present day, who relies upon the *needle*, is looked upon by ophthalmic surgeons generally, as an "old fogey."

My own personal experience had led me to conclusions very different from those of modern European surgeons, and although the needle in my hands had given results which would compare favorably with any statistics published, I was induced to think that, chance had favored me, or that there must be a larger proportion of *soft* cataracts in Alabama, than in those colder latitudes from which our cataract statistics mostly come. I ventured some two years ago to publish a very modest little article in this journal, stating my unexpected suc-

cess in cataract operations, with the view of eliciting information from others.

I stated that some twenty years ago I commenced operating in Mobile for cataract, and with the timidity of a young surgeon commenced with the needle, fearing to stake my reputation on the hazardous operation of extraction.

My first cases happened to be soft cataracts—too soft for displacement, and were broken up and left to be absorbed *in situ*. The cases did well, and I was encouraged to continue the needle operations. My number of cases has been steadily increasing and with more enlarged experience, I have seen no reason to change the mode of operating. About three out of four of the cataracts presented to me have been soft or capsular. When the eye was otherwise in good condition, I have never, that I recollect, failed in restoring good vision where the lens was soft, and have rarely failed when they have been hard, and required to be displaced. The membranous ones have sometimes had to be extracted through a small opening in the cornea.

Now, in my published article, alluded to, I ask the question, whether chance has thrown so large a proportion of soft cataracts into my hands, or whether they are peculiar to our climate? In adults even of sixty, seventy and eighty years of age, most of the cataracts which have fallen to my share have been soft; or at least soft enough to be easily cut to pieces with a delicate needle.

Much has been said about the tissues wounded and inflammation produced by the needle; but this is by no means in accordance with my experience. Much depends upon the construction of the needle; those usually seen in French eye cases, are much too large, and one who has not tested the fact, can form no idea of the advantage of a very delicate over a coarse needle; in fact it cannot be made too small if sufficient strength is preserved. I have sometimes had to operate a second or third time on the same eye at short intervals and without material inflammation.

I am perfectly aware, as the world goes, that the statistics of an individual, unconnected with a public institution, have little value in the eyes of the profession, and my main object at present is to attract attention to the article below, taken from the *Liverpool Medico-Chirurgical Journal*, January, 1858. My operations have never been secretly performed, but have, in every instance, been witnessed by my medical friends. The article below is the "*Report of the Liverpool Eye and Ear*



*Infirmary on Cataract; by Dr. Nevins, Assistant-Surgeon,*" and until statistics, as satisfactory with regard to the operation of *extraction*, are laid before us, I must believe that the needle properly used, as a general instrument, is more reliable than the knife, although neither should be exclusively used.

*Report of the Liverpool Eye and Ear Infirmary on Cataract.*—By Dr. J. Birkbeck Nevins, Assistant-Surgeon.—[In the following paper special attention is directed not only as to the immediate result of different methods of operation, but also as to the remote consequences.]

In looking at the reports of the Metropolitan Institutions, we find cases of cataract operated upon by extraction are reckoned by the hundred, whilst that is a mode of operating scarcely ever practised in this infirmary, the operation by displacement or solution being almost invariably adopted. Now it is acknowledged that the immediate risks in extraction are far greater than in the needle operation; but it is said that in the hands of an experienced man the results are so much more satisfactory as to place it decidedly above its more ancient rival; and I have heard of an experienced oculist in a very large ophthalmic institution, who has so uniformly adopted and seen others adopt this operation, that he has never seen that of displacement practised in a single case of hard cataract. By the advocates for extraction it is said, that the presence of the lens in the eye after displacement produces certain injurious consequences which do not manifest themselves for a long period—it may be years—after the patient is dismissed from the surgeon's care, and that subsequent amaurosis results from the irritation of the retina produced by the presence of a foreign body, such as a displaced lens. To this point, therefore, I have directed special attention; and I trust it is not presumptuous to hope, that the form in which the results of the practice of this Eye Infirmary are here detailed, may stimulate some of those who have equal experience in extraction cases, to publish their results in a similar form, that the oculist and the profession generally may be able to judge which is upon the whole the most satisfactory in its results—a point which I believe has never yet been submitted to so rigorous a test as I here propose.

To state simply that a patient operated on for cataract has been "cured," or "relieved," or "has derived no benefit," appears to me to be totally worthless; for the *cataract* may be cured, that is to say, it may have disappeared, and the pupil remain clear, and the eye free from inflammation or apparent disease, but the patient may nevertheless be totally blind from resulting amaurosis, and his own report of himself may be, that he is worse than before the operation, whilst it may be said—and in one sense with truth—that the cataract has been cured. I have therefore endeavored to employ a test of practical value, instead of a term which is open to such misconception, and in relating the results of the cases operated on by the needle, I have divided them into classes, according to their visual powers when discharged, without reference to the presence or absence of any remains of the lens or its capsule. Thus the first class consists of patients who "can see to

read," in which none have been included who have not recovered their sight so perfectly as to be able not only to read, but to pursue their ordinary avocations with ease. The next class are those who describe themselves by saying "they can see everything," and is confined to those who never could read, and therefore cannot be submitted to that test. The third class contains those who can "discern features," that is, patients who can recognise every one they see, but cannot decypher type, or see to sew or do fine work, but can follow household employments or laboring occupations. These may all be said to have regained good useful vision. The next class consists of those who "can see very imperfectly," which means that they can just see their way, so as to avoid running against obstacles. The fifth class contains those who have derived "no advantage" from the operation; and the sixth and last consists of those who are "worse than before it." With reference to these last two classes, some important points have to be taken into consideration, especially in cases of traumatic cataract.

It not unfrequently happens that amaurosis is associated with the cataract, and if the patient is blind from the first disease, the removal of the second will not restore vision. Such a patient will therefore have derived no benefit from the operation, though it may have been perfectly right to give him the chance of recovering a little sight. In many cases it is difficult even for an experienced oculist to be certain whether there is amaurosis before the cataract is removed, or not, and he will therefore give the patient the benefit of the doubt; and this is especially the case in traumatic cataract. In estimating, therefore, the result of the operation, I have given the total number of those who have received no benefit, in order that the report may be complete; and I have also given the return, deducting those who were entered in the report at the time of their admission as being amaurotic, or having the eye soft, or otherwise diseased, in order that the result of the operation upon healthy cataractic eyes may be fairly estimated.

A similar kind of qualification must be made in the cases which have been worse for the operation; but in these another disturbing circumstance comes into operation. Some of the patients recorded as "worse" did not leave the hospital worse, but better; but, in consequence of using the eye too soon, or of exposure to cold, they have returned in a few weeks with an inflammatory condition of the organ, which, in some cases, has ended in total loss of vision. I have not, however, deducted these from the list, because, although in one sense, it was not the operation which made the patient worse, yet, in another, this deterioration of the eye was a legitimate consequence of it; since the exposure would probably not have produced such destructive results had not the eye been previously operated on. I think, therefore, that in considering the probable result of the operation, we ought to make allowance for a certain amount of risk on this score, and accept it as a natural result if it does occur, instead of rejecting it, and saying that the operation has been successful, and the subsequent injury was the patient's own fault.

Such is the mode in which I have estimated our operations; and if the advocates for extraction will lay before the profession the results of their cases in a similar manner, it will be in a position to judge

more impartially and intelligently between the two methods than I believe it can do at present.

*Results of the Operations.*—It is necessary to state in this place, that these results only refer to cases of needle operation, whether it has been performed for the purpose of displacing a lens, or of cutting it up in order to obtain solution. The results of extraction cases will be noticed further on. One hundred and thirty-four eyes have been operated upon with the needle during the last two years, under my observation, and they have yielded the following results:

53 patients, or	$39\frac{1}{2}$ per cent.,	“ could see to read.”
20 “	$14\frac{1}{2}$ “	“ could see everything.”
21 “	$15\frac{1}{2}$ “	“ could distinguish features.”
—	—	—
94	$69\frac{1}{2}$	

That is to say, above two-thirds of the cases recovered good useful vision, and in above half it was sufficiently perfect for the most minute purposes of business or daily life. In addition to these, one in twelve gained imperfect vision; *i. e.*, they could just see their way about the house or the streets: and a single patient is recorded as “improved,” but I have no more accurate account than this.

We now turn to the unfavorable cases which bear a small proportion to the successful ones. Twenty-one patients, or  $15\frac{1}{2}$  per cent., had “no improvement;” but of these, 14 were entered on admission, as having amaurosis, or some disease of the globe. It was not, therefore, to be expected that they would be improved; and when they are deducted, we have a remainder of 7 patients, or 5 per cent., who derived no benefit, though they were no worse for the operation, and they had had the chance given them of obtaining some degree of vision.

The list now to be given, contains those patients who were worse at an interval of some weeks or months after the operation than before it, and of these I have records of 9; of which 3 were entered as having amaurosis, or softening, etc., previous to the operation; leaving 6 cases, or  $4\frac{1}{2}$  per cent., in which a favorable result might have been hoped for, but in which loss, and not benefit, was ultimately experienced.

The general results, then, of the needle operations, as detailed in this report, have been, that 70 per cent. of the cases have recovered useful or perfect sight, and 9 per cent. are able to see their way; whilst 9 per cent. who had a prospect of recovery, either derived no benefit, or were worse than before. In addition to these, 12 per cent. were operated upon in order to give them a chance, though with little prospect of benefit to begin with, and the result corresponded with the anticipation.

These results must be considered as highly satisfactory; for they show that where no fatal disease of the eyes can be recognized beforehand, in addition to the cataract, not one patient in ten fails to regain either perfect vision, or a degree of improvement from the operation; and of those who do not benefit by it, above half are no worse for it.

It also shows that not one patient in twenty who had a prospect of recovery is a loser by the operation, and that many even of those who

suffer ultimate loss, might have escaped it, if they had not exercised their newly-acquired powers too soon, or exposed themselves imprudently to injurious influences.

*Number of Operations Requisite.*—One great objection to the needle operation, and (as I think this report will prove,) the only really valid one, is the length of time requisite for cure, and the number of operations occasionally necessary. In the operation by extraction, the patient speedily knows the best or the worst that he has to look for; whilst in the needle operation the treatment may extend over months, and the necessity of repeated operations discourages the patient, and makes a demand upon his time which, in the case of an operative, is often of serious consequence. If, however, it should prove that the ultimate result of the one is decidedly more favorable than the other, there are few patients who would not prefer the suspense of a half year, with a high probability of ultimate useful vision, to the certainty that at the end of a fortnight they will be either cured or blind for life. The following table shows the number of operations in a hundred cases, of which I have kept a detailed account:

In 37 cases 1 operation sufficed.				
44	"	2	"	were requisite.
14	"	3	"	"
4	"	4	"	"
1	"	5	"	"

Five is the greatest number I have yet known to be required.

It will therefore be desirable to tell a patient beforehand, that only about one case in three is cured by a single operation, and that he will be almost certain to require two; but the chances are in his favor that a third may not be necessary.

*Length of Time required for Accomplishing the Solution of a Hard Cataract.*—Another of the objections entertained to the operation for displacement of a hard cataract, is based upon the assumption that the lens will remain in the eye as a foreign body, and produce the injurious consequences likely to result from the presence of a foreign substance in so delicate an organ. It therefore becomes a matter not merely of curiosity, but of practical importance, to ascertain whether the lens does so remain, and produce the consequences anticipated; and also, how long it will probably be before it disappears, if it should ultimately disappear at all. The following table and remarks show the rate of solution in thirty-three cases of hard cataract which were displaced by the needle:

In 1 case, the lens had gone in 11 days.				
3	"	"	"	16 to 19 days.
1	"	"	"	" lens almost entirely gone" in 17 days.
5	"	"	"	lens had gone in 2 months.
1	"	"	"	" lens nearly gone" in 2 months.
3	"	"	"	lens had gone in 3 months.
2	"	"	"	5 "
1	"	"	"	6 "
6	"	"	"	12 to 18 months.

In three other cases, raising the number to 26, the lens was much reduced in size, but had not entirely disappeared at the end of three months, though there was every reason to expect that it would do so shortly. In three other cases, portions of capsule were still present, not interfering with vision; and in four, the lens was still visible to a greater or less extent, but very rarely interfering with the sight, after periods varying from ten months to seven years. It appears, therefore, that in four-fifths of the cases of hard cataract the lens is entirely dissolved within eighteen months, and in above half solution is complete within six months: whilst in several cases the lens had entirely disappeared within three weeks. And even in several cases in which solution was never perfected, the remains of the lens or capsule did not practically interfere with useful vision.

[Mackenzie says, "Displacement is only to be thought of when some insuperable objections exist to division and extraction—chronic inflammation within the eye, dissolution of the hyaloid membrane, the amaurosis, and, I believe, the almost invariable results of a cataract of any considerable bulk continuing undissolved in the situation assigned to it by displacement." The author, speaking from experience, believes that the fear above expressed is groundless, for though it is true that an undissolved lens does operate prejudicially, yet it is so rarely that a lens does remain undissolved, that in practice, the objection falls to the ground.]

*Results of Extraction*—For these I am chiefly indebted to Mackenzie (4th edit., p. 748,) as our experience of the operation derived from the infirmary is so limited, that its advocates might very properly decline to accept the inferences drawn from so small a number of cases. So far as I know, during the last ten years or more, the operation has only been performed for idiopathic cataract in two instances in the infirmary; in the first of which the eye was totally lost, after many weeks' suffering; in the second, the operation was successful, and the patient left the hospital able to do everything except read, for which she was not tested, as being too early. I have records of 18 operations by extraction, 12 of which resulted in total loss of vision, and 6 in good sight, *i. e.*, all but one could see to read. These operations were performed by various persons, and although some of them are dead, and I have no means of ascertaining their surgical reputation whilst alive, the majority have been performed by oculists of wide and deserved celebrity, both metropolitan and provincial. When these are added to Mackenzie's list, the results are as follow:

	Perfect Sight.	Indifferent.	Failures.
La Faye, - - - - -	2	2	2
Poyet, - - - - -	2	2	3
Roux, - - - - -	97	10	72
List as above, - - - - -	6	0	12
	107	14	89

When these are reduced to per centages, to make them correspond with the results of displacement, we have—

Successful.	Indifferent.	Failures.
51 per cent.	6½ per cent.	42½ per cent.

In addition to the above, Mr. Sharp operated upon 19 cases, "in above half of which he had what he thought 'tolerable success.'" And Dr. Tartra operated in 71 cases by extraction; but as the results of 37 "were either unknown or more or less unfavorable," in addition to "6 indifferent results and 8 total failures," making 45 unknown or unfavorable results out of 70, or rather more than 64 per cent. The return is too imperfect to be worth anything for comparison. Dr. Tartra, however, did not confine his operations to extraction, but his results after displacement are almost equally uncertain with those after extraction. What he does give definitely is, that 27 per cent. of the extractions recovered perfectly, and 56 per cent. of the displacements; or two to one in favor of the latter operation, and that 9 per cent. of the extractions, and 12 per cent. of the displacements, were total failures; the extractions in this respect having the advantage.

In bringing our definite results, then, into a collected form, we find the following materials for making a comparison:

	Successful.	Indifferent.	Failures.
Extraction - -	51 per cent.	6½	42½
	57½		
Displacement - -	69½ per cent.	9	9½ & 12 known to be 21½ amaurotic before operation.
	78½		

Such, then, is the present state of our definite materials for forming an opinion upon the respective merits of the two kinds of operation, as tested by their results; and it will be for those who are interested in Ophthalmic Surgery to consider whether extraction, rather than displacement, is, upon the whole, the best mode of removing a hard cataract.—*Liverpool Medico-Chirurgical Journal*, Jan., 1858.

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ART. II.—*Lying-in Hospital, Dublin, October 11th, 1858.*

DR. STANFORD CHAILLÉ—*Dear Doctor:* I have been intending for some time past to send you some account of the practice, etc., of the Dublin Lying-in Hospital, but have been prevented by various circumstances from doing so before this. This Hospital was founded more than a century ago, and is the largest of the kind in the United Kingdom. It contains one hundred and thirty beds, fifteen of which are reserved for chronic diseases of the uterus and its annexes. About two thousand women are confined here annually, making an average of a little more than five *per diem*.

The medical staff consists of a master, Dr. McClintock, and two assistants, all of whom reside in the Hospital. The master is elected by the Board of Governors, and holds his office for seven years. The assistants each pay five hundred dollars *per annum* for the privileges they enjoy. The *Interne* pupils pay one hundred dollars for six months, or half that amount for three months, besides their board. The advantages here are very good, though not as much so as could be wished. Students are not permitted to perform operations, however well qualified they may be, but can only look on while they are performed by the master or his assistants. At Vienna, I am told, students enjoy the privilege of applying the forceps, or making version when necessary.

The ventilation of this Hospital is very good, and every attention is paid to cleanliness; but notwithstanding every precaution, there are occasional epidemics of puerperal fever. In each ward is a couch on which the patient is delivered, and two hours afterwards, she is removed to her bed. A purgative is usually given on the second day after delivery, and if the patient goes on well, she is allowed to sit up on the fifth day, and is discharged from the Hospital on the eighth day. Prolapsus uteri appears to be very common among the lower classes of Dublin, which is doubtless owing, in many cases, to getting up too soon after confinement.

Dr. McClintock lays great stress upon the use of the binder after delivery, to keep the uterus firmly contracted and to diminish the size of the abdomen. In the Paris Hospitals, the binder is considered useless, and is not usually applied. The patients here are always delivered on the left side, not only in natural labors, but also in all operations. In Paris they are always delivered on the back. In natural cases I think the side position is preferable, but when an operation is to be performed, I think it is much more convenient to place the patient on her back with the hips drawn over the edge of the bed.

In all obstetrical operations here chloroform is usually given, unless there is some special contra-indication. It is especially useful when it is necessary to make version, or to introduce the hand to bring away a retained placenta.

An interesting case occurred a few days since showing the advantages of chloroform. It was a case of retained placenta caused by

spasmodic contraction of the cervix uteri. After waiting four hours without result, Dr. McClintock decided to administer chloroform, and to introduce the hand; gradually dilate the os uteri and bring away the placenta. The patient was with considerable difficulty brought under the full influence of the chloroform, and on introducing the hand into the vagina, the os uteri was found to be dilated and the placenta extruding, so that it was easily extracted, without introducing the hand into the uterus, which may justly be considered one of the most dangerous operations in obstetrics.

The only forceps used in this Hospital are the straight forceps, but with longer blades than the ordinary short forceps. The following are considered indispensable conditions in order to render the forceps applicable: 1st. "That the child be alive, or at least that there is no reasonable ground for supposing it to be dead." When the child is ascertained to be dead, the perforator and crotchet are used, being considered as less dangerous to the mother than the application of the forceps. The stethoscope renders most valuable aid in ascertaining the condition of the fœtus. If the fœtal heart is heard in the early stage of the labor, and afterwards the most careful examinations fails to detect it, the death of the fœtus may be considered as almost certain if not absolutely so. Hence it is an established rule of the Hospital to ascertain if the fœtal heart is audible, when the patient enters, so that if the labor becomes difficult, and instrumental aid is required it can be ascertained with almost absolute certainty whether the child be alive or dead. 2nd. "That the head has remained stationary within reach of the forceps, for six hours at least." This rule, of course, is not adhered to when there exists any pressing complication, such as hæmorrhage or convulsions, etc. 3rd. "That the membranes be ruptured and the os uteri fully dilated." 4th. "That the head of the child be so circumstanced that the ear can be distinctly felt, without the use of any force or violence on the part of the examiner." Dr. Rigby, of London, does not approve of this plan of feeling for the ear. In his "system of midwifery," he very justly remarks: "The blades should always, if possible, be applied one on each side of the head, the position of which must be determined by the direction of the fontanelles and sutures, *not by feeling for the ear*, as is usually recommended in this country. The ear can seldom be reached without causing a good deal of pain, even under the most favorable circumstances," etc., etc. 5th. "That the state of the soft parts be such as



denotes the absence of inflammation ; in other words, that they be free from undue heat, dryness, tumefaction, or morbid sensibility." These rules are extracted from Hardy and McClintock's report of the Hospital practice.

When the above conditions are absent and instrumental aid is necessary, craniotomy is performed with the perforator and crotchet. The cephalotribe is never used—it being considered a dangerous and unnecessary instrument. The ergot of rye is used with great caution during labor, as experience has shown that it exerts a very injurious influence on the fœtus, unless it be born soon after the ergot has been given. In cases of inertia uteri, when the head presents and the os uteri is fully dilated, and stimulating enemata of salt and water have failed to excite efficient contractions, ergot, ℥ss., is given and the dose repeated in twenty minutes if necessary. But if the child is not born within *two hours* after the administration of the ergot, the forceps is applied and the child speedily extracted, as the experience of this Hospital has been that the child was generally still-born, if not born within two hours after the ergot was given. Dr. McClintock is opposed to the administration of ergot during the third stage of labor, to produce the expulsion of the placenta. He says that if the placenta should happen to be morbidly adherent, the ergot might cause hour-glass contraction, and render the operation of introducing the hand into the uterus to detach the placenta, much more difficult, and, consequently, more dangerous to the patient. It is the common practice here to push out the placenta by grasping the fundus uteri and making considerable pressure which is generally effective, if the placenta is detached.

We have recently had quite a number of cases of puerperal fever. The usual treatment consists in local depletion by means of leeches to the seat of tenderness; mercury and opium internally and mercurial inunctions to the abdomen. I cannot say that this treatment has been more successful than many others that have been recommended for the cure of this terrible malady. The muriated tincture of iron has recently been very much in vogue in London, as a remedy in puerperal fever. It was tried in several cases here, with varied success. Two cases appeared to be much benefitted, while two others died. I have not seen a sufficient number of cases to judge of its efficacy. It was not thought much of here, and was soon abandoned for the old treatment. One of the cases that appeared to be benefitted

ted at first, died after a month's illness from exhaustion produced by a phlegmonous inflammation of the calf of the leg. Twenty-four leeches were applied in the commencement of the attack, and then the muriated tincture of iron, (ten drops every two hours) was given. The other case appeared to be convalescing, when she was attacked with arthritis in the wrist. She was removed from the Hospital by her friends, before convalescence, so that I am not able to say whether she recovered or not.

To give you an idea of the advantages a student has for seeing obstetrical operations, I will mention that during three months that I have been an *Interne* in this Hospital, there have been eight forceps cases, three craniotomies, and two cases of version.

A very unnatural and interesting case of complicated labor occurred a few weeks since, the details of which may interest you: B. B., æt. thirty-seven, primipara, was admitted into the Hospital at noon on the 14th of September. She stated that at 9, A. M., on the 12th, she had been delivered of a dead child, at a village seven miles from Dublin. Her medical attendant discovered that there was another child in utero, and after waiting two days for its expulsion, advised her to come to the Hospital. Accordingly, she was placed in a cab and brought in. On examination the os uteri was found to be half dilated, the membranes unbroken, the breech of a second child presenting, and the cord of the first hanging out of the vulva. In a short time the second child was born alive, and apparently of about eight months' development. It was now discovered that there was a third child presenting also by the breech. There being some hæmorrhage, it was deemed expedient to bring down the feet and hasten the delivery. The third child was also born alive. The uterus contracted, and the hæmorrhage ceased; but commenced again in about fifteen minutes, and the placentæ were still retained. There was no time to be lost; if the hæmorrhage was not speedily arrested, the patient would bleed to death. Although she was very much exhausted, it was deemed necessary to introduce the hand and bring away the placentæ without delay. This was accordingly done, giving her, at the same time, stimulants. There were two placentæ, both of which were partially morbidly adherent. The uterus now contracted firmly and all hæmorrhage ceased; but in spite of all efforts to sustain her vital powers she sank rapidly, and expired in a few hours after delivery, from the combined effects of exhaustion, arising from the

protracted labor, the shock of delivery, and hæmorrhage. The pulse continued perceptible to the moment of dissolution. The great error committed by the "village doctor" in this case, was in not rupturing the membranes of the second fœtus, soon after the birth of the first, and before the os uteri had time to contract. Had this been done, the patient would have stood a much better chance of recovery, although the adherent placenta and hæmorrhage would have rendered the prognosis very unfavorable.

The most remarkable feature of the case, however, was revealed at the autopsy: The uterus was very large, and on its anterior surface was an *enormous fibrous tumor*, seven or eight inches long and four or five wide. The woman stated on coming in, that she had perceived a tumor in the abdomen for about two years. This shows conclusively that organic disease of the uterus does not prevent conception, and it is not a little remarkable, that utero-gestation should have gone on almost to the full term with triplets, while, at the same time, there existed such a large amount of organic disease in the body of the uterus.

The children were in a healthy condition when taken from the Hospital.

Yours, very truly,

W. A. MCPHEETERS, M. D.

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ART. III.—*Natural History of the Amphiuma.*—*Remarkable instance of the Tenacity of Life in the Amphiuma Tridactylum.*—(CUVIER: Congo Eel of the Negroes.)

PROF. J. L. RIDDELL stated, at the meeting of the Physico-Medical Society, Nov. 6, 1858, that in July, 1856, twenty-eight months ago, Dr. A. S. FOX presented him with a fine living specimen of that large Louisiana reptile, the *Amphiuma Tridactylum*. Its length is three feet, weight, four or five pounds. It has been all the while kept coiled up in a glass jar, about eight inches in diameter, covered with turbid Mississippi water. The jar is of near two gallons capacity and has a perforated tin cover.

For the first eight months, the Mississippi water was renewed every week. It was then left unattended to for four months. The water evaporated leaving merely a damp mud. The reptile was found alive, but somewhat wasted away, and not very active. For the next eight months, water was added and renewed about once a month. It has been wholly neglected for eight months now past, and at the present time, the *Amphiuma* is still alive; quite active, and apparently in better condition than a year ago. During the first year of his imprisonment, the *Amphiuma* was slightly wounded in the tail, perhaps a dozen times, to obtain for microscopic observation, some of his blood; the corpuscles of which are larger than those of any other known animal; being about one-third larger than those of the *Proteus Sanguineus* of Europe; and cubically estimated, about one thousand times larger than the blood corpuscles of the human system.

Prof. R. had in three instances observed rattle-snakes confined in cages to live near eighteen months, without food or drink. But the persistent vitality of the *Amphiuma*, under circumstances seemingly more adverse, and for a period one-third longer, is much more wonderful.

In the summer of 1857, the water dried down so as to leave a little plastic mud beneath the reptile, his head being thrust into the dampest place. At all other times there was water enough in the jar for his complete submersion. S. C.—*Abstract from the Minutes of the Physico-Medical Society, Regular Session, Nov. 6, 1858.*

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ART. IV.—*Natural History of the Amphiumide, with Remarks and Observations on Organic Science, Physiology, Crocodilian Hibernation, and Instinct:* By BENNET DOWLER, M. D.

*All the forms are similar, none are exactly alike. A group indicates a secret law, a sacred ænigma: Every animal is its own purpose, originates perfect in Nature's womb, and generates perfect children. All its limbs develop themselves in conformity with eternal laws, and the rarest form follows in secret the primitive type.—GOETHE.*

*All animals are but fetal forms of Man.—OENEN.*

NEITHER the faunæ nor floræ any more than the epidemics of a geographical region, can be invariably explained by known climatic conditions. For example, the equidæ, coëxistent with the historical æra

and most widely diffused in the old world, did not exist in America at its discovery, though at present, the whole continent, particularly within the equatorial regions and temperate zones, near the tropics, abounds with horses and asses as well as horned cattle of imported origin, showing that the climates of the New World are better adapted to these animals than any other, as unnumbered millions of them, without human care, in a wild state, darken the almost boundless Llanos,\* Pampas and Plateaux, from the Venezeula to the Argentine Confederation. Why are the tree-eating Elephantidæ, not to name other animals, nowhere found in the immense forests of the Oronoco, Amazon, and Rio de la Plata? The palæozoic ages of geology, the stone records of the remote past, plainly show that in the same region successive superimposed formations contain different or altogether dissimilar fossiliferous animals, with an hiatus strongly marked between each, showing, however, an elevating or progressive tendency towards the existing faunæ, which, on the whole, is organically better than any preëxistent one. Nevertheless, the principle of adaptation of the faunæ and floræ to the climate, is more or less apparent in earth, air, seas, districts and zones.

Professor Owen, one of the greatest living naturalists, in his recent Address to the British Association, gives the following brief but lucid exposition of these interesting topics: "The earliest testimony of the living thing, whether shell, crust, or coral in the oldest fossiliferous rock, is at the same time proof that it died. It has further been given us to know, that not only the individual but the species perishes; that as death is balanced by generation, so extinction has been concomitant with creative power, which has continued to provide a succession of species; and furthermore, that as regards the varying forms of life which this planet has witnessed, there has been "an advance and progress in the main." Geology demonstrates that the creative force has not deserted this earth during any of her epochs of time; and that in respect to no one class of animals has the manifestation of that force been limited to one epoch. Not a species of fish that now lives, but has come into being during a comparatively recent period; the existing species preceded by other species, and these again by others still more different from the present. No existing genus of fishes can be traced back beyond a moiety of known creative

\*The Llanos, covered with nutritious graminæ, according to Humboldt, extend over an area of 105,200 square leagues—an area exceeding that of the Desert of Sahara and the Mediterranean Sea united.—(*Person. Narr.* vi. 71.)

time. Two entire orders (Cycloids and Ctenoids) have come into being, and have almost superseded two other orders (Ganoids and Placoids,) since the newest or latest of the secondary formations of the earth's crust. Species after species of land animals, order after order of air-breathing reptiles, have succeeded each other; creation ever compensating for extinction. The successive passing away of air-breathing species may have been as little due to exceptional violence and as much to natural law, as in case of marine plants and animals. It is true, indeed, that every part of the earth's surface has been submerged; but successively, and for long periods. Of the present dry land different natural continents have different faunæ and floræ; and the fossil remains of the plants and animals of these continents respectively show that they possessed the same peculiar characters, or characteristic *facies*, during periods extending far beyond the utmost limits of human history."

This quotation and the preceding introductory observations have been suggested by the significant fact that, in the wide world, it appears the *habitat* of the *Amphiumidæ* is no where found but in Louisiana and the Southern States, and the conterminous territories of the Mexican Republic.

The genus *Amphiumidæ*, of the family *Salamandridæ*, has two known species, namely, the *Amphiuma tridactylum* (Cuv.,) and the *A. means*, (Garden, *et al.*)

Of the anatomy of the *Amphiumidæ*, there is, I believe, much to learn. A specimen in spirits, kindly sent to me for examination, during the past year, was so much decomposed before I had an opportunity to dissect it, that it was thrown away as useless. Another specimen which I found in the environs of New Orleans, was in a like condition. Unable myself to make an original contribution concerning this animal, which, physiologically speaking, is perhaps the most interesting reptilian of Louisiana, I propose to reproduce from sources supposed to be accessible to few readers of this journal, the contributions of others, so far as is known to me, hoping that some of the friends of science will observe, describe, and publish experimental investigations illustrative of Southern Zoölogy. The South is a rich, if not a cultivated region, for original research in natural history and comparative anatomy and physiology. It is a very praiseworthy fact, that, of late the General Government, in its geographical explorations,

has sent naturalists over the continent, with nearly every expedition, in order to enlarge our knowledge of Natural History by making new researches.

Dr. Harlan says concerning the *Amphiuma means*, the two-toed species: "I am informed by Major Wace, that they are sometimes discovered two or three feet under mud of the consistence of mortar, in which they burrow like worms, as was instanced in digging a street in Pensacola, where great numbers were thrown up during the winter season. This animal is called in Florida the Congo snake by the negroes, who believe it to be poisonous, but without foundation."—(*Mus. Anim. Nat.*, ii. 135.)

Dr. A. Garden, (born 1728—died 1792,) a correspondent of the celebrated Linnæus, in a letter to the latter, dated at Charleston, S.C., in 1771, appears to have been the first to notice this remarkable reptile. This interesting discovery was made known to the great Swede towards the close of life, but too late to be inserted in the last edition of his celebrated *Systema Naturæ*, published not long before his death, which occurred Jan. 10, 1778.

It is asserted that the Amphiumidæ live for the most part in water for the purpose of obtaining food, but that they can live equally on dry land and in water. Analogy would indicate that of the two media, the open air and water, the latter element would be the least eligible, or rather altogether impossible in consequence of the absence of branchiæ in the adult state at least.

The late Dr. Harlan relates, that an *Amphiuma Tridactylum*, (Cuvier,) "in the possession of Dr. Mease, escaped from the vessel where it had been confined, and when found several days afterwards, was brisk and lively."—(*Nat. Cyc. Lond.* i. 646.)

*Dissection of a Batrachian Animal in a Living State:* By Richard Harlan, M. D., Professor of Comparative Anatomy to the Philadelphia Museum. Read February 5, 1823.

The specimen was sent from Georgia to Dr. Mease of this city. An account of a similar animal has lately been published under the name of "*Chrysodonta larvæformis*."\*

Having of late been familiar in the dissection of Proteiform animals, "les reptiles douteux" of Humboldt, and having had the opportunity of observing and dissecting this specimen in a living state, I ex-

\* Vide Medical Recorder, July, 1822, No. 19.

perience less hesitation in making the following observations, more especially as the account alluded to above, is by no means free from imperfection and error.

The animal I dissected was eighteen inches in length; the branchial-cartilages are four in number, united to each other at their inferior end; but unconnected with the other parts of the skeleton; the branchial orifice is situated between the two inferior, the other cartilaginous slips are covered by the internal lining membrane: these orifices cannot be considered as connected with the process of respiration, are by no means breathing holes, not being furnished membranous fringes, and would appear to subserve no other purpose than to evacuate the water taken into the mouth with the food of the animal.

The nostrils are small and situated near the point of the snout, they communicate with the fauces, opening immediately behind the palatine row of teeth.

The lower jaw contains a single row of teeth of about thirty in number; the upper jaw contains a row on the maxillaries, and another on the palatine surface, consisting of about forty in number; they point backwards, are very minute, the tips reflect the golden rays, provided they be viewed through the medium of a microscope, they are not processes of the jaws, but are attached to the bones at their bases by a slightly moveable articulation, somewhat similar to the teeth of the shark: that is to say, neither by gomphosis or anchylosis.

On the top of the head are the orifices of two rows of glands, extending from the eyes to the tip of the nose: the eyes are covered with cuticle as in the Siren and Proteus.

The tail is short, round at its base, and flattened vertically towards the extremity.

There are no ribs, except the motionless rudiments, resembling in this respect, the *Proteus anguinus*, and differing from the Siren and Tritons, which have moveable rudiments of ribs.

The *tongue* is cartilaginous, possessed of very little freedom of motion. In the appearance of the circulating system, the alimentary canal, the cellular lungs, and the urinary organs, this animal presents no material difference from the *Siren*.

The testicles are flat in this animal and cylindrical in the Siren. The parts about the region of the cloaca being somewhat mutilated, I was unable to determine exactly where the ureters entered the bladder.

The animal cannot be considered, strictly speaking, as amphibious (breathing in air or water) not being furnished with branchiæ, and is not calculated for progression upon land. Indeed the most remarkable peculiarity in its organization, is its four *boneless* legs terminated by two toes, the external toe being the longest.

In the *Dictionnaire Chronologique et Raisonné des Découvertes*, (17 vols. Paris, 1822,) Art. *Crocodile*, it is stated as a discovery of M. de Humboldt that this reptilian (the American Alligator) can live two days without breathing at all (*sans respirer tout*, iv. 266). This statement,



which I have never met with in the voluminous works of the illustrious author of *Cosmos*, would so far as mere appearances go, seem applicable to this animal during its winter hibernation. But during the hot season in Louisiana, I have reason to believe, after years of personal observation, that a privation of air for so long a time, cannot be endured without suffocation, notwithstanding the vast capacity of its lungs, by which, it can lay in a large supply of atmospheric air for temporary purposes during voluntary submersion. I have several times endeavored to confine alligators under water, in the summer, and although I have not fully succeeded, I have seen that such restriction from the atmospheric air, except for a short time, is not willingly submitted by the great saurians of Louisiana.

Although in a sudden change\* of weather they seek to guard against it by voluntary submersion for considerable periods, probably some hours, they doubtlessly lay in a good though temporary supply of air. I have been assured by a reliable physician, that during his hunting excursions, he has readily drowned alligators by forcibly holding them down under water by means of a stick or pole; but in this case their reservoir of air was probably discharged to a great extent from the lungs during their violent struggles to escape from confinement—an event, which in spontaneous submersion, would not happen.

Persons who accidentally fall into the water are often drowned, being frightened; they make violent efforts to escape from impending death, thereby exhausting the supply of air, which would, otherwise, in a calm and self-possessed state, buoy them up. Hence, Dr. Franklin directs, what a frightened person, however, never practises, that on falling to the water one should turn up his face horizontally, keeping his arms and the residue of his body wholly immersed, in order to prevent drowning. In this position, he can breathe and float in safety, the body being specifically lighter than the water, while respiration continues to inflate the lungs.

But throughout the whole hierarchy of organic Nature, there is not, perhaps, a single animal, aquatic or terrestrial, which can long survive when totally deprived of “the breath of life,” oxygen, all having respiratory tracheæ, branchiæ, stigmata, cilia, or other strictly homo-

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\*The alligators of Louisiana and the colossal turtles from the coasts of the Peninsula of Florida, are reliable thermometers for all degrees above the freezing point, at least in the shade, excepting in very rapid alternations of the atmospheric temperature, during which they neither reach the maximum nor minimum so quickly as the mercury.

logical mechanism for the performance of this fundamental function. Nevertheless, in the inactive hibernating state of animal life, there is either a complete temporary suspension or an inappreciably low respiratory condition, which seems to invalidate this law. But as life persists in the absence of its usual phenomena, unseen, may not an appreciable respiration also? May not the reservoir of air present, when the animal enters upon the long sleep of hibernation, virtually supply the place of the usual or more obvious respiratory act, probably, by means of aërial endosmosis? Perhaps the observation may apply to frozen fish which may be resuscitated. In the *American Journal of Science and Arts*, for May, 1852, numerous memoranda are given, showing that, with few exceptions, fish suddenly and completely frozen, will, after many days, return to life, on being thawed. They may be, in this state, carried about—to market, and be roughly handled, in the meantime. If frozen very slowly, they usually, if not always die. Fish thus frozen, lose their sight in all cases on being resuscitated; at least, such is the case according to the statements alluded to.

In his *Personal Narrative of Travels to the Equinoctial Regions of the New Continent*, (7 vols. Lond.) Humboldt says, "It is a very curious physiological phenomenon to observe the alligators of North America plunged into a *winter sleep* by an excess of cold, at the same period when the crocodiles of the Llanos of South America begin their *siesta* or *summer sleep*—habits that appear essentially linked with their organization." (iv. 501-2.)

Hibernation (a most significant subject and starting point for the physiologist, though almost entirely ignored in the text books of physiology) has always been defined according to its literal or etymological meaning as relating to the winter (*hybernus*) season, although the word is not found in Dunglison's dictionary at all. Hoblyn defines it as "a reptile state of the functions, which occurs in some animals *in winter*." Brande's Dictionary contains an article on this subject, the theory of which is altogether visionary; the theory is, that animals which hibernate, do so on account of the scarcity of food during a part of the year! This is certainly a wise, economical and easy method of escaping from impending starvation during a famine. But Swift says that "this stoical scheme of supplying our wants by lopping off our desires, is like cutting off our feet when we want shoes." The Amphiumidæ, the Crocodilidæ and other hibernat-

ing animals of Louisiana, fond of good living, must be demented to adopt this theory, for they can get more food in winter than in summer, as the rivers, lakes and lagoons, after the summer inundations, most abound in food, not to name migratory birds which come to winter in the luxuriant South.

The alligator is a great epicure as to its choice of food when it has an opportunity to select, (Cuvier to the contrary notwithstanding). It eats nothing, however, for six months of the cool season unless summer temperature should return for several successive days. When alligators pass from hibernation to active life before eating anything, they appear fully as fat and large as ever, although a contrary condition, that of weakness and extreme emaciation is constantly attributed to hibernating animals at the close of their siesta. I can affirm positively of a large alligator which I kept for vivisection for two years, that during the second winter, when it eat but a few times during a few warm days in mid-winter, it became much more rotund during hibernation, which I then attributed to a morbid or dropsical effusion, but which, in the spring, when the vivisection was made in the presence of many medical witnesses, proved to be large normal masses of fat, the animal being extremely vigorous and totally free from morbid appearances. That an enlargement of the body from a deposit of pure fat, should take place during hibernation with only one brief supply of raw beef, during six months, will be credited by no one but myself.

It is time, however, to return to the starting point. Adieu! for the present, to hibernation. In the sequel further observations will be given.

The parthenogenesis, embryology, and morphology of the young *Amphiumidæ* are probably altogether unknown. Much physiological interest attaches to these primordial conditions, and the more so, because in the adult orifices are found upon the sides of the neck, at the place where gills are developed in the water-breathing animals accustomed to perpetual submersion, as fishes. Are these apertures the remains of gill-orifices of early life? As these animals are not pennibranchitate, analogy shows that they are not really amphibiæ. The animals do not respire like fish, which respire the air dissolved or mixed with water.

Two fish, says Cuvier, put into a vessel filled with water and closed so that the atmosphere was not in contact with the same, died—the

one in 18 and the other in  $18\frac{1}{2}$  hours. A fish put into a vessel nearly filled with water, having a slight stratum of air resting on the surface, lived a little longer. On adding a quantity of oxygen to a similar quantity of air, a fish lived still longer than in the latter case. *D'Anat. compar.*, vii, p. 7.

Whatever may have been the case during the early settlements of North and South Carolina, at present this animal is certainly rare, as none of our museums contain a single specimen, nor was I aware that a specimen had ever been sent to Europe, until I was informed by Dr. De Kay, of New York, (after having finished this description) that a similar animal had been noticed by Dr. Garden, in "*Smith's correspondence of Linnæus*," under the name of "*Amphiuma means*," on referring to which work I found that this animal had indeed been noticed under that name.\*

It will be observed that the description of Dr. Garden agrees with mine, with the exception of a few minor differences as respects the tongue, the pulmonary system, etc. Dr. Garden did not seem to be aware that the *Amphiuma* respired with two cellular lungs; by his own account, the specimen he described had been *preserved in spirits*, which circumstance will sometimes give rise to inaccuracies.

From the above description the "*Amphiuma*" must be acknowledged as generically distinct from the Batrachian animals hitherto described; the similarity of internal organization would place it between the *Proteus* and *Siren*.

This very curious animal lived for several weeks in the possession of Dr. Mease, by whose request a drawing of the living animal was taken by Mr. C. A. Lesueur. To the former gentleman, who has shown

\* Extract from a letter of Dr. Alexander Garden, to Mr. Ellis, dated Charleston, May 15th, 1773.

"I have not as yet been able to procure another of the *Amphiuma means*, which he (Linnæus) calls *Sireni similis*. This appeared to me to be a still more singular animal than the *Siren*, as you might observe by my remarks, etc., etc."—Vol. I. p. 599.

In a letter to Linnæus, with which he sends a specimen, Dr. Garden gives the following descriptions of the *Amphiuma*:

"I must now say something of an unknown animal, which you will find in a glass bottle, and which I have no doubt will afford you much satisfaction; the specimen here sent is the only one I ever saw, and I shall think myself fortunate if it reaches you in safety.

"When I first received it the length was 37 inches, though the animal was then become somewhat contracted. At first sight I suspected it to be another species of *Siren*, but upon nearer examination I found so many differences, that there proves to be no relationship whatever between them. Can this animal form a link between the Lacerte and Serpentes? is it allied to *Anguis quadrupes*?"

"It differs in many particulars from the *Siren*, most evidently in the following. This animal has four feet, with two toes to each, without claws. The *Siren* had only two feet. It wants the gills and their wing-like coverings. It has no scales, nor, which seems to me very singular, any tongue! all which are found in the *Siren*. I have opened the throat, and satisfied myself respecting the presence or absence of the gills. The following are the characters I have drawn up of this ugly animal:

"*Head*, rather long, depressed, tapering, serpent-like.

"*Mouth*, extending half the length of the head.

"*Lower jaw*, furnished with a single row of sharp, distinct teeth.

"*Upper*, with four rows with similar curved teeth.

"*Upper lip*, covering the under one.

"*Tongue*, none. *Nostrils*, two openings at the very extremity of the upper lip.

"*Eyes*, dull, at the upper part of the head, on each side, covered with a thick tunic.

"A thin retractile membrane covers each cartilaginous lateral spiracle or orifice, by which the animal breathes.

"*Body*, thick, nearly cylindrical, tapering and keeled at each side, beyond the vent. Tail recurved. There is no lateral line. *Vent*, a large opening immediately behind the hinder legs.

"*Feet*, four, two of them before, close to the spiracles, each with two toes, destitute of claws, two behind, at the bottom of the belly, with similar toes.

"Inhabits deep ditches, and lakes of fresh water."—Vol. I. p. 333.

himself on many occasions active in the cause of science, I am indebted for the opportunity of dissection.

This specimen is deposited in the Philadelphia Museum, under the name of *Amphiuma means*. (*Garden.*) *Jour. Acad. Nat. Sci.*, iii. 54 et seq. Phila. 1823.

(AMPHIUMA. Garden. Linn. Harlan.)

*Character of the Genus.*—Teeth in both jaws: legs four, slender and jointless: toes before and behind jointless, clawless.

The genus consists of a single species, the

AMPHIUMA *means*, Garden.

SYNONYMA. *Amphiuma means*, Garden. Smith's correspondence of Linnaeus.

*Amphiuma means*, Harlan. Journal of Philad. A. N. Sc. Vol. 3—and Annals of the N. Y. Lyceum of Nat. Hist. Vol. 1. p. 269. pl. XXII.

*Sireni simili*, Linn. Smith's correspondence of Linnaeus.

*Chrysodonta larvæformis*, Mitchell, Med. Recorder, No. 19. Vulgo, Congo snake.

*Char.*—*Color* dark-brown, or slaty, with a bluish tinge on the sides, and the belly rather lighter: *head* long, tapering, depressed: *mouth* extending half the length of the jaws: *teeth*, two rows above, and one below, with their points somewhat flattened and reflected: *legs* small, distant, with rudiments only of bones concealed in the flesh, and with two clawless toes, the external the longest.

*Habit.*—Burrowing in the mud, in swamps, or in the vicinity of streams, where it searches its food, and hibernates; occasionally visiting the dry land.

Growing to the length of three feet.

Inhabits the Southern Atlantic States, from South Carolina to Mexico. Specimens are common of late, in most of our museums. The species was for a long time neglected or unknown.—*Jour. Acad. Nat. Sci.* vol. v. 318. Phila. 1827.

*Note on the AMPHIUMA MEANS, described in Vol. III. of this Journal.* By R. HARLAN, M. D.

Since writing a note on this animal, published in the *New York Lyceum of Natural History* subsequently to the account in the *Journal of the Academy*, some additional information has been obtained.

In a Memoir read before the French Academy of Sciences, November, 1826, by Baron Cuvier, another species is described, the *A. tridactylum*, accompanied also with a very detailed account of the anatomy of these reptiles. In addition to the anatomical particulars previously known, Cuvier states that there exist rudiments of false ribs at the extremities of the transverse processes of the few upper vertebrae; and also that vestiges of bones in their rudimental legs were noticed. The author attributes the same habitat to both species; and thinks it probable that during the earlier periods of existence the *Amphiuma* is furnished with branchiae.

Within a very few days past I have received thirty-four specimens

of the *A. means*, through the politeness of Mr. F. Nesbit, of Alabama. They are of various sizes; one two feet ten and a half inches in length, and measuring six inches round the head. Some are very small. The rudimental legs do not increase in size in proportion to the size of the animal, but are proportionally smaller in the largest specimens. On dissection the stomachs of several were found filled with small fish, beetles, salamandræ, etc.

Mr. Nesbit informs me that the whole number were caught during the present year, in the neighborhood of Pensacola; all of them being of the same species, would appear to indicate that the habitat of the two species is not the same. Since the appearance of Cuvier's essay I have met with several specimens of his new species, always from the Mississippi and Arkansas rivers, or some of their tributaries.

In several individuals one limb was wanting, without doubt a congenital deformity.

The great number of these reptiles which have passed under my observation, varying in size from 3 inches to nearly 3 feet, induces me to believe that they are never furnished with branchiæ: the perfectly developed state of the lungs, is another argument in favor of this position.—*Jour. Acad. Nat. Sci. Phila.* vol. iv. p. 147. Philad. 1829.

The fundamental idea of a true amphibian predicated on, and derived from, physiological anatomy (more correctly termed physiology and teleological anatomy,) comprehends branchial and pulmonary organs; the one for aquatic, the other for atmospheric respiration. The *Amphiumideæ* viewed from a homological stand point present neither the affinities nor homologies of true amphibians. Hence the experimental evidence to entitle these animals to this rank should be absolutely demonstrative.

I have occasionally noticed that the decapitated alligator, breathes, but I found that in this headless condition it was speedily drowned by submersion. This animal is in no sense an amphibian. It has capacious lungs which it can expand to an enormous extent, especially when angry. It then swells up and blows like a bellows. Even during hibernation, while frozen in under a thick stratum of ice, it places an inch or less of the tip of its muzzle or nares so as to be completely out of, or above the ice, in advance of the congelation, as I have repeatedly observed. In this condition, as in a large tub or trough, there is neither perceptible breathing nor other movements of the body for weeks. No agitation of the water can be discerned below this transparent ice, as would necessarily occur in either inspiration or expiration. This is a delicate test, showing the perfect inertia of the body and of its respiratory organs. Nevertheless, as there is

an open avenue always preserved between the lungs and the atmosphere, even during hibernation, there must be physiological purpose in this arrangement. Is oxygen absorbed? Or does the atmospheric air enter, on mere physical or mechanical principles, so as to mix with or dilute any carbonic acid gas that may generate or accumulate in the lungs in the apparently unbreathing, torpid state? It is really a land animal, which inhabits the water either for its convenience in obtaining food, or for the regulating or equalizing its temperature. It is more readily and completely killed by ligating the trachea than by decapitation. After the most complete apparent death from the tying of the trachea, it may be so far resuscitated by pulmonary insufflation as to regain sensibility and voluntary motion even after decapitation.

To the student of Natural History and comparative physiology no apology is necessary for briefly alluding in this place to some of the luminous routes now pursued by profound naturalists and physiologists, which have led to marvellous successes, theoretically, practically, synthetically, and analytically, in organic science, and which have revealed the conditions of life, if not its essence or essential nature. These are what the poet Goëthe called "sacred riddles," and constitute the fundamental idea of unity of plan in organic composition, which he announced—which Cuvier and St. Hilaire subsequently worked out with unparalleled ability. The golden thread of teleology runs through Cuvier's *Règne Animal, anatomie comparée*, and his palæontological researches, (*Ossemens Fossiles*) which he arranged according to organization and its purpose or final cause. When Goëthe, then in his eightieth year, the mighty stream of his intellect flowing strong and clear as ever, heard of the successful workings of his morphological doctrine among the *savants* of the Academy, his almost childish joy was unbounded. Of "this synthetic manner of looking into Nature," he says, "it cannot be kept back any longer. From the present time Mind will rule over Matter in the scientific investigations of the French. There will be glances of the great maxims of creation—of the mysterious workship of God! Besides what is all intercourse with Nature, if we merely occupy ourselves with individual material parts, and do not feel the breath of the spirit which prescribes to every part its direction, and orders or sanctions every deviation by means of inherent law? I have exerted myself in this great question for fifty years. At first I was alone, then I found sup-

port, and now at last, to my great joy, I am surpassed by congenial minds." (Life and works of Goëthe, by Lewes, ii. 443.)

In order to illustrate existing views in regard to these topics, a few paragraphs from the Address of Professor Owen (President of the British Association), delivered at the meeting at Leeds, Sept. 22, 1858, will be subjoined as follows:—(*From Athen. Am.; Jour. Sci. and Arts.*)

*Homology.*—After pointing out the distinction between Affinity, which indicates an intimate resemblance, and Analogy, which indicates a remoter one, Prof. Owen continued—The study of homologous parts of a single system of organs—the bones—has mainly led to the recognition of the plan of archetype of the highest primary group of animals, the Vertebrata. The next step of importance will be to determine the homologous parts of the nervous system, of the muscular system, of the respiratory and vascular system, and of the digestive, secretory and generative organs in the same primary group or province. I think it of more importance to settle the homologies of the parts of a group of animals constructed on the some general plan, than to speculate on such relations of parts of animals constructed on demonstratively distinct plans of organization. What has been effected and recommended, in regard to homologous parts in the vertebrata, should be followed out in the articulata and mollusca. In regard to the constituents of the crust or outer skeleton and its appendages in the articulata, homological relations have been studied and determined to a praiseworthy extent, throughout that province. The same study is making progress in the mollusca; but the grounds for determining special homologies are less sure in this sub-kingdom. The present state of homology in regard to the articulata, has sufficed to demonstrate that the segment of the crust is not a hollow expanded homologue of the segment of the endo-skeleton of a vertebrate. There is as little homology between the parts and appendages of the segments of the vertebrate and articulate skeletons respectively. The parts called mandibles, maxillæ, arms, legs, wings, fins, in insects and crustaceans, are only "analogous" to the parts so called in vertebrates. A most extensive field of reform is becoming open to the homologist in that which is essential to the exactitude of his science—a nomenclature equivalent to express his conviction of the different relations of similitude. Most difficult and recondite are the questions in face of which the march of homology is now irresistibly conducting the philosophic observer. Such, for instance, as the following: Are the nervous, muscular, digestive, circulating and generative systems of organs more than functionally similar in any two primary provinces of the animal kingdom? Are the homologies of entire systems to be judged of by their functional and structural connexions, rather than by the plan and course of their formation in the embryo? It may be doubted if embryology alone is decisive of the question whether homology can be predicated of the alimentary canal in animals of different primary groups or provinces. It is significant, however, of



the lower value of embryological characters, to note that the great leading divisions of the animal kingdom, based by Cuiver on Comparative Anatomy, have merely been confirmed by Von Baer's later developmental researches. \* \* \*

*Parthenogenesis.*—John Hunter first enunciated the general proposition that “the propagation of plants depended on two principles, the one that every part of a vegetable is ‘a whole,’ so that it is capable of being multiplied as far as it can be divided into distinct parts; the other, that certain of those parts become reproductive organs, and produce fertile seeds.” Hunter also remarked, that “the first principle operated in many animals which propagate their species by buds or cuttings;” but that, whilst in animals, it prevailed only in “the more imperfect orders,” it operated in vegetables “of every degree of perfection.” The experiments of Trembley on the fresh water polyp, those of Spalanzani on the Naïds, and those of Bonnet on the Aphides, had brought to light the phenomena of propagation by fission, and by gemmation or buds, external and internal, in animals, to which Hunter refers. Subsequent research has shown the unexpected extent to which Hunter's first principle of propagation in organic being prevails in the animal division. But the earliest formal supercession of Harvey's axiom, “*omne vivum ab ovo*,” appears to be Hunter's proposition of the dual principle above quoted. The experiments of Redi, Malpighi and others had progressively contracted the field to which the “*generatio aquivoca*” could with any plausibility be applied. The stronghold of the remaining advocates of that old Egyptian doctrine was the fact of the development of parasitic animals in the flesh, brain and glands of higher animals. But the hypothesis never obtained currency in this country; it was publicly opposed in my “Hunterian Lectures,” by the fact of the prodigious preparation of fertile eggs in many of the supposed spontaneously developed species; and in then suggesting that the *Trichina spiralis* of the human muscular tissue might be the embryo of a larger worm in course of migration, I urged that a particular investigation was needed for each particular species.

Among the most brilliant of recent acquisitions to this part of Physiology, have been the discoveries which have resulted from such special investigations. Kuchenmeister and Von Siebold have been the chief laborers in this field. After noticing some of the results of those labors, Prof. Owen said: Since the time when it was first discovered that plants and animals could propagate in two ways, and that the individual developed from the bud might produce a seed or egg, from which also an individual might spring capable of again budding—since this alternating mode of generation was observed, as by Chamisso and Sars, in cases where the budding individual differed much in form from the egg-laying one—the subject has been systematized and generalized, with an attempt to explain its principle and greatly advance, especially and in a highly interesting manner, in Von Siebold's late treatise, entitled “*Wahre Partheogenesis bei Schmetterlingen und Bienen*,” in which the virgin production of the male or drone-bee is demonstrated. Von Siebold, having subjected to the closest microscopic scrutiny and experiment the conclusion to

which the practical Bee-master Dzierzon had arrived, relative to the cause of queen-bees with crippled wings producing a swarm exclusively of drones, has demonstrated that the male bee is produced from an egg which has been subjected to no influence save that of the maternal parent; whilst such egg, if impregnated, would have produced a female or working bee. The now well-investigated phenomena of parthenogenesis in Hydrozoa have resulted in showing, as in the analogous case of Entozoa, that animals differing so much in form as to have constituted two distinct orders or classes, are really but two terms of a cycle of metagenetic transformations—the acalephan Medusa being the sexual locomotive form of the agamic rooted budding polyp, just as the cestoid tænia is of the cystic hydatid. In Hydrozoa (hydroid polyps or sertularians) the young are propagated, as in plants, by “buds,” and also, as in most plants, by “germs” or “seeds:” these latter are contained in “germ-sacs” projecting from the outer surface, which is another analogy to the flowering parts of plants. The first acquaintance with these marvels excited the hope that we were about to penetrate the mystery of the origin of different species of animals; but as far as observation has yet extended, the cycle of changes is definitely closed. And, since one essential step in the series is the fertilized seed or egg, the Harveian axiom, “*omne vivum ab ovo*,” if metagenetic phases be ascribed to one individual, may be still predicated of all organisms which bear the unmistakable characters of plants or of animals. The closest observations of the subjects of these two kingdoms most favorable to clear insight into the nature of their beginning, accumulate evidence in proof of the essential first step being due to the protoplasmic matter of a germ-cell and sperm-cell; the former preëxisting in the form of a nucleus or protoplast, the latter as a granulose fluid. In flowering plants it is conveyed by the pollen-tube, in animals and many flowerless plants, by locomotive spermatozoids. The changes of form which the representative of a species undergoes in successive agamically propagating individuals are termed the “metagenesis” of such species. The changes of form which the representative of a species undergoes in a single individual, is called the “metamorphosis.” But this term has practically been restricted to the instances in which the individual, during certain phases of the change, is free and active, as in the grub of the chaffer, or the tadpole of the frog, for example. In reference to some supposed essential differences in the metamorphoses of insects, it had been suggested that stages answering to those represented by the apodal and acephalous maggot of the Diptera, by the hexapod larva of the Carabi, and by the hexapod antenniferous larva Meloe, were really passed through by the orthopterous insect, before it quitted the egg. Mr. Andrew Murray has recently made known some facts in confirmation of this view. He had received a wooden idol from Africa, behind the ears of which a Blatta had fixed its egg-cases, after which the whole figure had been rudely painted by the natives, and these egg-cases were covered by the paint. No insect could have emerged without breaking through the case and the paint; but both were uninjured. In the egg-cases were discovered, 1st, a grub-like larva in the egg; 2d, a cocoon in the egg containing the

unwinged, imperfectly developed insect; 3d, the unwinged, imperfectly developed insect in the egg, free from the cocoon, and ready to emerge.

From this splendid tableau of organic nature, turn once more patient reader, to Dr. Garden's "ugly animal," which has served as the point of departure for this desultory paper.

In Cuvier's classification, the *Amphiuma* is placed in the fourth order of the Reptilia, among the *Batrachians*, and which is by him described as follows: "Has an orifice on each side of the neck; body extremely elongated; limbs and feet but little developed; the palatal teeth form two ranges. (From North America.)"

Several of the Salamandridæ, with which the Amphiumidæ are most closely allied, are endowed with an extraordinary tenacity of life, and are, therefore, well adapted for physiological experimentation and vivisection. They possess also reparative powers after mutilation of a most surprising character. Experimenters have ascertained that the *Triton* reproduces as many as four crops of legs and tails, after as many amputations, including bones, nerves, blood-vessels, etc.

Dr. Felix Raubaud, in his scientific review for the week ending June 26th, 1851, reported (*L'Illustration, Journal Universel,*) that M. Duméril, had preserved a decapitated salamandre, alive and active, the wound of the trachea and gullet having healed; and Dufay had preserved a terrestrial salamandre under water in a healthy state.

#### CROCODILIAN HIBERNATION. INSTINCT.

1851. December 5th; sunrise; air 40°; clear; high wind, N. W.; river 49½°. Zip Coon (for such was the name of this fine specimen of the *Crocodylus Mississippiensis*) has during the late cool weather been coiled up, motionless, in a large tub of water, the tip of his muzzle resting on the margin of the vessel, the residue of the body being submerged several inches beneath the surface; sleeps profoundly, but when goaded or otherwise disturbed, opens his mouth and eyes a little, but with difficulty, and hisses feebly; pupils contracted; falls back gradually into a deep sleep, the pupils contracting into a mere straight and almost vertical line. Water 40°. During the next thirteen days, the cold increased to an extraordinary degree in this city. The following is the temperature for this period, with two omissions, at sunrise: 50°, 54°, 55°, 62°, 62°, 60°, 51°, 47½°, 33°, 33¼°, 28°. This latter temperature occurred on the 18th of Decem-

ber; the air, however, on the east side of the house, in the lot where Zip's den was, fell to  $26^{\circ}$ , being in an exposed situation.

Dec. 18th. Zip's muzzle rests on the margin of the tub exactly as at the last report; is frozen in; ice nearly half an inch thick, apparently adherent to the muzzle in places; a hole was made in the ice and the water found to be  $33^{\circ}$ . On pinching the locked up muzzle, which was not frozen, however, a slightly perceptible motion took place in the body. On the following day the ice over Zip was  $1\frac{1}{4}$  inches thick, and the water was  $32\frac{1}{3}^{\circ}$ ;\* air at sunrise,  $29\frac{1}{2}^{\circ}$  at the house; in exposed places near the den,  $27\frac{1}{2}^{\circ}$ . During the day, as the weather was becoming milder, the ice was broken; Zip was pinched; he raised his head, but could neither hiss nor open his eyes.

During all this time while shut in by the ice, he was repeatedly noticed, except at night. No perceptible respiratory motions could be detected.

Dec. 20; air, sunrise,  $37^{\circ}$ —21st, sunrise,  $61^{\circ}$ —22d, sunrise,  $61^{\circ}$ . On the 20th, after a sleep of twenty-four days, he slowly crawled out of the water, apparently very fat and full, but occasionally reëntered the water as if too cool himself, the temperature having suddenly risen in a few hours, from  $37^{\circ}$  to  $61^{\circ}$ .

The last week of the previous month, November, two days excepted, had been very cold; on the 26th, the air at sunrise was  $38^{\circ}$ , at which time I found Zip immersed in the tub, as above described; eyes closed and scarcely able to open his enormous jaws or to hiss.

1852. Early in January, the weather becoming cold, Zip reëntered the water exactly as he had done in November, and was again torpid and ice-bound excepting the nostrils.

Jan. 13th; air, at sunrise,  $28\frac{1}{2}^{\circ}$ . Great was the astonishment of Orleanians on awaking. The snow two inches deep, dry, and drifting and forming, in some places, extensive and deep embanked masses. The snow fell upon Zip's upturned nostrils, and being somewhat protected from the wind, remained unmelted, and unmoved by respiration, if breathing took place at all.

Jan. 14th; air, sunrise,  $27^{\circ}$ ; den,  $25^{\circ}$ ; Jan. 18th; air, sunrise,  $60^{\circ}$ ; Zip out and active; but the next day went to sleep as before, the air at sunrise,  $33^{\circ}$ . On the 20th, the air at sunrise was  $18^{\circ}$ , and in

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\* Some years previously, I lost two alligators in one night for want of water or other medium necessary for protection against sudden cold. They do not perish in water, however cold, short of congelation.

exposed places 100 feet from houses,  $14^{\circ}$ , (an unparalleled cold in New Orleans) but rose next day to  $39^{\circ}$ . Zip was again sealed up in thick ice, which adhered to his muzzle.

Jan. 25th; air, at sunrise,  $45\frac{1}{2}^{\circ}$ ; ice in the tub somewhat melted; Zip straightened himself, and got partly out of the water; the next day became warmer; he opened his eyes, and hissed when offended.

1852. March 6th. After several weeks of warm weather, Zip being lively, I gave him a small piece of pork, which, after many attempts, he at length swallowed, but for several days after eat freely, and then no more until the following month. He came out of two hibernating periods very active and excessively fat as the appearance, and really was so as the vivisection proved, on the 13th of May, 1852.

\* Messieurs psychologists and physiologists, will you explain the following facts either by the doctrine of chance, or the theory of animal prevision? Zip was captured in New Orleans and carted to my office, early in the summer of 1850. In the fall I dug a slight trench near the wall of the house, and covered it with a plank which leaned against the wall at an angle of forty-five degrees, thinking he would seek this, though a poor shelter, during hibernation, which he did, although the large tub of water above-mentioned was then, as well as during the following year, standing in his den. The alligator is obliged to seek a protection from cold in advance, for by delaying until the cold weather overtakes it, it would be unable to move an inch.

The winter of 1850-1, was very mild, and the trench answered every purpose. But during 1851-2, the temperature of this trench fell several times from  $6^{\circ}$  to  $16^{\circ}$  below the freezing point. Had Zip wintered in it, he would have inevitably perished. It was reasonable to suppose that having been habituated to this hibernating place during the previous winter, he would reënter it. But, on the contrary, as soon as the weather became cool, in advance of frost and severe weather, he coils himself carefully in the tub under the water; he is shut in by thick ice, in December; the cold relaxes; he crawls out; January sets in cold; he returns to his tub; the ice thickens over him, the air being  $18^{\circ}$  below the freezing point!

I will neither affirm nor deny that this is an example of prescience, but certain it is, that no meteorologist, can lay claim to such foreknowledge. The word *Instinct*, is vainly resorted to in order to explain a *thing or action*. A bookful of definitions might be given, in which all knowledge, or purpose is denied to animals because many of their

actions imply a more exact knowledge and a more speedy arrival at a perfect end than that of man, who is doomed to long years of laborious experience in order to gain knowledge and form judgments essential to his well-being and high destiny. Knowledge is not the less real because it is a congenital, primordial endowment, or instantaneous revelation which arrives intuitively, at once at the realization of its finality or end, without a long course of experience and dubious reasonings. Animals are comparatively non-progressive, being speechless, bookless, and on the whole, vastly inferior to man, the supreme head of Organic Nature.

The squirrel surpasses man in judging whether hickory nuts are good or bad. In the autumn, before the nuts are ripe enough to fall spontaneously, the squirrel begins to pull them for food, examining them by cutting into the outer envelope, perhaps, by percussion, without penetrating to the kernel; if bad, he drops them, (and sometimes by accident, a good one). Let the human nut-gatherer crack the rejected nuts, as is very common, and he will almost invariably find, to his disappointment, the kernel defective or blighted. Tried. When the various nut crops (mast) fail east, or west of the Ohio river, these animals, in immense numbers, migrate, and swimming the river, go to the land of plenty, upon surer information than the telegraphic dispatches give concerning the sugar or cotton crop of Louisiana.

If an animal unerringly adapt means to ends, why should this process be called blind, unintelligent, somnambulistic, while, in man, who often fails in accomplishing the same result, this process is called intelligential? Because it is in the latter acquired slowly and imperfectly by experience? Instinct as a primordial endowment is not for that reason necessarily the antithesis of knowledge.

Do not the poets rival the physiologists in expounding the philosophy of Instinct? To answer this query an example of the best from each class, will be submitted "to a candid world."

Cuvier, an illustrious expounder of the Animal Kingdom, speaks first:

"The most perfect animals are infinitely below man in their intellectual faculties; but, it is, nevertheless certain that their *intelligence* performs operations of the *same kind*. They move in consequence of sensations received, are susceptible of durable affections, and *acquire by experience a certain knowledge of things*, by which they are *governed independently* of actual pain and pleasure, and by the simple *foresight of conse-*

quences. When domesticated they feel their subordination, and *know* that the being who punishes them may refrain from doing so if he will, and when *sensible* of having done *wrong*, or behold him angry, they assume a suppliant air. In the society of man they become either corrupted or improved, and are susceptible of *emulation and jealousy*: they have among themselves a *natural language* which, it is true, expresses only their momentary sensations; but man teaches them to understand another, much more complicated, by which he makes known to them his will, and causes them to execute it. In short, we perceive in the higher animals a certain degree of *reason*, with all its consequences, good and bad.

“In a great number of animals, however, there exists a different faculty of *intelligence*, which is named *instinct*. This prompts them to certain actions necessary to the preservation of the species, but often altogether foreign to the apparent wants of individuals; frequently, also, very complicated, and which, to be ascribed to intelligence, would suppose a foresight and knowledge in the species that execute them infinitely superior to what can be admitted. These actions, the result of *instinct*, are not the effect of imitation, for the individuals that perform them have never seen them performed by others: they are not proportioned to the ordinary intelligence, but become more singular, more wise, more disinterested in proportion as the animals belong to less elevated classes, and are, in all the rest of their actions, more dull and stupid. In order to have a clear idea of instinct, it is necessary to admit that these animals have innate and perpetual images or sensations in the sensorium, which induce them to act as ordinary and accidental sensations commonly do. It is a sort of dream or vision that ever haunts them, and may be considered, in all that relates to instinct, as a kind of somnambulism.”—(*Règne Animal*.)

See how the French physiologist, in the end, snuffs out almost all of his own lights, amid “innate images,” “dreams,” “visions,” “sombambulism!” He leaves us in

——“The sphere of dreams,  
The bounds where true and false are passed.  
Then lead us on, thou wandering Glean!  
Lead us onward far and fast!”

“This high argument,” Pope clothes in beautiful versification, bringing in the following verdict; which, whether right or wrong, is, at least, expressed with a clearness, force, and brevity, rarely met with

in prose; nevertheless, the fundamental antithesis between reason and instinct and the identification of the latter with the will of the Deity, are scarcely tenable:

“——Reason

Stays till we call, and then not often near;  
 But honest instinct comes a volunteer,  
 Sure never to o’ershoot, but just to hit;  
 While still too wide or short is human wit;  
 Sure to quick nature happiness to gain,  
 Which heavier reason labors at in vain.  
 This too serves always, reason never long:  
 One must go right, the other may go wrong.  
 See then the acting and comparing powers,  
 One in their nature, which are two in ours!  
 And reason raise o’er instinct as you can,  
 In this ’tis God directs, in that ’tis man.  
 Who taught the nations of the field and wood  
 To shun their poison, and to choose their food  
 Prescient? \* \* \*

Who made the spider parallels design,  
 Sure as De Moivre, without rule or line?  
 Who hid the stork, Columbus-like, explore  
 Heavens not his own, and world’s unknown before;  
 Who calls the council, states the certain day;  
 Who forms the phalanx, and who points the way?  
 Whether with reason or with instinct blessed;  
 Know, all enjoy that power which suits them best,  
 To hllis alike by that direction tend,  
 And find the means proportioned to the end.  
 Say, where full instinct is the unerring guide,  
 What Pope or council can they need beside?”

Is an animal which possesses the inborn or acquired power of adapting certain means to the necessary ends of its well-being—means and ends which compared with those which concern man, are few in number, limited in their range, and attainable without the slow processes of speculative or abstract reasoning—is an animal, because it is thus highly endowed, necessarily destitute of all knowledge or purpose? Must the very perfection with which they perform *intelligential* actions, in which they surpass man, be adduced as proof that they know nothing more than mere machines? The attempt to degrade animals to mere *automata*, which act always in one way, which they cannot deviate from, is not only indefensible in itself, but fatal to the science of experimental physiology which has been built up, not by the vivisection of mechanical or automatic animals, or clocks, destitute of sensation, purpose, contrivance, and voluntary motion, but by the vivisection of creatures which see, hear, taste, smell, and feel and act



no more like *automata* than man himself. Is comparative physiology, based on automatic "reflex action, definite as the solar ray's reflection and physical in its nature?"

The following statement which critics admit to be authentic, quoted by Dugald Stewart in the first volume of his work on the *Elements of the Philosophy of the Mind*, taken from M. Bailly's *Lettres sur les Animaux*, is submitted for the consideration of those who advocate the theory of automatic action in animals: "A friend of mine," says M. Bailly, "a man of understanding and strict veracity, related to me these two facts of which he was an eye-witness. He had a very intelligent ape, with whom he amused himself by giving walnuts, of which the animal was extremely fond. One day he placed them at such a distance from the ape, that the animal, restrained by his chain, could not reach them; after many useless efforts to indulge himself in his favorite delicacy, he happened to see a servant pass by him with a napkin under his arm; he immediately seized hold of it, whisked it out beyond his arm, to beat the nuts within his reach, and so obtained possession of them. His mode of breaking the walnut was a fresh proof of his inventive powers; he placed the walnut upon the ground, let a great stone fall upon it and so got at its contents. One day the ground on which he had placed the walnut was so much softer than usual, that instead of breaking the walnut, the ape only drove it into the earth: what was the animal to do? He takes up a tile, places the walnut upon it, and then lets the stone fall, while the walnut is in this position."

I may be allowed to say in conclusion, that Natural History besides the positive knowledge it conveys, is very useful to the diagnostician in disease, accustoming his mind to make comparisons, to detect likenesses, to notice analogies, and identities, and to achieve general and special classifications, in vital and pathological phenomenology.

The great advances in human physiology by means of experiments on the inferior animals, are, it is hoped, preludes to similar advances in pathology and therapeutics which may in like manner be more fully illustrated from the same source, that is, from artificially produced morbid conditions and from the therapeutic results of medicinal agents in the animals which approximate man most closely. Experimental pathology and therapeutics and experimental physiology, should proceed hand in hand.

In the study of Natural History and comparative anatomy, will it be seen that, superiority of organization, is, on the one hand, only

achieved by a corresponding organic complexity, while, on the other hand, the intensification of differentiations among the special organs of the individual, combines the whole into unity so as to produce the maximum of well-being, that is, man, the head of the Organic Kingdom. Great is the hiatus between his rich and varied vital capital and that of the simple primordial cell! Man is at once the complex of almost all vital relations and the nobilitation of the whole organic series from vegetable life to the summit of the animal kingdom, which, he best illustrates, and by which he himself is best illustrated.

“The *more imperfect* a being is,” says Geoffroy St. Hilaire, “the more do its individual parts *resemble each other*, and the more do these parts *resemble the whole*. The *more perfect* the being is, the more *dissimilar are its parts*. In the former case the parts are more or less a repetition of the whole. The more the parts resemble each other, the less *subordination* of one to the other. Subordination of parts indicates high grade of organization.” A quotation from an elegant writer, Mr. Lewes, will illustrate these postulates in Organic Nature: “Take a polyp and cut it into several pieces; each piece will live and manifest all those phenomena of nutrition and sensibility which the whole polyp manifested. Turn it inside out like a glove, the internal part becomes its skin, the external part becomes its stomach. The reason is, that it is the simple structure of the polyp, the parts resemble each other and resemble the whole. There is no individualized organ, or apparatus of organs, performing one function, such as nutrition, and nothing else. Every function is performed by every part; just as in savage societies, every man is his own tailor, his own armorer, his own cook, and his own policeman. But take an animal higher in the scale, and there you find the structure composed of dissimilar parts, and each part having a different office. That animal cannot be hewn in pieces and each piece continue to live as before. That animal cannot have his skin suddenly turned into a stomach. That animal, in the social body, cannot make his own clothes or his own musket; the division of labor which has accompanied his higher condition has robbed him of his universal dexterity.”

The same writer thus expresses his views of animal and vegetable Morphology which, however, contain some disputable topics not yet fully accepted: “Place a flower in the hands of the cleverest man of your acquaintance, providing always he has not read modern works of science, and assure him that leaf, calyx, corolla, bud, pistil, and stamen, differing

as they do in color and in form, are nevertheless all modified leaves; assure him that flower and fruit are but modifications of one typical form, which is the leaf; and if he has any confidence in your knowledge he may accept the statement, but assuredly it will seem to him a most incomprehensible paradox. Place him before a human skeleton, and calling his attention to its manifold forms, assure him that every single bone is either a part of a vertebra, or the appendage to a vertebra, and that the skull is a congeries of four vertebræ under various modifications; he will, as before, accept your statement, perhaps; but he will, as before, think it one of the refinements of transcendental speculation to be arrived at only by philosophers. Yet both of these astounding propositions are first principles in Morphology." The fundamental doctrine of cell-anatomy and physiology, which is not included in this exposition, claims a much greater universality and has a far greater significancy for the biologist, than the morphological theory, to which it is, if not a complimentary, neither is it a contradictory but another method of viewing Organic Nature. The cell-type is paramount, supreme, and universal, whilst the leaf-type and the vertebral type are limited, and, in some plants and animals wholly absent.

Neither the mere surgical hewers of human flesh nor the fossilized routinists in medical practice, can fulfill the high mission which the existing state of science and the claims of humanity require. Embryology, development, metamorphosis, comparative anatomy, experimental physiology and experimental pathology, are, in a good degree, new fields of research, and have already furnished abundant specimens of pure ore, indicative of the mines and the methods from which medicine may be enriched, and parted from the base metals and dross of gross empiricism.

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ART. V.—*Discussion on the Radical Cure of Inguinal Hernia.*—(Abstract from the Minutes of the Physico-Medical Society of New Orleans, Nov., 1858.)

DR. STONE stated that, when in London, he made inquiry as to the result of Wützer's operation for the radical cure of inguinal hernia. This operation, you know, is a modification of Gerdy's, which was

made by inserting the integument of the scrotum and plugging up the inguinal canal and confining it there by a ligature. According to a report of Rothmund of Munich, this operation occasionally proved fatal, and this improvement has been substituted. The skin is inverted in the same manner but it is held there by a plug or cylinder, and instead of the suture of Gerdy, a needle is passed through the cylinder and out near the end through the soft parts in front. Over this is placed a concave cover made of hard wood, and rather wider than the cylinder, with an opening to admit the point of the needle, and a screw at the other end by which the cylinder and cover can be screwed together. In this manner the plug is retained in the canal and more or less inflammation is produced by the needle, and perhaps by the pressure exercised on the parts between the cylinder and cover. But these operations have been fully described in the journals. Dr. Stone was told that the plug of skin soon came down after the instrument was removed, and that the cure was only temporary.

Mr. Lawrence of St. Bartholomew, and Mr. Brook of Westminster Hospitals, had experimented pretty faithfully; and Mr. Brook said that, as the plug of integument did not remain, it must be rather a disadvantage by keeping the rings and canal dilated during the stage of inflammation, and thought the operation would be better without the cylinder.

Acting upon this information, I had an instrument constructed by Tiemann of New York, consisting of the cylinder with a canal opening in front near the point, through which is passed a needle or stylet, in the same manner as with Wützer's instrument. The needle, however, is large and of silver, with a stout point which unscrews, and a button is screwed on in its place. The cylinder is merely used for the purpose of conveying the needle or stylet through the inguinal canal, and is then withdrawn and the needle is left so that the inflammation and thickening may take place throughout the whole length of the canal undisturbed by the plug and cylinder.

It is plain that no operation for this disease can be successful in old hernia when the obliquity of the canal is destroyed, but in an earlier stage anything that is safe, and that tends to strengthen the parts and restore the obliquity of the canal is worthy of a trial. The silver wire can by this instrument be passed so as to favor the restoration of the canal, and will certainly produce more or less thickening of the parts, according to the time it is left and will be free from the danger of destructive inflammation.

Dr. Stone directed Mr. Tiemann to send this instrument to Mr. Syme, on Canal st.

Dr. TRUDEAU remarked that in Wützer's modification upon Gerdy's method, he could see no more safety than in the original operative procedure. In both, peritonitis must be feared, as well as erysipelas and abscesses in the region operated upon. Every one will readily acknowledge that an unfortunate result may take place, as well from the presence of the needle as from that of the thread. It has been contended that the great advantage of Wützer's procedure consisted in keeping the invaginated part of the scrotum in contact with the whole length of the inguinal canal, while in Gerdy's method the contact was more limited. This point was thought to be of great importance by Gerdy himself, who sought to make the contact more perfect by causing inflammation of the invaginated portion of the skin by means of irritating applications, such as ammonia and cantharides ointment, and finally by stuffing it with lint which acts like Wützer's *invaginatorium*. Should it be thought necessary, two and even more stitches might be applied without more danger, and with a better prospect of exciting adhesive inflammation. The operation, thus modified, has been found effective and offers this advantage of requiring no special instrument.

Dr. RICHARDSON (Professor of Anatomy in the University of Louisiana,) remarked that he had received from a gentleman\* in Ohio a communication detailing the treatment of three cases of inguinal hernia by what purports to be an improvement upon Wützer's method. The instruments essential to the performance of this new operation are a large curved needle, a strong silk ligature about two feet in length, and a perforated ivory ball sufficiently small to enter the abdominal opening without any great distension. The mode of operating is substantially as follows: The ligature is passed through the needle and doubled, and the ball is then passed down upon the ligature tied firmly within four or five inches of the lower extremity of the latter. The hernia having been reduced, and the skin invaginated with the forefinger of the left hand, the needle is passed along the palmar surface of the latter as far as the internal ring and there made to penetrate the structures lying in front. The ligature is then drawn, carrying with it the ivory ball which is in this manner lodged

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\*Dr. Hackenburg, of Springfield.

at the bottom of the *cul de sac*, and is retained in this situation by fastening the anterior division of the ligature to an apparatus designed for the purpose. Inflammation is soon excited by the presence of the foreign body, and when suppuration is fully established, the fastening upon the abdomen is loosened and the ball withdrawn by means of the lower division of the ligature which was left hanging from the mouth of the *cul de sac*; the latter subsequently closes by granulation.

Of the three cases treated in this manner, the writer of the article reports two entirely cured, and the third relieved.

[The radical cure of hernia is one of the great achievements of modern surgery. We will briefly mention here, the older methods in use, before the improvements effected of late. Unlike them, these operations are ineffectual and many of them highly dangerous.

*The Golden Stitch.*—A golden wire was passed by means of a strong, curved needle around the sac, the spermatic chord and vessels; it was then tightened.

*Castration.*—This, the favorite operation of Paulus Egineta, is barbarous and useless.

*The Royal Stitch.*—The ligature in this method, was applied around the sac, avoiding the cord and vessels.

*Scarification.*—A more rational, but yet very dangerous method. Some cures were obtained by Ambrose Paré and his pupil Pierre Pigray.

*Incision.*—In this operation, praised by Jean Louis Petit and Abernethy, the hernia was reduced, the sac laid open, and filled with lint. Like all the preceding operations, it has been rejected, although often performed successfully.

*Compression*, by means of an appropriate truss, is the most simple and effectual method practised in olden time; it is still in use and is the only manner of operating upon children. But after twenty years of age, and especially when the hernia is large, it cannot be relied upon.

The methods now under experiment are: 1st, *Autoplasty*, proposed by the late Dr. Jameson of Baltimore. The canal is laid bare, and stopped with a flap dissected from the neighboring integuments. Performed three times successfully by its originator, once by Dieffenback. It is to be regretted that science has no more facts in favor of this simple, yet beautiful method.

*Invagination.*—Proposed by Gerdy. The hernia is reduced, the skin of the scrotum carried into the inguinal canal, above the tendon of the external oblique and kept *in situ* by the quilled suture. It may be removed in five or six days.

This method was subsequently modified by Wützer, and requires for its performance, an instrument which deserves a description. It consists of a cylinder of hard wood, like ebony or box-wood, through which passes an elastic steel needle, which comes out on the upper surface, near the point. Over this, there is a concave cover perforated at one extremity, in order to secure the point of the needle, and attached by a screw to the handle of the cylinder. The fold of the scrotum is pushed in the inguinal canal, as for Gerdy's operation; the cylinder is then substituted for the finger, the needle pushed through the integuments above the internal ring, and the cover screwed on. The instrument is retained several days *in situ*, till sufficient inflammation is excited in the inguinal canal.

*Seton.*—Under this head we may mention Belma's method of exciting inflammation in the canal by means of a gold-beater's skin cylinder, inflated after its introduction, and Mösner's operation, which is yet quite recent, and deserves trial as well as Belma's. Mösner operates as follows: The skin of the scrotum is invaginated, and a needle carrying a strong thread, is passed through the inguinal canal as in Gerdy's method. The finger is then withdrawn, and the thread remains like a seton. It may be removed after eighteen or twenty days.

*Injections.*—Irritating injections into the hernial sac have been proposed by Velpeau, and experimented by Pancoast, Jobert, and Ricord. The tinct. of iodine has been recommended for the purpose. The success has not been gratifying.—J. S.]

STANFORD CHAILLÉ,  
Recording Secretary.

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ART. VI.—*Case of Scrofula: Treatment: Cure.*—By GREENEVILLE DOWELL, M. D., of Brazoria, Texas.

FEB. 8th, called in to see negro girl Olivia, belonging to Mr. T. R. Phillips of this county. Patient's age 18; dark mulattress. Had been subject to scrofulous ulceration for five years. Had had it in the arms

so bad as to induce the attending physician, the late Dr. Davenport, to propose amputation; also, once in the chest so bad as to expose the membranes around the heart and lungs, producing absorption of the upper portion of the sternum.

*Present Condition.*—Left arm anchylosed at the elbow joint, but free from any sores. Both legs ulcerated from the knee down to the feet. Both legs thin and emaciated. Left foot appeared to have burst open and was discharging a great deal of pus from around the tarsophalangeal joints. Legs and left foot appear to be a mass of corruption and very offensive to the smell. Otherwise her health was good; good appetite. Had never menstruated. She has been under the treatment of several physicians, all of whom had abandoned the case as hopeless. Upon a full history and close examination of the case, I came to the conclusion that nothing could be done in her present condition; but if both legs were amputated and then keep her on an anti-scorfulous diet and treatment, that a cure might be expected. As she had lingered on for five years, I thought if this mass of corruption was removed she would recover, and be at least a seamstress and not a nuisance as at present. I so stated my opinion to her master, and proposed to remove the limbs at once. I, therefore, appointed a day to amputate, and invited my partner Dr. Porter, and Drs. Morris, Weems and Brooks, to assist me, and see the operation.

We all met at Mr. Phillips' on the 11th, for this purpose. After examining the case, the other physicians came to the conclusion that even amputation would do no good, and might hasten her death by causing tubercles to form in her lungs; and that a cure was improbable and almost impossible, if the operation was performed. This opinion was stated to Mr. Phillips and the girl. And as all the others were against me, I abandoned the operation and asked for a prescription. One physician declared that nothing could be done, and to prescribe was useless. The others thought there was a chance with the plan I proposed. We then proposed to her master that if he would pay for the medicines, we would experiment on her and try to effect a cure, urging that it was a pity to leave her in her present condition. To this he readily agreed, and I commenced with the following prescription:

Simple Syrup of Sarsaparilla,	-	-	i pt.
Iodide of Potash,	-	-	i ℥



Dissolve the iodide of potash in an ounce of water, and add the syrup of this; give tablespoonful three times a day, two hours before each meal.

I directed the ulcerations to be covered with oil cloth, and to be bathed with the following wash, three times a day:

Chloride of Zinc,	-	-	-	iʒ.
Water,	-	-	-	i pt.

This treatment was alternated every two weeks with the following:

Impalpable Powder of Iron, (Iron reduced by hydrogen)	iʒ.
Simple Syrup of Sarsaparilla,	i pt.

Give tablespoonful three times a day.

The ulcerations to be washed every morning, or once a day, with

Kreosote,	-	-	-	-	i f. ʒ.
Water and Alcohol, equal parts,	-	-	-	-	i pt.

The chloride wash to be used as before, twice a day. Diet to be generous and unstinted—just what she preferred and most relished.

Under this treatment she gradually improved. In three months the ulcerations had nearly healed, and she began to menstruate, and has gradually improved until now she is able to walk and is quite full of habit and looks well every way. Her legs are still small, but gradually increasing in size, and the skin is assuming a healthy and natural color.

I am now using sugar of lead as a wash to harden the skin. She is still taking the iodide of potash and sarsaparilla, but in reduced doses, which I think of keeping up until next spring.

Nov. 13, 1858.

ART. VII.—*Contribution to the Pathological Anatomy of the Large Intestine, including Pathological Remarks, followed by a translation from the French, on the Faecal Retention, etc.* By BENNET DOWLER, M. D.

IN writing this article with the journalistic brevity which the exigencies of the case requires, I regret that illustrative examples of the above named lesion of the large intestine, cannot now be adduced, although, I believe my post-mortem examinations recorded in XXII MS. volumes, contain between one and two hundred cases.

I propose to divide this paper into three parts; namely, 1st, my announcement of a lesion of the large intestine sixteen years ago; 2nd, general remarks on the same; 3d, translations concerning Fæcal Retention, etc.

PART THE FIRST.

In *The Western Journal of Medicine and Surgery*, published in Louisville, Kentucky, October, 1843, Art. I., the following extract from an extended paper, extitled *Contributions to Morbid Anatomy*, will be found. It is now reproduced, because it is deemed important; because it has been most thoroughly confirmed by prolonged and laborious researches subsequently made; because anterior to this publication, this lesion was either little known, or, at least, little noticed even in yellow fever; and, lastly, because the article alluded to, of which this extract is only a small portion, was probably not seen by one in fifty of the readers of the *New Orleans Medical and Surgical Journal*.

IV.—CORD-LIKE CONSOLIDATION OF THE INTESTINE.—*Anatomical Characters*:—The large intestine chiefly, though not exclusively, is the seat of this lesion; the cæcum, colon,, and rectum, are found firm, round, bloodless—strong, tenacious, elastic, white—often resembling in color the breast of a boiled chicken, or prepared tripe; the intestinal cavity obliterated, not admitting a body as large as a quill without forcible dilatation; desiccation of the mucous membrane; all the coats much but equally thickened; the external dilatations, notchings, and puckering, as well as the internal sacculated pouches obliterated, leaving the longitudinal muscular stripes distinct; the consolidated portion probably diminished in length; in most cases empty; sometimes containing solid pebble-shaped fæces, from the size of a hazel-nut to a hen's egg, or even to a goose's egg. These fæcal indurations, when numerous, give the large intestine the appearance of a string of onions, being of the consistence and color of dried or baked clay, closely surrounded as with a skin, by the intestine, emitting little if any odor.

If we deduct for the apparent loss in the mucous, serous, and sanguineous fluids natural to the parts, it is probable that the bowel has suffered no loss of weight by this lesion. This change, which is a very acute one, since it happens for the most part in fevers lasting from three to four days, is quite different from atrophy, or collapse, which

happens to a muscle or organ from long disuse of its natural function. It does not appear to be occasioned by mere emptiness, a condition to which the large intestine is often subject in sickness and health—distention appearing to be its natural condition—differing in this respect from the urinary bladder, which habitually contracts or dilates more or less, according to the quantity of fluid which it may contain: on the contrary, the large intestine when most empty of faecal accumulations, is most distended with its peculiar gases, at least, such is often the case in the dead body.

There can be no doubt when this lesion is established from the valve of the caecum to the anus, as is often the case, that defecation cannot be effected; and, therefore, when purgation is essential to life, this lesion must be highly dangerous or absolutely fatal. From the universal stricture, dryness, and, sometimes, almost cartilaginous firmness of the intestinal walls, enemata, or gum-elastic flexible tubes would alike fail in many cases to render the canal pervious for the purposes of defecation. When we reflect on the anatomy of the colon, including that enormous triangular bag called the caecum at its origin, its great size, its constant gaseous distention, its thinness (being naturally the most attenuated of all the bowels,) it appears difficult to account for this lesion. Do the muscular fibres of the intestines undergo a rigidity and contraction, analogous to tetanus or lock-jaw in the external muscles? Is this an intestinal tetanus, so intense as to survive the relaxing power of death itself? From the thickness, muscularity, and diminished calibre of the rectum, one might expect that this portion of the great intestine would be more completely closed than any other; but, the contrary happens. Two or three inches above the anus, small portions of enemata are sometimes found.

Without entering upon the pathology of this lesion, I may be allowed to remark, that I have heard a number of yellow fever patients affirm that, during their whole illness, they never had a single defecation or stool. I have known several, both in convalescent and fatal cases, who, after free purgations during the first day, had no further defecations, or such, only, as I have often found in this lesion, and usually described by the patient as small dry pills. Some of these patients had taken quinine. Is not this salt a constringer of the bowels? I regret that my attention has not been arrested by this

inquiry in time to afford me a sufficient number of observations upon which to form a satisfactory opinion. The advocates of the quinine practice would do well to inform the medical public on this subject, as nearly all other modes of treatment, in which quinine is only an occasional element, are too complex to afford exact and unmodified results.

Though this lesion is not positively known to have any connection with the quinine practice, yet, such practice may increase the morbid alteration, instead of diminishing it.

The above description, it is believed, will apply to a majority of cases in which this lesion is found, though it will, as might be expected, transcend the appearances in some instances: with these exceptions, the picture I have drawn, will not, I trust, be chargeable with exaggeration.

The ante-mortem history or symptoms of this change, though little known, will doubtless be ascertained to a greater or less extent before long. A timely and well conducted course of purgation, with enemata or lavements, assisted with suitable instruments, would probably prevent this affection altogether.

*Case.*—Large man from Louisville: the cæcum, colon, and rectum contracted to the size of the thumb, scarcely pervious to the scissors, containing nothing but a handful of small, dry, inodorous, friable, black crumbs or scybalæ, from the size of a pea to a hazel-nut; the whole of the large intestine being firm, strong, elastic, thickened, blanched, bloodless, and scarcely moist within. I add the following facts for comparison: mouth and gullet full of blood; stomach small, blanched, four ounces of black-vomit, without any bloody tinge; jejunum, blood and black-vomit; the upper third of the ileum gray chylous paste, the residue black-vomit again.

#### PART SECOND. REMARKS.

In the cord-like contraction of the large intestine, the sacculated or pouched cavities are, as above stated, completely obliterated so as to lose their natural configuration both within and without the tube. The longitudinal bands of the muscular coat or the length of the bowel appears somewhat shortened, although actual measurements are still wanting to decide this point positively. The contraction of the circular muscular fibres, by obliterating the convex salient bulgings, pro-

per to the normal condition would reduce the apparent length of the intestine, even though the longitudinal muscular bands (the *ligamenta coli*) might be in no degree shortened.

This remarkable lesion though probably often secondary like many others, appears to be similar neither to the transient spasmodic contractions which probably occur in colic, nor to the permanent stricture, both of which are usually very restricted in extent, while this cordiform lesion includes, in many cases, the entire cæcum and colon.

It is possibly a primary lesion, in some cases of yellow fever and in other diseases. In some persons not habitually costive, that condition precedes or accompanies the attack of yellow fever. From the induration, dryness and rounded form of the fæces, and the firm contraction and exact adaptation of the intestine to these configurations, this lesion must occur anterior to the latter stage of the disease, as many post-mortem examinations indicate. In any case, it is a prominent organic change, and consequently, implies a previous functional derangement of a significant character, particularly in reference to the equilibrium of the circulation.

This lesion is more common in yellow fever than in any other fever. It is also found in congestive, and even in typhoid. I have found it in bilious-remitting fever, also. It is probably in a modified form, often the cause of obstinate constipation, colic, ileus, and may be superinduced by lead poisoning. In regard to this latter malady, I have not been able to test this conjecture by post-mortem examinations. This contraction, it may reasonably be supposed, simulates scirrhus strictures, invaginations and fæcal and calculous concretions. In all of these conditions the diagnostic symptoms are, unfortunately, very obscure and equivocal so far as the mere contraction of the intestinal calibre is concerned.

In the rectal portion of the large intestine, simple contraction is the physiological condition when this organ is not distended by the occasional descent or temporary delay of the fæces, while, on the contrary, the cæcum and colon are distended with gas in the natural state, even when there is little or no fæcal accumulation present. The rectum is more muscular, is thicker, is more sensitive, and more completely connected with the spinal sacral nerves, being more richly supplied with blood-vessels than the residue of the intestine, which is more intimately identified with the sympathetic system, and has a

lower degree of sensibility and of contractile power. This naturally contracted state of the rectum, excepting its pouches just within and above the sphincter, when not loaded with fæces, should be constantly borne in mind in administering medicated injections, which, if large, will often be expelled involuntarily and without delay; if the dose be small, as an ounce or two, and unaccompanied with injected air it will generally be retained. By the way, this mode of medication though repulsive to taste among most patients, has many advantages and should be far more generally practised; it may, in a two-fold sense be called *fundamental*, anatomically and therapeutically, thereby economizing the quietude of the stomach or becoming its most powerful ally.

The cord-like contraction of the alimentary canal is a lesion as much as hyperæmia, ulceration, or perforation, be its pathological import what it may. It occurs in the involuntary muscles of the intestinal tube, often suddenly and in a tissue supplied with nerves chiefly from the sympathetic—a system whose morbid phenomena in the absence of positive inflammation, are expressed scarcely at all by acute pain, and, are, therefore, treacherous, difficult to recognize, being, so to speak, unseen, while its antecedents or causes, are obscure. It is far different both objectively and subjectively, with the phenomena attendant on involuntary contractions, convulsions, spasms, and rigidities of the voluntary, muscular system, as in epilepsy, hydrophobia, chorea, hysteria, wry-neck, priapism, cholera, cramp, tetanus, trismus, etc., which the physician sees, the patient acutely feels. But the danger is not the less because central, unseen. Intestinal contraction however, leaves in the dead body a much clearer imprint than most convulsive affections of the voluntary system, in which pain predominates. But on this subject I do not wish to speculate diffusely at present.

It is by no means absurd to assume that the contraction or virtual occlusion of the intestine might be, in many cases, the cause of death, whether this lesion be a primary or a secondary complication. Suppose a case of yellow fever to consist of two factors—the one yellow fever proper and primary—the other secondary and incidental; suppose again that the former either from its natural tendency, or from the interference of art, would eventuate in recovery, were it not for the supervention of the latter, and that the combined result of both,

must work out a fatal result; then, the latter is, if not eliminated, the determining cause of death. Hence the prevention or removal of the complicating element, becomes fundamental, paramount.

Bad theories have often led to better; or, at least, they have in an experimental point of view furnished useful information. Thus the recent reign of hepatic, gastric, and bile theories of disease, led to a careful examination of the secretions and excretions, particularly those of the alimentary canal. The decadency of these theories and their complemental therapy based almost wholly upon active purgatives and repeated mercurials, was followed by a corresponding neglect of the alvine excreta. Constipation for days and weeks so far from giving dissatisfaction, is now by some physicians regarded as a favorable condition, particularly in some of the idiopathic and exanthematous fevers, as typhoid, scarlatina, etc., while in most other maladies indifference in this regard, is as compared to former times, very common, and is, as I humbly conceive, unwarranted, being convinced that a stercoral, infecting accumulation long retained in contact with an inflamed or morbid state of the mucous membrane, is more dangerous and painful than occasional purgatives. The absorption of stercoraceous matter into the circulation, gives an addition to the poison of fevers, etc.

Dr. Richardson, Professor of Anatomy in the University of Louisiana, in his excellent work, *Elements of Human Anatomy, General, Descriptive and Practical*, says of the cæcum: "Its great size, which sometimes nearly equals that of the stomach, is no doubt partly due to its depending position and consequent liability to accumulations of fecal matter, which often give rise to serious constitutional disturbance."

I may here remark by the way, that the narrow view which many pathological anatomists take of the doctrine of lesions is little calculated to advance morbid anatomy. It is erroneous to regard the stomach, or any, or all of the internal organs as the exclusive seats of disease. The skin, the cellular, muscular, and other peripheric structures should be as carefully examined, as the mucous membrane of the alimentary organs.

Once more: the rectal division of the great intestine from its contiguity to, and intimate connection and sympathies with the pelvic viscera, as the bladder and organs of generation, all highly sensitive, ought, theoretically speaking, to be a seat of medication inferior only to the stomach. Practically it is often more eligible, many medicines, par-

ticularly the most soluble, are quickly absorbed by this organ, whether absorption be a vital, endosmotic or a merely physical imbibition, as Magendie would designate that process. "Liebig states that a solution of common salt, in the proportion of one part to eighty of water, disappeared so completely in the rectum, that an evacuation one hour afterwards was found to contain no more than the usual proportion of salt." It is, probably, to the capillary veins rather than to the lacteals that the absorption of medicinal substances must be referred. The lacteal system, possessing as it does the power of selecting nutritive substances, to a great degree repels all else.

Among the abdominal viscera there is not one, the spleen perhaps excepted, whose special function is either so little understood or so little investigated as that of the large intestine. There can be little doubt that its physiological mission is neither that of furnishing a mere stercoral *dépôt*, nor that of simply elongating the alimentary canal. Its great volume, gaseous distention, vast pouches, great arch, sigmoid flexure, and anatomical organization indicate, if they do not reveal, the important purposes of this organ in the economy.

In his Human Physiology, Professor Carpenter thus explains the function of the large intestine: "The undigested residue of the food, mingled with the products of secretion that have been poured into the alimentary canal, gradually acquires, in the large intestine, the ordinary consistence of fæces through the continuance of the absorbent process, whereby the superfluous fluid is removed." § 116. This brief explanation is itself "superfluous," inasmuch as the dissectors of the dead will often find perfectly elaborated fæces of the natural consistence in the ileum after sudden death; and also, after death from both acute and protracted diseases, as I have often witnessed. Fæcation (if there be not such a word there should be) differs in the small intestine often in nothing from that in the large, except in giving a diminished transverse diameter to the stercoral aggregations corresponding to the diminished calibre of the canal. Professor Dunglison's exposition is similar: "When the food has attained the lower part of the ileum the process of chylification has been accomplished. It is sent on through the valve of Bauhin. The chymous mass is sufficiently soft to pass readily, etc. Before the excrementitious matter reaches the lower portion of the small intestine it has not the full fæcal odor." *Phys.* i. 185-6. Stercoraceous vomiting in intussusception, in strangulated hernia; also fæcal effusion from stabbing or puncturing the ileum



in its lower portion (followed by universal peritonitis, and death, and verified by post-mortem examination), I have witnessed in my own practice, all going to show that fæcation may, and often does occur in the small intestine. Moreover, stercoraceous fistula or artificial anus originating in the small intestine, has often occurred from which normal fæces have been discharged for years.

While writing this page, I stopped and turned to MS. Vol. XII., written sixteen years ago, and immediately found such records as the following; two made on the same day, and one a little later. This volume, its predecessors and ten successors, probably contain a hundreds of similar examples.

Extract from MS. Vol. XII.—*Post-mort. Examin.* of S. H., who died of abscesses of the liver and lung, after thirteen weeks' illness:

1843. July 8. Small intestine contained abundance of healthy fæces; much healthy fæces in the large intestine.

Same day: N. R., from five to ten minutes after death from dysentery; small intestine normal—contained natural fæces more or less consistent, some portions having a faintly greenish hue.

1843. July 16. P. T., dead eight hours; congestive fever; pulmonary congestion: Bowels distended with gas; both the small intestine as well as the large contained much semi-solid, natural fæces.

The special physiology of the large intestine is not, therefore, that of fæcation solely. Messrs. Todd & Bowman, in their *Physiological Anatomy*, under the head "of Digestion in the Large Intestine," say: "Many facts lead to the opinion that the intestinal contents undergo some further digestion in the cæcum; the mucous membrane of the cæcum resembles that of the stomach, and is supplied with glands like the solitary glands, which pour out an abundant secretion. No material change takes place in the fæces as they pass through the large intestine, excepting such as is produced by the absorption of fluid from them by the mucous membrane." 608.

I beg leave once more to return to the general subject before dismissing this part of the present inquiry. Some of these fæcal concretions are as round as bezoards found in the alimentary organs of various animals, being uniformly dense, nearly dry, and darkish throughout, having neither a nucleus nor perceptible strata; others have a firm nucleus of natural fæcal matter, as large perhaps, as a nutmeg, which forms the core of a hard, inodorous ball, as large as

a hen's egg, which is not coated with mucous or other exudation as is usual in the seybala of dysentery.

The mechanism or physiological anatomy of such formations, has not, so far as I know, been explained. The large intestine is far more entitled to be called the *jejunum* or *empty bowel* than that portion of the canal thus named. The so-called *jejunum* is the *great store-house of elaborated nutriment or chyme*, only the *caput mortuum* of which is received into the capacious and comparatively large intestine. The latter in its normal state contains a large amount of gas, the origin and uses of which are unknown. Those physicians who have been accustomed to the making of post-mortem examinations, and, who, open the *entire alimentary canal* from the mouth to the anus, find with certain exceptional cases, due to incipient putrefaction, that persons dying of the ordinary diseases, and being examined soon after death, contain less offensive matter, gaseous and solid, in the bowels, particularly in the large intestine than such as are suddenly killed while in full health. The normal state of the large intestine being that of gaseous distention, comparative emptiness, and, so to speak, dilatation, the fæces are not moulded or compressed by its contractile action as they are by that of the rectum and its sphincter. Neither in persons suddenly killed nor in those dying of diseases is the large intestine ordinarily found to be contracted upon its fæcal contents. Such contraction is, therefore, altogether abnormal. The distention of this intestine is maintained by gas rather than by fæces. The non-elimination of gas probably paves the way for contraction. The pouches before obliteration serve, perhaps, to arrest and give a rounded form to the fæces; or their circular fibres may be the first to contract and obliterate the cavity of the tube; or, the tube may contract irregularly as it certainly does in some cases, as proved on post-mortem inspection. Now, in any of these events, a persisting normal or even an inverted peristaltic action, would force the fæces in the longitudinal direction against the circular contraction of the tube, so that this double force would naturally compress the contents of the bowels into masses, which would be more or less globular, from the nature of the compound forces and the anatomical configuration of the bowel, which would make the compression circular in the transverse direction, while both the proximal and distal aspects of the cavity thus contracting would give a rounded form even in the longitudinal direction. Obstruction of the bowel from intussusception, strangula-

tion, or contraction, is followed often with an anti-peristaltic action, or, at least, by a reflow or back-water current, which, however, in the large intestine would be arrested by the ileo-cæcal valve. Upon this obscure subject the ablest physiologists are very silent, or give vague and contradictory neurological expositions which throw light neither upon the peristaltic action nor its aberrations.

The morbid element, that is, spasmodic contraction being admitted, the normal anatomy of the muscular tissue of the large intestine serves more fully to illustrate this lesion: Of this tissue, Professor Richardson of the University of Louisiana, says, that "its fibres belong to the involuntary or non-striated variety." "The *longitudinal* fibres of the large intestine are collected into three flattened bands, which are nearly equi-distant and extend from the cæcum to the rectum where they become spread out into an uniform layer of considerable thickness. These bands are considerably shorter than the intervening walls of the bowel, and give rise to the sacculated form of this part of the tube, as may be proved by dividing them at various points, when the saccules will be found to *disappear*, and the length of the bowel to be materially increased. The *circular fibres* pass transversely between the longitudinal bands, and are very thinly scattered over the convexities of the saccules, but in the intervening spaces, they are collected into small bundles, which project internally to form the inter-saccular septa." (*Human Anatomy*. 324.)

I will here add a few illustrative notes which happen to be the most convenient—perhaps not the most striking of this lesion:

1842. July 30; S. B. E., aged 19, born in Massachusetts, resident one month; yellow fever; on the third and following days took quinine, carbonate of soda; was cupped twice on the nape—bled from the arm fourteen ounces—during the following night the ligature became displaced accidentally; lost much blood in the bed—sinapisms; blisters; camphor and quinine each sixty grains in three enemata, etc.

The pains in the forehead and back which occurred in the early stage declined after blood-letting, but subsequently returned to the head. For two days, he had what the nurse graphically termed "*the trembles*"; intense thirst; dry red tongue; vomited an offensive turbid mucosity.

Pass on to the last hour: Aug. 5th, 11 o'clock, A. M.: He understands a question, and speaks readily, but wanders, and is incoherent when his attention is not aroused. Has universal tremors, an

spasmodic rigidity, running by wavy paroxysms all over the body, contracting some of the muscles into lumps; bending the trunk backwards and forwards and then relaxing. Demands water, but appears to have pain, or inability, with convulsive trembling in swallowing it; jaws nearly closed; has difficulty in putting out his tongue, which is rough and dry. His skin, on the face, neck and arms, is of a blue, leaden hue mixed with yellow; breathing and pulse irregular and hurried. Abdomen free from distention, but rigid, especially along the recti muscles; extremities cool; eyes slightly injected and yellow, their globes rolling about by spasmodic jerkings of their muscles. No description can give a just view of the pain, and excess of sensibility, which he seems to suffer. To touch him anywhere causes him to start, like a shock of electricity. His blisters appear to cause the severest pain; complained of them until within five minutes of his death. No febrile smell, or petechial spots. Died at 12 o'clock, M., the first victim of the epidemic of 1842.

In twenty-five minutes after death the body was universally rigid, especially the muscles of the abdomen. The fingers contracted, could not be straightened without great force. The jaws firmly closed. The extremities cold; the veins collapsed. The face and neck marbled with purplish spots. The abdomen where blistered, had three dry, hard, black spots (gangrenous), as large as a quarter of a dollar; body growing hot.

*Autopsy*, one hour and fifteen minutes after death. Muscles natural; adipose tissue scanty and moderately yellow; quantity of blood in the great vessels inconsiderable, etc.

I omit for the present, the report of the examination of the individual organs of the head, chest, and abdomen, excepting the large intestine: About half of the length of the colon, commencing at the cæcum, was *impacted* with dry, friable, clay-like, inodorous faeces; *colon contracted*; the rectum contained an anæma, apparently of quinine and camphor.

The next victim which I opened in that epidemic, had the large intestine, excepting six inches of its rectal end, *converted into an uniformly consolidated round cord*, etc., etc.

I have seldom examined, for scientific purposes, dead bodies in which the putrefactive process had made much progress; but in even these exceptional cases, if my recollection be not at fault, I have met with several examples of *cord-like contraction* of the large intestine in

subjects in which the residue of the alimentary canal was every where enormously inflated, while the thoracic and abdominal cavities, including the integumentary tissues, were enormously distended with putrefactive gases. I beg leave to refer for a moment to this subject with more detail, because the temporary persistence of contraction in the large intestine in defiance of this gaseous expansive force resulting from decomposition, is significant of its pre-mortem development as well as of its lesional intensification.

The rapidity with which the dead body is sometimes decomposed in New Orleans, when heat and humidity prevail, though remarkable, is by no means peculiar to yellow fever subjects, but occurs, perhaps equally, in cases of sudden death from sun-stroke, accidents, etc. It is not improbable that a peculiar, perhaps electrical, condition of the weather sometimes prevails, not always explicable by mere heat and moisture, in which decomposition, as compared with weather apparently equally hot and moist, proceeds with extraordinary celerity. The only case, however, which I have seen, according to my present recollection, in which decomposition occurred almost immediately after death, was that of a yellow fever cadaver *early in the winter and in the absence of warm weather*. In this case, there was no gaseous distention of the cavities or of the cellular membrane; the body emitted a putrefactive smell and assumed the greenish tint of incipient decomposition; neither muscular contractility nor the rigor *mortis* existed; the muscles non-elastic and doughy; the cuticle non-adherent or easily parted from the skin by pressure.

#### PART THIRD. FECAL RETENTION.

[I have placed the following observations as a sequel to the preceding, because *Fæcal Retention* is probably due in many cases to a *morbid contraction* of a portion of the large intestine somewhere between the cæcum and the sigmoid flexure of the colon. In clinical practice the much neglected cæcum should always be examined, as, of all the intestines, it is the most generally diseased, and is, at the same time the most accessible to the touch or responsive to pressure. Its lesions, however, may be for the most part, only secondary, and, although often slight and insufficient to explain the cause of death, would be deemed of great pathological significance if found in the stomach, duodenum, jejunum, or ileum. Early in febrile and some other diseases, especially in cases wherein patients complain of pain

in the right side of the abdomen, a slight pressure over the cæcum will often enable the practitioner to diagnose *typhloenteritis*. It should be borne in mind that the cæcum when suffering from tubercular, catarrhal, follicular, or dysenteric affections cannot tolerate stercoæal accumulations without an aggravation of the original malady. The fæces, however natural, become, in a diseased condition of the organ, highly deleterious. Not only so. Their accumulation and retention may be the primary sources of disease, as the following observations, not to produce other evidence, sufficiently prove. Here is an *internal* foundation for the "dirt theory," and miasma, often as potent for evil as the *external* dirt of streets and the fermentation of swamps.]

A paper was read in the Society of Medicine at Paris, May 7, 1858, by Dr. Boys De Loury (*Gaz. Heb.*, July 9), upon the diseases produced by the retention of fæcal accumulations in the large intestine, particularly in the cæcum, of which the following is a condensed translation:

Dr. Boys De Loury considered that although fæcal retention was not very rare, the phenomena to which it gives rise may be confounded with those of other morbid affections. This kind of disorder not being very common the attention is not usually fixed upon it, and being badly understood it may continue for a longer or shorter time producing the most aggravated symptoms of disease.

Dr. Boys De Loury relates a number of cases illustrating the evil, and even the most dangerous effects, of fæcal accumulations. He was called in haste to a lady who was shrieking with severe abdominal pain, which had continued for several days, which did not permit her to repose for an instant; abdomen tumid; excessive tympanitis, obstructing the diaphragm, causing dyspnoea; diffused pain, but most severe on the right side in the iliac fossa, and in the course of the colon, especially on pressure. Notwithstanding the severe meteorism, pain, and repeated vomitings of bile and matters as fœtid as fæces, the fever was comparatively light; but the pulse was hard as in peritonitis; face not shrunk; tongue not dry.

These symptoms had continued long; her physician had acknowledged that she suffered from a tumor, which was confirmed by a surgeon consulted in this case; the treatment had been with iodide of potassium internally; and with a view to the resolution of the tumor, ointments were applied to it, and alkaline baths were used. The pain, nevertheless, continued with increasing force.

For several years a resident in Paris, this lady's health had become deranged; her bowels were almost always constipated. For more than eight days she had had no evacuation. I considered her disease to be a violent inflammation of the large intestine, from fæcal retention in the cæcum and arch of the colon. Leeches were applied to the abdomen and over the cæcum; emollient narcotic fomentations to be followed by cataplasms, baths, etc. The pain continued several days notwithstanding these means. Finally, purgative enemata were used, which discharged fæcal matter in round masses, like balls, the consistence of which was remarkable for hardness. The patient was only partially relieved. Oily purgatives alternated with prolonged bathing were now resorted to, and gave issue to enormous quantities of fæcal matter, which was very foetid but less indurated. The meteorism subsided. At the expiration of twelve or thirteen days, the abdomen was still tender, especially in the region of the cæcum. The doctor upon reëxamination found a tumor-like collection still remaining in the large intestine, which had not yet been dislodged by the purgatives, but which, in twelve days disappeared altogether, although the pain had not been completely dissipated until six weeks after his first visit.

Dr. Boys De Loury gives a number of interesting cases of a similar character which occurred either in his own practice, or that of his *confrères*, including a synopsis of a case by Dr. Powell, published in the *New Orleans Medical and Surgical Journal*, and republished in the *Gazette Hebdomadaire*. He also gives an account of a Memoir by M. Cloquet, on the *entérolithe*, or intestinal concretions formed often in cæcum, which vary surprisingly (*étrangement*) in weight, volume, and color, ranging from two grammes to two thousand, distending the intestines, destroying their contractility, causing irritation and inflammation, together with hypertrophy of the intestinal walls, etc. M. Cloquet has seen true calculi formed in the intestines from the use of magnesia.

[Caroline, the Queen of George IV., died, it is reported, from magnesian concretions in the bowels, having in an injudicious and extravagant manner, used magnesia.—TRANSLATOR.]

Dr. Boys De Loury arrives at the following conclusions: 1st, that the retention of fæcal matters in all parts of the large intestine is far more frequent than is generally believed; 2d, that the cæcum is the chief seat where these accumulations exist; 3d, that the cæcum thus

filled with fæces may assume the character of cyst, ovarian-like tumor or abscess in the iliac fossa; 4th, one characteristic symptom, not hitherto recognized in this affection, is a painful sensation extending throughout the large intestine and which may be excited principally by pressure upon the cæcal region; 5th, that acute enteritis thus occasioned by retained fæces, may not only invite a violent inflammation throughout the mucous tissue of the bowels, but end fatally by a consecutive peritonitis, and perforation of the intestine; 6th, that the prolonged retention of fæcal matter, may from the absorption of their poisonous exhalations, give rise to affections of a typhous nature. He, therefore, insists that constipation, being injurious, should be prevented by attempting the act of defecation. One of the best means of regulating the bowels and increasing their contractile force, is that of the *douche ascendante*.

M. Boys de Loury, advises for severe constipation the *douche ascendante* or bowl-bath, to be repeated every four or five hours, as the best means of restoring the muscular contractility of the intestine. He does not consider these douches as safe until all inflammation has subsided in severe cases of fæcal retention, as the walls of the intestine may be too weak to resist jets of water, for a month after having been disembarrassed. If given sooner, perforation and death might be the result. He advises after the subsidence of inflammation, the regulation of the bowels by means of mild purgatives, especially by castor oil; he enumerates full and prolonged baths as valuable auxiliaries in this treatment. M. Cloquet advised persons who had suffered from prolonged fæcal retentions to use an enema amounting to *two litres or two quarts* daily.

In the discussion which ensued in the Society upon the above paper, M. Briquet coincided with M. Boys de Loury. M. B. had witnessed similar facts and dangers from fæcal accumulations; in some cases the retention of fæces in small quantities produced serious consequences; he had at the time the case of a young wife recently delivered, who had the symptoms of peritonitis, as he judged, from fæcal retention, who was almost immediately relieved by purgation, and although some new symptoms were afterwards developed, showing that all danger had not yet passed away.

M. Gros maintained that retention of fæcal matters was constant in dysentery; that their contact with the mucous membrane of the



superior portion of the large intestine, gave rise to irritation, pain, and tenesmus; and that purgatives by removing these fæcal matters were often of great utility in this disease.

M. Boinet, five or six years since, having been called, in consultation, to a lady who had suffered for two years with a voluminous tumor in the abdomen, which had been diagnosticated and treated unsuccessfully as an ovarian cyst, found in the left iliac fossa, a tumor which was hard, lumpy, and without fluctuation. He found that pressure upon this tumor altered its form, the impressing force causing an indentation, from which, and from a vaginal examination, he concluded that this was a stercoral tumor. He prescribed senna and sulphate of soda, which produced enormous evacuations of fæcal matter. The tumor and pains subsided. The patient was altogether cachectic, and subsequently had a cancer of the breast which was extirpated.

The following case, taken from *Ranking's Abstract*, will serve still further to illustrate the practical aims of the preceding observations:

*Case of Prolonged Constipation:*—By MR. GAY. *Pathological Transactions*, vol. v.—This case was exhibited before the Pathological Society on the 18th of October, 1854, as showing the connection between the constipation and an attack of dysentery, and as proving that persistent and complete constipation of the bowels might exist for four months without occasioning any interruption to the processes essential to the general function of nutrition.

*Case.*—A lad, æt. 7, of healthy appearance, was admitted into the Royal Free Hospital, in July, 1853. Four years ago he had an attack of typhus fever, accompanied with abdominal tenderness and dysentery.

On recovering, his bowels became so exceedingly torpid, that it was necessary to administer strong purgatives or enemata, in order to procure any evacuation from them. This torpor gradually increased, so that after about two years these means failed of having any effect whatever. *During the three months prior to his admission, nothing whatever passed from his bowels;* and he was accordingly sent from Rochford to Mr. Hogg, of Finsbury, and he then came under Mr. Gay's care. Notwithstanding this condition of the bowels, his health had not apparently suffered in the least degree; his appetite had in no respect failed him; nor had he been sick but on one or two occasions, and then in consequence of his having taken unwholesome food. His body, however, had gradually enlarged—to the size of forty-nine inches in girth; but without material inconvenience to his respiratory organs. On examining the abdomen, it was found to be uniformly very tense; the recti muscles were rigid, but the oblique and transversales, espe-

cially on the left side, were flaccid, and had evidently yielded more passively than the recti, to the distension within. Along the left side there was a considerable prominence or broad ridge, corresponding to an enlarged descending colon, and its sigmoid flexure. Just below the navel a portion of intestine had protruded, apparently through a rent in the linea alba; it could be reduced, but not retained within the abdominal walls. The abdomen was in parts (varying, as was afterwards found, from day to day) resonant on percussion, but, for the most part, dull; and on palpation distinctly gave an impression as though it was distended with solid lumpy matter. A series of remedies were used, but without effect; and it was not until after the expiration of three weeks that any fæcal matter was obtained from the bowels; and then only by the following means: A speculum was passed into the rectum; and, after dilating the sphincter, the tube of an enema syringe was passed high up into the bowel, and its contents washed out by a stream of warm water, which was kept continuously playing upon them for the space often-nearly half an hour. The distension of the sphincter seemed to excite peristaltic action, and thus materially to assist in dislodging the contents of the bowel.

A large quantity of fæcal matter, hard and black, and much resembling common cinders in appearance, was by this means brought away. This operation has now been repeated several times with similar results, and with the effect of reducing the size of the abdomen to that of twenty-six inches in circumference.

At present, on passing the tube into the bowel, there is little doubt but that it enters a capacious and tolerably flaccid sac; and that this sac is formed by a distended and, in all probability, a palsied condition of the descending colon, and its sigmoid flexure. A bandage is kept constantly applied around the abdomen; the confection of black pepper, aloetic purgatives, strychnia, and other remedies, have also been given, but as yet no spontaneous effort whatever has been at any time made by the bowels to relieve themselves. The urine has been constantly of a deep color, of a high specific gravity, and laden with lithic acid and lithates.

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ART. VIII.—*Practical Remarks on Yellow Fever, with special reference to the Treatment:* By EDWARD JENNER COXE, M. D.; Visiting Physician, Charity Hospital, New Orleans.

VIEWED in all its bearings, who can question, that to the city of New Orleans, as to her more rapid permanent prosperity, to her inhabitants, more especially those unprotected by acclimation, the subject of Yellow Fever, and the treatment most likely to prove generally successful, are matters of vital interest? Notwithstanding the extensive re-

search and consequent voluminous works upon this subject, by the most able of the profession, in present and past days, may it not fairly be questioned, whether some important points, referring to the cause or causes, the origin, and mode of communication of the disease, are better understood at this day, than was the case with those who first grappled with such abstract, and most probably incomprehensible mysteries? An attentive perusal of the unapproachable work of Dr. La Roche, on Yellow Fever, will substantiate such an opinion, for who, after its careful study, will acknowledge, that upon any one of the many important points, therein so ably handled, he has been enabled to arrive at a positive conclusion? and will he not have to confess that an array of talent has been expended upon both sides of the questions? Upon many of the debatable points, an equal amount of interest may not be felt, but on the most important part of the subject, the treatment, in which all are equally interested, there will not be found a greater unanimity of opinion, whether regard be had to the general course to be pursued, or the individual remedies specified.

Who, unacquainted with the last part of the subject, practically, would be able to decide upon the merit due to, or the confidence to be reposed in, any one of the means, so fully set forth? Who can decide, when Doctors disagree? would be the most natural exclamation. Should the idea suggested by Dr. Nott, of Mobile, and acquiesced in by others, that a visitation of this fever may possibly be inflicted upon some of the northern cities, be realized, there can be no doubt that those upon whom the duty will devolve to combat the unwelcome visitor, may not find these remarks valueless.

Engaged in a business for sixteen years, to which by continued ill health I was forced, but few have elapsed, since I was enabled to resume the practice of my chosen profession, and it was on the occasion of the philanthropic visit of my friend Dr. Fenner, to assist in alleviating the sad scenes at Norfolk, that in taking charge of his wards, I had an opportunity of seeing Yellow Fever upon an extended scale. With thanks to the administrators of the Charity Hospital I have since that time enjoyed the advantage of attending to the sick, in the wards under my charge, and during the recent epidemic, I paid a morning and afternoon visit from the 20th of July, until within a few days, when the necessity no longer existed. During that period, there were brought into my wards, 32 and 33, one hundred and ninety-two

cases of well-marked yellow fever, necessarily embracing every degree of violence; ninety-nine cases were in ward 33, and ninety-three in ward 32. In ward 33, sixty-two were cured, and thirty-seven died. In ward 32, fifty-five were cured, and thirty-eight died: 192 cases; 117 cured; 75 died.

From the number of deaths, I think I am entitled to abstract two who died within an hour of their entrance, after having thrown off a large quantity of black vomit, which, in one case, was forcibly ejected a distance of about seven feet from the bed.

Deduct about thirty who died within fifty hours from the time of entering, when from the first moment the faintest hope of averting a fatal issue could not exist, my opinion is that I have no reason to complain of the result, although, in reality, I am far from being satisfied. Whether the results of such an opportunity of combatting the most insidious and treacherous disease, that it is possible to encounter, will effect any improvement, or greater certainty, as to a correct course of treatment, to be depended upon, in future times, should such occur, it is not for me to determine. I fulfill a duty; others must judge of the result, and the means employed, to be fully noticed in a future article.

There are several points of no little interest about this disease, frequently alluded to, upon the recurrence of an epidemic, which it may not be amiss to notice. Is yellow fever contagious? It appears to me, after a due consideration of the facts and observations deduced therefrom, to be found most impartially collected in the able work of Dr. La Roche, as well as from what has actually occurred in this city, bearing upon that point, I cannot but believe that it is not. By this I mean, that one or more persons, with the disease on them in full force, at any stage, if removed to a healthy spot, without clothing, goods, or chattles, this disease cannot be communicated to any one with whom they may come in contact, however closely or faithfully nursed. Again. That this disease is capable of being communicated, or in some manner, transmitted to other points, from that infected must be admitted, nor can the many strange or remarkable facts, developed during our past severe epidemics, be in any respect understood or explained, without such an admission.

In what manner, such duly recognized effects have been produced, has proved an enigma to abler heads than mine, satisfactorily to ac-

count for ; therefore, on that point, perfect ignorance is candidly admitted. Also, to my mind, it is considered clearly proved that yellow fever is of local origin, although, upon that point, all are aware of the extreme difference of opinion, maintained by opposite parties, which it is reasonable to suppose, never can or will be reconciled. That the quarantine, for the purpose of preventing the ingress of yellow fever into our city, has signally failed, and the same it is well known has occurred at other points, is considered proved. While admitting the propriety and necessity of having proper sanitary measures, legally established, as well for internal or common, as external and rare sources of disease, I cannot but believe, that, as long as contagionists, and anti-contagionists, so strenuously insist upon their respective views, or opinions, just so long will a rational or common sense compromise, be withheld from the community.

In reference to the possibility of establishing a course of treatment adapted to the ordinary run of cases, which might per chance meet the approbation of the profession, if such a marvel could occur, I venture the opinion, that if all, or the greater part of the physicians of New Orleans, who for the last thirty years have enjoyed such ample opportunities of treating the fever, had given true reports of their successful, as well as unsuccessful cases, it being well known, that the latter are frequently even more instructive than the former, there can scarcely be a question of the possibility, that the essentials of the treatment, would have become well established, and in all probability, the successful management of the disease more certain. Is this suggestion unworthy of serious consideration ?

Appertaining to the treatment of yellow fever, there are two subjects, according to my views of the nature of the disease, not only of great importance, but most intimately connected with whatever general plan of proceeding may be considered best calculated to effect a cure. I allude to the amount of covering, or number of blankets to be used, with regard to the present comfort and future safety of the patient. The other is, what amount of fresh air should be allowed ?

I know, and all must agree in opinion, that in treating any other febrile disease, in which inflammation does not exist, or at least to any great degree, *the feelings of the patient are, and ought to be the only guide.* A departure from that universal rule, would be a transgres-

sion of the laws of nature, which cannot be done with impunity, present or future. Upon what principle the very reverse should be acted upon in the yellow fever, one in which the heat of the skin or fever continues gradually but regularly to increase for a certain time, passes my comprehension and cannot be rationally accounted for.

The facts, proved by the innumerable experiments of our learned *confrère* Dr. B. Dowler, are conclusive on the subject of animal heat. *The only guide should be the feeling of comfort of every individual patient*, for in that respect, as in all others in connection with the human system, do not all differ? They are the sufferers, and in such a case, in my opinion, they ought to be permitted to make themselves *comfortable*.

If regarded medically, as to the absolute necessity of keeping up a profuse sweat, which is not necessary to effect a cure, for although I know and act upon the principle of maintaining a healthy condition of the skin, and resort to various means for such an object, still I maintain that to cure this disease profuse sweating is not indispensable; on the contrary, in my belief, much more frequently than is thought to be the case, the unnatural habit of piling on blankets, not only does not effect what is desired, but may prevent it, although the sweat may wet all the clothing of the body, as of the bed. Tucked around the body, scarcely allowing the head to be free, how can the *evaporation go on*, which would diminish the febrile heat, as well as that unusual feeling of hot skin, peculiar to the disease, with an amount of fever scarcely appreciable? Besides, if it is wished to eliminate the poison from the blood by the efforts to cause sweating, how can it escape from the body, seeing that it is all covered with a load of blankets? The power of absorption by the skin, who questions? and will not, and does not the poisonous exhalation from the skin, which you will not let escape, become reabsorbed, and play a very important part in transferring upon the stomach and kidneys an increased poison, very likely to eventuate in black vomit, increased irritability of stomach, or suppression or retention of urine, and what then? Sufficient for the present about an irrational and destructive prejudice.

As regards the *admission of fresh air*, which cannot, under ordinary circumstances, be too much indulged in, or enjoyed, I would remark, that when the body is in a free perspiration no one, in his senses, would

advise it to be exposed to a draught; for who has not felt the ill-effects from it in health? But, if the animal heat, or temperature of the body, in a state of fever is beyond the natural point, feeling hot and dry, no cold or cool air, will then do harm. On the contrary, under those circumstances, a few buckets of cold water thrown over the body, if the patient then goes to bed, and covers himself with one blanket, he will be almost certain to have a free sweat induced. Protect from a draught, and give fresh air in the room, and around the bed; and make your patient comfortable, and he is on the road to a cure.

If the observations made this season, if the coincidence of opinion with many of my *confrères*, with whom I have conversed, if the fact of several deaths, (resulting, as I am firmly convinced, from blankets in excess), if, I repeat, I am correct in the above, and I am confident of being so, how could I conscientiously omit making the above remarks about *clothing and fresh air*?

Although at the risk of encroaching too largely upon your pages, I must say that when I consider the size of my wards, the number of windows, the two doors well placed for a fine draught of air, most generally open, when I think of the moderate amount of clothing permitted during the first half of the epidemic, and the absolute order in both wards, after having become, from painful experience, convinced that *excessive covering would kill*, when other circumstances fully warranted the hope for success, that but one sheet and one thin blanket should be permitted upon any patient having the fever, unless any one might complain of *feeling cool or cold*, when another might be put on. Besides, I took care to tell every patient, that if he felt too warm, and wanted the blanket off, off it might go, and no one interfere. In no one instance did the least injury result.

Of the 117 cures out of 192 cases of yellow fever, the following number were discharged on the days specified, namely: 3 the third day; 7, 4th; 14, 5th; 8, 6th; 14, 7th; 12, 8th; 7, 9th; 10, 10th; 2, 11th; 7, 12th; 4, 13th; 6, 14th; 2, 15th; 2, 16th; 4, 17th; 3, 18th; 1, 19th; 2, 20th; 1, 24th; 1, 25th; 1, 27th; 1, 31st.

Of the 75 deaths, the following number died on the days specified: 15, 1st day; 10, 2d; 16, 3d; 8, 4th; 15, 5th; 2, 6th; 3, 7th; 2, 8th; 2, 10th; 1, 13th; 1, 21st.

Some of the above cures and deaths were extremely interesting cases, the particulars of which I shall relate in one of the articles in continuation of the subject.

(*To be Continued.*)

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ART. IX.—*Sporadic Case of Cholera.—Cure.*

On the night of the 12th of September, 1858, I was sent for to see Frederick Struton (a distance of eight miles) who was suffering, as the messenger informed me, very severely from cramp, vomiting and diarrhœa.

He was taken sick about 5 o'clock, P. M., and I did not see him until 2, A. M., when he had passed through the first and second stages. His features were pinched and shrunken like a corpse; extremities cold; pulseless, and bathed in profuse perspiration and the characteristic rice-water discharges were passing off every twenty or thirty minutes. I thought the man would very soon be dead, as did several of his friends; and as his whole desire seemed to be cold water, I gave it fresh from the cistern, a tumbler full at a time, as often as he called for it. The first five tumblers of water were thrown up, but the sixth one was kept down and gave immediate relief to the intense burning, which is always so annoying in cholera. I continued to give the cold water, and the patient continued to improve; first the stomach was relieved from the fire that seemed to burn within; then followed a cessation of discharges from the bowels, and next the cramps gave way, while the extremities gradually grew warmer. In two hours my patient was out of danger. Very much emaciated and a good deal nervous, I prescribed for him  $\frac{1}{2}$  grain sulphate morphia, which gave him a nap of six hours, when he woke up hungry. For a couple of days he was allowed to have tea and toast, corn-meal gruel, and light soups. The man is now in your city, and will, in all probability, add one more to your daily list of deaths.

BENNET DOWLER: Dear Sir,—I send you the above case, because it was a bad one, and yielded to a very simple course of treatment; that I found laid down in your journal, by Dr. Pinckard.

This is the first article that I have ever offered for the press, and it



is with no small amount of diffidence that I send it, to be done with as you see fit.

Respectfully yours,

ROBT. G. RICHARDSON.

Port Gibson, October 5th, 1858.

P. S. My little son was severely attacked with cholera morbus. I gave him blue mass, paregoric, prep. chalk, and gum arabic, without checking the disease. He cried constantly for water, I gave it freely. The effect was speedy relief.

ART. X.—*Pulmonary Tuberculosis*: By WARREN STONE, M.D., Professor of Surgery in the Medical Department of the University of Louisiana.

OUR knowledge of tubercular diseases, particularly of the lungs, is very thorough and very correct. After the disease is fairly established, we can, from the physical signs alone, point out with great accuracy, the pathological condition of the lung, and mark its progress from day to day; but all this enables us to do but little more than to pronounce with more certainty the sad fate of the patient. We can, to be sure, with our improved knowledge of the proper therapeutic agents, prolong life in many cases; arrest the progress of the disease in a few cases; and effect cures in a very few favorable and particular cases. But this is not making the utmost use of our knowledge: for, if the same agents that will arrest the progress of the disease, are applied and brought properly to bear, in due time, they will prevent the disease altogether. Phthisis pulmonalis generally makes its approach slowly and insidiously; but then, it always casts its shadow before, and the close observer is able to detect it. A large number of the cases of phthisis inherit the diathesis; and so well can this fact be known, that there is no necessity for any such to be taken by surprise. There is, however, a strange tendency on the part of such subjects to deceive themselves, and to lull themselves into a feeling of security, and to do improper things in a spirit of bravado; but, by submitting to proper discipline, they should acknowledge a delicacy of system. Physicians too often aid in this deception, sometimes ignorantly, and sometimes knowingly, but with kind motives: but, I think, with mistaken kindness.

No one can be induced to submit to discipline, and to make sacrifices, unless he is persuaded of the necessity of it. The subjects of this disease are generally hopeful; but really as much in love with life, as the desponding hypochondriac; and only require to know their true condition, to be persuaded to adopt any course that is required. Most families have their medical adviser, and it is his duty to know the distresses and constitutional tendencies of his constituents, and if this duty is strictly performed, and all parties properly instructed, most cases of phthisis ought to be detected in the very incipiency, or even before any tubercular deposits have taken place. Tubercular deposition is not the first of the morbid changes that take place in the beginning of phthisis; this process does not begin while the subject is in health, but is secondary to other changes which produce a visible change in the system. A change in the color is the first to be observed, showing imperfect assimilation, emaciation soon follows, and the pulse becomes frequent, much more so than is found in any other state of ill-health, unattended with fever. There are generally other corroborating symptoms, but if these leading ones are present, it is folly to delay until auscultation reads by the usual signs the presence of tubercle in the lungs. I feel confident that the changes which precede tubercle can be, in most cases, readily detected. In cases where the diathesis is acquired by sedentary habits, improper diet, confinement in a vitiated atmosphere, or by any or all the causes which tend to lessen the vital action, and disturb the assimilation. I think, the approach of the disease is not so insidious, that is, the threatening symptoms are more or less apparent, before tubercular deposits actually take place.

In addition to the unhealthy hue of skin and frequency of pulse, and emaciation, the throat is often affected; sometimes there is hoarseness, sometimes there is thickening of the mucous membrane of the fauces, and more frequently there is a kind of softening, which I have been in the habit of calling scrofulous softening, which is attended with a secretion of viscid pasty mucus which causes the patient to make frequent attempts to clear the throat. The nature of these constitutional affections of the throat are often misunderstood. They are too often treated as local diseases, and treated by clipping, trimming, and cauterizing. These patients are not unfrequently a little hypochondriacal, and if they have ever been exposed to be tainted with syphilis, are induced to attribute the disease to it, and it is difficult to persuade

them to the contrary, or keep them from adopting an injurious treatment. I believe that improper treatment of syphilis often brings about this affection of the throat indirectly, by impairing the health and impoverishing the blood.

But it is not my intention to write a dissertation upon the minute and special symptoms of this disease, but to make some general remarks upon its treatment, and to urge the importance of making an early diagnosis, and of resorting to the proper remedies, before the mischief is done, and while the prospect is fair that the patient will be rewarded for the sacrifices he may make.

As soon as it is determined that the tubercular diathesis exists and that the changes are taking place, which render it almost certain, that phthisis is about to be developed, we should urge the proper means to counteract it with as much earnestness as we would urge the free use of the cautery to the bite of a rabid animal. The one possibly might not have phthisis, and the other might not have hydrophobia, but it would be equal folly to neglect the very sure antidote.

In this diathesis, or in the state in which tubercular deposits take place, it is now well known that there is a deficiency of corpuscles, and an excess of albumen in the blood, and some other deviations from the normal proportions of the healthy constituents of the blood, and that certain articles of diet supply this defect in a remarkable degree. There is, however, a radical defect in the digestion and appropriation of the food, as the first cause, or (as Churchill believes) there may be some defect in innervation or in nerve force, which is the cause of all; and that, it is more important to attend to this than to the quality of the food. There certainly has been too much reliance placed upon cod-liver oil, porter, and other stimulants, and too little to pure, bracing air; active exercise; cheerful occupation of the mind; and such remedies as may be required for the digestive functions. Cod oil is very valuable when there is much poverty of the blood, as it is assimilated more readily than anything else, and more readily supplies the deficient corpuscles by a judicious use of it, so much nutriment may be supplied, in addition to what the natural powers of the system will supply. It scarcely requires the process of the first, or gastric digestion, and if taken two hours or more after the regular meals, it passes from the stomach, with the chyme without taxing its powers, and is readily taken up. This is best administered in a little porter, as it

disguises the taste and in a large majority of cases, not only agrees with the patient, but seems to be of service of itself. Most phthical patients, however, have been placed under unfavorable circumstances, have not had proper exercise, diet and occupation, and it is unreasonable to expect of cod oil, with the addition of stimulants and the best selected diet, more than temporary relief, unless all the unfavorable circumstances that tended to develop the disease, are changed or done away with. Experience has shown, that steady and persistent exercise in the open air, prompted by some laudable and satisfactory purpose, do more towards restoring the appetite and digestion, and invigorating the system, in a word overcoming the diathesis, than any one thing besides.

Two years ago last June a gentleman who called on me for advice, was about to visit some of the springs for his health. He had been closely confined to the desk for several years, and led a solitary bachelor's life. Upon examination, I found a cavity in the upper part of one of his lungs, but the tubercular deposit seemed to be circumscribed. He was emaciated, and had a pulse of a hundred and ten to the minute. On inquiry I found he had friends in Virginia and Kentucky, and I advised him to get a horse and make his tour on horseback. I saw no more of him for two years, when he called on me for advice for some other complaint; but I would not have known him, if he had not called my attention to his case, and asked me to examine his chest. The cavity had closed, and there was a sinking of the ribs over the seat of it. His general health was very good. He told me that he followed my advice; bought a horse in Alabama, rode over the mountains to Virginia, and rode daily among his friends, and recrossed the mountains into Kentucky, and in all he spent five months in active exercise, and with the pleasant association of his friends. I had advised him, that if exercise and change did not soon improve his appetite, to take cod liver oil for a time; but he said he had no occasion for it, and his cure was entirely due to the radical change in his habits and mode of living.

Great importance is placed upon climate by many, and there can be no doubt, but some climates are preferable to others; but there is much less difference than many suppose. In advising a climate, I first inquire, where the patient will have the happiest associations and the best means for regular exercise in connection with some pleasant pursuit; and if there is no serious objection, I select this place; but

if I can choose the temperature, I always select a moderately cold one. The main objection to a very cold climate is that the patient may not always be able to take out-door exercise. Phthisical patients who have had the commencement of the disease in this section, almost always improve by spending their winters in Tennessee and Kentucky, provided the disease is not far advanced, and the patient is able to exercise freely. It is a long time since I have recommended a patient to any of the tropical islands. The steady heat of the climate is injurious, and there is but little advantage in the monotonous equanimity of temperature. I verily believe that changes of temperature are of an advantage, provided patients guard against the immediate effect. My situation here in New Orleans, at the outlet of the Great West, has given me an opportunity of witnessing the sad effects of sending patients in the second and third stages of phthisis, from the comforts of home to those hot and debilitating climes. A dry, bracing atmosphere is best, and it is none the worse for being pretty cold if the patient can exercise with comfort in the open air. Some will bear a lower temperature than others, determined mainly by the stage of the disease and habits of the patient.

Intimately connected with climate and temperature, are the habiliments. Most persons of this class clothe too heavily and sleep in too confined rooms, and by this means aid much in bringing about what they labor so assiduously to avoid, that is, frequent catarrhs, and besides creating a susceptibility to cold, debilitate themselves by an undue weight of clothing and heated and confined air. The clothing should be no warmer than is required under exercise in ordinary weather in the open air. For riding or cold changes it is better to use a temporary dress for the occasion. To derive a tonic effect from cold air we must feel it, and react under it by exercise; and to enable ourselves to feel cold with impunity we must accustom ourselves to it. There is a general injurious habit, of clothing the chest warmer than the other parts of the body. The chest from its central position requires naturally less protection than the extremities, and it is my experience that this acquired habit of wrapping it up in furs and plasters, rather tend to invite cold and catarrhs than to keep them off. It is well known now, that those who are liable to sore throat from changes of the weather, and have been in the habit of clothing the neck warmly, become entirely exempt, by going with the neck bare in all weather. The chest, however, cannot be treated like the neck, but care should be

taken not to dress generally too warmly, and to dress the chest at most, only in proportion to the rest of the body. Any one who has not made the observation, will be surprised at the power of adaptation in the human system, and how far it can be accustomed to exposure, without injury.

Great advantage may be derived from this habituation, for it allows us the full benefit of the tonic influence of cold, and to escape all ill effects from the thousand unavoidable exposures, which would injure the delicate and pampered invalid. I do not underrate the danger of catarrh or cold, but I insist that, the course usually pursued to avoid it, is the very course calculated to render us liable to it. But enough has been said upon this point.

The means that have been mentioned in general terms, if rightly employed, will ward off threatened phthisis in a large majority of cases, and restore the system to its original vigor. The general fault, both with physicians and patients is, to rely too exclusively upon some one popular remedy. Cod liver oil is probably the most valuable remedy we have for immediately supplying the deficiencies of the blood, but its effect will be but temporary, unless it is accompanied, and followed up, by all the other means that are calculated to give tone and vigor to the system, and enable it to furnish healthy supplies to the blood from the customary food. It is folly to rely upon medicine to cure permanently the diseases resulting from our violations of the laws of health, unless we change and live in obedience to the laws of health. The oil is simply a nutriment, and is valuable because it will be appropriated and supply the deficiencies of the blood when ordinary nutriment would not, and thereby relieve the patient of the immediate danger of tubercular deposits; or, it may arrest the deposit when it is going on. This is all the oil can do, and it is quite useless to continue its use under all circumstances, and as long as there may be the appearance of illness. It has its specific object, and when that is accomplished and the patient can take and appropriate more palatable food, it will do as well as the oil. The cod liver oil may be aided by many things that tend to improve the assimilating powers. Iron is often indicated, and of the preparations of this metal, I prefer in most cases, the muriated tincture; but of this I will speak in another place.

In some cases I feel confident of the good effects of the phosphate of lime, on the principle laid down by Burke. He asserts that this

salt is deficient in the blood in the tubercular diathesis, and it is well known it is essential to healthy cell-formation in all organizations. But this salt probably will be entirely superceded by the hypophosphite of lime which has been brought into notice by Dr. Francis Churchill of Paris. Dr. Churchill considers, however, that the virtues are entirely due to the hypophosphoric acid in the salt, and he is using now, the hypophosphite of soda, which he seems to think preferable to the lime. I had some interviews with Dr. Churchill last August, and visited his dispensary, and what I saw and learned from him, led me to believe that this remedy will prove highly useful. He believes that it acts as a stimulant to the nervous system, relieving the disordered nutrition and promoting nutritive action. It may not be profitable to speculate upon what cannot be well demonstrated, but we know whatever tends to depress the nerve force, is an exciting cause of this disease; and if this remedy tends to rally this force, and give vigor to it, it must be useful.

Churchill asserts that patients feel the invigorating influence, generally, as an immediate effect, and after its use a little time, the effect is shown upon nutrition by increase of color in the tongue, lips; and generally, there is some puffiness about the eyes. The catamenial discharge, which had been suppressed often, returns, and slight hæmorrhage from some part of the mucous surface not unfrequently takes place. In this case he lessens the dose or withholds its use for a short time, and then resumes its use in smaller doses. The dose is twenty grains in the morning for a medium, but it is varied according to circumstances. Dr. Bigelow, a very intelligent American physician of Paris, has had some experience in its use, and he prefers to give it in smaller doses, but twice or three times a day. I do not know why Churchill prefers the soda-salt, for on careful inquiry, I find there is no difference in the two salts in the amount of hypophosphoric acid. As the soda salt is deliquescent and the lime salt is a permanent one, the latter is preferable for this reason, independent of any good effect which the base might have. But if too much is expected of this remedy by those who may try it, they will be disappointed and probably throw it aside. The truth is, but few medicines afford such uniform and decided good effects, as to maintain general favor under the abuse which all valuable medicines will ever receive. Every one is familiar with the safety-lamp of Sir H. Davy. It was hailed as a great discovery, and the English Government gave him a

handsome pension for it. Miners could enter their mines without the danger of explosions and death by fire; but recent investigations by a Committee from Parliament have shown that the deaths in the mines have greatly increased since this useful discovery; for being secure against fire, the miners go boldly into the mines and die of suffocation.

The profession will pardon me, I hope, and not rank me among the disciples of Sir John Forbes, if I say that I think that some of our most valuable medicines have exercised as unfortunate an influence on human life as the safety-lamp of Sir H. Davy. These hypophosphites, however, are not open to the objections that might be urged against active remedies, and if they have any virtue, the real amount ought to be ascertained, as well as their mode of action, so that they may have their proper place in our *Materia Medica*.

But I will again urge the importance of employing all the agents that tend to overcome this diathesis and not to depend upon any one thing alone. Since writing the above, an intelligent young student called on me, who had been sent to this city from St. Louis, and is now leading a life of close confinement in the college at Boulogny. He has had slight hæmorrhage from his lungs, has emaciated, is of a sallow pale color, has a pulse of a hundred and ten in a minute. Upon exposing his chest for examination, I found he had on two flannel shirts, one cotton shirt, and a linen one over them all. These shirts were all damp with perspiration. He said he could not bear exposure, but acknowledged that flannel was uncomfortable to him until he had acquired the habit of wearing it. This patient, as might have been expected, had no appetite, but was faithfully forcing down cod liver oil, according to advice. This is a fair sample of the management of the cases that consult me, though it may be a little extreme. What chance has cod liver oil to be of permanent use under such disadvantages? or anything else? I advised the continuance of the oil and advised him to make any sacrifice to obtain active exercise in the open air, to get rid of most of his flannel, to accustom himself to the influence of cold as fast as possible, and to expose himself to the cold and maintain animal heat by exertion. To get accustomed to cold, I suggested that he should wash his chest with alcohol or cold water, in the morning, rubbing with a coarse towel briskly; dress immediately, and if necessary make some active exertion so as to secure reaction. This can be done so as never to give cold. Cold bracing atmosphere



has a better influence upon the nervous system, if it is properly made use of, than any tonic that can be given, and no doubt, is better than the hypophosphites, which Churehull gives for a similar purpose, (if I understand his views).

The same error with regard to exercise exists, as of the influence of cold. When patients are delicate and must be bundled up and rendered more delicate, they are feeble and must lay about and become daily more feeble, when the truth is, this class of patients not only profit by free exercise but by absolute hardship, provided the moral energy keep pace with the physical exertion. The general rules here laid down to ward off phthisis are applicable to the disease in any stage when there is any prospect of doing anything.

There is one thing, however, that deserves notice, and that is the use of cough mixtures; they ought to be stricken from the list of remedies in phthisis. There is no class of nostrums that do half the mischief, that the cough mixtures and pectoral syrups that are so freely patented, and sold in every shop. All these mixtures contain something to nauseate, which is called expectorant, and opium in some form, which serves for the time to kill the cough. But there is not one of them that does not indirectly do harm, by diminishing the appetite and increasing the tendency to sweat. It is remarkable that if the appetite and digestion improve, the cough subsides; and the reverse. Cough medicine taken freely may put off the cough for the time, but I am satisfied from experiments I have made, that those who take nothing, cough less in the whole course of their disease, than those who are furnished with the best cough mixtures through all their variations.

Patients are sometimes so much disturbed at night that they cannot get the proper amount of sleep and it is occasionally proper to give something, and I prefer a plain opiate; but take great care that the patient do not form a habit of taking it. A few drops of Battley's sedative, or a little paregoric will do much less injury to the stomach than cherry pectoral, and do quite as much good. The truth is, patients get in the habit of taking cough mixtures, more for the effect of the opium they contain, than from any other cause.

In a former page I mentioned the muriated tincture of iron, and I think there are certain cases in which it is particularly effective. Occasionally the first symptom of phthisis is hæmorrhage more or less severe, and it is not unfrequently that these cases progress ra-

pidly to a fatal termination. This preparation of iron not only has more effect in arresting the hæmorrhage than any other astringent I know of, but I think it has a great effect in overcoming the erythema of the tissue of the lung, and prevents the rapid tubercular deposit that sometimes takes place. There is, no doubt, in these cases, some previous tubercular deposit, but the immediate cause of the hæmorrhage is a kind of erythema of the lung which leads to engorgement, hæmorrhage, and sometimes to rapid tuberculisation, and occasionally exudation of croupous lymph, and rapid softening or disintegration. The effect of this preparation upon the capillary circulation in this erythematous or semi-inflammatory condition, is shown by its effects in certain forms of erysipelas, when it is given freely. I saw it used in the Charity Hospital twenty-five years ago in an accidental prescription, that is, the physician who gave it evidently had no definite idea of its effects, as he never prescribed it again to my knowledge; but the effect was so remarkable that it attracted my attention, and I followed up its use in analogous cases, and I am satisfied that it has a strong effect not only in checking hæmorrhage, but in correcting the condition upon which it depends, and preventing tubercular deposit.

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## PROGRESS OF MEDICINE.

ART. I.—*Therapeutics*: Selections from Foreign Medical Journals.  
Condensed by J. P. BARBOT, M. D.

i. *Inefficacy of the Hypophosphites of Lime and Soda in the treatment of Phthisis Pulmonalis.*

IN the *Journal de Pharmacie* of February, 1858, we find an extract from a memoir of M. Vigla, who, having seen that M. Trousseau, and others, had been unsuccessful in their treatment of consumption by the hypophosphites, resolved to give them a fair and extensive trial. M. Vigla used hypophosphites manufactured expressly for his hospital patients by the respectable house of Menier & Co., and tried them on twenty patients without the slightest benefit. Two patients in private practice were ordered to procure the salts from the Chemist Robiquet himself. Consequently, there can be no doubt as to the

purity of the hypophosphites used. M. Vigla took, besides, special pains to see that the medicine was regularly and properly taken. There was no selection of patients either, but they were taken indiscriminately, as soon as auscultation and percussion had placed beyond a doubt, the existence of a tubercular diathesis. The condition of the patient and the effects of the remedy were carefully and regularly noted down, in the presence of all the students, who besides, attentively watched the cases, to note down the results of M. Vigla's experiments. These results were entirely negative: there was no improvement in the patient, no modification of any of the various symptoms of this fearful malady. It did not, in any manner, allay the cough, pains in the chest, hectic fever, and wakefulness; did not check the diarrhœa, the vomiting or emaciation.

As these results confirm the experiments of M. Trousseau and others, M. Vigla thinks he is justified in pronouncing the hypophosphites of lime and soda totally inefficient in the treatment of phthisis pulmonalis.

ii. *On the use of Collodion in causing the nipples of suckling women, when flat or sunken, to become prominent.*

Whenever, either from a natural want of development of the nipple, or from a neglect to prepare it during gestation, for the function of giving suck, the nipple seems to disappear under the influence of the lacteal tumefaction, and the child is unable to take the breast, the following method will remove the difficulty. Cover the cuticle around the nipple with collodion, for the space of four *centimetres*. The compression produced by its retraction will be found sufficient to permit the infant to suck even when it at first seemed impossible. The suction by the child, and consequent thereupon, the reduction of the mammary tumefaction will ensure its restoration to the physiological condition. Care must be taken however, not to cover the internal half of the areola with collodion. (*Med. Zeitung Rugl. and Rev. de Thérap. juillet.*)

iii. *Iodine as a cure for Corns.*

Drs. Varges and Wager recommend the tincture of iodine as a cure for corns. It is applied by means of a camel-hair pencil, several times daily, till a cure is effected. After each application, the pains diminish, the cuticle becomes supple, and the callosity is greatly diminished. When the corns are between the toes, a mixture of equal

parts of the tincture of iodine and of glycerine, applied by means of a piece of punk or a dossil of lint, night and morning, will be found sufficient. (*Zeit. für Med. Ch. and Revue de Thér.*, July.)

iv. *Treatment of Measles by Lardaceous Frictions.*

Some time ago, Dr. Schneemann, recommended frictions with large pieces of fat bacon, warmed and incised, all over the body excepting the head, several times a day, as a treatment of scarlatina. The good effects of this method of treatment induced several practitioners, justly celebrated for their treatment in diseases of children, namely, Mauthner, of Vienna, Ebert, of Berlin, Walz, of St. Petersburg, who had seen its good effects in scarlatina, to try it in rubeöla. Dr. Cornaz, of Neufchatel, in Switzerland, has also tried it in an epidemic of measles in the latter city. These gentlemen are all convinced of its great benefit and efficacy in the treatment of rubeöla. Statistics are published which we omit. (*Echo Medical de Neufchatel*, July.)

v. *Is Copper a Poison?*

Long since, Rademacher had spoken with contempt of the belief that copper was a poison. It was really disgraceful to students of medicine, he said, that they believed in this falsehood, when it was so easy and safe to disprove it, by experimenting on themselves.

Dr. Toussaint has made numerous experiments on this subject, in Königsberg, in Prussia, on himself as well as on hospital patients, and in a voluminous memoir, in which he enters into the minutest details, he publishes results confirming Rademacher's opinion.

From these experiments which are in the highest degree interesting and important, it appears: 1st. That neither metallic copper, its black oxyde, nor its sulphuret will disturb the general health, not even the ammoniated hydrochlorate, in twenty drop doses in Koëchlin's liquor. 2nd. That the ammoniated sulphate of copper in seven grammes; the iodide of copper in eight gram., the phosphate in ten gram., the carbonate in 10 gram., the acetate in 14 gram., and the nitrate in 14 gram. doses, will at first cause vomiting, but that much larger quantities of each, can be taken daily, in broken doses, without any unpleasant symptoms. 3rdly. That the food that is used while taking the salts of copper exerts no influence upon them. 4thly. That none of the salts of copper, whether soluble or not, can be traced in the urine. 5thly. That he never saw any of the symptoms, described in all the books, as witnessed after the long continued use of

the cupric preparations: as a blue circle under the eyes, tenderness on pressure on the abdomen, frequent vomiting, well-marked febrile action, etc., etc.

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Is there a copper-colic, as well as a lead-colic? Do we meet with cases of chronic poisoning by copper? are two questions not yet fully resolved. French and English authors have described cases of that kind, and German authors have republished them. Those physicians who do not seek for their facts in books, but rely upon their long practice and experience do not recognize any chronic poisoning by copper or copper colic; they maintain on the contrary, that copper manufacturers are remarkable above all others for their good health. Rademacher, Burg, Audonard, Pietra Santa, and Toussaint, hold the latter opinion.

Dr. Toussaint says, that whenever the workmen in the copper works or manufactories are attacked with the symptoms usually described as produced by poisoning by copper, these symptoms are due: 1st, to exposure to cold; 2nd, to intestinal disorders produced by mechanical obstructions such as are common to workmen in other metals and trades; 3d, to the metals often found combined with copper, such as lead, zinc, arsenic, etc., etc.

When the metallic nature of copper and its high specific gravity are taken into consideration, it cannot be denied that in certain cases, though not in all, when it was taken in immoderate doses, it may have caused death; but M. Toussaint completely denies that it will disturb the general health or deteriorate it, producing persistent debility as will corrosive sublimate, and arsenic. Consequently, he thinks he can affirm with some show of reason, that "copper is no poison."

Regarding the innocuousness of the use of copper-cooking utensils, a subject of paramount importance, in as much as, though these may be dispensed with in private families, they are indispensable in large candy and confectionary establishments, in breweries and distilleries, for the boiling of corn, etc., for distilling and other purposes.

Toussaint first enters into historical details; showing that from the utmost antiquity to the last century, no fear seemed to be apprehended that evil might result from their daily use; that the oldest work on their injurious effect was that of Schulze (1722), and the latest that of Piesdic (1752); but that, *per contra*, as early as 1854,

Eller, basing his conclusions on chemical experiments, had shown the utter innocuousness of copper-cooking vessels. Toussaint himself made many experiments on the same subject, which he published *in extenso*, from which it appears: 1st. That food can be cooked for a long time in copper vessels, without any injury to health, provided the vessels be cleaned immediately after they have been used. 2nd. Vinegar and other vegetable acids, as well as salt, will react on the copper, and dissolve a portion of it, but in so small a quantity, as not to be sufficient to disturb health. 3rd. Water, milk, beer, coffee and pure fat, may be allowed to cool in copper vessels, and will not affect or be affected by them. 4th. Acid food, on the contrary, if allowed to cool in copper vessels, will dissolve large quantities of the metal, and may bring on symptoms of poisoning, but will never, says M. Toussaint, cause death. In these cases, the par vagum nerve, acts as a certain preventive, producing vomiting as soon as the food ingested contains too much copper in solution.

M. Toussaint concludes that copper vessels can be used at all times with perfect impunity, if the following precautions are taken: 1st, to use none but vessels manufactured of pure copper, free from any admixture with lead or arsenic; 2nd, not to use them except when perfectly clean and polished; 3d, not to cook therein food containing free vegetable acids, and not to allow the food to cool in them.

In conclusion, M. Toussaint severely criticises the cases of poisoning by copper, published by various authors, and shows how carelessly and negligently these have been reported. Those cases that are most open to severe criticism, are the cases published since 1802, as cases of poisoning by copper, in the "*Traité des Empoisonnements*," of Drouard, as well as those published by a celebrated professor of Vienna, who drew conclusions from newspaper and oral reports. *Bulletin de Thérap.*

iv. *Treatment of Chronic Gonorrhœa, (Blenorrhœa or Gleet) and Chronic Leucorrhœa, by Sub-Nitrate of Bismuth.*

The Sub-Nitrate of Bismuth,\* seems destined to play an important

\* It may not be improper to give, in this place, the following statement from the November No. of the *American Gazette*, concerning the impurity of the Sub-Nitrate of Bismuth: B. D.]

SUB-NITRATE OF BISMUTH—CONTAINS ARSENIC!—This valuable remedy, so long employed, and more frequently prescribed of late than ever before, has never been suspected as at all hazardous, and is hence given freely, even in diseases of the stomach. Professor Rodgers, of Philadelphia, has recently announced the appalling fact, that he has proved by analysis, that out of ten samples of this drug, obtained from as many different sources, eight of them contained *arsenic!* The importance of the chemical purity of the bismuth, hereafter used in practice, and especially that arsenic is not concealed in it, will be apparent to every practitioner.

This discovery of Prof. Rodgers has already resulted in the acquittal of a prisoner, on trial for sus-

part hereafter in therapeutics. Until very lately, very little was positively known of its remedial effects, and even its dose was not fixed. It has been recently ascertained, that it can be taken almost *ad libitum*; at any rate, in much larger doses than those laid down in the standard works, provided the salt be pure. It is unfortunately apt to be undesignedly contaminated with arsenic, when prepared with bismuth not quite freed from the latter metal.

Dr. Emile Caby, *ex-intèrne*, of St. Lazare Hospital in Paris, has been using it extensively of late, in the treatment of these obstinate chronic urethral discharges, which are the opprobria and despair of the regular profession, and the source of revenue and credit to empirics. Those discharges, so trifling in appearance, obstinately resist the usual treatment by balsam, etc., internally and astringent injections. Should Dr. Caby's experiments prove as successful in other hands as they did in his, he will have deserved the gratitude and thanks of the whole medical community.

Dr. Caby claims that it is almost a specific against discharges from the genital organs in both males and females. In order to be beneficial however, it is absolutely indispensable, that these discharges be entirely free from any inflammatory character. It is especially in chronic discharges that it is remarkably efficacious, and superior to all other remedial agents hitherto employed. Few men had better opportunities than Dr. Caby had at the St. Lazare, to try it, and he cites cases, taken from forty-three persons on whom he tried it with perfect success. One case of gleet of thirteen months' standing was cured in three days; two others of eleven and twelve months' standing were cured in five days; one of ten months, in seven days; in fact, this tedious and obstinate disorder met with in individuals differing totally in habits, temperament, and social position, was invariably cured by this treatment in periods varying from three to twenty-one days.

Dr. Caby recognizes two kinds or classes of chronic gonorrhœa—blemnorrhœa or gleet. First, that which is the sequel of acute gonorrhœa, and second, the "*Blénorrhée, d'emblée*," (spontaneous chronic gonorrhœa,) an indolent, painless discharge, which is in males, the counterpart of fluor albus in females. This latter form is produced

pected poisoning with arsenic, and in which this metal had been found in the tissues of the deceased by the post mortem. It being proved that the patient had taken the sub-nitrate of bismuth among his medicines, the medico-legal evidence of Dr. R. acquitted the prisoner. May not this have a bearing upon a case now before our New York courts?

by various causes. In children, it is due to a vicious diathesis; in adults, to excesses in eating or drinking, to excessive fatigue, to certain articles of diet, whether food or drink, and oftener than either of these, to coition. But it is then the result of sexual connection with a perfectly healthy female, who is not even troubled with abnormal vaginal or uterine secretions, but still it is characterised by a profuse urethral discharge, sometimes staining the linen in the same manner exactly as the most acute gonorrhœa, but quite distinct from the latter in being entirely free from pain, for if pain existed, Dr. Caby says, it would cease to be a blennorrhœa.

On questioning the latter patients, it will be discovered that during childhood they had been frequently troubled with otorrhœa, nasal discharge, ophthalmia and diarrhœa; serofulous sears will often be discovered. The cause is evident: the same diathesis that induced a predisposition to chronic catarrhal affections of certain mucous surfaces in childhood, continues in adult life, and is developed in other mucous membranes by the slightest cause. All authors are agreed that the constitution and temperament of the patient exercise a great influence on the nature and treatment of these discharges, and that in early age, instead of a local, a general treatment tending to greatly modify and improve the general constitution must be resorted to, before advancing life renders it difficult, if not impossible.

Dr. Caby cites some interesting cases of the latter order treated by him with entire success. The first was that of a young man of twenty, whose mother (with whom he lived) kept a house of prostitution, and who was invariably attacked, twenty-four hours after having had sexual intercourse with a woman, by a discharge, which though free from pain and inflammation could not be stopped. This young man, of feeble constitution and lymphatic temperament, had tried in vain all systems of treatment under respectable physicians, as well as empirics. He was advised to consult Dr. Caby, by the female whom he charged with having inoculated him, and who being examined by Dr. Caby, was discovered to be perfectly healthy. Seven injections with sub-nitrate of bismuth sufficed to cure him completely.

A month afterwards, he called to consult Dr. Caby again. He had had connection with another woman, perfectly healthy, with the same ill-luck, the discharge having reappeared. He was again cured by injections of the sub-nitrate. Dr. C. then advised him to use the sub-nitrate by injection after each sexual connection, as a prevent-



ive. He did so, with perfect success. "He is now," says Dr. Caby, "perfectly satisfied, that he has a certain prophylactic. It may be objected," continues Dr. Caby, "that other astringent injections, such as those of acetate of lead, sulphate of zinc, tannin, etc., would have had the same result. I deny it; as I had often before tried the latter, with very little success."

Another case was that of a college lad of fourteen, addicted to masturbation, being of a feeble constitution and a most lymphatic temperament. His genital organs were largely developed. Without having had any sexual connection with a female, he had been troubled for eleven days, before consulting Dr. Caby, with an urethral discharge without any *ardor urinae*. Besides the discharge, he had ozæna, with discharge, which he said, had been produced by repeated coryzas. The urethral discharge was cured by injections of the sub-nitrate, and the ozæna by snuffing the dry salt up the nostrils. M. Monneret, had before this, cured ozæna by snuffing dry sub-nitrate up the nasal passages.

A third case was that of a young man, who, eighteen hours after his *first* connection with any female, was attacked with a very free urethral discharge, leaving greenish spots on the linen. There was no pain, however. In this case, again, the girl was examined by Dr. Caby, and found to be healthy. He was cured by injections of the sub-nitrate.

Dr. Caby uses the sub-nitrate of bismuth by injection only. His formula was: 30 grammes of the salt mixed with 200 grammes of rose water. This proportion of the salt may be increased or diminished *pro re nata*.

This injection was used like the ordinary astringent injections, except that, as the sub-nitrate is insoluble and very heavy, Dr. Caby would urge his patients to shake the bottle well first, and pour it into the syringe instead of sucking it up by it. He also insisted upon their emptying their bladder before using it, and holding the penis up for three or four minutes afterward, in order that as much as possible of the salt might be deposited along the parietes of the urethra. Dr. Caby says, that however thick this coating of the canal may be, it will never, in any way, interfere with free micturition. The only unpleasant effect it can produce, is a sense of dryness and tension of the urethra, in no way painful, which besides, only occurs after the first three or four injections, and then ceases entirely. On the contrary,

several of his patients, who had previously been troubled with *ardor urinæ*, assured him that they derived therefrom so much benefit, that they felt tempted to inject themselves every time the desire to urinate came on, in order to alleviate their suffering.

Dr. Caby found the sub-nitrate equally beneficial in the treatment of balanitis and balano-posthitis. It cured these affections as well as herpes preputialis, rapidly and without any pain.

The glans penis and prepuce were first well cleansed, and the dry salt freely applied over the affected parts, which were afterwards protected by a layer of soft lint or carded cotton. Chancres even, healed under the influence of the sub-nitrate, after having been freely cauterized.

In females, Dr. Caby describes three affections in which he used the tris-nitrate with perfect success: 1st, vulvar leucorrhœa (*leucorrhée vulvaire*), in which the discharge was confined to the external genital organs; 2d, vaginal leucorrhœa (*leucorrhée vaginale*), in which the vaginal mucous membrane was the surface affected; 3d, vulvo-vaginal, vulvo-urethral leucorrhœa (*leucorrhée vulvo-vaginale, vulvo-urétrale*), in which the mucous surface of the vulva, vagina, and urethra, were altogether the seat of the affection.

Under the name of vulvar leucorrhœa, vulvar discharge, hypersecretion of the vulvar follicles: M. Caby ranges these discharges that do not extend beyond the vulva, whether sequelæ of the affection so remarkably described by M. Huguier, vulvar folliculitis: these are the most numerous, as well those which appear without any precursory inflammatory symptoms: the latter is more commonly found in young girls in whom the whole vulva is continually kept moist by an indolent hypersecretion, free from any inflammatory character, but which as well as the other cases are attended by an alteration of the mucous follicles.

The most common cause by far of this affection is pregnancy; fifteen or sixteen women out of twenty affected are pregnant. It is also produced by want of cleanliness, by excessive *emboupoint*, the use of rancid salves, *pediculi pubis*, ascariides. Vermiculares in young girls, masturbation, and lastly, the bruising of the genital parts by coition, or by *attouchements*. The latter cause, was above all, most frequently met with in St. Lazare, and would principally be found to exist in those females who had become prostitutes but a short time previously,

and whose organs had not yet acquired the usual insensibility common to females of that class.

In these cases, there is rarely any very distinct inflammation: it is more a temporary over-excitement, which leaves as a sequel an abnormal secretion, not always curable by rest and emollients, but yielding readily to the sub-nitrate.

Excessive uncleanliness alone, sometimes causes this vulvar discharge. In these cases, the changes undergone by the mucous membrane, as seen after repeated baths and washings with warm water and soap, are different from those of simple vaginal leucorrhœa. In the latter, its color and consistence only are altered. In the former cases, it is softened, and peels off in scales; it is either universally or in spots of a greyish or purplish color; sometimes it has the bluish appearance of a dark-bearded man's face, newly shaved: and above all, there is found hypertrophy of the follicles, from each of which, exudes under the slightest pressure, an abundant sebaceous matter.

There is little or no pain, unless there be complication; the vulva is rarely swollen; but most generally there exists an intense pruritus; the upper part of the thighs are red and excoriated, and when in addition to the first great cause, *pediculi pubis* exist, it is impossible for these poor women to keep from continually scratching themselves.

Dr. Caby's method of using the sub-nitrate on females, was, first to ascertain that the parts had been thoroughly cleaned and purified as mentioned above; and after having carefully dried them well by means of soft lint, etc., to apply throughout, a thick coating of the dry tris-nitrate, being careful to insert it into all the folds. The patient was advised to protect herself by wearing a bandage such as females usually wear when troubled with catamenia. Six days or less usually sufficed to dry up this discharge; but Dr. Caby advised its continued use for three or four days longer to prevent any return of it.

Dr. Caby met with cases of vulvar discharge produced by masturbation, which differs from other leucorrhœas in being confined to the mucous membrane of the clitoris. Masturbation, singularly enough, is a frequent vice with prostitutes. In these cases the clitoris is greatly developed, its mucous membrane is thickened, insensible, and of a bluish tinge; sometimes it is very pale, and the subjacent cellular tissue is puffy. At first there is considerable discharge, accompanied by slight inflammation which soon subsides and is followed by

nothing more than a chronic hypersecretion of the follicles. In some cases, there was complete dryness.

Though this last affection is readily curable by the bismuth salt, Dr. Caby preferred cauterizing the patients freely with nitrate of silver, in order that the pain produced thereby, and the fear of being subjected to the same treatment, should they be again attacked, might exercise a salutary influence in curing them of this bad habit. It was often tried in St. Lazare, with perfect success.

*On the use of the Sub-nitrate of Bismuth, internally, for the Cure of Gonorrhœa:*—The success of M. Caby with the sub-nitrate in chronic gonorrhœa, induced M. Delamorière (Dr. Caby's successor at St. Lazare), to try it in acute gonorrhœa.

The following is the combination used daily in St. Lazare, with great success:

R Balsam Copaiba,  
Powdered Cubebs,  
Sub-nitrate of Bismuth, of each, 30 grammes.  
Oil of Peppermint, sufficient to flavor.

Mix well. From eight to ten grammes may be taken daily in azymous bread or a waifer.

This combination is easily retained by the most delicate stomach; it does not cause any fever, any epigastric heat or diarrhœa; in fact, its whole force seems directed to the genito-urinary organs, curing the patient more rapidly, more pleasantly, than the balsam administered the old way.

Those patients who prefer taking the capsules, can swallow immediately after or before the capsules, a dessert-spoonful of the sub-nitrate of bismuth, mixed to suit their taste.—*Bulletin Général de Thérapeutique, of Sept. 15, 1858.*

v. *Can a Person be Poisoned by Smoking Cigars impregnated with a Solution of Arsenic, or containing a Piece of the Solid Oxyde?*

On the occasion of a recent case of poisoning in Genoa, Dr. Bunsen of Heidelberg, made some experiments to solve the above questions, of great interest to habitual smokers. He discovered that from five to seven centigrammes can penetrate the mouth through a cigar which has been impregnated with arsenical solution, and that about an eighth of a grain will be inhaled with the smoke, if the arsenic be in the dry state in the cigar.

ART. II.—*On Animal Heat.*

[The following article, from an elaborate essay on ANIMAL HEAT in the October number of *Blackwood's Edinburgh*, consisting of a few extracts, will give the reader a fundamental view of the author's facts and conclusions. The entire argument and experimental proof if copied, would occupy more space in the *N. O. Med. & Surg. Jour.*, than is desirable.]

It may be laid down as an axiom, that every living organism has within it a source of self-supplying heat. Organic beings are thus distinguishable from inorganic in possessing, as a necessary consequence of their vital activity, a self-supplying source of heat; and organic beings are distinguishable *among each other* by the rapidity with which this heat is *supplied*, and the facility with which it is *radiated*, and not, as the current classification implies, into animals with warm blood, and animals with cold blood, and plants with no heat at all.       \*       \*       \*

"Food is warmth," says the physiologist; and in one sense this is strictly true, namely, that Food, by rendering a continuance of the vital processes possible, must bring with it the heat to be evolved in those processes. But it is *not* true in the sense in which the aphorism is frequently employed, namely, that food is the fuel which is burned for animal heat (like coke in a furnace), and that particular kinds, the so-called respiratory food, are those we ought to employ as fuel. The warmth you feel after eating a hearty dinner is not really an increase of your temperature, but a *diffusion* of it to the extremities and the surface. Place a thermometer under your tongue before dinner and after it, and you will find that, in spite of your sensations, the thermometer points to the same degree at each period. Yet, as this admits of another interpretation, we shall adduce the less equivocal observations of M. Martins. The ducks belonging to a miller near Montpellier were fed well on grains every morning before being turned out to enjoy themselves in the river, and every evening on their return they were fed again; close by, there lived a poor waiter on the loch, who also kept ducks, but could not afford to feed them on grains, like his richer neighbor, so that they were reduced to forage for themselves. Here accident had arranged the conditions of a good scientific experiment. Living in the same air, the same temperature, and in the same loch of the river, these two flocks differed only in respect of the grain on which one was daily fed. The influence of food would, therefore, here be manifest. What did observation detect? A superiority of temperature amounting nearly to a degree centigrade (about  $1\frac{1}{2}$  Fahr.) in favor of the well-fed ducks. M. Martins adds that he has since then often been able to affirm whether a bird has been well or ill-fed, by simply ascertaining its temperature.

On a superficial consideration, this would seem to be convincing evidence that those physiologists are correct who assert "food to be warmth," in the crude sense of food being fuel; but closer attention will show that the evidence supports our view of food. Indeed, M.

Martins has furnished us with irresistible evidence; "for," as he remarks, "we shall greatly deceive ourselves if we imagine that a better quality of food will suffice to raise the temperature in a *short period*. Two drakes, after five days of abstinence, were found to have a temperature of  $41^{\circ}.83$  centigrade (about  $107^{\circ}$  Fahr). I then fed them entirely on bran and herbs, and twenty-five days afterwards their temperature was  $42^{\circ}.14$  (not quite  $108^{\circ}$  Fahr). Two other drakes, with a temperature of  $41^{\circ}.40$ , were fed abundantly on maize and hay; at the end of twenty-five days of such diet their temperature was  $41^{\circ} 76''$ —that is to say, actually *less* than those which had been ill-fed! This may seem to be in contradiction with M. Martins's previous observation on the well-fed and ill-fed ducks; but the contradiction is only superficial; the reader will notice that, although the temperature of these well-fed drakes was actually less than that of the ill-fed, it was also less when the experiment began; and if we compare the rise in the temperature which took place in both, we shall find that in the ill-fed it was only  $0^{\circ}.30$ , and in the well-fed  $0^{\circ}.36$ . This difference, slight as it may seem, is in favor of the well-fed; and when such slight elevations are continued month after month, they may, and will attain a superiority amounting to one degree. Although, therefore, this experiment confirms the previous observation of the influence of generous food in elevating the degree of animal heat, it strikingly discredits the notion that the food is burned as fuel in the organism.

"Food is warmth," because food furnishes the pabulum of the tissues, and warmth is evolved in the chemical changes which go forward in the formation and destruction of the tissues. But food is not fuel only, as some physiologists would have us believe. If anything is burnt, it is the tissues not the food; our warmth comes from the organic processes which make and unmake the tissues. The proof of this is seen, not only in the foregoing experiments, but even more convincingly in the experiments on starvation which Chossat, and Martins have performed. We shall not here repeat those of Chossat, because they are well known, and the results are accessible in almost every text-book; but those of Martins will be new to our readers, and may therefore briefly be indicated. He took four drakes, and submitted them to several successive periods of abstinence, separated by periods in which they were abundantly fed. They were left in a tub of water in which to bathe, and their habits were unaltered; nothing but the solid food was withheld during the days of the experiment; and in order to avoid the diurnal variations of temperature from complicating the problem, the thermometer was always applied at the same hour of each day. The birds commenced the period of fasting with a temperature of  $41^{\circ}.20$  ( $108^{\circ}$  Fahrenheit), at the close of a period of ample nourishment.

After 24 hours' abstinence	the temperature	sank	to	$41^{\circ}.84$
" 48 "	" "	"	rose	to $41^{\circ}.89$
" 72 "	" "	"	rose	to $41^{\circ}.91$
" 96 "	" "	"	rose	to $41^{\circ}.94$
" 120 "	" "	"	sank	to $41^{\circ}.62$

Here we see that twenty-four hours' fast have produced a striking

reduction of temperature: and those writers who attribute warmth to the combustion of food may fancy they see evidence for their opinion in such a fact; but, as the fast is prolonged, the temperature does not continue falling; it rises: so that, after ninety-six hours of complete abstinence, the temperature has risen nearly to what it was when the animal was crammed with food. We have only to add, that this is in perfect accordance with the observations of Chossat, on pigeons. It is true that, after the fifth day, the temperature suddenly sinks; but the mere increase, as the abstinence is prolonged during the first four days, is sufficient to show that the animal heat is not evolved by the combustion of food.

Having examined the influence of age, sex and food, we may now cast a glance at the influence of the seasons. Although man preserves his standard of  $98^{\circ}$  in the tropics and in the arctic zone, he does so in virtue of the power his organism possesses of adjusting itself to changing circumstances. We adjust ourselves to the changing seasons. In winter we are as warm as in summer, because in winter we produce more heat, and lose less by evaporation and exhalation. A cold day in summer is incomparably more unpleasant and injurious than a day of equal temperature in autumn; and the coldest day in summer would be mild to us in winter. The reason is, that in summer the cold day finds us unprepared. The organism during summer has been adjusting itself to the production of less and less heat, and if a cold day now occur, we have less power of resistance; we are somewhat in the condition of the infant animal, which has not yet acquired its full power of heat-making. It is on this principle that we may explain the death of animals exposed during summer to a degree of cold which in winter would scarcely lower their temperature.

After having thus glanced at the chief phenomena of animal heat, we are naturally led to inquire more closely into the cause. If we could trust our text-books, no part of physiology is better understood; the theory of animal heat seems as simple as it is indisputable. There are, indeed, a few recalcitrant physiologists who will not accept the dominant theory; but on the whole, a very remarkable unanimity exists. We shall first state, as clearly as we can, what the dominant theory is, and then state the reasons which, if they do not destroy that theory, at least show the necessity for a reinvestigation of its foundations.

Animal heat, it is said, is the effect of which respiration is the cause. In respiration, oxygen is absorbed, which burns the carbon of the food into carbonic acid, and the hydrogen into water: in these acts of oxidation heat is generated, for no combination of a combustible substance with oxygen can take place without disengaging heat. No matter whether such oxidations take place in the body or out of it, rapidly or slowly, at a low temperature or at a high one, the amount of heat set free by the combination of a given quantity of oxygen with a given quantity of carbon or hydrogen, is always and everywhere the same. The oxidation of the carbon of the food will liberate precisely as much heat as if, instead of being spread over a long time, the combustion had taken place in a vessel of pure oxygen. Chemistry assures us of those facts. Physiology assures us that oxygen is inces-

santly absorbed in the lungs, and that carbonic acid and water are incessantly exhaled; and further assures us that, concurrently with this absorption of oxygen and exhalation of carbonic acid and water, there is an amount of heat generated which would be generated by an equivalent combustion of carbon and hydrogen out of the organism. "It is obvious," says Leibig, "that the amount of heat liberated must increase or diminish with the quantity of oxygen introduced in equal times by respiration. Those animals, therefore, which respire frequently, and consequently consume much oxygen, possess a higher temperature than others, which, with a body of equal size to be heated, take into the system less oxygen."

Such is the so-called "chemical theory of animal heat," which we will now proceed to criticise. We have already seen how little confidence is to be given to the notion of the food being burnt in the organism; and when, therefore, we hear "the carbon of the food," spoken of as passing into carbonic acid, and disengaging heat, we must understand by it the carbon of the tissues made from that food. The oxygen which is absorbed in the lungs does not then and there combine with carbon in the blood, and generate its due amount of heat; this, which was formerly believed, is now given up by all competent physiologists. In giving up this idea, we must follow the course of the oxygen in the blood, until we detect it, *flagrante delicto*, in the act of burning the carbon; but this has hitherto escaped all research. *We are in utter ignorance as to the origin of carbonic acid in the organism.* We have many plausible explanations as to how it *may* arise, but how it *does* arise we do not know. It is extremely doubtful, according to Robin and Verdeil, whether *any* direct oxidation of carbon takes place at all, and is quite certain that much of the carbonic acid is not so produced.\* Without venturing further on ground so delicate, we will sum up in the words of the distinguished chemist Regnault, who has specially studied this question: "It was long believed (and many chemists still believe it) that the heat produced by an animal in a given time is precisely equal to that which would be produced by the burning in oxygen of the same amount of carbon and hydrogen which is found in the carbonic acid and water exhaled in that time. It is very probable that animal heat is entirely produced by the chemical actions which take place in the organism, but the phenomenon is too complex to admit of our calculating it according to the quantity of oxygen consumed."† The simple fact that the carbonic acid exhaled, at times contains *more* oxygen than has been absorbed, although perfectly intelligible when we remember the influence of food on the exhalation of carbonic acid, is of itself enough to destroy all confidence in such calculations.

While, therefore, it is still undecided whether carbonic acid and water arise in the organism by a process of direct oxidation, the theory of animal heat, which is based on such an assumption, must necessarily be held questionable. Meanwhile we may look a little closer into the evidence which declares that animal heat is the direct

\* ROBIN et VERDEIL: *Traite de Chimie Anatomique*, li. 38 seq., 87, 163, 462, and liii. 183 seq.

† REGNAULT: *Cours Elementaire de Chimie*, li. 868.



product of respiration, rising and falling with it, dependent on it, as effect upon cause. That a mass of evidence can be adduced is perfectly true, because, whatever theory we may form, we must still perceive that *an* intimate relation necessarily exists between respiration and animal heat; if only on the ground that all vital processes are intimately related, and in the organism one function is necessarily dependent on another. The question, however, is not whether *an* intimate relation exists; but whether *the* causal relation exists, whether the two phenomena are in invariable correspondence, the one never feeble when the other is energetic—the one never acting after the other has ceased.

Disregarding the mass of evidence which may be adduced in favor of the correspondence, let us here fix our attention solely on some striking exceptions. The cases are by no means very rare in which a corpse has preserved a high temperature for many hours; and, as respiration must altogether have ceased, these cases have great significance for us. Dr. Livingstone mentions a case which came under his own eye, of a Portuguese lady, who died of fever at three o'clock in the morning of the 26th April. "The heat of the body continued unabated till six o'clock, when I was called in, and I found her bosom as warm as ever I did in a living case of fever. This continued for three hours more. As I had never seen such a case in which fever heat continued so long after death, I delayed the funeral till unmistakable symptoms of dissolution occurred." Mr. George Redford informed the writer of a case which he had under his own eye. A soldier, given to drink, died, we forget from what cause, and the next day Mr. Redford was quite startled at finding the body still warm. Dr. Bennet Dowler of New Orleans, has likewise observed that, in many cases, the temperature *rises* after death; and, as these observations are cited by so eminent an authority as Professor Dunglison, we must give them a credit which might perhaps be refused to the cases previously mentioned. Dr. Dowler found that where the highest temperature during life was  $104^{\circ}$  under the armpit, it rose to  $109^{\circ}$  in ten minutes after death; fifteen minutes afterwards it was  $113^{\circ}$  in an incision in the thigh; in one hour forty minutes it was  $109^{\circ}$  in the heart. Three hours after all the viscera had been removed, an incision in the thigh showed the temperature to be  $110^{\circ}$ .

When we remember that, even after death, processes of growth and secretion have been observed to take place, there is nothing incredible in these examples of continued heat after death; but we cannot see how the advocates of the respiration theory reconcile such facts as the complete absence of respiration during several hours, with no diminution of animal heat. According to theory, the two phenomena are in immediate dependence, the intensity of heat corresponding with the energy of respiration; but here there is no respiration, nor has there been any for some hours, yet the heat continues to be produced.

There are, moreover, numerous facts which show a similar want of correspondence between the energy of respiration and the intensity of heat. In tetanus, for example, the temperature has been known to rise to  $110^{\circ}$ —an amazing height; yet no corresponding increase of respiration is noted. In women the energy of respiration is strikingly

inferior to that in men: according to Barral, 40 per cent. Yet, although they "burn" so much less carbon than men, their temperature is scarcely lower, if lower at all! We lay more stress on this fact, because it is the expression of the normal condition of the organism. In all cases of disease there is a possibility of some totally new conditions which render our inferences inapplicable; but, in the natural breathing of ordinary men and women, we may expect to see the unobstructed action of the law which connects respiration with animal heat. According to theory, women ought to have a very much lower temperature than men, for they exhale so very much less carbonic acid in respiration, and must, therefore, "burn" less carbon. According to fact, women have as high a temperature as men. It looks so plausible when we read that the amount of heat liberated must increase or diminish with the quantity of oxygen introduced in equal times by respiration; yet this plausibility becomes troubled when we find animal heat sometimes bearing no such relation to the amount of inspired oxygen. The woman is as warm as the man, with feebler respiration.

A mouse eats eight times as much food, in proportion to its size, as a man, and its respiration is, according to Valentin,\* eighteen times more energetic; yet its temperature is little higher than that of man, and its power of resistance to cold is incomparably lower. Birds eat six to ten times as much as a man, in proportion to weight—respire much more vigorously—and lose less heat by evaporation; nevertheless, they are only a few degrees higher, and their power of resistance to cold is in general much less. Valentin says that a dog consumes twice as much oxygen as a man, in proportion; yet the difference in their temperature is very slight.

These illustrations suffice to show that no invariable constancy can be found between respiration and animal heat; even should the theory we are criticising ultimately turn out to be correct, the objections we have urged will still retain their force, not indeed against the truth of the theory, but against its inconsiderate interpretation; they will retain their value as indications of the presence of *physiological conditions*, and will show how varieties in the organism modify the operation of the general physical laws; thus removing the question of animal heat from the hands of the chemist, and replacing it in the hands of the physiologist. Treating the question as a physiological one, we are forced to consider animal heat as determined by the energy of two processes, one of production and another of radiation.

We know how intimately dependent the vital functions are on temperature, and in a previous article we saw how respiration in the cold-blooded animals uniformly increased in energy as the external warmth increased, up to a certain point; and we are therefore able to understand how it is that a low degree of vital activity will be found coincident with a feeble respiration and low temperature, while a high degree of vital activity is coincident with energetic respiration and a high temperature, without our being forced to admit that this coincidence implies a casual relation between energy of respiration and animal heat.

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\* VALENTIN: *Text-Book of Physiology*. Translated by W. Brinton, p. 351.

In conclusion, we may say that the hypothesis generally adopted respecting the production of animal heat is very far from possessing the evidence demanded by science. It may be true; we do not think it is true; and we are persuaded that it is not proven. It rests on two pillars, the very foundations of which are insecure. The first of these is the chemical hypothesis of direct oxidation of the carbon and hydrogen. The second is the assumed invariableness of the relation between intensity of heat and energy of respiration. We are not warranted in affirming either of these propositions; all we are warranted in affirming is this: Animal heat is evolved in various chemical and physical changes which occur in the processes of life, and is consequently in direct correspondence with their energy, rising in intensity as they become more active, and falling as they fall. We have every reason to believe that oxygen is the great *inciter* of such changes, the indispensable condition of vital activity; but we have no direct evidence that these changes are all oxidations; we have direct evidence that some of them are not oxidations, but are dependent on respiration only as one organic process is dependent on another, and as respiration itself is dependent on them.

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ART. III.—*On the Observation of Temperature of Patients.* By PROF. C.

A. WUNDERLICH and DR. L. MEYER.

ALREADY during the last century several eminent physicians endeavored to ascertain the temperature of the body in different diseases. These efforts, however, were soon abandoned again, and only quite recently the thermometric observations in patients have again received that attention which they so much deserve. Prof. Wunderlich has, in this respect, a rich experience at his command; accurate thermometric observations were regularly made in his clinic in more than 5,000 patients during the whole course of their sickness, and also in private practice he has convinced himself of the practicability and usefulness of this means of investigation. He considers himself justified, therefore, to pronounce the view so generally taken, and recently advanced again by Lasègne, (*Arch. Génér.*, May, 1856,) that thermometric investigations would never become very important to pathology, as perfectly erroneous. It is true, that for the theory of diseases these observations of temperature have remained as yet without direct use, and that they do not throw much light at present upon the nature of fever, inflammation, etc., but also in reference to theory they have afforded facts which are of great consequence for many important questions in relation to pathological physiology. Of far greater importance, however, are they in a *practical* point; Prof. Wunderlich considers them even of more value than most of the other means of investigation, provided that also in local diseases

the part taken by the whole organism is considered of sufficient moment. As proof of his statements, the author gives the following facts :

1. The observation of temperature offers the most reliable means for deciding *the importance of a disease of recent origin* ; with normal, or but little elevated temperature, the disturbances of health are, with some exceptions (cholera, apoplexy, pulmonary hemorrhage, strangulated hernia, poisoning, etc.), first of all of no importance ; an elevation of 2° R. or more, announces, however, with certainty, the commencement of a serious disorder. This circumstance is a valuable guide, particularly in cases of children, in which, as is well known, an insignificant disease is frequently accompanied by violent symptoms, as also in reference to the continuation of the patient's business, to his departure, or transport, etc.

2. The observation of temperature points out frequently *important, though still latent disturbances* ; an indisposition with much elevated temperature deserves always particular attention ; in the state of *reconvalescence* from serious diseases a relapse, or a secondary disease, is frequently indicated first by an elevation of temperature. This is particularly the case in typhus ; but also in intermittent fever an elevation of temperature without any other symptoms is frequently observed after an apparent cure, and a relapse can then only be prevented by continuing the use of quinine.

3. If the disease is developed the observation of temperature offers *the most reliable indications for the diagnosis*. Diseases in which the diagnosis of particular forms of the same or other of pathological processes can be decided in this manner are, according to Prof. Wunderlich, the following : typhus, (exanthematic ; enteric ; ) intermittent fever ; pneumonia ; meningitis, (at the base ; at the convexity ; ) serous and purulent effusions in the pleura or pericardium ; acute exanthemata ; internal suppurations ; peritonitis, (in lying-in women.)

4. The diagnosis being decided, thermometrical observations are of the greatest use in reference to the *prognosis*. *Intensity* and *character* of the disease, *its stage* the commencement of a complication which is often not indicated by any symptom, the usual *aggravation* and *increase* of the malady, as well as its *decrease*, can be recognized, as the author proves by many examples, the earliest and surest, sometimes even solely by the behavior of the temperature of the body. A certain height of temperature (about 34° R.) indicates, with certainty, a fatal issue ; perseveringly high temperature (over 32·5°) makes the prognosis always more serious ; a falling of temperature in a proper manner, however, permits predicting a favorable turn of the disease. In the state of *convalescence* changes of the temperature of the body deserve no less consideration as a means by which to recognize deviations from health, otherwise hardly perceptible. Insufficient falling of temperature indicates, in spite of apparent convalescence, an incomplete cure, and gives reason for fearing the development of a chronic disorder ; even a small increase of temperature challenges precaution in regard to diet and regimen of the convalescent.

5. Another great advantage derived from thermometric observa-

tions is the proof of a *regular typic course of numerous febrile diseases*; it is true that physicians of a former period supposed it to exist, but it cannot be demonstrated with certainty but by accurate observations of the changes of temperature of the body. In the same way *deviations* from this regular typic course are best recognized by the use of the thermometer, and we are thus enabled to avert them by removing the causes, to neutralize their consequences, or also, as many of such irregularities are of a more favorable character than the normal course of the disease, to bring them on by therapeutical means

6. From what has been said, the importance of thermometric observations for *therapeutics* is evident enough; they indicate where energetic interference is necessary, and when the disease may be left to itself again. The thermometer gives us, however, also reliable and accurate information in regard to the efficacy of certain remedies and methods of treatment used; for instance, on the effect of general bleeding, of calomel, digitalis, camphor, an emetic, and other energetic means in febrile diseases. The observations of temperature have, as the author shows, so great a value for therapeutics, particularly for the reason that the indications for treatment have more frequently to be derived from the general condition of the patient than from so-called local disturbances which usually disappear spontaneously, and in which direct interference is not of much benefit (*Arch. f. Physiol. Heilk.*, N. S. 1, p. 5.)

Dr. Meyer considers observations of temperature in insane patients of very great use, as they aid the physician in determining whether there exists a direct disease of the brain, or whether the latter is affected merely by reflex action from another organ. In the former case a corresponding change of temperature is observed; if the delirium is accompanied by elevation of temperature without remission, a direct irritation of the brain exists. In reflex alienations, however, this change of temperature does not take place; if the temperature rises in these cases, it indicates the occurrence of a complicating disease. These statements, the importance of which for the prognosis and therapeutics of mental diseases is very evident, the author proves by a condensed report of numerous cases (mania, progressive paralysis). As the peculiar character of insane patients does not permit a long continuance of the usual mode of observation, viz.: by placing the thermometer in the axilla, he prefers to insert it into the rectum.—(*Deutsche Klinik.*, 13, 1858; *Schmidt's Jahrbücher*, 6, 1858. *North Am. Med. Chir. Review.*)

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#### ART. IV.—*Electrical Anæsthesia.*

THE electrical anæsthesia or painless extraction of teeth under the influence of the electrical current, which recently originated with Dr. J. B.

Francis, of Philadelphia, has not heretofore been alluded to in the perhaps too skeptical New Orleans Medical and Surgical Journal, although the testimony during the past summer, by more than "two or three witnesses," has been highly favorable to the validity and utility of this announced discovery. Nothing would seem more simple, at the first view, than the testimony of a person whose tooth had just been extracted as to the presence or absence of pain in the operation, but even this is really liable to mistake.

"To be, or not be" pained "that is the question." Seeing, feeling or tasting is believing. But such is the uncertainty of human testimony that sweet is sometimes called bitter and bitter sweet! A few years ago (1845), a London clergyman (Rev. G. Williams, A. M.), wrote a book on the Holy City; in his account of the Fountain of Siloam, beginning with Josephus and coming down to the present, he found that Pagan, Jew, Christian, and Mahommeden differed about the taste of this water. The above author says: "Tastes must strangely differ; this water has been described as sweet, bitter, brackish, milk and water," etc.

It may be so with teeth. One gentleman reported to the Academy that 1,500 teeth had been extracted by the "nouvelle méthode," and almost always without pain. The fear of the operation, or the intense coëxisting pain, may perhaps be more severe than that of the extraction itself; hence it is possible that the pain of the latter might be overpowered by, or merged in the greater and more diffused general sensation of the electrical shock. But this may be mere speculation, and withal improbable as an explanation.

It appears that some thousands of teeth have been extracted by this method, but the results are not altogether satisfactory, particularly in France, where some gentle hints at American exaggeration are reiterated in, as well as outside of, the Imperial Academy of Medicine. In the session of the Academy, October 12, 1858, M. Velpeau reported six cases in which he had tried this new procedure, and from the whole he made the sage deduction, namely, that American teeth are not at all like French teeth. Perhaps American electricity differs from French electricity. Be this as it may, an European tourist admitted, a few years ago, the superiority of American thunder and lightning. As Franklin discovered the identity of lightning and electricity, in Philadelphia, it is hoped that Francis' Philadelphia discovery of the anæsthetic property of electricity will,

like that of Franklin, stand the scrutiny of talents, and endure until the last tooth of Time itself shall have been extracted. Electrical anæsthesia is gaining friends in France and England as well as in America.

M. Amédée Latour, the able and indefatigable editor of the *Union Médicale*, says with due skepticism, that the painless extraction of teeth, under the induction of the electrical current, is the happy news which has arrived from the country in which ætherization was discovered; but the experiments made in several hospitals of Paris, have singularly lessened the pretensions of this electrical anæsthetic, particularly the facts recited in the Academy by MM. Robert and Velpeau. It appears that in punctures, incisions, and in tooth-pullings, patients utter cries more vehement than they are wont to do under the simple bistoury without any electrical apparatus whatever. While M. Latour doubts the success of this anæsthetic in France, he thinks that the accounts of its success in the United States have been singularly exaggerated. (*L'Un. Méd.*, Oct. 14, 1858.) However, some of his *confrères* speak well of this anæsthetic. More light! Eighteen centuries may be insufficient among the Orientals to decide whether "The Pool of Siloam" is bitter or sweet, but the Occidentals are a fast people, and will soon ascertain whether tooth-pulling is pleasant or painful.

Hitherto, according to the poet of Human Nature,

—There never was yet philosopher  
That could endure the tooth-ache patiently;  
However they have writ the style of gods.

*Much Ado about Nothing.*

In the *Gaz. Hebdom. de Méd.* for Oct. 22, M. Verneuil sums up such scanty available evidence as he could obtain concerning the merits of galvanic anæsthesia, the tendency of which is rather unfavorable, but not conclusive in the premises. Nevertheless, he adds that at a late meeting of the attending physicians of the *Hôtel-Dieu*, M. Preterre, a remarkably skillful dentist, extracted teeth from three very intelligent persons (one a foreign physician), whose testimony perfectly agreed, and was altogether in favor of the new method; whereupon M. Verneuil wisely concludes, "*L'avenir devra juger la question.*"

The American Medical Journals being very silent upon this new

anæsthetic, it may be proper to get American news, *via* Paris and London. The following is from the latter city :

*Galvanism in Dentistry.*—To the Editor of *The Lancet* : Sir—The application of galvanism in the extraction of teeth is likely to occupy much attention, and as this new agent for producing local anæsthesia can be employed *without danger*, a great boon will be secured to the world, should the invention, after standing the test of experience, realize the sanguine anticipations of the originator. This being the case, perhaps you will kindly allow me space to give the history of the introduction of the agent into this country. Some statements have been made upon the subject in lay journals, which, I think, should have been reserved for professional publications; however, as these statements have been but partial, I will proceed to narrate how the invention was introduced to the notice of the profession in this country.

The first public mention of galvanism\*as applied to tooth-extraction is to be found in the last July number of the (London) *Quarterly Journal of Dental Science* (p. 254), wherein Dr. Berry, of Philadelphia (U. S.), writes that some few dentists had adopted the agent in America ; and that a gentleman, a fellow-citizen, claimed to be the inventor, but that he was under an impression that this was a mistake, and that the method of operating had been practised in England.

The next that we hear about the process is in the *American Journal of Dental Science*, which reached England about a month ago. From this journal it would appear that great attention has been given to the matter in the United States. Dr. J. B. Francis claims to be the inventor, and has secured his rights as such by letters patent. A committee of the Franklin Institute of Pennsylvania, had been appointed to investigate the value of the discovery. In the course of the report of this committee, the following observations are made : “ One hundred and sixty-four teeth were extracted in the presence of the committee.” “ The committee is satisfied—that in a large majority of cases of extraction with this apparatus, *no pain whatever* is felt by the patient.” “ To test whether the effect might not be merely mental, the current was broken without the patient being aware of it, when the usual pain was experienced, although, in the same patient and on the same occasion, teeth had been removed while the current was flowing without causing pain.” “ The operator requires no new instruments except the battery and coil.”

In the specification of the patent, which is published at length in the American journal, the *modus operandi* is fully given; and it would appear that no time was lost in this country in profiting by the circumstance, testing the value of the process, and publishing the result. Candor to our transatlantic brethren, however, demands that the source from which the idea was produced should be made known, in addition to the mere announcement of the fact that galvanism has been successfully employed by individuals in this country.

The whole subject will, I trust, be sifted by the College of Dentists; in the meantime whatever information can be collected by the med-



ical and dental journals will be of interest to the profession, and probably prove advantageous to the public. I am, sir, your obedient servant,

A MEMBER OF THE COLLEGE OF DENTISTS OF ENGLAND.

September, 1858.

*Electricity vs. Chloroform.*—To the Editor of *The Lancet*: Sir—Although I was the first to make, take, and administer chloroform this side the Tweed, I am by no means sorry to find that it is likely to be superseded, for allaying pain in dentistry and other minor operations, by electricity, which promises great results without danger. I have constructed an apparatus, inexpensive and self-acting, for its proper application. The importance of having an apparatus self-acting must be evident to all, so that the hands of the operator may be free to act, and the operation performed without the necessity of a third party. A few pence per week will keep the battery charged. I am, sir, your obedient servant, WILLIAM HOOPER, Operative Chemist.

Pall-mall East, September, 1858.

From other sources not recollected at present, it appears that in Insular Europe this agent is now used in ordinary surgical operations, by several surgeons, with satisfactory results.

The following papers are submitted to the reader, who will judge for himself as to their value. B. D.

*On the Application of Electricity in Local Anæsthesia in Tooth Drawing.*—The application of electricity for producing local anæsthesia in tooth drawing, is creating a considerable sensation. The *modus operandi* is thus described by Mr. Kincely Bridgman, of Norwich. The apparatus is simple, and consists principally of the common electro-magnetic machine used in medical electricity, a single cell and pair of plates constituting a Smcc's battery, and a small electro-magnetic coil with a bundle of wires for graduating the strength of the current. One end of the thin wire conveying the secondary current is attached to the handle of the forceps, and the other end of it to a metallic handle to be placed in the hand of the patient. The instrument touching the tooth completes the circuit, and the current passes instantaneously. The wire attached to the forceps should be made to pass through an interrupting footboard, so that the continuity of the wire may be made or broken in an instant by a movement of the right foot of the operator. The advantage of this arrangement is, that it allows the instrument to be placed in the mouth without risk of producing a shock in coming in contact with the lips, cheeks, or the tongue, which would interfere with the quiet of the patient. A hole drilled in the end of the left handle of the forceps, and the end of the wire tapered to fit rather tightly allows the substitution of one pair of forceps for another with scarcely a moment's delay. The importance of this subject is so great that numbers will, doubtless, immediately have recourse to it, so that we may soon expect its real merits to be fully ascertained.—*Athenæum.*—*Am. Jour. Sci. and Arts.*

Among its selections, the *Va. Med. and Surg. Jour.* for Nov. 1858, has reproduced the following paper without crediting it to any particular journal :

*On Local Anæsthesia and Electricity* : By BENJAMIN W. RICHARDSON, M. D., L. R. C. P., Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Physiology at the Grosvenor-place School of Medicine :—In this short communication, I beg to relate the history of certain experimental inquiries having reference to the application of the electric current for the production of anæsthesia. I would premise that it was no intention of mine to publish at this moment. The paper is one long held in reserve for the purpose, after further research, of being sent to one of the learned societies. But events often check dispositions, and do so now. In the *Times* of to-day (Saturday), there is a letter from Mr. Snape, of Chester, on the application of the electric current for preventing pain in dental operations; and as the subject of this letter, now widely circulated about the world, bears directly on certain points which have received from me much attention, I feel it a duty to hesitate no longer, but at once to submit such labors as I have performed to the notice of the profession.

It is a matter of regret that the results to be given are initiative only, and up to this moment negative in character: the circumstances which have led to their publication must be the excuse for all deficiencies.

My attention was first drawn to the subject in hand in the year 1853, in the following manner:

I was then engaged in investigating by experiment the influence of electricity on the blood in the living animal body. In one of these experiments a small dog was subjected to an electrical shock, resulting from the discharge of a battery of seventy-two Leyden jars. Wire chains ready for connecting with the battery were placed, one round the throat of the animal, meeting over the upper part of the head, the other round the lower part of the body at the loins. The whole charge was at once passed through the body. The animal fell without a struggle, and lay before me to external appearance dead. There was no respiration for several seconds, but the heart continued beating. A little later, and there was a feeble respiratory gasp. I pricked the nose of the animal with the point of a scalpel, and blood issued, but no indication of sensibility on the part of the animal followed. A minute more, and I had laid bare about an inch of the right jugular vein. I tapped the vein, drew off a few drachms of blood for after observation, passed a ligature round the vessel above the opening, brought the edges of the flesh wound neatly together and secured them by suture. By the time I had done, the signs of reanimation were well marked, but the operation had been performed without the slightest evidence of suffering. For a little time the respiration was short and irregular, but in a few minutes the animal rose slowly, looked about him, as if wondering where he had been, and recovered without a bad symptom.

This was probably the first instance in which any operation was

performed without pain, by means of electricity. The result of the experiment was purely accidental. The experiment was originally intended for a different object altogether; but accustomed to operate on narcotized animals, the new fact of the perfect production of insensibility by electricity changed the intention of the experiment entirely in my mind. The idea of producing general anæsthesia for the purpose of an operation, by a repetition of this one electrical experiment, was of necessity out of the argument, for it were impossible to adjust a shock as to produce a sufficient degree of general insensibility for an operation without the hazard of destroying life altogether. The fact of the production of insensibility was, however, striking; and this fact at once suggested to me that what could be done to the whole body might possibly be done to a part.

To carry out the inquiry which had thus been presented, I tried the effect of passing electrical shocks of varying intensities through the limbs of animals. The shocks were severely felt in these cases; but I could never detect that at any instant after the shock the sensibility of the parts through which it had passed was all at destroyed. There was often some temporary twitching of the muscles of the limb operated on, but the merest attempt to produce pain succeeded.

I next tried various experiments on myself. I charged twenty Leyden jars, and discharged them, either in combinations, or one after the other in rapid succession, through one of my fingers. The shocks were painful to bear, and when many were given, the last was felt as severely as the first; but afterwards, the finger was as sensitive to a prick from the point of a lancet as it had been previously.

I tried the local effect of the continuous current for long periods, but with as little success.

I passed the electro-magnetic current through one finger for long periods, modifying the intensity of the shocks; sometimes submitting the part for periods of an hour or more to a gentle current; at other times increasing the force till the pain produced was scarcely endurable. On January 3, and on August 8, of the present year, I kept a finger for two hours thus exposed; but in these, as in all other cases, without the slightest effect in removing sensibility. While the finger was being subjected to the current, I tested its sensibility by pricking it with a lancet or needle. This test is, however, unnecessary, for so long as the part operated on is sensible of shock, it is sensible to a cut or a puncture. An animal deeply narcotized with chloroform is as little sensitive to electrical shocks as it is to the knife.

In its local application, indeed, it seems to me that the electric current restores rather than destroys sensibility. One experiment will explain this. Let two fingers be placed in a freezing mixture and held there until the external surface is so benumbed that the prick of a needle is not felt. Let them then be removed, and let pass through one the current from the electro-magnetic battery. In the finger thus operated upon, it will be found not simply that the sensibility will come back more quickly, but so much the more quickly as to lead to the unpleasant and painful reaction called vulgarly "hot-ache." The current acts like warmth in this respect.

In one experiment the effects produced were very peculiar, and deserve special note. I placed the first and second fingers of the left hand in a mixture of ice and salt till they were entirely insensible to puncture. I then removed them from the mixture, and after well drying them I placed one wire from the electro-magnetic battery round the second finger, at a distance of three-quarters of an inch from the tip, and the other wire round the finger at the base. A gentle current was passed. For a brief period I was not conscious of the shocks, but suddenly the portion of finger included between the wires, from being white became red and injected, and therewith there was excited a degree of pain that was unendurable. By removing the wires and applying cold once more the acute pain passed off. But the most interesting point is, that while the first finger regained its normal sensibility in the course of an hour, and the second regained its normal sensibility in the parts which had been enclosed between the wires, the end of this second finger, from the point beyond which the upper wire had encircled it, remained completely insensible for four hours, and felt slightly numbed even thirty-six hours later.

From these experiments I have no alternative but to believe that the electric current cannot, according to our present knowledge of its application, be made practicable for the production of local anæsthesia.

The only way by which, as I would suggest, electric shocks can in any way be said to remove pain locally, is, that the pain which they excite creates a diversion, so that any new pain which may be inflicted on the part is not felt the less, but is lost in some degree in the pain which was preëxistent. I give a simple illustration. The school-boy tells his new comrade that he can remove a hair from his head without the removal being felt. The skillful operator seizes a hair with his left thumb and finger, pulls it out quickly, and at the very moment strikes the head of his dupe a smart blow with his flat right hand. The operation is performed, and it may be without the pain which would have been elicited by a simple pull. The pain, however, is not removed, but diverted. When my finger was painfully affected by the electric current, the entrance of the lancet or needle point into the skin caused sometimes a more acute, sometimes a less defined, pain than is ordinary. Mr. Louis Parnell, who allowed me to perform some experiments on his finger, expressed that his sensations were the same.

We have seen, nevertheless, in the first experiment related, that a powerful electric shock, sent through the whole body, will produce insensibility; why, therefore, should it not have the same effect in its local application? A dose of aconite tincture will render a body generally insensible to pain; a drop of the same tincture, put on the lip, will produce numbness of the lip. Here is brought out at once a general and a local effect, each alike in kind, but different in degree. Why, then, should not the same obtain with the electric shock? To answer this, the consideration of the modes in which insensibility is ordinarily produced is necessary.

My late friend, Dr. John Snow, did much in clearing up the mystery which interposes here. He has described, and to my mind

proved, that the sensibility of the body may be destroyed in two ways. First it may be destroyed by the direct effect of some benumbing agent on the extremities of the nerves of the part. Secondly, by the effect of the agent on the centres of intelligence; *i. e.*, by the destruction of consciousness. Thus, the effect of some narcotic vapors, taken into the system at large, may even be contrasted; amylene, for example, acts mainly on the extremities of the sensory nerves, interfering but feebly with the consciousness. Ether, on the other hand, suspends sensibility in proportion as it destroys consciousness.

Now, when a powerful electric shock is sent through an animal body it destroys sensibility, not because it is a local anæsthetic, but because it strikes out at a blow the consciousness. The animal for the time is dead to all impressions, pleasant or painful. An animal stunned by a blow is in the same condition; an animal in syncope is in the same condition. In this explanation, I read off the reasons why electricity, as generally applied, did, and, as locally applied, did not, produce insensibility.

In sending this paper to the press, it is no part of my object to interfere with the labors of Mr. Snapc. The present unsatisfactory modes of preventing pain in surgical operations must unquestionably be superseded ultimately by some better process. It shall be read of some day as rude science, that leads the whole body into the realm of dissolution, that one poor molar may be dragged out without a flinch. Dr. Arnott thinks it so now. A mode of producing local anæsthesia, I mean complete anæsthesia, lies open at this moment as the grandest practical discovery to be made in medicine; and he who makes it can be begrudged his well-earned fame by none but by the selfish and the foolish. I write, therefore, not critically, but to record my own researches and their results.

12 *Hinde street, W. Sept. 4, 1855.*

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ART. V.—*The Effect of Chloroform on the Heart:*

OF all the muscles, the heart most readily manifests the influence of chloroform, for it exerts a direct control over the contractility of that organ. When directly applied, it arrests, either momentarily or definitely, all cardiac movement, according to the quantity employed; and if these movements are resumed after their suspension, another dose of chloroform will again suppress them. In this manner, an idea may be easily formed, in advance, of the effect which will be produced, by considering the quantity of chloroform used, the actual condition of the organ, etc. There is another important observation to be made in this connection: Chloroform always exerts a specific influence on the heart, whether it be applied to its exterior, or intro-

duced into its cavities, however small the quantity employed; whilst upon the pericardium, it is powerless, because the extreme density of that membrane renders it impermeable. Experiment: I exposed the heart of a frog, and by leaving the animal alone, assured myself that it was capable of its natural movements. With a delicate brush, I dropped gently the smallest possible quantity of chloroform upon the anterior surface of the ventricle. I observed the contractions become convulsive immediately, and after a few moments of agitation, the heat diminished rapidly. In three minutes, there was a very slow vermicular motion visible, which commenced at the right auricle, descended to the ventricle, lifting the point and folding it lightly upon itself, and then ascended from the ventricle to the left auricle, until it grew feebler by degrees and ceased entirely. Notwithstanding this paralysis of the heart, the animal still moved with considerable activity for some time, when it fell into a state of complete immobility, both body and members being perfectly inert. As the movements both of deglutition and respiration were entirely absent, the animal might have been considered dead. This state lasted only two or three moments, when a motion was perceptible between the apex and the base of the ventricle. By degrees this movement gained strength, regulated itself, and was finally fully established throughout the whole organ. The animal remained for some time extended upon its back in a quiescent state; it then began to make some movement with its anterior extremities, and subsequently with its posterior, until at length it resumed its wonted activity. In a few moments, it attempted to escape by leaping towards the opening of the vessel in which it was confined.

2. It is well known that when chloroform is applied to the muscles of a frog, and has produced a local paralysis of their tissues, its effects are extended by degrees throughout the whole body of the animal. But when it is applied to the heart, it does not affect it in the same manner, for that organ after being completely stupified, is capable of returning to its original condition, and the effects resulting from this anæsthetic application confine themselves often times to that particular viscus. In all experiments of this character, I repeat, that the local effects of chloroform are immediate, and that the heart may be seen to relax or remit its movements according to the quantity of the agent employed. In some instances, the movements of the heart are entirely and instantaneously suppressed, when dogs are suddenly compelled to inhale an atmosphere thoroughly saturated with chloroform; but frequently, among those who died in this manner, I have seen the movements of the heart return and become very vigorous as soon as the organ was exposed to the air; whilst with those who were only anæstheticised, and who readily resumed their normal condition, the heart continued to beat with decided energy. The same experiment can be made on them as on the frog, and it will be found that by applying chloroform immediately to that organ, its movements may be retarded, suppressed, etc., according to the quantity experimented with.

3. Chloroform exerts upon the internal surface of the ventricles an action absolutely identical with that described in the subjoined ex-

periment. A heart having been taken from the breast, the ventricles emptied of blood, and washed, I passed half a gramme of chloroform through the pulmonary vein. The organ was very much excited at the moment of infection, but in an instant after, it became absolutely immovable.

4. It is known already that when a heart is divided in fragments, each of these parts is capable of contracting, dilating, etc., in the same manner as the organ itself. So chloroform has the same power over them individually, as over the organ in its totality; to such an extent is this true—that particular portions of the heart may be in a state of great activity, while others are in the most absolute repose.

5. The faculty of resuming mobility, after having been quiescent, seems to be inherent in the substance of the heart. This elasticity appears to withstand the most depressing influences; and when it has been forced to yield on that account, it waits but for the slightest occasion to surmount the obstacles by which it is restrained.

6. After death by hanging, strangulation, submersion, etc., it frequently happens, that the heart which has been motionless in the pericardium, resumes its pulsation for some time and with great energy when that membrane is opened, and atmospheric air admitted into its cavity. The same is true after inhalations of chloroform.

7. Whatever the power which constrains the heart to resume its pulsations after having once been suspended by death, the fact of this resumption, suggests the idea, that in reference to the influence of chloroform especially, this viscus sustains certain relations to the nervous system essentially different from those which exist between that system and other muscular organs. The opinion of Bernard is also well understood in regard to this point, and he affirms that the muscles of organic life, and the heart particularly, perform their respective functions in a manner peculiar to themselves, and without the intervention of that nervous influence so essential to contractile tissue in general, and which is only exerted upon them in order to suspend their action, or to change their normal condition of activity into some other state.

8. If instead of plunging the heart or its fragments into chloroform, they are exposed to the action of curare, the contractions of the organ will continue; so that an important difference manifests itself in this connection between a substance whose influence though essentially local is equally uniform, and one which, though possessing the most deadly activity in itself, requires for the exhibition of this quality, that it should be administered under certain definite conditions. If the movements of the heart were under the immediate control of the nervous system, curare, which has a specific influence on that system, would arrest them instantaneously; but on the other hand, as this organ is directed by a form of contractility peculiar to itself, and over which curare has no power, no effect is produced by bringing the heart in contact with that substance.

9. Between the effects of poisons which are really absorbed, and

those produced by chloroform, there is a difference which cannot be too much regarded. In those instances where chloroform is introduced into a vein emptying into the heart, death is the immediate result; that organ becoming instantaneously paralyzed. Whilst, if curare be injected into the jugular vein, the cardiac pulsations become feeble by degrees, losing gradually both in energy and frequency, but never ceasing entirely, until a long period has elapsed after the operation.

10. There is a vast difference in the effects of an injection into a vein and an artery, as well as in the results of a slow and a rapid operation. M. Gosselin has seen dogs die instantly after an injection of three or four grammes into the jugular vein; and on the other hand M. Gérardin is said to have thrown seventy-five grammes into the veins of a horse without even producing anæsthesia. There seems to be a contradiction in these two facts, but in reality they sustain each other; the only difference depending on a question of time.

When chloroform is rapidly thrown into a vein, there is a great mass of blood affected by it, forming a clot that is borne to the heart, the functions of which are interrupted mechanically by it as by a foreign body, and physiologically by the influence of the anæsthetic itself upon that organ. But "*au contraire*," when the liquid is injected slowly, there is but a small quantity of blood affected at a time, and the clot is carried by the circulating current to the lungs, where it is again brought back to its normal state under the hæmotic influences which are perpetually operating in that locality. And as there is no chloroform in a state of freedom, *i. e.*, uncombined as in the last experiment, there is no specific action on the heart, whatever quantity of the agent be employed, thus anæsthesia is rendered impossible, and the result is invariably the same under every variety of circumstances.

Again, if the chloroform be thrown into an artery, the clots formed are inevitably swept onward by the sanguineous tide, and instead of being carried to those parts where the work of reparation is in progress, they are directed towards the periphery of the body, where they are soon arrested in the delicate capillary vessels, producing a suppression more or less complete of their functions, paralysis, etc. Under these circumstances, the celerity with which the infection is made is of no importance, whatever the extreme to which it is carried; whilst a more moderate method of procedure is alike immaterial in this connection. So from whatever direction the observation is made, the same result will always be attained, *viz.*:—the part corresponding to the artery which has received the chloroform becomes paralysed after the infection.

12. From all these facts, it seems demonstrable that chloroform enters in its natural state into the blood, to be transported by it throughout the whole system, and to produce immediate paralysis of the heart when it comes in contact with the cavities of that impressible organ.—*Translated from the Archives Générales de Médecine, Juillet, 1858. Med. Jour. North Carolina, Oct. 1858.*



ART. VI.—*Impurity of the Sub-nitrate of Bismuth.*

[The following extraordinary statement, taken from the last number of the *Transactions of the College of Physicians of Philadelphia*, is calculated to throw doubt upon the purity and safety of this drug as ordinarily found in the shops, in Philadelphia, and probably elsewhere. In a preceding article this medicine is highly, perhaps extravagantly, commended. Those who may desire still further to test its efficacy, should at the same time test its purity.—B. D.]

*Arsenical Poisoning—Arsenic in Sub-nitrate of Bismuth.*—Dr. Rogers called the attention of the college to a recent case of suspected poisoning with arsenic, the peculiar circumstances of which, as developed in the course of the legal investigation, rendered it unusually interesting in a medico-legal point of view.

The trial was suddenly cut short, and the accused party released, on account of an accidental impurity discovered by him (Dr. Rogers) in the medicine which had been administered by the attending physician of the deceased.

His object was not to discuss the evidence of poisoning in the case, although he had no doubt that, with all the facts elicited at the trial at his command, such a discussion would both be interesting and instructive; he wished to refer only to the point already alluded to as having terminated the prosecution; and, at the same time, to put the fellows of the college on their guard against an accidental impurity of a common remedy, which had been hitherto unsuspected, viz, the presence of *arsenic acid in the sub-nitrate of bismuth*.

Dr. Rogers was unable, from his own knowledge, to detail the history of the case from the beginning. His connection with the investigation dated only from the time when the stomach and intestines were brought to him, carefully separated from other parts of the body, and for chemical analysis *alone*.

The examination was conducted exclusively for arsenic, since that substance was known to have been procured by the accused; while there were other circumstances tending to create the suspicion that it had been criminally administered.

The stomach and alimentary canal contained a very small amount of pulpy matter. This, taken from the stomach and then from successive portions of the bowels, was analyzed, and in each case exhibited the presence of arsenic.

The material of the stomach itself was next examined, and likewise found to contain arsenic. The bowels were then divided into five equal lengths, and each subjected to the proper tests, and with the same results.

The presence of the arsenic in all of these experiments was not only unmistakable, but very decided in amount. How much the patient had swallowed, even presuming that it had been given with intent to poison, it would have been impossible to determine by the most careful quantitative examination, since copious vomiting and purging had

prevailed during several days, and up to a short period before death, so that the stomach and bowels were almost completely emptied of their previous contents.

The presence of the arsenic in the substance of the tissues examined naturally induced the presumption that it existed in the liver, lungs, and other portions of the body of the deceased. He regretted that he had not had the opportunity to ascertain the fact, as nothing had been preserved and sent to him except the parts referred to, nor were these in a condition to enable him to recognize either the presence or absence of the physical signs of irritation or inflammation, such as might have been expected in a case of arsenical poisoning.

Hence, in the absence of any proof of an absolute amount of arsenic adequate to destroy life, the mere fact of the discovery of a poisonous adulteration of the medicine administered was considered sufficient to invalidate the evidence against the prisoner, and to justify her immediate acquittal.

During the early days of the trial, nothing occurred to indicate that arsenic had been administered as an adulterating substance of the medicine directed by the prescribing physician. At a later period, the court called for a copy of the prescriptions which had been ordered by the physician, and sub-nitrate of bismuth appearing as one of them, Dr. Rogers directed his efforts to determine how far that substance was pure.

He was unable to procure a specimen of the identical sub-nitrate which had been administered, not succeeding in finding the druggist who had sold it, but obtained a number of samples from individuals throughout the city, and prepared, many of them, by different manufacturers.

Of ten specimens, some of which were imported, which he examined, all were found to contain arsenic with the exception of two.

He did not experiment to ascertain the per centage, but inferred it to be small, too much so, he presumed, to have any material effect except in long continued and unusually large doses. It was not shown in this case, nor was it probable that enough could have been taken in that way to produce the vomiting and purging, much less the fatal result. It was to be remembered also that the symptoms attributed afterwards to poisoning, had begun and continued for some time before the bismuth had been taken.

He then exhibited to the college the result of some of his analyses in the arsenical rings deposited within the tubes employed.

He remarked further that the druggists applied to for specimens or samples did not appear to be aware of any liability to this kind of impurity in sub-nitrate of bismuth, a fact which rendered it still more desirable that the subject should be brought before the notice of the college—since the usual mode of preparing the sub-nitrate, unless great care was taken in washing the product, rather favored the presence of arsenic acid, unless the bismuth from which it is prepared be pure.

Considerable discussion ensued on the part of Drs. Bell, Gross, Coates, F. Bache, H. Hartshorne, Hays, Jewell and Levick, upon this

ease, and upon the use and effects of large doses of the sub-nitrate of bismuth.

From this it appeared that, although all the fellows present had been for several years accustomed to prescribing large and small doses of the preparation, and often for considerable periods, no symptom of arsenical irritation had ever been observed. The only unpleasant effect noted was the slight temporary discomfort which is well known to occur in some peculiarly susceptible patients, or from its long continued administration.

Dr. Leviek, among others, referred to a case of phthisis, observed by him several years ago at the Pennsylvania Hospital, in which, on account of an obstinate diarrhœa, twenty grains of sub-nitrate of bismuth were given four times a day during the last two weeks of the patients life. At the autopsy, the bismuth was found lining the whole length of the alimentary canal. This observation accorded with common experience; and, although affording no evidence of mischievous operation in this particular case, it had awakened his attention, and had ever since induced him to be more cautious and reserved in the resort to the large doses of the remedy which have been prevalent for some years past.

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ART. VII.—*Vesico-Vaginal Fistula, operated on by Dr. Bozeman, with his Button Suture.* BY GEORGE BUCHANAN, A. M., M. D., one of the Surgeons to the Glasgow Royal Infirmary.

MRS. Mary Cairney, aged 35, was admitted to the Royal Infirmary on the 18th of August, and gave the following history of her case: She has had two children, and on both occasions labor was tedious. At the birth of her first child, which happened ten years ago, she was attended by a midwife, and labor lasted two days. She was delivered without the aid of instruments, the child being born dead. Her recovery was not tedious, and she soon regained her strength.

About nine years ago she had a second child, on which occasion she engaged the services of a medical man. Labor was again lingering, and at the expiration of thirty hours she was delivered with instruments, though of what kind she does not know. During the use of the instruments she suffered great pain, and the child was born dead. Immediately after the birth of this second child, she found herself totally unable to retain urine and fœces. The dribbling of the urine annoyed her so much that she became a patient in the hospital of Enniskillen, where she was under treatment for five months; but her health failing, she returned home for awhile. After several months she was readmitted to the hospital, and was again under treatment for a second period of five months, at the end of which time she was dismissed in much the same state as before admission. During her residence in the hospital she was seen by a number of

medical men, and various plans of treatment were tried, but of what nature she is unable to explain; the result, however, was always unsuccessful. Since then, that is for about eight years, she has remained in the same state, and has not applied for relief to any surgeon, although she has always suffered the greatest inconvenience and annoyance from the urine constantly dribbling away, keeping her clothes moist and foul.

On admission to the Infirmary, examination of the parts disclosed a fistula the size of a sixpence, communicating between the bladder and vagina, the situation of which was about an inch internal to the orifice of the urethra, and in the mesial line. The perineum was found to be ruptured, and the fissure between the rectum and vagina to extend for about three inches, laying these two canals into one for that extent.

Dr. Bozeman, of America, being in Glasgow on a short visit at this time, he was requested to examine the patient, and he pronounced it a case in which he could nearly insure success by his new plan of operation.

A consultation of the physicians and surgeons of the Infirmary was called, and they unanimously agreed to request Dr. Bozeman to perform the operation. Accordingly, that gentleman, having shown and explained the exceedingly perfect and ingenious instruments which he had brought along with him, proceeded to operate in the following manner, in presence of the hospital staff, and several medical gentlemen who had heard of the case and were interested in the result.

The patient was placed on a table on her knees, leaning forward on her elbows, and the parts were brought clearly into view with the aid of a single-bladed silver speculum, in shape somewhat like a shoe-horn, bent at a right angle about three inches from its broad extremity. This bent part was easily introduced, and being pulled up against the rectum, disclosed the cavity of the vagina throughout its whole extent, the fistula in view now on its inferior wall. The operator carefully made raw the edges of the opening, by seizing first the anterior lip with a small hook, and cutting off a slice about an eighth of an inch in breadth, the edge being bevelled at the expense of the vaginal mucous membrane, so that the raw surface was nearly a quarter of an inch broad. The same kind of edge was formed on the posterior lip of the fistula, but this was accomplished with scissors ingeniously contrived to be used with the right hand, and to cut either from right to left, or the reverse, as might be desired. During this proceeding a good deal of oozing of blood took place, which somewhat retarded the operator; but by careful sponging, the parts were kept free sufficiently to allow all the steps of the operation to be clearly seen. When the fistula was converted into an oval opening with perfectly smooth edges, the patient was allowed to rest a little till the bleeding should cease. The edges were then brought together with four silver-wire sutures. The needles were introduced at the distance of about three-sixteenths of an inch from the cut edge, not pushed through into the bladder, but turned across the wound, beneath its mucous membrane, and so brought out at a corresponding point on the opposite side. In this way four silk threads were

passed, and to their extremities the silver wires were attached and drawn through. When the opposite wires were pressed together, it was found that the edges of the opening fitted perfectly to each other. The wires were then fixed *in situ* by the button shield, which is the peculiarity of Dr. Bozeman's plan of procedure. An oval disc of thin lead rather larger than the opening, bent into a convexo-concave shape, was pressed against the wound, and found to fit the contour of the vaginal wall. In this were four small holes, at equal distances along its long diameter, through which the four pairs of wire were passed, and the shield was then slid down, its concave surface being pressed against the incision. The wires were then pulled tight, and fixed by leaden bullets. A catheter with a double curve was placed in the bladder, to be retained till the cure was effected. The patient was then removed to bed, and had immediately a grain of solid opium, which was ordered to be repeated every sixth hour. A nourishing diet was prescribed, with as little fluid as possible, and a pint of London porter daily. Previous to the operation the bowels had been cleared out by a large injection.

*August 19th.* Patient has felt comfortable since the operation. Urine comes freely by catheter, and is collected in a small bed-pan. None dribbles away by the vagina. The bed is kept quite dry.

*21st.* Still going on well. No uneasiness in the wound, and no appearance of ulceration going on. No motion of bowels, or tendency to it since operation. The opium was diminished to a grain, morning and evening.

*26th.* The catheter has daily been removed, cleansed, and replaced. It remains *in situ* without apparatus, and the patient has remained steadily in one position. The bowels have not been moved, and there is apparently no ulceration around the shield. This being the ninth day from the operation, and the period which Dr. Bozeman recommends, I proceeded to remove the apparatus. The patient was placed on her knees, as at the operation; and the vagina being exposed, with the aid of a bent speculum, I removed the bullets by twisting them a little to one side with a long forceps, and snipping across the wires between them and the leaden shield. When the bullets were cut off the shield fell off, and the wound was seen perfectly cicatrized throughout its whole extent. Three of the wires I easily got hold of with a pair of forceps, and pulled out; the fourth had got imbedded in the soft tissue, and I could not find it. However, knowing that a metallic wire would produce no irritation, I left it in, rather than disturb the parts by a prolonged search. The patient was again removed to bed, the catheter retained as before, and she was requested to move about as little as possible for a day or two. The opium was discontinued.

*27th.* Urine passes entirely by the catheter. Complaints of headache and uneasiness in bowels. To have two drachms of sulphur and bitartrate of potash.

*Sept. 1.* Bowels moved freely yesterday by a large dose of black draught, after which the use of the catheter was discontinued. She

can now retain the urine for some hours, and pass it voluntarily. Dismissed to-day.

On the 7th September I visited the patient at her own house, and found that the cicatrix had become quite firm. She can retain urine for two or three hours in the recumbent position, but not so long in the erect. Still complains of headache and indigestion, with irregularity of the bowels. I ordered some Gregory's mixture and a small dose of quinine, twice daily.

On 16th September the patient was stronger, but still had some uneasiness in stomach and bowels. She was ordered to take a little exercise in the open air, having confined herself to the house since she returned home. In examining the cicatrix, which I found perfect-strong, I felt the sharp point of the wire which was left in the tissues when the shield was removed. It had produced no inflammation or ulceration, and I easily seized it with the forceps and withdrew it. The bladder had not yet entirely regained its retaining power when the patient stands or walks, but she has perfect control over the urine for several hours when in bed. The bed is never wet, as it used constantly to be, and both the patient and her husband express themselves as exceedingly happy at the result, and thankful for the success of the operation.

I have published this case at Dr. Bozeman's request, and because the merits of this operation are still doubted in some quarters. True, it has not succeeded in every case, and in some instances death has been the issue; but the same can be said of every operation in surgery, however simple. There can be no doubt, however, that a measure of success has followed this proceeding which cannot be affirmed of any other mode of treatment. I do not intend to add any statistical or critical account of its success, but it may be proper to allude to the plan of Dr. Sims, of New York, who claims a large amount of success for his operation. To him belongs the credit of having introduced all the preliminary steps of the operation as performed by Dr. Bozeman. The use of the single broad-bladed speculum, and the position of the patient on her knees, does away with a great part of the difficulty of operating on a part so concealed and inaccessible as the wall of the vagina. The formation of a broad, bevelled raw edge round the fistula affords an extensive surface for union. The introduction of the sutures at a distance from the edge of the wound allows an amount of traction and support which cannot be attained by stitches placed close to the incision. Above all, the employment of thin silver wire as a substitute for silk thread prevents the ulceration or cutting out, which is inevitably caused by organic substances imbedded in the living tissues. It has recently been found by various experimenters that it is of little consequence what metal is used, and iron, copper, and platinum wires have been found to act as well as silver; and the material which may ultimately be chosen as the best suture will depend on the flexibility of the metal.

Dr. Bozeman, in the accounts which he has published of his operation, freely acknowledges that, in the proceedings above mentioned he has closely followed the directions of Dr. Sims, and only claims for his method of fixing the wires by a metallic shield, an amount of suc-

cess which has not followed that of Dr. Sims, who employs two bars of lead, after the manner of the quill suture. This method of drawing the edges of the wound together, called by Dr. Sims the "clamp suture," is a decided improvement on the common interrupted suture; but the use of a metallic shield, as recommended by Dr. Bozeman, seems to make the operation perfect. By its use the wires can be pulled perfectly tight, and the vaginal aspect of the incision is drawn up into the concavity of the shield; so that towards the interior of the bladder is presented, not the line of the wound, but smooth lips of mucous membrane. The shield also prevents the vaginal and uterine secretions from coming in contact with the wound, which is thus protected, both externally and internally, from the presence of irritating fluids. In the case detailed, the menses flowed during the progress the cure; of and had it not been for the shield, there is little doubt that a considerable irritation would have been set up.

It is much to the credit of American surgery that this distressing affection can now be treated, with a probability of success even greater than many other surgical diseases; and the surgeons of this country cannot fail to recognize the claims both of Dr. Sims and his follower, Dr. Bozeman. Dr. Sims has done much by perfecting the operative part of the proceedings, but we cannot deny to Dr. Bozeman the merit of adding the last, a very essential part of the treatment. It only remains to add, that the whole of the hospital staff, as well as the other medical men who were present at the operation here detailed, besides being satisfied with the perfect adaptation of the operation to effect the end proposed, were unanimous in according to the skill of the operator a large share of its successful result. There was but one opinion, that while he was entitled to share in the credit of devising the means, his mode of employing them exhibited the master-hand of an accomplished surgeon.—*Glasgow Med. Jour.* October, 1858.

*Vesico-Vaginal Fistula—Dr. Bozeman's Operations.*—So much interest is excited just now by Dr. Bozeman's suture in the treatment of vesico-vaginal fistula that it may be well to inform our readers of the result of his practice in this country. We learn from the last number of the *Edinburgh Medical Journal* that Dr. Bozeman operated in two cases in Edinburgh; one of these was perfectly successful; in the other case, the patient died of pyæmia, and the fistula was found after death completely closed, there being a small suppurating point on the neighboring mucous membrane of the bladder. In the last number of the *Glasgow Medical Journal* a case is recorded in which he was successful. We have only heard of one case in London in which he has operated, which was at University College Hospital. This proved unsuccessful; so that of four cases we have one death, one failure, and two cures. His suture was tried two months ago in the perineo-plastic operation for prolapsus uteri at the Samaritan Hospital, and with complete success, by Mr. Spencer Wells. The union was perfect, and the suppuration along the track of the deep sutures, which is sometimes so troublesome, was entirely averted. Professor Simpson has quite recently made a great improvement upon Bozeman's

button, by substituting for it a sort of wire splint, which is lighter than the button, and keeps the parts more free both from longitudinal and transverse movements than the clamps or button of Sims or Bozeman. Dr. Simpson has already had two successful cases. He prefers iron to silver wire.—*Med. Times and Gaz. Med. News.*

*Ethical and Critical Remarks on Vesico-Vaginal Surgeons and Surgery.*

The great success of Dr. Bozeman in the treatment of Vesico-Vaginal Fistula, both before and since Dr. Sims's unwarranted denunciation of B. (in a publication entitled "*Silver Sutures*") must be particularly gratifying to his numerous friends, while it, at the same time, affords the most satisfactory refutation of the disparaging reflections cast upon his professional character by Dr. Sims, whose *silver suture* censures seem to exemplify one of Rochefaucauld's maxims, namely: "Those who imitate us we like much better than those who endeavor to equal us. Imitation arises from esteem, competition from envy."

Dr. Sims in claiming exclusively for himself the discovery of the silver suture, and in proclaiming this assumed discovery "the great surgical achievement of the nineteenth century," errs, but injures no one but himself in the premises; the moral code is not thereby infringed. But neither medical ethics nor courts of justice will sanction a publication declaring that an able and honorable physician is destitute of professional character or "*any professional position.*" Were this charge altogether true, its *publication* is contrary to the law of the land and the public good. It is, however, far different in impersonal or scientific disputes. Blessed is the man who invented controversy and contradiction; for they have paved the way to inquiry, discovery, and the well-being of society.

The absolute and paramount superiority claimed for silver wire for closing wounds, etc., is imaginary. With the common sewing needles applied to wounds of the face, for example, the surgeon will succeed just as well as with anything metallic.

Neither clamps nor buttons should be designated the greatest discoveries of the XIX century. Celsus, who lived in the days of the Cæsars, was not ignorant of fibulæ, clamps, etc.: *Fibula*, a clasp, buckle or button. Celsus quotes the Greek word *anteras*, which he latinizes by the word *fibula*, which is generally translated into *suture*. Mr. Lee, in his *CELSUS*, gives a critical note upon this term: "*Fibula*, literally translated, signifies a clasp or buckle in classic authors; but the antiquarian critics have yet to determine the nature and



form of CELSUS's *fibula*," etc. Although Mr. Lee translates this word, *suture*, yet, on reading the judicious advice of the Roman doctor concerning the application of the fibula, it appears that he uses the term as comprehending both pins and ligatures (L. V. c. 26).

Dr. Sims's claim to the greatest discovery of the nineteenth century, founded on the application of silver sutures to the living tissues, is wholly gratuitous. The following extract is taken from a note book, anno 1829: It appears from the experiments of Dr. Levert (of Alabama), made upon dogs, that metallic ligatures, as lead, *silver*, etc., applied upon arteries "*are free from danger.*" When arteries had been tied with silk ligatures and the like, he found on killing the animals, abscesses at the site of ligatures, but when lead, silver, etc., had been used, these latter were *found encysted, without either inflammation or sup-puration.* I quote this note, because I have not access to the original paper which appeared in the *American Medical Journal* for May, 1829.

M. L. J. Bégin, in an elaborate article on the ligature (*Dict. de Méd. et de Chir. T. onzième*, Paris, 1831), gives a summary of Dr. H. S. Levert's experiments with metallic ligatures: "Dr. Levert, of Alabama," says M. Bégin, "having observed that bullets, pieces of iron and other analogous substances, often remained for long periods imbedded in the economy without injury, was led to think that threads of gold, *silver*, or platina, might, in like manner, be applied with the same impunity, a deduction which he in part confirmed, by experiments on living animals."

Dr. Bozeman has repeatedly acknowledged Dr. Sims's great merits, priority, and former successes in the speciality which both have cultivated with unrivaled skill. If Dr. Bozeman has improved upon and surpassed Dr. Sims's operative procedures and successes, so much the better. No one is injured. Many are benefitted. Dr. Sims should be proud of his pupil in this department of surgery. Jobert, Sims, and Bozeman may be surpassed by their successors.

Jobert, Sims, and Bozeman, now probably the most prominent and successful operators in vesico-utero and recto-vaginal fistulæ, have been preceded in these operations by their cotemporaries as Mettauer, Hayward, Pancoast, Brown, and others, whose procedures were virtually similar though not identical in detail.

In making these remarks, no motive at all hostile to Dr. Sims has had any influence. On the contrary, notwithstanding the exception-

able character of the book on "Silver-Sutures," admiration for his deeds, if not for his published words, is paramount.

The writer of these lines is, with the exception of the "Silver-Sutures," exactly in the position of the Ass which stood equi-distant from two haystacks, having no stronger motive to go to the one than the other, and, therefore, could not move either way, while at the same time, he yearned towards both. They are both doers of good, and should love each other.

Dr. Sims will probably not accept the writer's admiration, much less return it upon the usual but fallacious principle of reciprocity. He who esteems another has no reason in the world to claim a similar return. Esteem or true love is involuntary, being founded on real or supposed merit. Hence an anonymous poet justly says:

"Adieu! adieu! I'll think of thee,  
But dare not add, remember me."

B. D.

ART. VIII.—*Clinical Lecture. Tonics and their Administration*: By Dr. GULL, Guy's Hospital.

GENTLEMEN,—I wish to speak to-day, in form of clinical lecture, of a subject that—to use a common expression—has been weighing on the mind of myself and more than one of you for some weeks; it is what may be the nature of the "general debility" that has been so prevalent during August this summer, in almost all our patients in this hospital? I told you in the wards I should take the first opportunity I could to enlarge on this point, and strive to explain its value or signification.

We have passed through a curious phasis of belief in the medical schools, that purging and bleeding were necessary to stop inflammation, and now we are fluctuating towards the opposite extreme, or all traveling on an opposite road. It is now thought that the chief diseases are due to debility, and inflammation must be stopped by stimulants. It was at the confluence of such logical cross-roads, Lord Bacon set up his crucial instances, or finger-posts, or crosses to direct inquirers. That case which occurred to us this week is a case in point. I said I thought it softening of the brain, to be treated by generous diet and stimulants, but it turns out, on the post-mortem table, a case of sthenic inflammation. I told you at the time of the autopsy I should make some observations on stimulants and their value in cases of debility. I will make a few observations on this term "debility," as I know it quite falls in with your wishes, as ex-

pressed in the wards. Does this debility really and positively exist, or is it a cloak for our ignorance? We hear it said every day, "What did he die of?" "Oh, general debility!" I believe the term is a lazy one; it is an indefinite term, it is a negative term, and so it is a dangerous term for us to use. Do we find in the dead-house any disease that Dr. Wilks could term, pathologically, "general debility?" Certainly not! Now, a negative thing is one of the most difficult things possible to establish, or to argue about. I find diseases of the circulating system especially connected with what is called general debility; then, again, I meet patients sent to me dying of this general debility,—but I find albumen in their urine; this directly weakens them. I find in another that he has decided urea in his blood; some change going on in his system, for which I advise change of air, and he gets well. A lady called on me yesterday, suffering from "general debility," but I detected that she had been suffering from a "miasm;" in two other cases I found the general debility to arise from slow poisoning by lead. Dr. Addison was so struck by this asthenia, or debility, that he was led to examine all the organs very closely, and only then found the supra-renal capsules diseased—this disease probably interfering with the ganglionic nerves and function of healthy blood! You will not be long in practice, depend on it, when you will be consulted about general debility—the cause of which you must find out for yourselves, if you intend to remedy it. It may have its seat in local pleurisy or consolidated lung, and your patient becomes weaker on any extra exercise having been taken. The existence of such a disease leads to errors of circulation, or sanguification, with which, no doubt, you are all familiar. But in whatever mode the disease is caused, I wish especially to fix your attention on the fact that the disease will be presented to your notice as what the French call *malaise*, but patients term general debility. "Only cure them," they will say, "of this general debility and sense of sinking, and they are sure they will get well." This is why sea air is so useful in August or September.

What is strength or life? A child is comparatively weak, or not strong, as compared with an adult, but we do not call that debility; the tissues that exist may be in full activity, but the tissues, *quoad* function, are not yet fully developed. Old people, again, are weak, but here the tissues are debilitated, or becoming worn out. I do not think in adult people all the tissues are changed every seven years, as popularly believed to be the case. Now, I should ask, especially in adult patients, What is strength? and you at once see it is a very complex thing. When the patient advancing in years says he is suffering from general debility, you will probably find also some local disease reacting on his organism. Our tissues represent to us curious and beautifully elaborate parts in a state of vital "tension." A spring of a watch and its wheels are elaborate, but of no use till the spring be placed in a state of tension. Food (and vegetable food especially) supplies the something we call tension, but in the healthy capacity of each patient to exhibit or sustain this tension, consists his strength; in a word, the power of assimilating food through all its different stages. Now, we know that the impure,

close air of cities, in August and September, is very unhealthy, whether from absence of oxygen, or from the vegetable world being in abeyance as regards its function of absorbing sewage gases, we do not know ; but the impure close air of London—in August especially—keeps the spring unbent ; children especially do not take their food, and debility with them becomes very prevalent, with diarrhœa, etc.

I am often reminded (in practice) of an excellent observation also, by my friend Mr. Hilton ; he says : “ If limbs remain not used, they waste away—thus, by contracting the muscles ; this very functional activity necessitates their nutrition.” Now, amongst young ladies, in private practice, you will find the limbs and muscles remain not used ; their tissues do not waste enough, and they may be actually debilitated or weak, because they appear so strong ! Waste of tissue and nutrition, by some wonderful law, go together. Exercise in open air also gets rid of *excreta* that lurk in the system and cause debility. Lying in bed is a very common cause of debility—a proper waste of tissues does not go on.

Patients often come to you also with some obscure but active inflammation in the chest—a common cold or influenza—and they invert the old rule, as they “ feed a cold, but starve a fever.” They are wretchedly debilitated, though, perhaps, taking beef steaks or porter. You prescribe for such cases a saline with antimony, and vegetable diet, and mild starvation for a few days, and they will grow strong, which they would in vain strive to do previously by taking food that was never assimilated. In the same way, you may have good nutrition, but too much exertion of muscle and waste of tissue, as in the very poor class of patients that come to hospital. Lying in bed is a cause of debility, as I have just said ; hence, surgeons put on starch bandages in fractures, and send such poor patients into the air. If you find in a patient a very irritable nervous system, you will have also waste going on, waste even during sleep. Excessive exercise also will waste the muscles, but proper or moderate exercise strengthens them. In London, one finds both extremes—one set of people all idle or sedentary, especially patients of the upper classes, or of the female sex ; their general debility is cured by prescribing exercise. They tell a good story of Dr. Jephson, who used to cure his delicate lady patients at Leamington, by taking them out in a carriage,—but, suddenly, something happening which obliged the debilitated patients to walk back over a rough road, three or four miles. We have then, patients also of the other extreme, who are “ walked off their legs ” by the toil of business. A few glasses of wine, or a little bark and ammonia does much good in such cases as tonics. You see at once, therefore, that it is a tedious process to find out if a patient owes his or her debility to waste of “ tension,” or diminished “ tension,” or whether you are to over-rest or to over-exercise. Sleep, too, I may tell you (at least, healthy sleep), very much depends on healthy waste of tissues and their normal reparation.

Now, about tonics. You naturally will say, what about quinine ? Muller says, we know of no “ direct tonics.” I believe, also, there are no tonics but those which wind up the “ tension ” of our tissues.

Whatever will do that has, for all practical purposes, been regarded as a tonic. Well, iron and quinine are the favorite tonics, but in some cases a purgative would be a better tonic, so as to act on the liver; in some other cases, wine is a direct tonic, or food. A gentleman recently came to me suffering under "general debility." I saw at once his liver was the diseased part. Some small doses acted, as I call it, as a direct tonic. In another case of debility, I detected "Bright's disease," with intense debility. Well! we "snuffed the candle." The flame thickening, and dull from excess of effort, tissue was changed by one or two doses of elaterium into the cheerful glow of comparative health; elaterium here I call a tonic. We took away what prevented the tension of the vital forces, and the elaterium produced a state of strength; in short, acted as the very best sort of tonic. We may scientifically vary our medicine, but I call them all "tonics," as establishing the normal "tension" of the constitution adapted to health.

In the recent "blood-letting controversy," or discussion as to the nature of what is called inflammation, almost everything turns on the meaning of the terms "debility," or asthenic power, active effusion in inflamed parts, or sthenic power, etc. Want of power or debility, is even oftener seen in surgery than medicine—as exhibited in want of "tension" of the system to carry patients through a formidable operation, and the subsequent reparation of the injury. Surgical erysipelas, for instance, is a disease of debility, and wine one of the chief remedies. In the daily practice of medicine, I find wine to be our almost single direct tonic, and to act as a food, or, at all events, to economise food. I agree with the views of Dr. Todd on this subject. Of course, wine is only adapted to some cases, such as wasting fevers, or surgical cases, for instance. Wine stops debilitating diseases like erysipelas, etc. Exercise may be a tonic. Iron may act as a tonic, or even minute doses of strychnine. But if you will apply the observations already made, you cannot be at a loss to find why they are tonics, and the best kind of tonics.

I have done enough to-day, however, if I have indicated a certain line of thought for you to follow. I could have wished that I could offer you something more of the vast number of facts bearing on this subject, and leading to this doctrine of debility depending on want of vital tension in the animal tissues. "It is not wings, but rather lead and weights," says Lord Bacon—"non pluma sed plumbum potius et pondera")—that the human understanding wants; something to restrain its tendency to leap and fly at once from particular facts to universal principles. You must not, therefore, from a few facts take these principles for granted till you have calmly worked them out for yourselves, in the practice of the wards of the hospital, if the subject is left with you in a fragmentary or imperfect shape—observed once of a lecture by Mr. Astan Key in this theatre—it is with the intention that you may go over it yourselves, and fill in the outline. One thing is, at least, quite certain—that the medical schools are passing out of the old doctrine of heroic blood-lettings and antiphlogistics; and the treatment by stimulants of this "general debility," of which I have spoken, has become a reigning fashion. Still, as I said

already, the term "general debility" is a negative term, a lazy term, an indefinite term, except we examine it below the surface, and prescribe accordingly.—*Med. Circular.*—*Med. Chronicle.*

ART. IX.—*On Periodic and Eruptive Fevers—Their Nature and Prevention.*—By Dr. FREDERIC JAMES BROWN, Chatham.—[Speaking of the nature of periodic fevers and of typhoid and typhus fevers, the author offers the following speculative considerations in the form of propositions:]

*First Proposition.* That enlargement of the spleen precedes or follows the first attack.

The researches of Surgeon Dempster and Major (now Colonel) Baker, in India, show conclusively that splenic enlargement is a direct effect of malarious influence. These researches establish the fact that a large per centage of the inhabitants of malarious districts present an anatomical element of disease; namely, enlargement of the spleen.

*Second Proposition.* That no poisonous matter is introduced into the blood.

I find periodic fevers arising in damp situations where there is no decaying animal or vegetable matter, therefore I cannot but accept the conclusion that periodic fevers are produced by some other cause than gaseous emanations or poisonous matter in the fluid or solid form, from decaying organic matter. I fully admit that fœtid gases produce an injurious effect upon the human organism; but this question is distinct from the subject under consideration.

The sequence of occurrences in periodic fevers is utterly opposed to our ideas of disease produced by a blood-poison. Take those affections in which a virus is introduced into the blood, such as syphilis and hydrophobia, and compare them with ague. It is true that syphilis presents many remarkable progressions and retrogressions in its course; but the cutaneous eruption, the contagious nature of the disease, and its capability of being propagated by inoculation, and other circumstances, mark a wide separation between this disease, the effect of a blood-poison, and ague, a disease caused by diminished nerve-force. The phenomena witnessed in periodic fevers are wholly different from those seen in diseases that clearly arise from the introduction of animal poison into the blood. With respect to diseases produced by vegetable poisons, I am not aware of the existence of any such. We are acquainted with diseases caused by mineral poisons; but poisons from the vegetable kingdom kill rapidly without inducing a train of symptoms that endure beyond a few days. In other words, the vegetable poisons produce acute toxæmia, whilst the animal and mineral poisons give rise to acute, sub-acute, or chronic toxæmia.

Another argument in opposition to the idea that periodic fevers are due to blood-poisons, is as follows: A man may contract ague in Essex, may go to Edinburgh (where there is no endemic malarious disease), may remain free from ague for a year or more, and be affected by the

disease on the occurrence of a cold wet day in spring. This is a series of occurrences never observed in any disease that is proved to be due to virus.

*Third proposition.* That damp earth is necessary to the production of periodic fevers.

Damp must be in connexion with soil for the production of these fevers. The damp decks of a ship will not originate this form of fever, although a recurrence of fever may take place from this cause. It may be objected to this proposition, that the bogs of Ireland have no influence in producing ague. It is true that the Irish bogs do constitute a remarkable exception. I put forth the hypothesis that the peat acts as a nervine tonic, and so enables the individuals residing on the damp land to bear the loss of an abnormal amount of electricity with impunity.

*Fourth proposition.* That periodic fevers are due to telluric causes, viz.: the abstraction of positive electricity from the human body by induction, in abnormal quantity, through the agency of damp earth.

The fact is well ascertained that positive electricity is removed in abnormal quantity under the circumstances just enunciated. The order of sequences I give hypothetically. Abstraction of positive electricity in undue proportion; disordered function of the sympathetic nervous system; changes in the constitution of the blood, viz.: first, hyper-nosis; finally, leukaemia; changes in the condition of the spleen and liver.

It may be objected to this proposition, that the lower animals grazing on damp land should experience the same ill effects as man.

I attribute the immunity of the lower animals to the natural clothing of hair and wool that they possess, which preserves the equable temperature of their bodies and acts as a non-conductor of electricity. It is certainly an extraordinary circumstance that the lower animals should enjoy immunity from periodic fevers, but it is a fact. It is equally certain that the drainage of marshes and the diminution of cases of ague in the malarious districts, bear a direct ratio one to another. Hippocrates banished periodic fevers from the city of Abydos by causing the marshes in its vicinity to be drained; and I believe that London might be similarly freed from malarious diseases. In my opinion convict labor might be beneficially employed in the drainage of the marshes bordering the Thames and the Medway. In this way, men that offend a civilized community might make reparation for their faults. Dr. Handfield Jones has drawn up a list of about a score of diseases allied to ague, such as neuralgia, certain forms of rheumatism, neurosis of the nervus vagus and solar plexus. All these diseases will disappear when we bring the *will* to bear upon them. The people have the *power*: they only want the *knowledge* and the *will*. It is for us to supply the knowledge and to stimulate the will.

I have next to inquire into the nature of typhoid and typhus fevers. I will premise by a few remarks on pyrexia in its most extended signification. Pyrexia consists in an augmented temperature of the skin, due to an augmented cremation of the tissues of the body. The increased temperature of the skin can be measured by the thermometer, and it is found even during the stage of rigors. Pyrexia attendant

upon inflammation I believe to be due to changes in the condition of the sympathetic nervous system, and to changes in the constitution of the blood, and to alteration in the condition of the tissues. Catarrhal and biliary pyrexia I attribute to similar changes, arising from arrested or altered secretions. Typhoid fever I believe to be caused by the application to the alimentary canal of the excreta of the intestines that have undergone alterations outside of the body. The sequence of occurrences is as follows: Faecal matters (altered in their nature) are swallowed in water in almost all towns, and even in lone houses in the country, in consequence of the proximity of wells to privies and drains; blood-poisoning and disordered sympathetic nerve-force result: the skin and the glands of the small intestine become affected: and the disease known as typhoid fever is produced, having a duration of from fourteen to twenty-eight days. Typhoid is essentially a privy-soil fever, but night-soil can give rise to other diseases beside typhoid fever, viz., cholera, diarrhœa, and dysentery.

I now proceed to consider the nature of typhus fever. I believe that typhus is produced by the respiring of air charged with a larger percentage than usual of animal extractive matter present in the expired air. The excretory matter from the lungs and skin, when respired in a concentrated state, as occurs in over-crowded apartments, gives rise to typhus fever, which has a duration of fourteen days, and which expends its force on the lungs and cerebro-spinal system. Typhus is thus an ochletic disease, or, in other words, a disease caused by overcrowding, and is essentially distinct from typhoid fever.

An interesting experiment was performed a short time since, on the continent. An animal extractive matter was obtained from the respiratory and cutaneous excreta of numerous individuals congregated in one apartment. This extractive matter was injected into the blood of a dog. The animal died of low fever in fourteen days.

Now, it will be observed that fourteen days constitute the full period of typhus fever. If this experiment shall be repeated many times with the same result, I shall deem the matter to be demonstrated.

That overcrowding is the cause of typhus fever, is further shown by the occurrence of this form of fever in jails, barracks, and the habitations of the poor in large cities. Civic populations usually suffer from both typhoid and typhus fever, because of the contamination of the water and the overcrowding of the inhabitants; but in towns occupying much space in proportion to population typhus bears a small ratio to typhoid fever. This is the case with Rochester and Chatham, as I showed in a paper read at the Epidemiological Society, in April, 1855.

In the third part of this paper, I beg leave to point out the measures necessary for the prevention of the fevers that we have been considering. Marshes must be drained; but until this can be effected the inhabitants of paludal districts should take a daily dose of quinine, and should wear flannel clothing and a non-conducting material within their boots. The conversion of marshes into bogs might prevent ague (could it be effected by the cultivation of the plants found on bogs or by other means), but it would not meet with



the approval of agriculturists and graziers; therefore drainage is the only practicable mode for the conversion of marshes into salubrious ground. Then as to the prevention of typhoid fever; this can only be obtained by a pure water supply from a source distant from towns, and by effective sewerage. The prevention of typhus would be certain if abundant space were included within the area of towns, together with an improved construction of houses and free ventilation. Our public buildings, such as hospitals and union houses, should be built after the model of the hospital at Bordeaux and that lately in use at Renkioi. Ancient Rome suffered from periodic fevers, because of the proximity of the Pontine marshes; and she probably suffered from typhus in consequence of the enormous population within her walls; but I should imagine that she was free from typhoid fever because of the purity of the water supplied by her magnificent aqueduct, and the effective sewerage that was employed.

Before concluding this paper I wish to remark that bad smells will not produce typhoid fever, under ordinary circumstances, although they cause nausea and depress the general powers of life. But now and then it occurs that the opening of privies long closed is directly followed by typhoid fever of a malignant type. Such an instance occurred in Rochester several years since. I believe that in these instances the faecal matters (altered in quality) are actually swallowed with the saliva,—minute particles impinging on the pharynx, and being thus conveyed to the lining membrane of the alimentary canal.—*British Med. Jour.; Braithwaite's Retrop.*

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ART. X.—*Practical Surgery and Medicine.* (Extracted from Braithwaite's Retrospect. 1858.)

1. *Unnecessary Orthopaedic Operations.*—Operations are resorted to too frequently in the cure of talipes varus. As a consequence, though doubtless the cure is more speedy, we have various permanent ill effects which might have been avoided. Thus, the fleshy part of the calf is too small and too high up in the leg from contraction of the muscle, and the free motion of the ankle-joint is impeded by the prolonged rest which has been necessary to the success of the operation. Where, then, the foot can be abducted by the hand of the surgeon, and whilst so abducted can be bent to a right angle, by the use of gentle violence, a cure may be effected without tenotomy, before the usual age at which sound children attempt to walk, and even if an operation be performed, mechanical treatment cannot be dispensed with before this age. When the surgeon cannot perform this movement with the child's foot, and the imperfectly developed os calcis seems tied to the back of the tibia and fibula by deep ligaments, and a deep depression exists between the great toe and heel on the inner

side of the foot, the case cannot be cured without operation. The secret of curing the case without operation consists in applying the bandage and splints (which may be made of gutta percha, moulded leather, or tin), so as not to distress the infant. They should be removed at least three times a day and manipulation practised, and re-applied more with a view of preventing relapse into a wrong position than of forcing the lapsed part into a better one; and during this treatment recollect first to obtain eversion of the point of the foot before attempting depression of the heel. Although a case may be apparently cured, yet in the course of time deformity may again return owing to the muscles and other textures on the originally contracted side of the limb not keeping pace in growth with the other. Such cases can generally be restored by due pains in efficient instrumental and manipulative treatment. (Dr. J. W. Little.)

2. *Talipes Varus*.—The theory of stretching the new material formed in the reparation of tendon, after division by the knife is altogether erroneous. The new material does not admit of being stretched like india-rubber, and then, unlike this material, of being retained at any required length. Our object must therefore be to regulate the length of the new material during its formation. In *slight* infantine cases, divide all the tendons necessary at one operation, and after the foot has been retained in its deformed position by a bandage and splint for three days, apply Searpa's shoe, and in the course of two or three weeks bring the foot into its natural position. In *severe* cases the treatment, both operative and mechanical, must be divided into two stages; the first to overcome the inversion of the anterior part of the foot, and thus convert the case into one of equinus—the second stage to cure this equinus. The first stage is accomplished by dividing the tendons which invert the foot, and then the cutaneous punctures being healed by the application of a bandage and splint to the outer side of the leg and foot, gradually to produce eversion. The second stage is accomplished by division of the tendo-Achillis, and then by the use of Searpa's shoe, to produce flexion at the ankle-joint. If at the end of the fourth or fifth week the tendo-Achillis should still appear to be strong the shoe should be worn only at night, and the frequent and regular employment of passive motion is more to be relied on than any mechanical aid. (Mr. W. Adams.)

In all *relapsed cases* of talipes varus, in which the deformity can be removed, the relapse is owing to some defect in the after-treatment. Among defects in the primary treatment are, omission of the division of the posterior tibial tendon, division of the tendo-Achillis at beginning instead of at the end of the operative treatment. Among defects in the after-treatment are omitting the use of such retentive mechanical means as the case may require, and of active and passive muscular exercise, in the right direction. The earlier the treatment is commenced the less is the tendency to relapse. In very rare cases there is a congenital defect in muscular development. Here some form of retentive apparatus must be worn by the patient during the remainder of life. In the treatment of these cases, where any marked degree of inversion remains, and is accompanied with rigidity of the

foot, treat the case throughout as if nothing had been done. In most cases division of the tendo-Achillis, or of this and the plantar fascia will suffice. (Mr. W. Adams.)

3. *Dr. Fell's Treatment of Cancer.*—If non-ulcerated, the skin must first be removed by some liquid caustic, as nitric acid. The tumor so exposed must be covered by a layer of chloride of zinc paste spread on linen. The following formula is used:—R. Sanguinariae canadensis,  $\bar{3}$ ss. vel.  $\bar{3}$ .; chlor. zinci;  $\bar{3}$ ss. vel.  $\bar{3}$ ij.; aquae,  $\bar{3}$ ii.; pulv. sem. tritic. hibern., q. s. Mix and form a paste the consistence of treacle. Through the slough so produced incisions are to be made with the knife, and strips of cotton spread with the paste introduced daily, till the whole tumor is seriatim converted into one large eschar, which drops out entire in about 12 or 14 days. To any secondary tumor or enlarged lymphatic gland, the following ointment may be applied:—R. Sulph. zinci,  $\bar{3}$ vi.; sanguinariae,  $\bar{3}$ ij.; myricae ceriferae,  $\bar{3}$ j.; extr. opii (aquos.), extr. conii, aa  $\bar{3}$ vi.; ungt. cetacei,  $\bar{3}$  vi. Mist. et fiat ungt. With this ointment one containing iodide of lead may be applied alternately every twelve hours. (Dr. Fell, p. 37.)

*Gastric Fever of Children.*—The following treatment is adopted by Dr. Farre, of King's College Hospital. To a child of a year and a half to four years old give a mixture containing a drachm each of the diluted nitric and hydrochloric acids to six ounces of water, in doses of half an ounce three times daily, with about four grains of grey powder every night at bedtime. If the disease is passing off, omit the powder, and give the acids in infusion of quassia or with bark.

4. *Treatment of Nævi.*—Pass two pins at right angles to each other under the mass, and place a ligature around the whole, tying it tightly and withdrawing the pins. At the end of four hours withdraw the ligature. A scab forms, which drops off in fourteen days or so, without any suppuration or open sore, and leaving but a very slight scar. By this means the current of blood is arrested, and that in the tumor becomes consolidated. One great advantage is, that the part encircled does not slough, but becomes atrophied and drops off. (Mr. J. C. Forster.)

Remove the epidermis by a blister, and then touch the denuded dermis with a pencil of charpie wetted with perchloride of iron, at 30° of the hydrometer of Baume (Sp. gr. 1.245.) (M. Paul Broca.)

In the treatment of nævi by caustics, none is preferable to chloride of zinc, it is more powerful in its action, and penetrates more deeply, than other caustics, and powerfully coagulates the blood in the tissues, surrounding the part to which it is applied. The surrounding parts having been protected by sticking plaster, or by a strong solution of gutta percha in chloroform, apply a paste composed of one part of the chloride of zinc to four of flour; this should be allowed to remain about twelve hours, when a white dense slough will have formed. As caustics are chiefly applicable where the nævus is large and superficial, it will seldom be necessary to repeat this; much pain is caused

the first few hours, but this abates; the cicatrix is usually small. (Mr. J. F. West.)

A case lately occurred to Mr. Hutchinson of the Metropolitan Free Hospital where there were more than a hundred distinct *nævi* of the most superficial character on the same child; all were cured, except about twelve on the scalp, by the continued application of the compound iodine ointment. To the remainder it is proposed to apply nitric acid should they not diminish under a continuance of the former treatment.

Mr. Haynes Walton has recently treated several cases of *nævus* in the following manner: A small tendon knife having first been pushed into the base of the tumor and moved about a little slightly, to break up the tissue, a solution of tannin, a drachm to an ounce of water, is injected by means of a syringe with a small nozzle. This must be slowly performed, and must be given up directly the tension of the tumor becomes apparent; by this means the blood coagulates, and after the lapse of some weeks the tumor disappears. There is no risk of sloughing taking place as when the muriated tincture of iron is used.

Apply a mixture of corrosive sublimate, one part, and collodion, eight parts; the eschar falls off in from ten to fourteen days, and is not followed by supuration. No pain is produced, and scarcely any cicatrix is left. For very small *nævi* one penciling is enough, but in larger ones this has to be repeated, and in these it is best to effect their destruction gradually. (Dr. Cösfield.)

5. *Treatment of Uterine Polypi.*—For the removal of these tumors an instrument lately invented by Dr. Aveling, of Sheffield, will be found very useful. It consists of a long curved stem, at one end of which is a hook, and fitting into the concavity of this is a plate of metal, which by a screw in the handle is capable of being withdrawn lower down the stem. When used the plate is screwed half way down the stem, and the instrument is passed up so that the hook may be round the peduncle of the tumor; by means of the screw the plate of metal is now forced up into the concavity of the hook. Thus the tumor is removed, without the disadvantages of either the knife or the ligature, viz.: of the former, hemorrhage, which is here avoided, as the peduncle is not divided by a cutting but by a *crushing* action, and of the latter, the fetid discharge caused by the putrefaction of the Polypus. (Dr. J. H. Aveling.)

There is no class of tumors in which the *écraseur* is likely to prove more valuable than in these. The dangers resulting from the other modes of removal are avoided, viz.: inflammation of the veins, secondary abscesses, etc., if the ligature be used, and hemorrhage if the more speedy knife be employed. The great and only objection to the *écraseur* in these cases is the difficulty of applying the chain high enough up, so as to include the base of the tumor: this is in a great measure overcome by seizing the polypus with a hook and dragging it lower down. (Mr. G. M. Humphry.)

Mr. J. B. Brown says that the best mode of removing these growths, is to seize them with a pair of long vulsellum forceps, and

having dragged them into sight to pass a needle armed with a strong double ligature through the base, which can now be tied in two parts, then cut off the polypus just anterior to the ligature, and plug the vagina with oiled lint. This plan is infinitely preferable to that of either simply cutting off the polypus, or the more tedious process with Gooch's apparatus, where the sloughing of the polypus within the vagina generally causes serious constitutional disturbance, not unfrequently pyæmia, and sometimes death.

Dr. Savage, of the Samaritan Hospital, lately removed a vascular uterine polypus, without pain or hemorrhage, by means of an *écraseur* the curve of which can be made to fall into the hollow of the sacrum and the point to pass up into the uterus. When removed by this instrument, the polypus is first seized by a pair of ring forceps, and the chain is passed over these, and drawn tight, precisely as the cord in the ordinary operation by ligature.

6. *Therapeutic Uses of Arsenic in Cholera.*—Dr. Black believes arsenic to have a specific action in cholera, from an experience of nearly two hundred cases, in none of which has it ever failed to produce a speedy and permanent cure. It may in very severe cases, where the cramps come on every few minutes, and the vomiting and purging are all but incessant, be given in doses of five drops of the liquor arsenicalis every fifteen minutes until the symptoms abate, and then be given every hour. (p. 248.)

Dr. Begbie says that the lameness and deformities of chronic rheumatism frequently disappear under a prolonged use of arsenic. It is generally given up too soon. Two cases of this nature are related, in which the most marked benefit was derived from this mode of treatment. About five drops of the liquor arsenicalis should be given after each meal, and continued till the characteristic effects of this drug are produced, when it may be intermitted for a time. Many and obstinate forms of neuralgia are in a similar manner found to yield to arsenic. Arsenic holds the "foremost place" amongst the remedies employed for the cure of chorea, and the author has never yet seen it fail. Besides the various forms of skin disease in which its use is so well known, it has been used very extensively and successfully by Dr. Simpson in amenorrhœa and other disorders of the uterus, where iron appeared to be contra-indicated, as well as in that peculiar affection of the bowels which he has described as prevalent among females, and characterised by copious discharge of membranous shreds, and accompanied by great emaciation and a long train of neuralgic and other nervous symptoms. Dr. Simpson places most reliance upon small and very long-continued doses of arsenic, as two drops of Fowler's solution, or a pill containing the sixtieth of a grain of arsenite potass, taken three or four times a day.

7. *Treatment of Boils and Carbuncles.*—The manner of operating by incision is neither requisite nor safe, and is much surpassed in efficacy by the use of *potassa fusa* for this purpose. It should be fresh, and unaltered by exposure to the air, and may be inserted into a goose quill by way of handle. The best time for opening, is when the skin

has become dusky, and is perforated by pin-hole orifices, whence issues an ichorous oozing. To prevent the caustic running and destroying healthy skin, a dossil of dry, flocky lint should be applied upon and around the part to which the caustic has been applied. The carrot poultice is the best that can be applied afterwards, with a view of cleaning the sore, and a solution of chlorinated soda is very useful at particular stages of the sore. A weak caustic, like nitrate of silver, is of no use, but even prejudicial in these cases. (Mr. B. Travers, jun.)

8. *Chronic Eczema in Children.*—First get the scabs or crusts separated, by means of poultices, if the spots are limited, and are not settled on the head, face, and neck, where these applications are not suitable. Before applying them, it is a good plan to moisten the surface with a solution of carb. of soda (ʒii. to ʒviii.) When the surface is large, a water dressing is to be preferred; a little sub-carbonate of potash being added to the water. If situated on the head, face, or neck, paint the scabs over with a mixture of carb. of soda and cod-liver oil, removing the crusts carefully next morning, and moistening the surface with the alkaline lotion. This must be repeated as often as necessary, till crusts cease to be formed, and a red, inflamed, but painless surface is left. Next, we must remove this condition of the skin. One of the best means to this end, is the application of a solution of ʒi. of acetate of zinc, and the same acetate of lead, in ʒviii. of distilled water, adding to this, at the time of using it, an equal quantity of strong chamomile infusion. The last indication is to restore the activity and healthy tone of the skin, by such hygienic measures as fresh country air, free exercise, a well regulated and wholesome diet, and such local applications as weak solutions of alum, and sulphate of zinc. (Dr. Behrend.)

9. *Treatment of Purulent Ophthalmia.*—During the acute stage, you may: 1. Deplete locally by leeches. 2. Scarify the inner surface of the lids. 3. Divide the external canthus, or scarify the ocular conjunctiva in radii, if chemosis is high. 4. Apply nitrate of silver drops, the strength varying according to the severity of the symptoms. 5. Foment constantly with decoction of poppies, containing a little alum. 6. After the bowels have been well opened, for which purpose croton oil is the best, as being the most speedy purgative, exhibit mercury, which must be used guardedly if ulceration threaten the cornea. 7. Give quinine when debility prevails. 8. Moderately nutritious diet should be given, and progressively improved. In the convalescent or chronic stage, tonics with varied and mild local astringents and slight counter-irritation must be used to complete the cure. The above is the treatment adopted at Guy's Hospital. (Mr. J. F. France.)

10. *Treatment of Cynanche Maligna.*—Give powdered guaiacum in combination with chlorate of potash. Support the strength with beef-tea, mutton-broth, etc. Cause the patient to frequently gargle the throat with a solution of chlorinated soda; or, if he be too young or unable to use the gargle well, cause the throat to be mopped or sponged with it. The secretions of course must be attended to; and

in the latter stages of the disease, tonics, such as quina, or bark, and nitric acid will be beneficial. (Dr. W. A. Bryden.)

11. *Treatment of Dysentery.*—In subacute and chronic dysentery confine the patient to bed, and in the recumbent position that the bowels may be supported and kept quiet, and unirritated by the action of the abdominal muscles. Milk and farinaceous food are most applicable, as least stimulating peristaltic action, and being most likely to be assimilated. In the subacute and severer dysentery of this country no remedy is equal to Dover's powder in ten grain doses, with an occasional dose of castor oil, guarded by a few drops of laudanum, for the removal of fecal matter. If there be a deficiency of bile, give a little hydr. c. creta; if there be much pain or tenderness apply a few leeches to the anus. In acute cases confined to the rectum, medicines administered by the mouth do little or no good, here give soothing injections with opium.

In the purely chronic form of the disease, the treatment consists in the administration of mineral and vegetable astringents, combined with opium, occasionally using aperients for the removal of feces and foul secretions. When the ulceration is confined to the rectum or lower extremity of the colon, astringent injections of acetate of lead, nitrate of silver, sulphate of zinc, and gallic acid, will be most serviceable. But when the ulceration extends more or less throughout the entire extent of the colon, remedies administered by the mouth, especially the mineral astringents, act most effectually. In these cases slips of blistering plaster should be frequently applied. In the more advanced stages of the disease, and in the milder cases where we have a healing but lax state of membrane, the stools being bilious and containing but little blood or mucus, the vegetable astringents will answer best. A strong decoction of the *Ægle Marmelos*, or Bael fruit of Bengal, is very useful. A few drops of laudanum may be added to each dose of this. (Dr. S. H. Ward.)

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ART. XI.—*The Present Aspect of Conservative Surgery and the Chloroform Question.*—By F. C. SKEY, Esq., F.R.C.S., Surgeon to St. Bartholomew's Hospital.

GENTLEMEN:—I don't think I can better begin a course of clinical lectures, which it is my duty to deliver in this theatre during the new year, than by making some few remarks in reference to what is called, in the general parlance of the day, "Conservative Surgery." By conservative or curative surgery, as applied, in the first place, to injuries of joints or limbs, I wish to signify that practice in surgery which, in the hands of some one surgeon, leads to a conservation or saving of a limb or joint, as contra-distinguished from another practice, which, in an entirely similar case, sacrifices the same joint or limb by amputation.

By "conservation," or some such term, we properly signify the recognition of the power of nature to repair what may appear otherwise beyond repair, and it obviously implies on the part of the surgeon a deeper knowledge or experience of the subtle, yet marvellous powers of nature, engaged in such processes, as well as an instinctive recognition of these processes and the point where nature is assisting us, and proving herself a true *vis medicatrix*; or where, overborne by sundry morbid agencies, art, even at any cost of mutilation, must come to the rescue.

And this "conservative" surgery is no mean or little thing; it engages the moral feelings as well as an intellectual analysis of the surgical facts that have previously come under your notice—for I now take it that you will all and each of you be one day or another embarked in surgical practice—both, mind you, are indispensable, the moral feelings and the intellect; nay, more, we must in a measure make each case our own, and sympathize with our patients, if we wish to master each case presented to us.

A man properly imbued with the idea of curative or conservative surgery must calmly study Nature—he must forget self-interest. Oh! do not for mere *clat* of operations, as do some, commit some crime—I was almost inclined to say, do not do anything to your patient, be he rich or poor, that you would not after the gravest study wish to be done to yourself in similar circumstances! That, indeed, must be your guiding rule in all your medical intercourse with the sick, but more particularly in this department. Remember, if you save a man's arm, to him it is almost equivalent to his life, as it is the means by which he lives.

But you are young, and you say, "Why all this—the surgical profession is surely an honorable profession?" and you read the journals, and "we are all honorable men!" Be it so; that is drawing it very mild, at any rate. Mark! I make no imputation against the great operators; but while human strength is mixed up with human weakness, and while the line is not well defined in surgical operations when *esprit de corps* or mere surgical *clat* trespasses on thorough conscientiousness or honesty, while error is likely to prevail, then it is your duty to keep yourselves properly instructed in what the lessons of experience everywhere are teaching us, and to what principles of surgery the "finger-posts" of statistics are pointing.

You may depend on it that Nature has a disposition to effect singular cures if we only did not thwart her designs. Nature is usually at work building up healthy tissues. I believe this is evident in a large majority of surgical cases. Look at a broken bone uniting. Sometimes, no doubt, there was side by side with such healthy physiological action a certain pathological or diseased action or element, but let us take care that we do not encourage the latter in place of the former; say that we bleed in a fever where we ought to give wine, then the disease gets ahead; or say that a patient dies of amputation or resection, where such operation was unnecessary.

If you believe with me that there is this tendency to mistake many things in Nature's work for something pathological—that from *esprit de corps* we take for granted Nature is going pathologically wrong as



often or oftener than she goes physiologically right—yet if I can show you this is an error, surely there must be something in our conservative surgery that deserves more than a mere passing notice.

Let us take care that this so-called *esprit de corps* does not alienate the public too much from us. Bacon made this remark a very long time ago. It is as true now as it was then.

All very well ! But what evidence can I bring that Nature, as we find in her hospitals, is not always going wrong ? I now remember the wards of St. Bartholomew's Hospital for a period of about forty years ; and if I look back at amputations, for instance, I find they are diminishing in number year by year. What is the secret of this ? I'll tell you. Nature now cures hundreds of cases that were formerly mutilated and amputated. Do you think we have in this department reached the goal, that we have nothing like steel or cod-liver oil in numerous cases, for instance, totally unknown forty years ago, yet to be discovered ? Is chloroform nothing ?

But what a melancholy thing it is that some surgeons still adhere to old routine. You will be astonished, perhaps, that there is no standard practice in our sixteen or eighteen London hospitals. The public does not know that even with this *esprit de corps*—of which we make so much ado—that the leg which is saved in hospital A is cut off and few questions asked at hospital B, C, D ; ay ! we're "all honorable men," saved perhaps at hospital E, or put in orthopædic training, but cut off at F, G, H, I, etc. Is it not our duty to society to save limbs, to cure aneurisms without cutting operations, to take advantage of chloroform, etc. ? To society did I say ! Here a man feels the blighting influence of this so-called *esprit de corps* ; he is outvoted in consultation, and by apparently wise heads ; he must not infringe old routine or the stereotyped rules of years ! But I tell you, and you may rely on what I say, our first duty is to society, though it may ill understand us ; our next, ay *longo intervallo*, our next duty is to our profession !

I am sorry to say, too, that the more remote the distance from London the worse do things appear to become, so firmly rooted do old prejudices keep their hold where once they have been implanted. In one small provincial hospital, with forty surgical beds, I have recently known there to have been more mutilations or amputations in one year than there were in the same year (of twelve months) in all our 340 surgical beds in St. Bartholomew's. The fear of using chloroform—another error—adds to the horror of such old-world wisdom. All professions, perhaps, even to the judges on the bench, are "infirm of purpose," if it comes to be a matter of common sense *versus* some old precedent or rule—in other words, a matter of *esprit de corps*. Still, even this latter must occasionally yield to the public learning things themselves.

There is no large or capital operation in surgery that is not undergoing a change in the direction of greater simplicity. Where are our pulleys in recent dislocations of the hip ? What improvement ever equalled that of the introduction of chloroform, of anæsthetics ? Pain abolished at one glorious sweep ; you know that even within your own memory, let cavilers say what they please, it has become

the recognized practice of all good surgeons in America, Asia and Europe to administer chloroform. Let no silly errors persuade you to the contrary. Again, look at injuries of the skull and the use of the trephine, where formerly it was used on every occasion (realizing the lines to Sidrophel—

“He used trephining of the skull  
As often as the moon’s at full,”

where formerly we were told to anticipate symptoms); now the trephine is scarcely used at all; in Germany it is entirely condemned.

Let us take lithotomy operations again, and compare them with lithotripsy. Yet though the appliances and means of lithotomy are more brilliant or improved, (I now speak of the lateral operation) the deaths are at least 30 per cent.; those of lithotripsy, which is superseding it in private practice, are only 5 per cent.

Does the conscientious surgeon treat surgical aneurism now in the same manner that surgeons of the last century did? No! We have been compelled, by the facts brought forward by the Dublin surgeons, to adopt the mode of cure by compression; it is not so brilliant or full of *clat*, yet it is eminently conservative, and saves many valuable lives. Some London men say they like a cut at their cases, but the short and best cut of a case of aneurism is to cure it, and that is effected by pressure; the amputations and deaths after ligature, even for popliteal aneurism, were something terrible and enormous a few years ago; now we hear of internal aneurisms cured by specific methods.

Old dislocations are now reduced in people of advanced years even at sixty, though Sir A. Cooper advised the opposite, and in the manipulation of other cases under chloroform it is marvellous what may be done. These are the cases that will make or mar you in practice.

As to the special subject of saving limbs after surgical injury, such as compound fractures with and without injury of vessels, I wrote a paper in 1855. I am every day more and more convinced of the force of the truths I then enunciated; I will not go with much detail into it.

If a limb be cold or mortified and vessels impaired, there can, of course, be no second opinion as to the propriety of amputation; but I take cases on the confines of this extreme condition, and I ask myself very often, shall I amputate or not? Fifteen years ago I had a great deal of night surgery at this hospital; I may say I lived next door; and I have had many a half hour’s walk up and down the quadrangle—half hours of agony, thinking, shall I amputate or not amputate? A hand and forearm of a patient is sometimes fractured by machinery, by a printing press at night—the old rule, I now firmly believe “more honored in the breach” than in any, even at least slavish observance, was always to amputate; and mind, in doing that you take away the working man’s right hand perhaps; now that is a serious matter! We infringe the rule, and the infringements or errors of old rules taught us a great deal. I was every year more and more fortified in my

opinion, now fully established, that you are not called upon to amputate in almost any injury of forearm and hand. Nature will repair the mischief herself.

Take, again, diseases of joints, and what do we find? What has the last ten years taught us? That you cannot have ankylosis without removal of the enrusting cartilage of such parts as the head of the femur or humerus, etc. This is not disease—it is nature's own curative surgery; yet how are we every other day frightened at the words "ulceration of cartilages." Have you never seen this part finely injected with size and vermilion? If you have, you would see a beautiful ring of granulations marking out the process of absorption, and thus, *pari passu*, marking out nature's conservative surgery. Yet how many dozens of knees do we see amputated, or ankles removed, because we mistake a healthy reparative process, tedious it may be, for a diseased state.

But I come back to the subject of compound comminuted fractures. I do not like to lead the advanced guard of ultra conservatism. I see a good deal of small surgery, but though heterodox in a "clinical," I wish to teach you leading clinical principles, not small surgery. But I will give you a case; there is nothing like a case—it is a sort of diagram. A man was brought in, Nov. 5, with a very bad and formidable laceration of the forearm, the radius comminuted, the extensor muscles scooped away, the interosseous ligament and so-called interosseous artery (it's not an interosseous artery at all, by the way), all injured; he had a Guy Fawkes explosion, burning and destroying his arm. What was to be done? The students came crowding down for an operation; he had some amount of extension and flexion in the forearm. Well, I simply cut away all the jagged parts, I placed a splint over the front of the forearm; he has now a useful arm; but mind, if he had not had extension and flexion, it would be useless to try to save the parts. Here "antagonism," whatever it be in the moral or Guy Fawkes world, is everything to be desired. He had his supinators, else I would have amputated: it would never do to leave him a hand all flexors, bent like a bird's claw.

You have heard also, no doubt, (any of you industriously reading up for the college, at least), of "primary" and "secondary" amputations. Well, there has been a great deal written on this subject, and as it is made to bear on conservative surgery, it is good in its essence, but it is impracticable. I have often explained why I don't believe in it. It may answer on the field of battle, where the surgeon is here in a trench to-day, and ten miles off to-morrow. I believe it is ridiculous in civil hospitals; for I say if a man is not operated on till the termination of six hours, why not give him 24, ay, and 48 hours? In such cases I would say, in one word—do not take off limbs too soon; watch the case; watch it, that's the secret. I fought a case here the other day; I said give him 24 hours, and then three days: I will not tell you the result—but that is the principle. I want you now to observe for yourselves, and take this with you, that in the last ten years there is happily growing up more tenderness and sympathy for patients; there is more humanity amongst the true and great surgeons.

If we felt for patients more it would be no harm. I had a joke

with Sir B. Brodie a little while ago. He went out of town somewhere, and happening to meet with an injury, dislocated his humerus. "I am so glad, Sir Benjamin," I said to him, "as the Fates would have it so, that it occurred to such a good observer; you can now sympathize with patients, and tell us all about it; I am glad, at any rate, it was no worse." He laughed. "I agree with you," he said; "we do not sympathize enough with our patients. I don't exactly see the force of your congratulation, at least in your way, for I assure you it was horribly painful."

I would say to you, in conclusion, it is your duty, it is your mission, and there is no nobler one, to save limbs and to save life in every possible manner you can; the sooner you forget mere personal *eclat* the better. Take care that much of the *esprit de corps* I speak of is not pride and self-esteem.

You may rely on it, considering our increasing knowledge of therapeutics, that we have not yet tested nature's own conservative surgery enough, and that we deprive nature of much of the credit that is justly her due.—*Montreal Med. Chron.—Am. Med. Gaz.*

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#### ART. XII.—*Apparent Drowning.—Asphyxia.*

*Apparent and Real Death from Submersion.*—Submersion for two minutes is probably sufficient to produce apparent, and, in a majority of cases, real death, not from the admission of water into the lungs, but from the non-arterialization of the blood and from the non-elimination of carbonic acid of the economy. Were it possible to discharge all of the atmospheric air at one expiration, and then to inspire pure carbonic acid, it is probable that a fatal asphyxia or aerial drowning would follow as soon as from submersion. The left or arterial side of the heart is killed by carbonated blood, and consequently, the systemic circulation ceases, and with it the wheels of life stand still.

I have in a number of cases of submersion, even for a short time, always failed to effect resuscitation, although I have practised the usual plans recommended by writers.

Respiration *per se* is not the desiderated finality or end, but it is the means by which suspended animation is rehabilitated, that is to say, the means by which carbonic acid is parted from, and oxygen united to, the blood. The natural *respiratory act itself* is dependent on these two processes. The so-called artificial respiration, which though generally unavailing, is but auxiliary to this fundamental function which for a short time after submersion is inherent in the economy.

In view of the general failure of all methods hitherto proposed for the restoration of persons submerged for 100 or 120 seconds (some authors maintain that no case of resuscitation is known after four continuous minutes\*) would it not be proper to resort to transfusion? The venous blood taken from the arm of a healthy person is not wholly carbonated, and might transmit to the heart a sufficient amount of oxygen to renew its action, and the more so, as the circulation survives the respiratory act for a time? Might not arterial blood taken from an animal whose blood approximates that of man be still better for this purpose? I am not aware that this suggestion has been ever made, much less acted upon. Certain it is that no method yet adopted has generally proved successful in restoring persons submerged for one minute or more. Hence the propriety of calling the reader's attention once more to this subject as reproduced in papers which will follow as a sequel to these observations.

The insufflation of the lungs by the operator's blowing into the nostril, or by a bellows, or by any conceivable posturation, is altogether uncertain, and may be injurious; the breath of the operator is already carbonated, poisoned; it may enter the œsophagus, or return through the rima of the glottis without having entered the bronchial ramifications and air-cells of the lungs; if the atmospheric air be injected by the bellows, it may return in like manner; or, otherwise it may injure the pulmonary tissue, as it is impossible to graduate the injecting force upon any known scale. Hence I venture to suggest the propriety of tracheotomy—a suggestion based on a very decisive experiment upon the alligator, which is, I believe, reported in this journal, or in the *Boston Med. and Surg. Journal*, and which is due to the sagacity of my friend Dr. Cartwright.

Many years ago, a number of gentlemen had the politeness to attend some of my vivisections, sometimes at Mr. now Dr. Barbot's. On one occasion I commenced operations by ligating the trachæ of an alligator in order to ascertain how long it would survive in this condition, and then proceeded without delay, to vivisect another. In a very short time (I now write from memory), perhaps in three or four minutes, Mr. Barbot brought the asphyxiated animal out of its den

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\* It is usually difficult to determine the precise time of submersion. Moreover, the individual, while drowning, rises once or oftener to the water's surface, and instinctively inspires more or less air. Taylor, in his *Med. Juris.*, quotes various authorities showing that professional divers can in no case remain under water for two consecutive minutes. The best pearl divers at Ceylon, can rarely endure submersion for fifty seconds.

apparently as dead as it could possibly be. Dr. Cartwright, assisted by Prof. Forshey, performed tracheotomy below the ligature and inflated the lungs, whereupon the animal revived and struggled vigorously throughout a prolonged and painful vivisection.

In cases of submersion, tracheotomy would not only favor the postural plan, but afford facilities for cautious and effectual insufflation, for the discharge of water, and for the manipulatory removal of mucosity and froth from the trachea.

Tracheotomy has occasionally saved life in croup under circumstances always unfavorable to success, because the tracheal, including the bronchial, and often the pulmonary tissues are highly inflamed, and would generally cause death after the removal of the immediate asphyxia resulting from dense mucosity and false membranes developed by this malady.

Instead of speculating further, I will proceed to the statistics of submersion, from which it will appear that drowning is almost an endemic in New Orleans, and deserves an official investigation in order to devise the ways and means both preventive and remedial to save the lives of the people.

The loss of life from drowning in the Mississippi, its tributaries, its bayous and its lakes, particularly in lower Louisiana, during its long season of inundation, is probably much greater than in any other region. I believe that there is not a city in the world, where drowning is so common as in New Orleans; in proof of which, I offer such statistical evidence, taken at random and in haste, as is most easy of access.

According to the United States Mortality Statistics, compiled by Prof. De Bow, Superintendent of the Census, there were 2,357 cases of drowning in this Republic, in the year 1850; in the State of Alabama, 75 cases in a total mortality of 9,091; In Mississippi, 86 in 8,721 deaths; in Tennessee, 78 in 11,857; in South Carolina, 41 in 8,047; in Virginia, 117 in 19,050; in New York State, 337 in 45,600; in New York city, 54 in 11,883; in Vermont, 19 in 3,129; in Massachusetts, 169 in 19,404; in Maryland, 59 in 9,621; in Philadelphia, 53 etc., etc.

In this same year (1850), the Board of Health of New Orleans reported 97 deaths from accidental drowning; in the preceeding year (1849), the number was 92; in 1853, the number was 105.

In the city of Paris in 1821-2-3-4-5-6, the total number drowned was 526. (Taylor's Medical Jurisp. 525.)

The Official Report of the City Inspector of New York, for the year 1853, enumerates 86 deaths from drowning in a total mortality of 22,702, being one in 264. The Report for 1854, shows the total mortality of the same city to be 26,063, of which number, 183 deaths were caused by drowning, that is 1 in 142.39.

For seven months ending Dec. 15th, 1856, the Board of Health of New Orleans reported 76 deaths from drowning, or about one-fiftieth of the total mortality: 10 in May, 15 in June; 14 in July; 13 in August; 10 in September; 5 in October; 9 in November; averaging about 11 per month.

Thus it appears that whether the total populations or the total deaths of other cities be compared with those of New Orleans, the latter city loses by drowning from eight to ten times more than the former. Hence the professional and non-professional public have a tenfold interest and responsibility, both in a preventive and remedial sense, as it regards this neglected subject.

John Randolph maintained in Congress, that some of the Germanic States were so small, the sovereign could not shake his powdered wig without throwing dust over the whole realm. Be this as it may, the inundations of Louisiana during several months of the year, would submerge 24 of these States, and one or two more as large as Rhode Island. Hence with a denser population, drowning would be increased greatly in the region of the Lower Mississippi, as well as in the river proper, where there are many hundred steam and flat boats constantly in motion. During the recent crevasse near New Orleans a number of persons, at various times, were drowned.

B. DOWLER.

i. *New Method of Resuscitating Persons Apparently Drowned—The Silvester Method.*

This new method, according to its advocate, H. R. Silvester, Esq., M. D., possesses many advantages over the "ready method" of Marshall Hall; it is easy of performance, and may be employed along with those other means in which so much confidence has hitherto been placed.

The *Silvester Method* is "a simple initiation of natural deep inspiration, and is effected by means of the same muscles as are employed by nature in that process." In deep inspiration, we lift the ribs and sternum by the pectoral and other muscles which pass between the chest and the shoulders; so in the "new method" the ribs and ster-

num are lifted through the intervention of the muscles, *by steadily extending the arms up by the side of the patient's head*. In this way the cavity of the chest is enlarged, a tendency to a vacuum is produced, and a rush of air immediately takes place into the lungs.

Expiration is brought about by simple compression of the sides of the chest by the patient's arms. Thus "the arms of the patient are to be used by the operator as handles to open and close the chest."

The following rules are to be observed in inducing artificial respiration by this new method.

1. *Position*.—Place the patient on his back, with the shoulders raised and supported on a folded article of dress.

2. *To maintain a free entrance of air into the windpipe*.—Draw forward the tongue, and keep it projecting beyond the lips. By raising the lower jaw the teeth may be made to hold it in the proper position.

3. *To imitate the movements of respiration*.—Raise the patient's arms upwards by the sides of his head, and then extend them gently and steadily upwards and forwards for a few moments. [This action, by enlarging the capacity of the chest, induces inspiration.]

Next turn down the arms, and press them gently and firmly, for a few moments, against the sides of the chest. [Forced expiration is thus effected.]

Repeat these measures alternately, deliberately, and perseveringly, fifteen times in a minute.

The advantages of this method, as compared with that of Marshall Hall's, as stated by its author, are the following, and may be thus studied in comparison:

#### MARSHALL HALL'S METHOD.

Expiration is made to precede inspiration—the reverse of the natural order. In still-born infants forced expiration, at first (as they have never breathed) is, of course, impossible.

The warm bath cannot be employed during its adoption.

When the patient is turned on the face (pronated), and pressure made, the contents of the stomach are liable to pass into the œsophagus and trachea.

In the opposite position, "on the side, and a little beyond" (supinated), the tongue is apt to obstruct inspiration by falling back into the throat.

Both sides of the chest are not equally inflated.

The amount of air respired is exceedingly small, the *actual* capacity of the chest not being enlarged; (proved by experiment).

#### SILVESTER'S METHOD.

Inspiration may be made to precede expiration, or *vice versa*, at the will of the operator.

May be adopted when the patient is in the warm bath.

Contents of stomach not liable to pass into trachea.

Tongue effectually prevented from obstructing inspiration.

Both sides of the chest are equally inflated.

A larger amount of air is inspired than by any other method: (proved by experiment).

Lastly, we are told that whilst the Royal Humane Society directs its attention mainly to the circulation, and Dr. Marshall Hall chiefly



to the respiration, the new method of Dr. Silvester combines the advantages of both.—*British Medical Journal. Dub. Hos. Gaz., Aug. 1858.*

ii. *The Marshall Hall method of Treatment in Asphyxia.*

To the Editor of the LANCET:—Sir,—Your correspondent from Columbia, Tennessee, U. S., appears to be much interested in the method of treatment in asphyxia recommended by the late Dr. Marshall Hall, and generally known as the “Marshall Hall” or “Postural Method.” Allow me, through the medium of your journal, to call his attention to the results of my experiments and observations on this important subject.

The principal involved in the postural method is precisely the same as in a plan formerly much practised, in which simple compression and relaxation of the chest were employed to induce expiration and inspiration. The difference is this, that in the postural method the compression of the thorax is occasioned by the weight of the body of the patient resting on the chest during the prone position, followed by the removal of the pressure and consequent return of the thoracic parietes to their ordinary level on the patient assuming the supine posture. No attempt is made to enlarge the actual capacity of the chest by inducing an elevation of the ribs, such as takes place on making a deep inspiration.

This method of treatment has been found open to some objections; for instance—

1. Expiration is made to precede inspiration, whereas originally expiration is second in order, and not the primary act. In still-born infants, whose lungs have never been inflated, forced expiration at first is of course impossible.

2. The expansion of the thorax or inspiration, being dependent on little more than the elasticity of the tissues, takes place feebly, inefficiently, and therefore calls for some more active mechanical aid.

3. It is scarcely possible to use the warm-bath, if indicated, in the case of a still-born infant, during the adoption of the postural method.

4. The patient is liable to have the mouth and nose compressed, the face bruised, or the neck twisted, by the almost lifeless body being turned alternately on the chest and back fifteen times a minute for some hours; moreover to the operator this process is very arduous.

5. When the body is “turned on the face, and pressure made on the back” (pronated), the contents of the stomach are liable to pass into the œsophagus and windpipe.

6. When the patient is turned “completely on the side and a little beyond” (supinated), the tongue obstructs inspiration by falling back into the throat, the epiglottis resting against the back of the pharynx.

7. Both sides of the chest are not equally inflated, one side only being called into action at the same time to any important extent.

8. This process is not entirely in harmony with that of nature. It is not the way in which we generally breathe.

9. That the amount of air respired is exceedingly small, as has been

proved by actual experiment. This is doubtless in consequence of the actual capacity of the chest not being increased.

The method of performing the experiments, at St. George's Hospital, on the dead subject, which appeared to prove that in the Marshall Hall Method "nearly as much air entered the lungs as would be inhaled in an ordinary inspiration in a state of health," is, I believe, by no means satisfactory; it is, in fact, open to obvious objections. The tube of the pnaemometer was passed into one of the nostrils of the patient, the other nostril and the lips being closed with adhesive plaster. The want of rigidity of the cheeks, and the amount of air in the respiratory tract, and even in the stomach, etc., of the patient, could scarcely fail to render the indications of the instrument, however perfect in itself, liable to suspicion, if not entirely valueless, in point of scientific accuracy.

The method which I have ventured to bring before the profession, is not open to any of the above objections. It is a simple imitation of natural deep respiration, and effected by means of the same muscles as are employed by nature in that process. In ordinary deep inspiration, we lift the ribs and sternum by the pectoral and other muscles, which pass between the chest and the shoulders, and thus produce the threatened vacuum which inflates the lungs.

In my method we lift the ribs and sternum by the pectoral and other muscles which pass from the shoulders to the parietes of the thorax, by steadily extending the arms up by the sides of the patient's head; by elevating the ribs, the cavity of the chest is enlarged, a tendency to a vacuum is produced, and a rush of air immediately takes place into the lungs. Expiration is brought about by simple compression of the sides of the chest by the patient's arms.

The distinguished feature of my method is the actual enlargement of the cavity of the chest.

In order to maintain a free entrance of air into the windpipe, we draw forward the patient's tongue, and keep it projecting beyond the lips. If the lower jaw be gently raised, the teeth may be made to hold the tongue in the required position. Should it be found necessary, the tongue may be so retained by passing a handkerchief under the chin and fastening it over the head.

In this way the patulous orifice of the windpipe is raised and drawn forward, so that nothing intervenes between it and the natural channel of air through the nose. The tongue is entirely prevented from falling back into the throat, whilst the extent to which the windpipe put on the stretch is clearly indicated; the pharynx also is sufficiently opened to allow of the removal of liquids, etc., from the mouth, nose, pharynx, etc.

My method has been tested by experiment on the dead body with most satisfactory results; and a successful case of resuscitation has already been recorded.

Yours obediently,

HENRY R. SILVESTER, B. A., M. D., London.

*High street, Clapham, 1858.*

ART. XIII.—*On the Therapeutical Action of the Constant Galvanic Current.*  
By Dr. ROBERT REMAK, of Berlin.

AFTER the discovery of the voltaic pile (1800), many experiments were made with a view to the application of the galvanic current, as a means of treatment in diseases of the nerves and muscles. Stimulated by the admonitions of Alexander Humboldt, Loder of Jena, and Grasiengießer of Berlin, were amongst the first to treat various paralytic affections of the limbs and sensorial nerves in this manner (1801). The opinion at that time being, that not the continued action of the current, but the shocks produced by the interruption of the current, are the best means of exciting the normal action of the nerves and muscles. These trials could not result in success, because, as my researches have now proved, such shocks can in very few cases be advantageously employed. Thus for many years the electrical machine formed the only means of producing and applying electricity to the above-named diseased conditions; and the opinion was generally adopted by physicians, that it signified little from what source electricity for medical purposes was produced.

Although the discoveries of Becquerel and Wollaston (1820-30) supplied the means of producing a constant and equable galvanic or electric current, physicians did not avail themselves of these improvements, but preferred using the magneto-electrical and electro-magnetic machines which were constructed after the discovery of the current of induction, made about the same time as Becquerel and Wollaston's, by Oersted and Faraday. In experimenting with these instruments, the fact was overlooked that the current of induction cannot be applied to the living body without producing shocks (*i. e.* spasmodic contractions of the muscles), and that these shocks have a weakening effect. At last the chief consideration came to be, the easiest method of procuring electricity for medical purposes. So at last the magneto-electric machine (of Saxton, Stochrer, etc.) gave place to the self-acting electro-magnetic (galvano-magnetic) machine of Faraday, which was recommended by Duchenne, of Paris, and was much approved of in France and Germany. In the work of that physician it was stated that muscles are made to contract most readily by the current of induction, if certain points on their surface are touched by the electrodes. Having been occupied for many years with physiological researches upon muscular contraction, I was not a little curious to know the nature of these mystical points; and, on directing my attention to this subject, I soon found that they corresponded with the points of entrance of the muscular nerves, and that the degree of contraction of a muscle was proportionate exactly to the number of motory nerve-fibres embraced by the current at its point of application.

I stated the result of these physiological investigations in a small pamphlet, published in 1855, and drew attention to their value in a practical point of view. It seemed to me quite obvious that, for the successful therapeutical application of electricity, a better and more extensive physiological basis was required than was afforded by this

one fact ; so, in continuing my researches on the subject, I was led to examine the effect of the constant galvanic current ( as it is produced by the elements of Daniell, Grove, or Bunsen ) on the muscles and nerves of a healthy man. The results of these further observations I noticed in an appendix to the second edition of the pamphlet last quoted, also in a note sent to the Academy of Paris. After having continued such researches, made upon my own person, and upon many healthy men, for about six months, I was induced, in July, 1856, to apply the constant current as a means of treatment of contraction of muscles, in cases of hemiplegia from cerebral apoplexy. The most important result of this application was the fact, that the continued current applied for a few minutes to a contracted muscle had the effect of immediately relaxing it to a certain extent, and rendering it amenable to the influence of volition.

After repeating these trials upon 200 cases of different kinds of nervous diseases, I reported as the result of my further experience, that the continued application of the constant current to the nerves and muscles in rheumatism, paralysis, atrophy, neuralgic and spasmodic diseases, had in my hands proved of much greater service in a curative point of view, than any other application of electricity had hitherto done. I gave my results in a memoir which I read before the Academy of Sciences in Paris, in *séance* Sept. 22, 1856 ; and on the 29th of the same month I made some experiments before M. Rayer, who was representative of a commission appointed by the Academy for the purpose ; and on the following day repeated them before a number of physicians and naturalists.

From that date to the present, I have spent all my time in continuing these researches, and in endeavoring to develop the methods of application of the constant current. In the Society of Medical Science of Berlin, I stated ( Jan. 19, 1857 ) that the diseases in which the constant galvanic current is of service, are — 1. Rheumatisms, acute and chronic. 2. Cerebral hemiplegias. 3. Paraplegias. 4. Atrophy of muscles. 5. Chorea. 6. Stammering. 7. Trembling of limbs. 8. Cramps of writers. . And I have supported my assertions by presenting cases of these descriptions, which were either cured or improved by this mode of treatment. My subsequent publications upon the subject were directed principally to the treatment of atrophy and paralytic diseases. This was accounted for by the fact that certain physiological views, dating from the time of Volta Nariamni and Nobili, gave rise to the opinion that the continued action of the current had only a relaxing or weakening effect, and could do good only in spasmodic diseases. I have refuted this idea by showing that the continued current applied in a certain manner, is of use in many cases of paralysis in which the interrupted current of induction is even productive of harm.

One of the most curious results of my researches made during the last year is the observation, that the constant galvanic current, by dilating the blood vessels, and by promoting absorption of exudations, can be used with good effect in certain inflammations in which the other antiphlogistic and resorbent remedies are insufficient. There is no doubt that the electrolytical power of the constant cur-

rent (discovered by Nicholson and Carlisle, 1800) is a powerful means of producing these effects. I have already seen most convincing and gratifying proofs of this fact, not only in rheumatic and traumatic inflammations of joints, but also in the inflammatory states of the spinal marrow which sometimes precede atrophy of this organ (?). It may readily be supposed that the mode of applying the current must be very different in order to produce the various actions I have hitherto distinguished—viz.: 1st. The catalytical (*i. e.* electrolytic and antiphlogistic); 2d. The antiparalytic; 3d. The antispasmodic. But for information on this subject, I am obliged for the present to refer to my various publications.

In Germany my experiments have not yet been repeated; but in France, after my visit in September, 1856, some physicians made some trials upon the subject, and, as I am now informed, these were not unattended with success. In England I do not know if anything has been done in this way, but it is the object of this short paper to direct the attention of my English colleagues to this discovery (as I dare to name the methodic application of continued galvanic currents to diseased conditions), and I would feel satisfied if I thought that the methods described by me in a work I am about to publish, would in any way, assist them to the successful practice of their art.

Finally, I think it my duty to state distinctly, that all my experiments upon 700 patients have been made only with the constant galvanic current (of Daniell, Grove, and Bunsen), and that there is reason to doubt if these results could have been obtained by the use of other elements less constant.—*Med. Times and Gaz.*—*Braithwaite's Retros.*

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ART. XIV.—*Chemical Examinations connected with a Bullet which had been Imbedded for more than Forty Years in the Lung of a Man*; by HENRY WURTZ, of New York city.—Read before the American Association for the Advancement of Science, at Baltimore, 1858.

WHILE delivering the course on Chemistry in the medical department of Queen's College, Kingston, Canada West, during the past winter, the case in question came under my observation, and seemed to me a rare opportunity for a chemical examination of the action undergone by metallic lead when imbedded in a human viscus, and although the examination made, from want of time and other causes, was very imperfect, the results obtained are probably worthy of being recorded. The following history of the case was drawn up at my request by the gentleman who made the dissection, and found the ball, Dr. Alexander Bethune, now a graduate of the Institution :

William Kelly, æt. 76, a pensioner, a native of Ireland, was admitted into the Kingston General Hospital, September 25, 1855. He had *hemiplegia*, and as it was considered incurable he was sent to the House of Industry, where he remained only a few days, and then re-

turned to the hospital, in which he continued until the day of his death, December 17, 1857.

Post-mortem appearances: On removing the calvarium more than a pint of bloody serum gushed out. The dura mater was found much thickened and closely adherent. The gray matter of the right hemisphere was softened and contained a clot the size of a pea. The lining membranes of the ventricles were thicker than natural, and they contained serum and organized lymph, with traces of recent inflammation. When the thorax was opened a prominence was observed in the middle lobe of the right lung, and on cutting into it about the depth of an inch, a bullet was found, enclosed in a cyst, and firmly adhering to its wall at several points. On examining the walls of the thorax, a cicatrix was found between the fourth and fifth ribs, corresponding to the prominence produced by the bullet.

In the early part of his life, the deceased had been a soldier in the British army, and was engaged in the Peninsular war. In one of the engagements there he received a wound from a musket ball, which entitled him to a pension. As he was engaged in no war but that of the Peninsula, and had no marks of any other wound upon his body, the bullet found in his lung, more than forty years after, must have been that which produced the wound for which he received the pension.

ALEXANDER BETHUNE.

Queen's College, Kingston, Feb. 1, 1858.

I may add that the wound is believed to have been received at least as early as the last siege of Badajoz, which occurred in the year 1812, and consequently forty-five years ago; and that the hemiplegia was of the right side, or on the same side as the ball.

The ball, as delivered to me, appears to have been a common once musket ball, but is irregular in shape, having evidently undergone corrosion. It now weighs 370 grains, indicating a loss, if originally an ounce in weight, of more than one hundred grains. Two spots appeared on opposite sides, where adhesion to the walls of the cyst had existed. These spots are coated with a hard whitish substance, which cuts like horn, and blackens when heated, with a smell of burning animal matter, leaving afterwards an infusible white ash. Near one of these spots was a small incrustation of a chalky white color, about one-eighth of an inch in diameter, which, when detached, was found to penetrate somewhat beneath the surface of the lead, and when submitted to careful qualitative analysis, was ascertained to consist wholly of *chlorid of lead*, containing no trace of either phosphoric or sulphuric acid. The ball was then boiled in distilled water and gave a liquid hard to filter clear, which contained considerable chlorid of lead. In places the ball had a very thin brown coating, too small in quantity to be satisfactorily determined, but which was probably also chlorid of lead, colored with organic matter.

A piece cut from the wall of the cyst was treated with chlorohydric acid and chlorate of potash, and the solution examined for lead without success, but the material was so small in quantity that the result is not satisfactory. A piece of the lung itself, taken from the neighborhood of the cyst, was then treated in the same way. The

solution obtained gave with sulpho-hydric acid a small brown precipitate, which was collected on a small *purified* Swedish paper filter, and the latter washed, dried and burned to ashes. The ash, fused with carbonate of soda on charcoal, and manipulated in the ordinary way in the agate mortar, gave a multitude of spangles of a soft white malleable metal, which being collected on a watch-glass, dried, dissolved in a drop of nitric acid and the solution evaporated to dryness, gave with a drop of a solution of sulpho-hydric acid a deep brown coloration.

I was very desirous to obtain a portion of the *liver* (which was not yet exposed when I viewed the body), and left directions that it should be saved, which were unfortunately not complied with, the liver having been thrown among a number of like viscera from other subjects in the dissecting room, so that it could not again be identified. A piece of *muscular tissue*, however, taken from the muscle of the diaphragm, was examined and lead found, apparently in greater quantity than in the lung.

It is evident from these results, that the lead of the bullet has undergone solution in the cyst as chlorid, and passed as such into the circulation, to be deposited again in insoluble forms in other parts of the system, as is usual with the so-called *cumulative poisons*.

The question whether the paralysis and post-mortem appearances had any connection with the presence of the lead, I shall leave for the consideration of medical men and toxicologists.—*American Jour. of Sci. and Arts.*

ART. XV.—*On Chinese Poisons* : by D. J. MACGOWAN, M. D.

IN consequence of the atrocious attempt of the Cantonese to poison the foreign community at Hong Kong, applications have been made to us from various quarters for information on poisons known to the Chinese. An investigation of Chinese toxicology would require much time, and special study, which we despair of being able to devote to that interesting subject. In default of presenting anything of value to the scientific inquirer, we submit for the perusal of the general reader the limited information we possess on Chinese poisons. From the period of the Han dynasty to the present day poisoned arrows have been employed, both in the chase and in warfare; less, however, by the Chinese proper than by the so-called aborigines or Miautzs. In the hands of the latter, they are formidable weapons. Instant death is inevitable from the slightest abrasion. The Chinese possess no agents for counteracting the poisons of the hill tribes; the *Pun Tsau* states that these Inner Barbarians apply borax sometimes with success to poisoned wounds; and it is commonly reported that for their various virulent poisons they possess perfect antidotes.

We are acquainted with only one of the *inoculating* poisons of the Chinese—a watery extract of the root of *tsau wu*—a perennial creeper found on the hills of the central provinces. Its botanical character,

as well as its precise physiological action, we have yet to ascertain. Its active principle appears to be highly volatilizable, and great exactness is needed in preparing it. Small animals are kept in readiness for testing the strength of the extract; they are punctured with a point charged with the poison, and when its virulence is sufficient to occasion instant death, it is boiled no longer. A native physician who saw it prepared by the hunters of the adjacent district of Funghwa, states that sometimes they prick their arms to let blood flow to the wrist, and after carefully wiping the part near the wound, apply some of the pasty extract to the lower end of the line of blood, which it rapidly blackens through its whole length; and it is affirmed, that were the continuity with the incision unbroken the morbid action of the subtile agent would penetrate the wound and prove quickly fatal. It is sometimes applied to the tongues of the unwary as a practical joke—when it occasions a keen sensation of formication. The dried root is administered where the nerves of motion are impaired. The Funghwa hunters traverse in winter the mountainous regions of Chihkiang in pursuit of tigers that are sometimes taken by being pierced with arrows, the necks of which are bound with filaments of cotton saturated with the poison; at other times, springs charged with these arrows are adjusted near the lair of the animal for his destruction. When a limb is struck the beast writhes awhile before expiring, but when wounded in the body, he leaps forward, staggers and falls dead immediately.

Attached to the imperial body guard is a corps of hunters, who when on the chase in pursuit of edible game, provide themselves with envenomed missiles to be employed against wolves.

Latterly, poisoned arrows have fallen into comparative disuse as implements of war, owing to an increasing familiarity with fire-arms. We are acquainted with the contrivor of a machine which was designed to be used against the English during the late war. It succeeded so well in picking off goats which were led over the cords communicating with it, that the military commission strongly recommended its adoption for destroying barbarian bipeds. Unfortunately for the patriotic inventor, the treaty of Nanking caused the dispersion of the game just as it was ready, as he supposed to be largely secured. It need hardly be added that in the hands of assassins, this easily attainable poison would prove a potent means of destruction, as a slight puncture of an instrument charged with it, would, from its rapid absorption, be a sure *coup de grace*.

It is not our purpose to point out all the virulent agents which unscrupulous Chinamen are likely to employ against an enemy. We shall only add one more of the inoculating class, and that chiefly on account of its novelty. When the late commissioner Lin was devising means for the extirpation of English barbarians at Canton, some of the gentry of that city actually proposed to His Excellency to rid the place of certain prominent and obnoxious individuals by infecting them with *leprosy*.

Lin indignantly rejected the proposition as unbecoming a civilized people. That Chinese statesman was a brave and honorable man; his present Manchu successor has no claim to such virtues.



His chief objection to the use of leprous virus was, it is said, the tardiness of its action; it is believed that more than a month elapses after the introduction of the animal poison before it begins to show itself in its victim. Our knowledge of this foul malady is too imperfect to justify us in pronouncing such an inoculation as impracticable. Nor have we any remedy to propose for an officer who finds himself thus tainted.

There are poisons also which are *inhaled*. A person who aided a magistrate in the administration of a poison of this class gives the following narrative of the transaction, which was a *ffiliicide*. (Our apology for coining this word is, that it is needed to denote a crime, or rather extra-judicial practice not uncommon in China, resorted to for making away with dangerous adult sons). The son of that officer was a lawless and incorrigible character, who by misdemeanors periled the safety of his family, and they determined on his removal. To effect that object without publicity no small finesse was requisite on the part of his father and friends. Suspecting their designs he became excessively wary. On the day agreed for his execution, the father feigned to be withholding the son's much loved opium, until he could induce the hapless youth to take a draught of tea, which he was artfully led [to suppose was drugged. At length, affecting to be wearied by the son's contumacy, the father gave him his opium pipe, mixing with the genial papaver another drug intensely poisonous. After a few inhalations the victim fell into a stupor, which was followed by convulsions to which his athletic frame succumbed in less than six hours.

To the same class belong those drugs which are employed by burglars for stupefying the inmates of the house to be robbed. Ever since we read in commissioner Lin's anti-opium diatribe, references to medicines used by robbers, kidnappers and sorcerers, which that statesman compared to the prohibited narcotic, we have been vainly endeavoring to investigate their nature. There is, however, abundant evidence that such agents are employed to induce stupor for criminal purposes.

Kidnapping male children for sale in Siam or the Straits has long been a common practice on the seaboard, and a short time since, to meet the demand for Chinese females in Cuba, many girls were kidnapped. The provincial capital—Hangchau, was thrown into consternation in consequence of the paucity of slave labor in the West Indies. Popular placards and official proclamations were posted everywhere, warning the inhabitants against the villainous agents of foreign barbarians who were prowling in search of female children. Two miscreants were detected with their little prizes and immediately beaten to death. From the documents published on that occasion it appears that for very young children a drug is employed which on being applied to the face produces a degree of insensibility enabling the operator to lead the little one away unresistingly. For those of a larger growth, as well as for adults, something is given which by irritating the throat causes aphony. At other times they seem to employ an anæsthetic for criminal purposes.

We must adduce one instance of a proposal to stupify by drugs of this character. When Ningpo was in possession of the English, a bold effort was made for its recapture by surprise. A large number of "braves" was secretly housed in the city, and a few hours before the time appointed for rising, the mandarin sent for distribution among a portion of the concealed force a quantity of stupefying drug in the form of pastiles. Arrangements were made for a simultaneous attack on the west and south gates by night, and while some of those within the city were to set off noxious vapor in houses occupied by the English, others were to force open the gates. The plot failed through the dilatoriness of the fumigating squad. They arrived too late. The noise of the attack aroused the English, and its partial success, enabling a large force to enter the city, caused a dreadful carnage among the spirited assailants. Yet their confidence in the utility of fumigation was not impaired, for it was not long after that a barbarian soldier was captured by this means and beheaded. Many were the plans set afoot for the capture of the English chief *Pu-ting-ché*—for whose head ten thousand taels were offered.

It is said that for nearly every poison its antidote may be found. These soporific pastiles come within the number. The difficulty in poisoning cases is to know where to look for the desired neutralizer. That our toxicological notes may not be wholly devoid of practical information, we shall give some simple directions for the guidance of those whose premises are invaded by fumigators. It is assumed that you are wide awake at the time, for if you are caught napping there is no remedy. In the first place, then, take care that you do not open your mouth; but holding your breath, rush at the intruder and seize his cue in your left hand, while with the thumb and index finger of the right, you lay hold of his nose just above the alæ with a firm grip; a bolus will drop out of each nostril, with which you are to plug your own nasal passage (be not fastidious) and you may then commence breathing (your mouth still closed)—for these medicated pledgets possess the property of decomposing the somnolent gas. If successful in these manipulations you will have the gratification of seeing your foe gasp and tumble over *hors de combat* into the pit he had prepared for you.

Allusions are often made to a mysterious and extremely virulent poison taken by men of high rank when on the eve of execution; it is derived, according to vulgar belief from a protuberance on the head of a species of stork. Whatever the agent may be, it is unquestionably one of great potency. An officer who was eye witness to the decapitation of the venerable and lamented General Yu Tsien, informs us that when that victim of Manchu malignity was led out to slaughter, something was placed on the tip of his tongue, which, without the infliction of pain, rendered him insensible in less than half an hour. Friendly mandarins then retained his moribund frame in an erect posture to receive the painless death blow.—*American Journal Science and Arts.*

## JOURNALISTIC MUSINGS.

TIME has poured out the last sands of his glass for 1858. He has measured, confined and interred the year 1858, and with it, nearly five thousand victims of yellow fever, in New Orleans. For the unacclimated, 1858 has proved a year of disaster and dismay. But the wifeless, the widowed, the orphaned, and the bereaved who survive, have suffered on the whole, no more than what all must suffer soon or late:

—All here are men  
 Condemned alike to groan,  
 The feeling for another's pain,  
 The unfeeling for his own.—GRAY.

Hope, at least, is still left for the most inconsolable:

Hope, like the glimmering taper's light,  
 Adorns and cheers the way,  
 And still as darker grows the night  
 Emits a brighter, a brighter ray.—GOLDSMITH.

Passing by these sad souvenirs resulting from yellow fever, a retrospective view of the past year will prove satisfactory as it regards the health and prosperity of the Southern Cities and States.

Crevasses and disrupted Atlantic cables, erratic comets and more erratic governmental cabinets, may excite wonder and arouse the passions of the multitude; but such events affect a medical editor very little. He is absorbed in *his* Journal. When he has been lying-in of six numbers a year, his is a hard labor, and when at the end of the year he sees the whole family complete, *he* thinks (others may think differently) that this is at least for him, an event, and a great one too. Smile, if you will, at his rejoicings in his study, his midnight flickering lamp, ink-bottle, skeleton, and proof-sheets before him, his exchange journals and dusty volumes around him. BEHOLD THE FIRST NUMBER OF THE XVI<sup>TH</sup> VOLUME OF THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL! Other journals, books, and encyclopædias may be better. Judge ye. But if the editors fail to make this journal a good one, it will not be owing to the want of a wish or a will. To fulfill this high purpose is their ambition, and the more so, because its patrons are rapidly augmenting, and, at the same time, a vast proportion of them make in return prompt payments never before equaled since the first number was issued. This is as it should be. Journals should be at least self-sustaining, if not remunerative. Duty and interest are one and indivisible. The subscriber to a medical journal who pays promptly enables the publisher generally to send in return a journal superior in quality or quantity. Outside of the editorial contributions to this journal, it is believed that the *quality* of its articles is improved and

improving; at all events, the *quantity* of its reading matter in this and in its future issues is and will be augmented by eight additional pages, so that each number will contain 152, instead of 144, as hitherto. Each number will be printed on superior paper, and will be bound for easy preservation.

The present editorial organization of this journal has existed for nearly a year and a half, and the managing editor takes this occasion to say that not only has the utmost harmony prevailed, but that the journal was never freer or more independent of interested biases than since its present arrangement. It is the organ of no college, school, party, or person. Its sole mission is to diffuse useful knowledge as extensively as possible, particularly in the South; and, with this view, medical men are earnestly entreated to coöperate by contributing their observations, experiences, and conclusions in the art and science of healing, including the collateral sciences.

Some of the most timid rural and village contributors to this journal have been the most esteemed at home, and the most honored by having their papers translated abroad. *Vale!* B. D.

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.—*From the Medical Purveying Office, U. S. Army, N. O. New Orleans, La., Lat. 29 deg. 57 min. 30 sec. N.; Long. 90 deg. W. Altitude of Barometer above the level of the sea, 35 feet.*

1858.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
MONTHS.	Max.	Min'm	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
October ...	7 A. M. 16th. 30.332	2 P. M. 24th. 29.900	30.136	9 P. M. 7th. 84	9 P. M. 29th. 70	76.52	2 P. M. 7th. 84	7 A. M. 29th. 64	74.96
November	7 A. M. 26th. 30.440	2 & 9 P. M. 21st. 29.750	30.152	9 P. M. 1st. 72	7 A. M. 10th. 56	62.19	2 P. M. 1st. 73	7 A. M. 24th. 43	54.73
Dec. 15 ...	7 A. M. 9th. 30.530	2 P. M. 14th. 30.010		Several, 76	7 A. M. 10th. 54		2 P. M. 7th. 76	7 A. M. 10th. 41	
1858.	HYGROMETER.			PREVAILING WINDS.			RAIN.		
MONTHS.	Max.	Min'm	Mean				Days.	Quantity.	
October ...	Several 80	7 A. M. 29th. 63	73.16	East, North East and N. W.			12	4.13 inch.	
November	2 P. M. 1st. 70	7 A. M. 24th. 42	52.99	N. W., North and North East.			9	3.20 inch.	
Dec. 15 ...	9 P. M. 7th. 74	7 A. M. 6th. 40		South, East, and North East.			4	4.89 inch.	

REMARKS.—November 6—Frost in many parts of the city.

9—Thin ice in parts of the city.

24 and 25—White frost in the morning.

*Medical Department of the University of Louisiana.*

The friends of Southern Medical Schools will be gratified to learn that the Medical Department of the University of Louisiana has now (Dec. 20th) recorded on its books three hundred and six matriculates. The steady increase of this College gives ample evidence of its future destiny. Whilst adding annually to its members, it has acquired a reputation and has afforded the essential facilities for medical education, which render New Orleans the great medical centre of the South West.

CHAILLÉ.

*Mortality Statistics of New Orleans, from Oct. 1st to Dec. 12th, 1858, compiled from the Weekly Reports politely furnished by Dr. Baldwin, Secretary of the Board of Health. Population of New Orleans estimated at from 150,000 to 200,000.*

<i>Time.</i>	<i>Total Deaths.</i>	<i>Children under 2 yrs.</i>	<i>Under 20.</i>	<i>U. States.</i>
October (5 weeks).....	2315	302	777	936
November (4 weeks).....	834	190	340	455
December (2 weeks).....	272	73	122	142

<i>Principal Diseases.</i>	<i>October (5 weeks.)</i>	<i>Nov. (4 weeks.)</i>	<i>Decem'r (2 weeks.)</i>
Still born.....	31	25	12
Tris. Nascent.....	33	22	7
Cholera Infantum.....	5	5	1
Infant. Convulsions.....	47	31	14
Infant. Marasmus.....	24	21	5
Teething.....	19	10	1
Croup.....	11	9	11
Scarlatina.....	7	2	2
Rubeola.....	0	0	1
Variola.....	0	4	5
Diarrhœa and Dysentery..	73	69	17
Gas. Enteritis.....	8	2	5
Inflammation of Liver.....	6	4	4
Inflammation of Throat...	22	18	9
Inflammation of Lungs....	11	34	25
Phthisis.....	72	80	31
Apoplexy.....	11	7	3
Congestion of Brain.....	29	11	9
Tetanus.....	6	4	5
Fever Typhoid.....	21	20	10
“ Miasmatic.....	94	40	6
“ Yellow.....	1517	224	3

The weekly mortality from Yellow Fever, from June 20th to Dec. 5th was, 2, 8, 9, 20, 25, 70, 140, 286, 312, 402, 448, 472, 460, 444, 380, 390, 308, 265, 174, 136, 66, 17, 5, 1. It will thus be seen that this disease has existed in New Orleans twenty-four weeks, that it reached its culminating point in the 12th week, and that it prevailed as an Epidemic about sixteen weeks.

CHAILLÉ.

## EDITOR'S OFFICE.—NOTICES.

[Managing Editor's Office and Residence, 89 Constance, between Melpomene and Thalia streets; box 106 D, Post Office; box of the Journal at Mr. Morgan's book store, same place, where communications, etc., may be received.]

JANUARY, 1859.

### NEW MEDICAL JOURNALS.

*The North American Medical Reporter*: Edited by W. Elmer, M. D. October, 1858. New York: Published quarterly, by W. A. Townsend & Co.: 1858

*The New York Medical Press: a weekly Journal of Medicine, Surgery, and the Collateral Sciences*: Edited by J. L. Kiernan, A. B., M. D., and W. O'Meagher, M. D. No. 1, Dec'r 4, 1858. New York: 1858.

*The Journal of Materia Medica, and Pharmaceutical Formulary*: devoted to Materia Medica, Pharmacy, Chemistry etc.; conducted by Joseph Bates, M. D., A. Hutchins, and H. A. Tilden. December, 1858. Published by Tilden & Co., New Lebanon, New York: 1858.

### BOOKS AND PAMPHLETS RECEIVED:

*Lectures on the Principles and Practice of Physic*: delivered at King's College, London; by Thomas Watson, M. D., Fellow of the Royal College of Phys., etc.; a new American Edition, with additions, by D. Francis Condie, M. D., Fellow of the College of Physicians of Philadelphia; Member of the American Philosophical Society, etc., etc.; with 185 illustrations on wood. Pp. 1224, 8vo. Philad'a. Blanchard & Lea. 1858. From Messrs. J. C. Morgan & Co., Booksellers, Exchange Place, New Orleans.

*The History of Prostitution: its extent, causes, and effects throughout the world.* (Being an official Report to the Board of Almshouse Governors of the City of New York.) By Wm. W. Sanger, M. D., Resident Physician, Blackwell's Island, New York city; Member of the American Association for the Advancement of Science, etc., etc., etc. Pp. 685, 8vo. New York. Harper & Brothers. 1858. From Messrs. J. C. Morgan & Co., Booksellers, Exchange Place, New Orleans.

Proceedings of the Academy of Natural Sciences of Philadelphia.

Report of the Superintendent of the U. S. Coast Survey, during the year 1856. 4to. Washington. 1856. From the Hon. John Slidell, U. S. Senator for La.

*Lectures on the Diseases of Women*: By Charles West, M. D., etc. Part 2d. Pp. 308—500. 8vo. Philadelphia. Blanchard and Lea. 1858. From Mr. T. L. White, Bookseller, 105 Canal street, New Orleans.

*Diseases of the Urinary Organs: a compendium of their Diagnosis, Pathology, and Treatment*; By William Wallace Morland, M. D.; Fellow Mass. Med. Society, etc., with illustrations. Pp. 579. 8vo. Philadelphia: Blanchard & Lea. 1858. From Mr. T. L. White, Bookseller, 105 Canal street, New Orleans.

*A slight examination of this work has sufficed to produce the conviction that it is a very meritorious one, and will prove an useful guide in the treatment of the diseases of the urinary organs. It is hoped that some of the readers or editors of this journal will furnish an accurate estimate of its plan, execution, and practical value.*

*A System of Human Anatomy, general and special*: By Erasmus Wilson, F. R. S. A new and improved American edition, from an enlarged London edition; Edited by Wm. H. Gobrecht, M. D., Professor of Anatomy in the Philadelphia College of Medicine, Fellow of the College of Physicians of Philadelphia, etc.; with 397 illustrations on wood. Pp. 616. 8vo. Philadelphia: Blanchard & Lea. 1858. From Mr. T. L. White, Bookseller, 105 Canal street, New Orleans.

Professor Bennett's *Lectures on Clinical Medicine*, mentioned in the catalogue already published, was received from Mr. T. L. White, 105 Canal street, N. O.

*The Physician's Visiting List, Diary, and Book of Engagements for 1859.* Phila. Lindsay & Blakiston. From Messrs. J. C. Morgan & Co., Booksellers, Exchange Place, New Orleans.

*A Manual of the Practice of Medicine*: By T. H. Tanner, M. D. F. L. S.; author of a manual of Clinical Medicine, etc.; Licentiate of the Royal College of Physi-

## E R R A T A .

ART. II.—*Floral, hydro and terrene Dynamics.*

Page 177.—Eleven lines from bottom, “small vessels taxed heavily for *tonnage* cannot compete with vessels of heavier tonnage,” read “small vessels taxed heavily for *towage* cannot compete with vessels of heavier tonnage, sailing from the Atlantic ports.”

Page 183.—Seventeen lines from bottom, for “*studies*,” read “*studied*.”

Page 186.—Three lines from top, “constructs its nidus the zymotic germs,” read “constructs its nidus. The zymotic germs,” etc.

Page 190.—Sixteen lines from top, “Every planter occupying a higher land,” etc, read “Every planter occupying *no* higher land than that on which New Orleans stands contrives to keep it dry.”

Page 191.—Eight lines from top, “carried off,” read “*carted* off.”

Page 191.—Fourteen lines from top, “each *generator* forms an additional layer,” read “each *generation*,” etc.

Page 191.—Twenty-two lines from top, “*Palustro*,” read “*Palustre*.”

Page 192.—Three lines from bottom, “another field, *when* a bridle is put upon,” etc., read “another field *where* a bridle is put upon,” etc.

Page 192.—Fourteen lines from top, “*waters*,” read “*water*.”

# TABLE OF CONTENTS.

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## ORIGINAL COMMUNICATIONS.

ART.	PAGE.
I.—Continuation of Remarks, having special reference to the Treatment of Yellow Fever: By Edward Jenner Coxe, M. D., Visiting Physician, Charity Hospital, New Orleans.....	153
II.—The Influence of Floral, Hydro and Terrene Dynamics on Health, Agriculture and Commerce. Read before the New Orleans Academy of Sciences, December 19, 1858: By Samuel A. Cartwright, M. D.....	175
III.—Surgical Memoranda (continued from the March No. of this Journal, 1858, page 183): By J. J. McElrath, M. D., of Camden, Ark....	195
IV.—Fracture of the Skull: By J. M. Owens, M. D.....	200
V.—Asphyxia.—Resuscitation from Apparent Drowning: By Edward Jenner Coxe, M. D.....	203
VI.—Intestinal Wounds: By J. J. McElrath, M. D., of Camden, Ark.....	205
VII.—Illustrations of Fever: By Bennet Dowler, M. D.....	207
VIII.—Observations and Reflections on Yellow Fever: By M. Morton Dowler, M. D.....	220

## PROGRESS OF MEDICINE.

I.—Lithotrity .....	235
II.—The Ecraseur.....	252
III.—Irrigation in the Treatment of Lacerated Wounds: By J. D. Chep- mell, M. R. C. S., Faubourg St. Honore, Oct., 1858.....	255
IV.—On the Theory of the Production of Hernia: By Professor Roser (Arch. für Physiol. Helk., 1858.....	257
V.—Wounds of the Chest—The new Bullet.....	259
VI.—On Hospital Gangrene: By M. Maupin. (Mémoires de Médecine Militaire, tome xx, p. 368.....	261
VII.—Scott on the Treatment of Joints and Ulcers, and Chronic Inflamma- tions: Edited by William H. Smith, Esq.....	263
VIII.—Treatment of Pthisis Pulmonalis by Caustics: By Dr. Roualt. Translated from the French, by J. P. Barbot, M. D.....	264
IX.—On Consumption: By Dr. Richard Payne Cotton, Physician to the Hospital for Consumption and Diseases of the Chest.....	268
X.—Electro Galvanic Anæsthesia.....	269
XI.—Anæsthesia and Anæsthetics. (Original and Selected.).....	272
XII.—Anæsthetic Discovery—A Grain of Wheat from a Bushel of Chaff....	281
XIII.—Hints to Craniographers: By J. Aitken Meigs, M. D.....	285
XIV.—Radical cure of Hernia.....	292
XV.—The effects of certain substances upon the exposed hearts of Ani- mals: By S. Wier Mitchell, M. D.....	299
XVI.—Académie de Médecine. Award of Prizes for 1858.....	299
XVII.—New Operation of Lithotomy.....	300
Notice to Subscribers.....	302



THE  
NEW ORLEANS  
MEDICAL AND SURGICAL JOURNAL.

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MARCH, 1859.

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ORIGINAL COMMUNICATIONS.

ART. 1—*Continuation of Remarks, having special reference to the Treatment of Yellow Fever*: By EDWARD JENNER COXE, M. D., Visiting Physician, Charity Hospital, New Orleans.

IN continuing these remarks, there are many points of interest well worthy of notice, before commencing the particulars of the treatment, and I think it proper to state thus early, that, whether the opinions entertained and expressed, are acquiesced in or not, those opinions, or, as I believe, those facts have been forced upon my mind, by bedside observations, and serious reflection, resulting, in no small degree, from the great loss of life among those, of at least, ordinary health and strength of constitution, at an age, when, with reason, it might be supposed that the human system could more generally and successfully resist the morbid influence of the powerful, though unseen poison.

While firmly believing in the controlling power of appropriate judicious medication, to successfully combat a disease universally acknowledged, and proved at the bedside, to be of a most dangerous and treacherous character, under all circumstances, my first requisite was, the being able, from the recorded observations of others, to decide upon some course of proceeding, which might have received the approbation of at least a portion, if not of a majority of the profession, who had enjoyed so many opportunities during the numerous epidemics, to decide upon that important point, and so confer a

boon upon their successors. Where could such be found? Even the remarks of the late Professor J. Harrison, in vol. 2, 1845, of this journal, admirably correct, and replete with sound practical doctrine, as they are acknowledged to be, did not, in all respects, meet my wants. Expressing the opinion of an individual who yields to no one, in the high appreciation of his profession, I withhold not the remark, that the impossibility of finding a well digested course of treatment, for the ordinary run of cases, clearly laid down, redounds not greatly to the credit of the passed veteran M. D.'s of the by no means unfrequent epidemics. Feeling the want of such a digest, the great work of La Roche, as well as what has been recorded in our own medical journals, was carefully perused, and there was found truly a mighty mass of conflicting opinions upon all practical points, rendering the arrival at a fixed decision, as to the best course to adopt, to meet the various indications pointed out by the symptoms, utterly impossible.

The following remarks, by Dr. M. Morton Dowler, found in vol. 11, p. 493, of this journal, substantiating my views, I extract with pleasure :

“ A threefold 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, etc. Again, circumstances such as these, the yellow fever should be the grand and engrossing medical topic of the day; and every physician amongst us should be considered as failing in duty, who fails to contribute to our recorded stock of knowledge, in relation to this disease.”

Mark what Dr. J. Harrison says: “ Hence the causes of this disease, its pathology, and the methods of treating it, become of immense importance to the public, and hence, too, a duty rests upon those who have seen much of the disease, to impart their experience.”

Remarks of similar import, made by Dr. Merrill, in an article on the yellow fever, at the Bay of St. Louis, in 1820, are to be found in the number of this journal for July, 1851. It is more than probable, I may not succeed in filling the assumed void, but, having enjoyed a

pretty fair opportunity of combatting the enemy, it will be my aim to perform the duty to the extent of my ability, and earnestly request the many, equally, if not more capable, to surpass these humble efforts in so good a cause.

With a firm belief, that successfully, and with some certainty, to conduct a case of yellow fever through all its stages, much, very much is required, in addition to, and commenced simultaneously with the medicinal treatment strictly so called. I make no apology for trespassing on these pages, by offering additional remarks, as proofs, that the opinions here put forth, have been, and are now entertained by others of greater note. To combat a deeply rooted, and in my opinion, a fatal prejudice, is, I am well aware, an arduous, and not very enviable undertaking, but, fortified, as I hope to prove I am, by high authority and the laws of nature, I fear not the result. The essence of the whole subject is embraced in a few words. Is the principle assumed, right or wrong? If right, who shall say, the result will not redound to the welfare of those who may be induced to cast their lot in this city?

With great and unfeigned satisfaction, I extract the following apposite remarks, peculiarly appropriate to this portion of my undertaking, from an article on Refrigeration in Ardent Fevers, in the number of this journal for September, 1856, by the indefatigable and learned, Bennet Dowler, M. D. This article I did not see or read, until after having commenced these remarks, and I cannot but regard the coincidence of opinion, on such important points, as enhancing greatly their value.

“Refrigeration harmonizes with most theories of the treatment of fever, in all of which, whether primary or secondary, free caloric must act mechanically, chemically, morbidly, and fatally, as is well known from experiments upon animals, in which it proves fatal when augmented from  $10^{\circ}$  to  $15^{\circ}$  beyond the normal standard. The animal metamorphoses, decompositions, recompositions, and the many altogether new changes, which the organism must undergo from such an all-pervading and all-powerful agent, are great, paramount, and even in a physical point of view, comprehensible. Every tissue, every atom, every normal chemical combination, whether gaseous, liquid, or solid, must be presumed to undergo deleterious alterations from a great morbid heat. Normal physiology, and physics too, indicate the great principle of refrigeration beyond mis-

take, and an enlightened pathological physiology must accept it as fundamental in the therapeutics of maladies which are characterized by an unnaturally high temperature, as ardent fevers, etc. Many of the phenomena attendant upon these maladies, as they progress, when left wholly to nature, together with most of the remedies applied by art, combine to prove that the removal of morbid heat is a grand desideratum." Alluding to Thompson, or his theory, Dr. D. says: "He also used steam, a purer and better article than the steam of the patient's body, *inclosed* in the bed clothes, so much relied on by some in New Orleans." "It is well known that a high humid heat is far more oppressive, unendurable, and irrespirable than a dry one of the same temperature. A man immersed in boiling water, and in water even at a temperature much below that, is instantly killed; though a dry air, heated two or three times hotter, has been frequently borne for many minutes without injury. Steam of a high temperature, like hot water, is injurious, fatal. *The hot steam of perspiration confined around the body by blankets* is more pernicious than ordinary steam of the same temperature, inasmuch as it is loaded with a most deleterious gas, namely, carbonic acid—a cutaneous excretion—thrown off from millions of pores. That carbonic acid gas is eliminated from the skin, has been proved by direct experiment, by inclosing a limb in an air-tight vessel. It must be observed that this hot steam or air, saturated with humidity, and deleterious gases confined around the body, prevents the aëration, or oxidation of the blood, which, to some extent, is carried on by this tissue, the cutaneous, as in some of the inferior animals. An impermeable varnish placed on the skin soon proves fatal to animals, as proved by many experiments. Air, garments, sheets, and blankets, saturated with humidity, and confined around the body, so far from increasing, diminish, and would wholly stop perspiration, or evaporation, just as in the case of an impermeable varnish were the saturation and confinement of the air complete. The illusory appearance of wet garments, as indicants of abundant perspiration, misleads the unscientific observer. These garments once wet, would be always wet, were the air wholly excluded. A free current renders perspiration invisible, though not the less real for that reason. Sensible perspiration thus becomes insensible, [passing off, unseen, in vapor. Dry garments, and warm, dry, free air, may lessen apparent, not real perspiration. The fundamental idea, in the popular mind, that sweat and

wet, perspiration and water, are synonymous terms, is a fallacy which the medical profession should not countenance. The artificial application of heat may increase sensible perspiration ; but the hot, forced sweat, thus excited, as it is sometimes in yellow fever," [almost invariably, E. J. C.,] "which, in the early stage is one of the hottest diseases known, is generally mischievous, often resembling that malignant form of yellow fever, in which the patient, from the first hour of his disease, until his death, is bathed in a hot perspiration, the heat declining but little, even in *articulo mortis*. For although the general tendency of perspiration be refrigeratory and curative in yellow fever, yet heating measures to excite it during the hot stage of the disease, augments the danger upon the whole. The forced sweating not neutralizing the new element of mischief, introduced and combined with the preëxisting preternatural temperature inherent in the malady itself. In these persistent hot cases of yellow fever, nature generally sets up the sweating and *evaporating* process, which is favored upon physical principles, by free ventilation in warm, dry, lively currents of air, unless the body be almost hermetically sealed in blankets, non-conductors of heat.

"Among the most efficacious of all remedies for the treatment of the early and middle stages of ardent fevers, are, free aërial currents, and cold water. The application of these remedies, of course, requires skill and discrimination, yet owing to their cheapness, simplicity, pleasantness, or to prejudice or some other cause, they are not only very often repudiated, but very opposite methods and agents are substituted in the treatment of febrile maladies. In New Orleans, the refrigeratory treatment is underrated, not a few patients being treated by other means, chiefly in yellow fever, as hot mustard baths, warm drinks, blankets tucked around the body to exclude the air, closed windows, etc. This practice, which, it appears, originated with a portion of the medical faculty, has, during the late epidemics in New Orleans, taken a deep hold upon the minds of many, inso-much that the writer," [Dr. B. Dowler,] "has, in his own practice, in some instances, been unable to carry into effect a better mode of treatment, even in the hot stage of yellow fever. In fact, he has found patients willing to take any drug he might prescribe, who, nevertheless, refused to change their wet body linen and sheets, for a week, who wrapped themselves in blankets, who hurriedly put out their wrists, in order that their pulses might be felt, returning the

same quickly under cover, lest the sweating should stop, who, *instigated and advised*, by their *deluded friends*, allowed no windows to be opened, nor cold spongings to be applied to the skin, nor iced drinks to be taken.

“Candor obliges me to say, that a fair proportion of patients thus refractory, as it respects purity of air, light covering, sponging, etc., recovered, nevertheless. Nature, instinct, and feeling, are not always wrong. Nothing but a hope, or belief, that heavy coverings of blankets, or of feathers, are necessary to the patient’s cure, reconciles him to bear them during the hot stage of fever.

“In some instances, however, when the physician is prejudiced against ventilation, but not against cold beverages, the latter serve to lessen the increased heat from excessive coverings, and the absence of free ventilation, by their refrigerant action in the centres, and by their secondary action in the form of both sensible and insensible perspiration, or transpiration from the surface. Nature is great, and seeks to compensate herself on many occasions in the sick room, in despite of false theories and bad therapeutics.

“The impure air, originating from dense masses of human beings, whether in palaces, jails, ships, or camps, is, when not removed by ventilation, known to be the antecedent, or cause of typhus fever, etc.; and how can it be dispensed with, or substituted by drugs, its opponents have not explained. Exclude all air from a room, and its inmates must inevitably die, with a rapidity proportioned to the area and the number of individuals, just as in the case of the men confined in the Black Hole of Calcutta.

“Experimenters find that both carbonic and nitrogen gases are parted from the system, through the skin, by the sudoriparous glands. And it is highly probable that the poisonous agents, the *materies morbi* of diseases are, for the most part, eliminated from the skin, in the form of sweats, gases, odors, eruptions, etc.

“Those who have no faith in the existence of perspiration, which they can neither see nor touch, should know that experimental physiologists have obtained data, by which they are able to estimate the actual weight of matter parted from the body in health by aëriform exhalations, etc., chiefly from the skin, at about five pounds daily, and this, too, without it may be, the appearance of liquid sweats, or wet garments. Among those attacked in the great epidemic of 1853, from personal experience, among patients under my care, or seen in

consultation, a considerable number had spontaneous, profuse, hot perspirations, through the whole course of the disease, which saturated their garments, beds, and bedding. In a few instances, my authority was wholly insufficient to cause these latter to be changed during the entire course of the disease.

“Neither could I induce the friends of the sick to permit the access of pure air from without. Deprived of ventilation, these patients respired foul air, and also, weltered in clothes, and skins loaded with viscous, oily, acid, saline matters, and cuticular exfoliations, originally parted from the body as effete or deleterious, but which, nevertheless, were carefully confined around their bodies, for fear the sweat should dry up. Effete deleterious matter thus confined in a liquid or humid gaseous form upon the cutaneous surface, would probably, to some extent, undergo reabsorption. In the first decennium of the present century, Prof. Mussey found, after immersing himself in a madder bath, that for the two following days he had plenty of madder in his urine. An infusion or cataplasm of tobacco applied to the skin, speedily produces severe nausea, prostration, fainting, vomiting, etc. All know that not a few articles of the *Materia Medica* act through the skin, upon the whole system, as belladonna, cantharides, etc. Hence the term, endermic medication. Medicines and poisons enter the economy by pulmonary inhalations; the deleterious exhalations discharged from the lungs of patients, in close, unventilated rooms, and in hospitals, reënter the system by this same route, which, upon every rational principle of analogy, not to say direct experiment, must be injurious to both the sick and the well. The inhalation of effete gasiform animal matter is a most unfavorable but unavoidable circumstance incidental to crowded hospital practice.

“Sponging cleanses the skin, keeping the outlets of the sudoriparous ducts open, thereby favoring the escape of sensible perspiration, which facilitates the discharge of oily matter from the sebaceous glands, promotes the elimination of carbonic acid, and perhaps also, the deleterious principle which constitutes fever, *per se*, or its *causa vera*.”

Thus far we have a series of undeniable facts, recorded by the well known editor of this journal, which cannot fail to receive the most serious consideration from every member of the profession. The following will be found equally conclusive upon the important subject under consideration :

In the number for March, 1856, of this journal, in detailing the general plan of treatment adopted by Prof. Shanks, of Memphis, (Tenn.) during the epidemic of 1855, the following language occurs, which, from its appropriateness to the main object of this part of my subject, I consider well worthy of extracting. "The encouragement, or production of perspiration, during the stage of heat and excitement, by hot diaphoretic drinks, wrapping up in blankets, and keeping the room heated and confined, was soon found to be most decidedly injurious. The morbid heat was thus retained, and the excitement, restlessness, pain and exhaustion, greatly increased, without any compensating benefit. Some of the most favorable recoveries occurred, when the liver and kidneys acted well, without any greater action of the skin, than the insensible perspiration, and a gentle moisture."

Be it remembered, that the mode of treatment which, upon trial, had to be condemned, as prejudicial, had been imported from New Orleans, which might reasonably have been regarded as the *fons et origo* of correct treatment, due regard being had to the many opportunities of treating the disease, our physicians had enjoyed for so many years.

Under the head of Treatment, in the work of Dr. La Roche on Yellow Fever, are found the following extracts: "*Cool Air*.—There are few physicians, conversant with yellow fever, who have not had ample opportunities of appreciating the advantages of placing the patient under the influence of cool or fresh air. The good effects of it, during the epidemic of 1793, were, according to Dr. Rush, obvious in almost every case in which it was applied. It was equally proper, whether the arterial system was depressed, or whether it discovered in the pulse a high degree of morbid excitement.

"Dr. Griffiths furnished a remarkable instance of the influence of cool air upon the fever. Upon my visiting him, on the morning of the 8th of October, I found his pulse so full and tense as to indicate bleeding; but, after sitting a few minutes by his bedside, I perceived that the windows of his room had been shut in the night by his nurse, on account of the coldness of the night. I desired that they might be opened. *In ten minutes afterwards, the doctor's pulse became so much slower and weaker*, that I advised the postponement of the bleeding. The cool air was improper only in those cases where chilliness attended the disease. Dr. Cathrall says that the chambers of the



sick should be spacious and airy, kept cool, and frequently sprinkled with vinegar. Great attention, adds Dr. C., should be paid to these circumstances, as they are of infinite importance to those who frequent the apartments of the sick, and are highly *conducive to the cure of the disease*. Dr. E. H. Smith found the same benefit to accrue from this means in New York, during the epidemic of 1795. The advantages which resulted were," he says, "universal and wonderful. To this end, the sick were ordered to be placed on a hard bed, with very little covering, in the middle of the room ; and the doors and windows were, as much as possible, kept open day and night. In all the stages and circumstances of this malady, of whatever form, says Dr. Dickson, we must give the sick man fresh air. If his apartment be close and unventilated, he must needs die. Similar observations, continues Dr. La R., have been made elsewhere, whether within, or beyond the tropics, and to enhance the effect, it is often found advantageous to sprinkle the room floor with water alone, or mixed with vinegar. The results are the more marked, when the air is not only cool, but also when it is pure, its renewal unimpeded, and its circulation free. For this reason it is that the mortality from the fever is greater where these advantages cannot be obtained. Hence the practice, in the West Indies and elsewhere, of removing the patients from hot and confined localities, to spots where the air is cool and pure, and the ventilation as complete as possible ; or, where such changes are not attainable, to place them, if possible, in an upper and well shaded room.

"The necessity of these means has long been recognized. In these matters, as in most others, there is nothing new under the sun. Galen, some two thousand years ago, in treating of the synochus, directed the apartments of the sick to be kept cool, and well ventilated, and to have the floors sprinkled, and rendered fragrant by means of suitable flowers."

Dr. La R. cites his authorities for the above facts.

"Now, if a comparison be justly drawn between the above unquestionable natural rules, *life-aiding* they may properly be termed, as being strictly in accordance with the laws of nature, reason, and common sense, and those customs so long stereotyped in the city of New Orleans, excessive covering, close rooms, and doors, and even the crevices stuffed, no less properly termed, Death-assisting aids. Is it to be wondered at, that the difficulty of successfully treating

yellow fever has been great, or that the mortality has been excessive, far beyond what would reasonably be expected, seeing that there always are, in every epidemic, very many cases of a mild type, which do and will end favorably, without much medication, provided the laws of nature are observed, and the real necessities of the case in hand be looked at and closely watched, all, without so much ado about incessant nursing? and is it not a fact, that some do recover, in spite of all obstacles, even that most reprehensible of all, the abandonment of the case, as hopeless, by the medical attendant before death has occurred, as though the wonderful power of nature, in its proper acceptation, was not behind the curtain? Is not the assertion an almost demonstrated fact, that thousands, aye, tens of thousands, have been immolated upon the altar of erroneous opinions, and the practice deduced therefrom? I need not be told that I have drawn a fancy sketch, for I speak that which I do know, and have seen. All know, that in the main, the allegations are true, and, if necessary, the most abundant proof could be adduced, even from non-medical observers and thinkers, of the self-evident fallacy of excessive covering, and seclusion of fresh air, in the treatment of Yellow Fever.

Having sufficiently dwelt on the preceding vitally important subjects, may I not hope, satisfactorily? I now direct attention to another, scarcely inferior, and intimately associated with any plan of treatment which may be decided on, as best calculated to lead a case of yellow fever, or the various symptoms therein presented, to a successful termination.

The subject alluded to, is the nursing of the patient, with suggestions as to the propriety, or safety of admitting friends, acquaintances, or visitors to the chamber of the sick, during the progress of a case of this fever. Known, and recognized as a fact, beyond cavil, that, at the commencement, or forming stage of yellow fever, in the majority of cases, it is absolutely impossible for any one, however experienced he may be, to foretell what course it will pursue, whether it will pass quietly and slowly, without much suffering, to a speedy and happy issue, or whether, after having at first appeared in a mild form, it may, unexpectedly, and suddenly, put on the most dangerous symptoms, possibly setting at defiance the highest medical skill. Fortifying my assertions, or opinions, by the remarks of others, well known to the profession, the following is from the pen of Dr. M. M.

Dowler, who says : " 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 art, can have no assurance, as to whether their cases shall be so mild, as to yield to the therapeutics of nature, or so deadly, as to defy the power of medicine." As no one who has seen much of yellow fever, can question the possibility of this uncertainty in a matter of great moment, how manifestly important becomes the subject of nursing? All, the faculty, as well as the public, are well aware, how essential good nursing has been, and is regarded in the city of New Orleans, and in this belief I most heartily coincide.

Adopting, as do many of the faculty, ably seconded by so-called nurses, the erroneous practice of excessive blanketing, and the exclusion of fresh air, ought it to be considered a matter of astonishment, that, with the body at an elevated, or febrile temperature, it is found impossible to keep the clothing constantly adjusted around the body, without the unceasing aid of the nurse, for nature will speak, by kicking off the covering? Now, if such interference is essential for the safety of the sick, which unequivocally I deny, what can result, but the constant annoyance of the patient, and generally, an increase of the existing fever? What but injury can result from such a course, scarcely to be believed, if not known to be a fact! From personal experience, I do know that this irrational and murderous course has been regarded, and acted on as the law, and even at this distant period, it is not without a shudder, that I revert to what I was forced to endure, when passing through a most violent attack of yellow fever in 1839, an attack rendered what it was, by what I shall ever regard as mal-practice.

Should my efforts to overthrow such a fatal prejudice, which is known to exist, and be daily practised during the existence of an epidemic of yellow fever, in any degree cause the important subjects alluded to, to be seriously considered, ample will be the reward, as far as my labor is concerned, while to others it may prove of still greater value in the preservation of life.

On the other hand condemning any imprudence, by adopting the opposite rational, because natural course, which allows the real sufferer to think a little for himself, as to the quantity of clothing and

fresh air adapted to his feeling of comfort, when scorched by the fever; and this ought to be regarded as the only true guide for such matters, for an individual in his senses; does it not follow, as a necessary consequence, that the nervous irritability, so constantly present in the febrile stage, often continued throughout the disease, will become tranquilized, other symptoms ameliorated, and who will venture the assertion, that the appearance of many dangerous symptoms would not thus be prevented? Is it not a legitimate assertion that in gaining such self-evident advantages, the necessity of the customary, unceasing watching on the part of the nurse will be greatly lessened, and the not unfrequent changes in the disease materially diminished? Acknowledging in all sincerity, my high appreciation of the real benefit resulting from the attendance of a good and faithful nurse, more easily talked of than found, during an epidemic, there is no question in my mind, that the majority of hired nurses, notwithstanding the five or ten dollars a day, more frequently consult their own ease and comfort, rather than the welfare of the sick under their charge. The self-sufficiency, or officiousness of many hired nurses, as well as of not a few falsely called friends, is manifested in many ways, almost always to the detriment of the patient. It is unnecessary to enter into particulars. The remark, however, of one of our most experienced physicians, who had attended about one hundred cases of fever, during the recent epidemic, will show that similar opinions are entertained by others, competent to judge, and express an opinion about the subject. Said he, "as regards the generality of self-styled nurses, in yellow fever, I would prefer an ordinary field hand, who made no pretensions, but who would keep awake, and strictly obey all the directions left for her guidance." Believing that the main object in alluding to subjects of such real importance, will be equally, if not better attained, by giving a few directions for the proper management of the sick room in yellow fever, rather than by complaining of what, in justice to all parties, I deem the results of thoughtlessness, I append the following:

The first direction is embraced in the remark, that, in the treatment of any serious disease, more especially, one, at times so violent in its approach; frequently so rapid in its career, for weal or for woe, as yellow fever is known to be, there should be a strict observance of all directions given by the physician in attendance. The fact should be not only distinctly recognized, but constantly kept in

mind, and acted on, that, as far as human means are concerned, the life of the patient is under the charge of the physician in attendance ; with, and upon him, rests the responsibility, and he can only predicate his hope of success in the treatment of the case, on the faithfully carried out instructions which have been given.

2nd. Who will question the fact, that the best of all nurses, is the wife, the sister, or a real friend, assisted, it is true, by a nurse, thus combining feeling with the labor necessarily attendant upon the treatment of the case.

3rd. For reasons too obvious, to need explanation, there should be as little talking in the room as is consistent with the necessities of the case. Hence, the importance of there being in the sick room but one, or at most, two persons, for it has frequently been confessed by the sick, that, but for the too much whispering, many a doze would have been enjoyed, thus, unintentionally, no doubt, interfering with the real well-being of the sick friend. It should always be borne in mind, that a sweet sleep, if only for an hour, not unfrequently proves sufficient to turn the scale in favor of recovery.

4th. As a necessary consequence of the above, no friends or acquaintances, should desire, or be allowed to enter the sick room for any purpose whatever, for who knows not, even supposing no talking to be done, that more or less excitement is almost invariably the result. The mere standing at the bedside is exciting to many, and had better be avoided.

5th. As far as practicable, whatever may be required to be prepared for the sick, the process should not be carried on in the sick room.

It is possible that a few may offer objections to such, apparently rigid directions, but believing, from what I have seen and heard, that positive injury has resulted from their non-observance, I am sure all will admit the propriety of adopting the safe course. By such means it is not impossible that we may find the disease to be more under the control of human means, and as a natural consequence, the dread of an attack of yellow fever, becomes materially lessened.

Having upon the above important points said all that seemed called for, or was necessary, to convey my views clearly, not wishing to be misunderstood upon matters of no mean value, if the desire exists to make the fever more amenable to treatment, I pass on to the consideration of a few points, not devoid of interest.

What is yellow fever ?

To give a satisfactory answer to this question, I will first state the definition given by Fordyce, of the word, fever.

“Fever, says Dr. F., without alluding to any particular form, is a disease that affects the whole system ; it affects the head, the trunk of the body, the extremities ; it affects the circulation, the absorption, and the nervous system ; it affects the body, and likewise the mind. It is a disease, therefore, of the whole system, in every kind of sense. It does not, however, affect the various parts of the system, uniformly and equally ; but, on the contrary, sometimes one part is affected more in proportion to the affection of another part.”

If, to the above attributes of fever, all of which, the disease under consideration, possesses in a marked degree, we add the facts, that the stomach and kidneys are more prone to manifest the poisonous effects of the first cause, at a much earlier period ; that the disease is more violent and dangerous, as to symptoms and effects, with a tendency to a fatal termination, more rapidly and certainly, than, as a general rule, is found to be the case in the ordinary course of any other fever, the question as to what is yellow fever, may be considered answered.

In reference to the particular type of the fever, during the past epidemic, it was regarded as a continued fever, of one paroxysm. In all respects, in my opinion, yellow fever is to be looked on as a disease *sui generis*, differing in so many essential points from the bilious, the remittent, or the intermittent fevers, that I could regard it in no other light, although, from incidental circumstances, it might have, in a few cases, put on some of the characteristic features of those forms of fever. To my mind, it is impossible to find in any other fever, however severe the type, such an assemblage, or variety, of the most dangerous symptoms, at as early a stage, frequently, and most unexpectedly ending fatally, without the slightest hope of affording relief, the idea of effecting a cure would be a pure absurdity.

During the continuance of the epidemic, so frequently were the following facts observed, appearing to have a direct bearing on the mooted point, the part first affected in an attack of yellow fever, that, in my opinion, their importance deserves attention.

Many were brought into my wards, complaining solely of a feeling of great soreness of the whole body, or in some cases of certain

parts, the back and lower limbs ; headache was present in the majority, though absent in some, there was no evidence of fever, as indicated by the pulse or skin, there was an absence of the injected and peculiar appearance of the eyes and face, so constantly present, as to constitute it the pathognomonic sign of yellow fever, which, when absent, would cause great doubts as to the genuineness of the attack.

In the absence of almost all of the more usual symptoms of the first stage of yellow fever, while good reasons existed for its being such, in an embryo state, I uniformly was satisfied to prescribe one or two modified blue pills, followed in two, or three hours by a moderate dose of castor oil, one, or two tablespoonfuls, with, if required, in a few hours, an injection of salt water, to insure a free action of the bowels. This ordered, I would wait until the following morning, or evening visit. At the next visit, how manifest the change ! how clearly marked, and fully developed, all of the characteristic features of the fever ! The eyes presented their significant appearance, more closely resembling those of one just out of a drunken debauch, coupled with a slightly dirty yellowish tinge of the white. The headache was far more severe, and always present, the face more flushed, or red, the skin intensely hot and dry. Now, there could be no mistake ; and to what conclusion could I arrive, but that the nervous system was the part of the body first acted on by the invisible poison, which had so mysteriously and powerfully impressed the entire system ? The nervous system, once impressed, who will affix a limit to the velocity with which subsequent impressions shall pass from that system, to the blood, and upon it, exert its most powerful influence ? This secondary effect produced, what is there to protect any one of the vital organs from the destructive agent, now coursing rapidly throughout the whole system, selecting any weak organ, upon which to expend its poisonous influence ? Who can prove the contrary ?

Subsequent events, developed during the treatment, throughout the whole course of the fever, fully satisfied me of the necessity of closely watching the frequent and various manifestations of the nervous system, and, as far as possible, to endeavor to control them by such remedial measures as might have been indicated in each case.

“The nervous system,” says Dr. J. Harrison, “is particularly liable

to derangement in yellow fever ; the most fatal of all the symptoms are those which indicate affections of the brain."

Not irrelevant to the disease, or the treatment, I consider it proper to state that frequent opportunities were presented, during the epidemic, of noticing an affection of the whole system, which, not only in hospital but in private practice, has been of frequent occurrence, at all times, and irrespective of other diseases, an affection not admitted into the catalogue of positive diseases, although well meriting specification as such. I allude to bilious derangement, which existed to a greater or lesser degree, in quite a large number of the cases of yellow fever, brought into my wards. As a complication, it plays a most important part, but, in no one instance, were effects produced, calculated to induce the belief, that the important organ, the liver, was in any way or degree, more than functionally deranged. As a purely functional derangement, it was regarded as performing a very important *role* in the grand assemblage of the symptoms of yellow fever, demanding for itself a particular treatment, because of indirectly exerting a most powerful and baneful influence upon the fever itself, and materially interfering with the administration of the remedies considered appropriate for the judicious management of the main disease, the yellow fever.

With the exception of the above noted derangement, I do not believe the liver to be an organ, seriously, if at all impressed by the poison, or first cause, which opinion is substantiated by the following remarks of Dr. J. Harrison : "As for the liver, the symptoms of the disease, throughout its whole course, as well as post mortem examinations, show that it is, by no means, particularly affected." And again : "I have never seen any lesion in this organ, which could be attributed to the effects of the yellow fever."

*Jaundice.*—Of this peculiar yellowness of the skin, generally termed jaundice, which no doubt does in some cases result from bile, there is reason to believe, that much more frequently, it is to be considered as the effect of a morbid condition of the blood, although practically I do not regard it of much importance. Jaundice in yellow fever, is represented by most authors to be of frequent occurrence ; in my wards but few instances occurred, which I attributed to the general course of treatment pursued. By far the most severe, and general case of jaundice occurred in a man who had a relapse of the fever after having been fairly convalescent for about a week. He was about gain-



ing strength by means of diet and tonics, the bowels were regular, in all respects, yet when least looked for, he had a relapse, which proved more severe and dangerous than the first attack, by no means mild in character.

*Tongue.*—In reference to the appearance of the tongue, so entirely different did it present itself, that no especial value was attached to it as a sign of the disease. Most generally, it was heavily coated, of different thicknesses and color, at times red and dry, and occasionally quite natural in appearance. “In truth,” says Dr. Harrison, “those who see much of yellow fever, soon learn to distrust the tongue in their diagnosis.”

*Restlessness*, to a great degree was noticed in the majority of cases, and it was observed as a prognostic fact of no mean import, that those who remained perfectly tranquil, uniformly passed through the disease with few if any draw-backs. In several cases of a most violent grade, this fact was specially noted.

*Delirium* was of rare occurrence, but when it did occur, as a general rule, it was found a most dangerous, and generally a fatal symptom.

*Hiccup.*—This truly distressing symptom, fortunately occurred in but four cases. It is to be looked on as a very dangerous attendant, generally of fatal augury, and always adding to the difficulty of the case.

*Urine, suppression, or retention of.*—But two instances of retention of urine were observed in the whole number of cases, and not one of the suppression, at least in that stage of the disease when looked for, although in those cases of severity, which from the first offered no reasonable ground to hope for relief, it is presumable that suppression may have existed. In consideration of the few cases in which any obstruction was observed in the free and regular secretion and discharge of urine, I am forced to believe that such immunity was the result of the free use of iced drinks, the remedies employed, and the small amount of covering allowed.

*The Pulse.*—In general, the fever being well established, the pulse has been found full, varying in hardness, and frequency to such an extent that to it alone, I confess, I did not attach as much importance, as I know it is most generally supposed to possess, but taken in connection with all of the other symptoms, it is entitled to a diligent attention. One fact in reference to the pulse was perceived, and in every case particularly watched. It was, that however hard and

full, it was with ease compressed by the fingers, showing that it did not resemble the full hard pulse of other fevers. It was an apparent, not a real force, which in most cases after twenty-four hours' treatment was found less frequent, hard and full. There were but two cases of the fever in which I was induced subsequently to believe that the general and local loss of blood would possibly have averted the fatal issue, but in all others however apparent, the necessity for the abstraction of blood, subsequent events in a short time fully convinced me that in the yellow fever of this season in my wards, the resort to that remedy as a means of cure, would have proved disastrous. That there may be some cases in which the local determination, especially to the brain, may be so severe that it would be unsafe to dispense with that powerful revulsive, I do not question but even in those cases I should prefer the free abstraction of blood locally by cups. A close observance of the effects of the disease and the remedies proved the fact, that in a short time the system required all of its physical strength to enable it to pass safely to convalescence, and convinced me that more harm than good would necessarily result from the loss of blood, whether general or local, and according to the ideas entertained of the disease, I believe as a general rule, that great source of depression of the vital force had better be dispensed with in the treatment of yellow fever at all times, with the exception of those few cases, which it is admitted may and do occur.

*Black Vomit.*—This dangerous or more correctly, this almost invariably fatal symptom, was of frequent occurrence in my wards the past season. Often have I left patients very ill, but without any one especial fatal sign, complaining of no pains in any part, with bowels regular, kidneys acting freely, skin natural, and only a sensation of fulness, or uneasiness at the lower part of the abdomen, not however very sensible to pressure, and considering them in a fair condition; yet at the following visit they would be found dead, having previously thrown up large quantities of black vomit. The quantity in some instances thrown off was very large, three or four basins full in the space of a few hours, and more than once in cases that had not been in the ward twelve hours. One man who, to all appearance, was out of danger at least, reclining on his elbow, remarked that he felt no pain, would like a little more diet, and thought he would be able to go out in a few days. In less than six hours he ejected an enormous amount of black vomit, and in a few hours was dead. Truly may

this fever be called treacherous. When black vomit occurs at the latter end of the disease, however favorable all other symptoms may be, I doubt if any will recover, but, when it appears at an earlier stage before the system has been excessively prostrated, and this may occasionally occur, I think there are good grounds to hope that by the use of appropriate means it may be arrested, the condition of the stomach upon which it depends—now to a certain extent local—may be overcome and recovery secured. Under all circumstances it is our duty not to abandon any case however apparently hopeless it may seem until the most conclusive evidence exists, to prove that Nature herself can not interpose her power. Have not cases occurred even with black vomit, other fatal symptoms, and the confession of the physician, that he could do no more, in which, by the unceasing exertions of the family and friends, a recovery has been finally gained, aided, it is true, by that ever to be recognized, though unseen power of Nature, the true friend of patient and physician, at all times? That black vomit does occur in other diseases, of a very different character from yellow fever, and that blood from any hemorrhage passing into the stomach, and there commingling with the acid usually present, will produce real black vomit, are admitted facts, but when the season with all antecedent symptoms of this fever are taken into consideration, how can we fail to recognize in black vomit a pathognomonic sign of yellow fever? It is well known that the presence of black vomit is not essential to establish the fact of the existence of yellow fever, for in the majority of cases, whether resulting in a cure or death, not a trace is seen, and yet, at times, its presence is necessary in order to determine the true nature of the case of fever which may have existed. Upon the recurrence of the season of the year when yellow fever may be reasonably looked for, should the first cases be of a mild character, so similar in all respects are the symptoms observed to those occurring in any ordinary fever, more particularly if the attack be severe, that it is absolutely impossible to decide positively whether it is yellow fever or not. Is not that fact proved at each succeeding year? and if further proof be required it can be substantiated by the printed opinions of some acknowledged judges.

I allude to this fact for the express purpose of exonerating from censure those who, at some distant place, may for the first time fail to correctly diagnose a case of yellow fever appearing in a mild

form. As additional proof of the possibility of error in that certainty of diagnosis in this fever, so much talked of, let me state that in 1839, in this city, I had an attack of fever, moderately severe in character, after recovering from which it was asserted that I had passed through the yellow fever, was congratulated upon the event, and felt sure of the opinions of some experienced medical doctors. In about five weeks from that time I was most suddenly seized by the simple fever, in the most violent and dangerous form, such as few recover from. For years I suffered from the effects of that true acclimation. Am I not correct in my skepticism as to the unerring diagnostic signs of yellow fever in its forming stage?

Facts developed during the epidemics of 1853 and '58, in reference to the important subjects of acclimation, the result of passing safely through a *bona fide* attack of the fever, as well as those connected with individuals who had presumed upon having obtained the same exemption by long residence, require those points to be alluded to. I will first give an extract from the valuable remarks of Dr. J. Harrison, on yellow fever, embodying, as they do, the opinions entertained at the period he wrote (1845) by the majority of the profession. A departure on the part of the disease, during the past epidemics, proves the disease in those years to have been of a more violent character than usual, and that in some respects those recognized facts have been in some measure weakened. Says Dr. Harrison: "The yellow fever attacks only strangers, those born in the city being perfectly exempt from the disease, though it is still a question whether they do not pass through it in infancy. The creoles of the State, residing out of the city, and never subjected to the disease, are as liable to attacks of as grave a character as those born further north. Upon recovery the citizen is said to be acclimated, and enjoys an immunity from the disease; but this, though general, is not universal, for I have known several who have been attacked a second time. I have never known, however, such cases to terminate fatally. Some persons, also—and those not a very few—pass through the most violent epidemics without being attacked at all, and are then considered fully acclimated. This rule, however, does not apply to those who have remained in the city during what are termed mild epidemics; in other words, those who pass through such epidemics as occurred in 1833, 1837, 1839 and 1841, without an attack, are considered as safe as those who have recovered, otherwise not. Per-

sons coming from cities where the disease prevails, and in this respect similarly situated to New Orleans, also enjoy an immunity from the disease. The susceptibility to the disease seems to be renewed by long exposure to cold weather. Negroes certainly have the disease in a much milder form than the whites, and the mortality is far less among them. I cannot say that I have observed any difference as regards the sexes, either as to the malignity or the number of the cases. As a general rule, children have it milder than adults."

During the epidemic of 1858, not a few children of various ages, born in the city, never out of it, had moderately severe attacks of the fever, distinctly marked. In one of the cases under my charge, a boy, aged five years, black vomit occurred in about forty hours after the first symptoms, and continued for nearly thirty hours; but he was finally saved, and is now in perfect health. The particulars to be given under the head of treatment. My impression as to a perfect acclimation is, that not only should the attack occur during an epidemic year, but that the attack should be of a sufficiently severe character, in all respects. A very mild attack, with but very slight fever, and the absence of the peculiar eye, may prove sufficient for acclimation; but such an attack, as well as one occurring in a non-epidemic season, should be cautiously and doubtfully recognized as certain, which in my opinion is clearly warranted by the experience of 1858. In conclusion, I express a decided doubt of a really severe attack, with all of the symptoms well marked, ever having been followed, without regard to time, by a second attack.

There remains one subject in connection with the cause of yellow fever which it may not be profitless cursorily to notice: I allude to the matter of filthy streets and gutters, as an alleged cause of yellow fever in New Orleans.

Coinciding in opinion, in all respects, in the views of Drs. M. M. Dowler and J. S. McFarlane upon that point, I embrace the opportunity, in extracting the following remarks of Dr. M. Morton Dowler, to observe, that I am, and ever have been, a firm believer in the absolute impossibility of adducing a solitary reason in proof of the assertion that so serious an effect could result from such a cause. On the contrary, many localities could be cited to prove that far more filthy streets and gutters than have ever been found in New Orleans have a perpetual abiding-place without producing yellow fever. Such an ad-

mission, however, does not authorize the assertion, that because of their unbelief in that cause, that they do not on other grounds condemn such nuisances most strenuously. There can be no doubt, in the mind of any reflecting man, that perfectly clean streets and well watered gutters, combined with an overabundant supply of wholesome water for the free internal and external use of every one of the population, whether of a large or small city, does and will always contribute, in a great degree, to the formation of a good constitution, and, as a necessary consequence, the blessing of health.

Let those health-preserving measures be properly carried out—let all of our streets be paved with granite blocks, and then finish by having a constant and full flow of water from the inexhaustible Mississippi, through the gutters, and who will assert that positive good will not result—perhaps to the prevention of yellow fever?

After a due consideration of the above, let us direct attention to the more than possible evil effects resulting from the manner of construction of our numerous frame buildings—or shanties, as some are. The joists, the flooring, imbedded in a wet soil, not an atom of air by any possibility allowed to pass through or over such a mass of filth as is seen whenever a floor becomes so rotten that it must be removed. Add to this the number of individuals, of all ages and sexes, as well as the remarkable cleanliness in all respects there seen, and then say, with such an atmosphere as must there exist to be breathed, whether those various combinations of causes are not more reasonably to be considered probably efficient for the production, if not of yellow fever, certainly of impaired health, and a great susceptibility to contract that disease, the true cause of which, it is well to confess, will most probably elude all investigation. Says Dr. M. M. Dowler: "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 investigation."

(To be continued.)

ART. II.—*The influence of Floral, Hydro and Terrene Dynamics on Health, Agriculture and Commerce.* Read before the New Orleans Academy of Sciences, December 19th, 1858: By SAMUEL A. CARTWRIGHT, M. D.

IN no State in the Union, probably in no country on the globe, are the dynamic forces of vegetation, water and earth, working such rapid and extensive transformations as in the State of Louisiana. New Orleans rests upon no less than six superposed cypress forests, as proved by Dr. B. Dowler, from examining the excavations at the gas works and other places. Within a comparatively recent period the silent and unappreciated powers of floral, hydro and terrene dynamics have built up the largest and most fertile portion of the State of Louisiana, and are still at work in elevating its terrene tableau and enlarging its boundaries by encroachments on the sea. Their influence upon health, agriculture and commerce cannot well be appreciated or turned to practical account without a systematic geological survey of the State and a thorough exploration of its waters, its organic and inorganic productions. Louisiana opens a richer field than any other land for original research in geology, chronology, physical geography, natural history and particularly zoölogy. A hundred years have made greater changes in the chorography or physical geography of Louisiana than two thousand in any country on the Mediterranean. Those ancient countries are still recognized by delineations and descriptions made twenty centuries ago, while lower Louisiana can scarcely be recognized by any delineation or description of it, no further back than a single century.

The silent and unperceived action of floral, hydro and terrene dynamics, in progress for many ages, has produced some changes in the world, known to the ancients, and generally for the worse, destroying empires, by causing some extensive regions to be sickly, which in ancient times were healthy; ruining cities, by obstructing the channels of commerce, and paralyzing agriculture, by converting fertile fields into arid wastes. Slow decadence and deterioration seem to be the general result of the operation of floral, hydro and terrene dynamics in the old world, while rapid progress and improvement mark their action in the new, and no where more conspicuously than in Louisiana. So vigorous are those dynamic forces, in a country so new and fresh, that it is dangerous to interfere with them, without calling upon a union of

sciences to direct the movement. The physician, the geologist, the hydrographer, the chemist, the microscopist, the botanist and zoologist should accompany the civil engineer, and be consulted in forming or carrying out any programme of internal improvement, for the benefit of the planter or merchant. Those artificial embankments, called Levees, built in ignorance of the sciences, are sure to disappoint expectations, and often do more harm than good. Their giving way at the hot season of the year—as they always do—besides the damages done to agriculture, directly tends to convert a naturally healthy country into a sickly one. The proof will be found in the Report of the Swamp Land Commissioners to the Legislature of Louisiana, where the facts in regard to the Levees, constructed without a proper base, of improper materials, and an insecure foundation, are fully stated, all of which is the natural consequence of the geology of the State, being a sealed book to the engineers and levee builders.

The money spent by the State Government to reclaim some three or four millions of acres of swamp lands belonging to the State, and the vast sums annually expended by the Federal Government in dredging those outlets of the Mississippi river into the Gulf of Mexico, called the *Passes*, has been expended with eyes closed to the influence of floral, hydro and terrene dynamics upon health, agriculture and commerce, consequently resulting in little or no practical benefit. Levees made of peat or other inflammable earth, as some have been, which floated off when dry, or took fire and burned up, were not less useless expenditures of labor and money than the works the Federal Government have so long been engaged in to remove the obstructions at the outlets of the Mississippi river.

The Mississippi river has no mouth, properly speaking—such as would serve the purposes of commerce by admitting vessels of heavy burden, drawing over eighteen feet water, as the river itself will. All the treasure of the Federal Government would be insufficient to make a mouth for it, or to counteract the influence of floral, hydro and terrene dynamics, which are constantly tending to bring the very bottom of the channels the Government is trying to deepen to the surface, and to cover it with vegetation.

The Mississippi river, from the Gulf to New Orleans, is deep enough to float the heaviest vessels known to commerce; for instance, the East Indiamen. Such large vessels as the East Indiamen could trans-



port freight from New Orleans to Europe as cheap, if not cheaper, than the smaller vessels which can be got over the bar at Sandy Hook can transport it from New York to Liverpool. But certain mud banks, from eighty to an hundred feet high, obstruct the pass way between the Mississippi river and the Gulf of Mexico, effectually excluding vessels of heavy draught. If it were possible to remove them, nothing could prevent their being speedily formed again by the sedimentary deposits of the river.

It is believed that a thorough hydrographical and geological survey would demonstrate the practicability of getting large sea vessels into the river through the collateral aqueducts called *colmates*—contrivances to put water under the will of man, and which M. Thomassy, a distinguished hydraulic engineer of France, has come from Italy and Holland, where they are in use, to introduce in Louisiana.

But if this could not be done the heavy sea vessels could at least anchor in Ship Island harbor, and communicate with the city by means of lighters, conducted into and out of the *river by colmates*. So expensive is the foreign trade of New Orleans in the small carriers, which can be got over the shallow bars at the Balize, that nothing saves it from annihilation but the cheap transportation the river itself affords to the products of the Mississippi Valley. Flour, for instance, costs only 20 cents per barrel from Cincinnati to New Orleans, while its transportation from the former city to New York costs a dollar, and never can cost less. Yet it is cheaper to the exporters of Ohio flour to pay the dollar and ship it to Europe by the way of New York than to pay only 20 cents and send it by the way of New Orleans. The reason is, that none but small light draught vessels can be got from the sea across the bars into the river, and being taxed heavily for tonnage, cannot compete with vessels of heavier tonnage sailing from the Atlantic ports. It costs more per ton to bring a vessel from the Gulf of Mexico to New Orleans, only about one hundred and ten miles, than it costs per ton to transport a similar vessel from the Bay of Fundy through the Welland Canal to Chicago, upwards of a thousand miles. The great incubus on Southern commerce will be found in the fact, that after the Mississippi river passes the lines of levees, inclosing it on either side, it breaks over the low grounds, washing for itself numerous channels, which become shallower as they approach the sea. One of these, called the *Jump*, has been lately formed, and is so deep and rapid

where it debouches, as to be dangerous to the shipping and boats navigating the river; because when sucked into it they cannot get out nor follow the current into the sea, owing to its shallow embouchure. All the aqueducts from the river are lost in a low ground or swamp of their own making, while the water of the three main channels, called the *Passes*, instead of flowing into the sea by any thing like a proper mouth, disembogues through numerous crevasses in mud banks of its own creation, rising from eighty to one hundred feet above the level of the bed of the river itself. The depth of water on those mud banks, after all the dredging and piling to deepen it, is not as great now as it was a hundred years ago, before any levees were built; as will be seen from an old hydrographical chart in the library of this Academy. Instead of more depth of water over the bars, when the river is high, there is always less than when it is low, because, perhaps, more sediment is deposited in the former than in the latter case.

Bars or sedimentary deposits always form in the river below a crevasse, from the giving way of the artificial embankment, called the levee. The bars, by obstructing the current, create a necessity of adding to the height of the levees above the crevasse, to prevent the lands on either side from being inundated by the next freshet. The Bell Crevasse, the last season, a mile above the city, has caused a large bar to form, which can not fail to make the levees for some distance above the bar more insecure than previously to its formation. It is not the levees, but the breaking of the levees, which does the mischief. The almost annual giving way of the levees, at some point or other by forming bars, tends to elevate the bed of the river, and to create the very evil which the levee system was intended to prevent. The fault is not so much in the system itself, as in the unscientific manner of constructing the levees, and in not preventing them from being damaged. The geologist, if consulted, could save the levee builders a great deal of useless labor, who, in some kinds of soil, dig a trench unnecessarily deep to reach a firm foundation to erect the levee upon, while in other kinds of soil the trench is not dug deep enough. The caving of the banks of the river endangers the levee only at certain points, while the whole line of the levee is always in danger, when the river is high, from the burrowing of *crawfish*, the *Astachus Bartonii*. These are not aquatic or amphibious animals as has been supposed. They cannot live but a

short time *under* water. They build their houses near the base of the levee and next to the river, for the convenience of catching fish, shrimps, etc. When the water comes up against it they burrow through the levee, and go on the other side to prevent being drowned. The most effectual method to drive them away is to throw on the base of the levee the crushed stalks of the sugar-cane, called *bagasse*. Some years ago I got Professor Riddell to assist me to examine through his famous microscope, the effects of cane-juice upon the infusoria. It killed them instantaneously—quicker and more effectually than any other substance in his laboratory. Young Riddell, the son of the Professor, had been in the habit of bringing the infusoria to life, which had been killed by chloroform. He tried his hand upon those which had been immersed but an instant in cane-juice, but could not succeed in resuscitating them. They were seen to die under the microscope the instant that the cane juice came in contact with them. About the same time I made a number of experiments with the sugar cane, and was forced to the conclusion that it was highly nutritious and wholesome to man and the higher animals, and rank poisonous to the lower orders of creation, such as insects, worms, reptiles and the articulata, including the crustacea to which the crawfish belong. The crawfish have done more damage to the agriculture of Louisiana, by burrowing through the levees and causing them to leak and break, than all other causes besides. The planters now are beginning to use the sugar cane against them, and have driven them entirely away from many miles of levee, by putting *bagasse* or crushed sugar cane on its base where the crawfish build their houses. Experience proves that they soon abandon their tenements. The entire line of levee could be thus effectually protected, and would be, if the influence of floral dynamics upon those mischievous decapoda was generally known.

My experiments with the sugar cane, and various monographs on the subject, were published several years ago in the Medical Journals, where they sleep, and have not yet been brought into the field of practical utility, except in a few limited instances, awaiting a stronger hand than mine to bring them into that field.

The crawfish are the principal agents in causing the levees to break. The breaking of the levees, by causing bars to form below the break, tends to elevate the bed of the river and to make agriculture less secure than if there had been no levees at all, and the sys-

tem of colmates, instead of embankments, had been adopted. This elevation of the bed of the river causes its waters, after passing the termini of the walls or levees inclosing it, to rush over the unleviced banks on the right and left, forming bars below, on the same principle that bars are formed below a crevasse—thus artificially erecting a more serious impediment to the commerce of the Mississippi valley than in former times, before the levee system went into operation.

The injury done to commerce and agriculture by the breaking of the levees, which have been previously riddled by the burrowing of the crawfish, although great, is small in comparison to the injury done to health. And this brings me to the main part of my subject: the influence of floral, hydro and terrene dynamics

#### ON HEALTH AND IN THE PRODUCTION OF YELLOW FEVER.

The injury to health from the breaking of a levee and the consequent inundation of a large district of country previously dry, is not confined to one season, but extends through many. It may not be perceived at all during the year of its occurrence. The reason is, that there is a reciprocal balance or an equilibrium in every healthy locality among the plants and animals adapted to that locality—the several species of organized beings therein being dependent on one another for their health, sustenance and existence.

The inundation of any extensive portion of that splendid tableau of organic nature, lower Louisiana, by the breaking of a levee, necessarily destroys some species of plants and animals, disturbs the equilibrium among the various members, and thereby converts a most healthy region into a sickly one. So well balanced was every thing in Louisiana from its first discovery to a comparatively recent period, that all the historians and travelers who visited it, and they were very numerous, were unanimous, through a period of time exceeding an hundred years, in according to it the highest degree of salubrity. Du Pratz's three volumes of history were published more than a century ago—1758. He agrees with La Harpe, the agent of the French Government, who arrived in Louisiana in 1718, remained five years, spent most of the time in New Orleans and its vicinity, kept a regular journal, showing that the air was most salubrious and the inhabitants entirely exempt from the fevers which desolated other parts of North America. Lozières visited Louisiana twice, and wrote two volumes, nearly fifty years after Du Pratz. He confesses

his inability to account for the remarkable healthfulness of the country and the longevity of its inhabitants. Robin, who traveled three years in Louisiana, and who published the result of his travels, (3 vols., Paris, 1807) notices not only the freedom of the inhabitants from acute diseases, but their almost entire exemption from those dreadful *chronic maladies* which so greatly afflict the people of the rest of the world. The Count de Vergennes, in his memoir on Louisiana, adds his testimony in favor of its healthfulness. (See Dr. Dowler's Review of the Old Historians, published in 1852.)

We have only to look into the mimic oceans made of glass, now so fashionable an amusement of the aristocracy of Europe, to understand at a glance why lower Louisiana, after having been regarded for upwards of an hundred years as one of the most healthy regions of the world, should have lost its reputation abroad to so great a degree that the people of the upper and middle portions of the Mississippi valley are tapping the river by railroads, under fears that if they trade with New Orleans they will almost necessarily perish with disease ; yet Dr. Dowler's Necropolis of New Orleans proves that human life was formerly more secure in it than any other city.

One look into these mimic oceans shows Louisiana as it was before the hand of man went to work in executing an imperfect system of internal improvements, thereby disturbing the healthful equilibrium previously existing among the members of organized nature within its boundaries. Another look into these mimic oceans, after some officious interference has disturbed the equilibrium among the marine plants and animals therein, will show the woes unnumbered which the levee builders, State and City authorities, refusing to be guided by the light of science, have brought upon New Orleans and Louisiana.

The warm, moist climate, and the fertility of the soil of the delta, are highly favorable to the generation and existence of an abundant flora and fauna, which do not thrive so well, or are not to be found in higher latitudes and altitudes. The waters are literally covered with aquatic plants, and the animals, though of small size, are exuberantly abundant. When a microscope of high power is brought to aid the eye, a new and beautiful world, filled to overflowing with animal and vegetable life, in its most gorgeous forms, opens to the beholder's vision.

It is an admitted fact, that in most other countries a few square

rods of stagnant water, as that of a mill pond, occasionally scatters pestilence in its neighborhood. There are no living organisms to consume the impurities of stagnant pools in a high latitude on the approach of winter—nothing to cause organic life to bound into being ; hence those unconsumed impurities, though small in amount, are sufficient to contaminate the air a mile or more around a mill pond. The healthy piney woods of the South are apt to become very sickly in the beginning of autumn, in the vicinity of obstructed water courses or stagnant pools, the soil being too barren to impart to the water the fertilizing properties which aquatic plants require or the elements on which the microscopic organisms feed. Diseases called zymotic are the natural consequences of a deficiency of organized vegetable and animal life to consume the impurities ; hence bilious, intermittent and congestive fevers scourge the people in the vicinity of a marsh or a mill pond, or a handful of decaying animal or vegetable matter, while the inhabitants of lower Louisiana, although surrounded with stagnant water, swamps, marshes, vegetable and animal matter in great abundance, in every stage of decomposition, continued in the full enjoyment of vigorous health, and were almost entirely exempt from malarious and zymotic diseases for a period of a hundred years or more.

Dr. McFarlane and Judge Gaiennie both bear witness to the fact, that the citizens of New Orleans, some thirty-five years ago, residing in that portion of the city where Poydras Market now stands, enjoyed excellent health, although living in the immediate vicinity of six whole squares of filth, and a thousand hogs wallowing therein ! But every inch square of that filth, if it be like the filth of the gutters and sewers of the city at the present time, was swarming with microscopic animal and vegetable life sufficient to consume its deleterious elements. The microscope demonstrates that the foulest water, filled with the most abominable filth, speedily becomes pure and sweet, if it contain a sufficient number of infusorial and cryptogamic organisms, which it always does in this city, as Professor Riddell, who made the discovery, has verified by actual demonstration, aided by the microscope.

THE YELLOW FEVER—HOW CAUSED?—Ever since the yellow fever has become such a terrible plague to New Orleans, there has been an incessant war waged against filth, which has proved to be most expensive to the tax-payers and worse than useless as a preventive of

that disease. Almost every measure taken to prevent yellow fever in New Orleans has been dictated by the exploded theories of the speculative philosophy of the last century. The obsolete ideas of contagion and non-contagion have alternately ruled, and both have failed, because neither is true. All diseases are contagious or non-contagious, according to circumstances.

For a great many years past the sediment in the gutters and sewers has been condemned as filth, and its removal strictly enjoined. It is accordingly shoveled out on each side of the streets every day, and left to dry in the sun, when it is carted off. If the microscope had been consulted, it would have proved that every shovelful of the alleged filth contains more living beings, in the form of infusoria, than there are inhabitants on the globe, abundantly supplied with microscopic flora, to afford them sustenance. These living infusoria and living cryptogamia are not filth, because when put in the foulest water they speedily purify it ; but the dead infusoria and dead cryptogamia, killed by exposure to the sun on the side walk, are filth, and cannot fail to load the air with foul exhalations from their decomposition under the influence of heat and moisture. It has been alleged that the sediment would fill up the gutters and render them useless as drains, unless shoveled out as fast as it accumulates. Hydrodynamics is full of powers—stronger than the spade—to remove it, if its laws were studied and put in force. It would remove the sediment out of the gutters and sewers, and would not destroy the microscopic flora by exposing it to the sun in the streets, with the countless myriads of organized animals which feed upon it, to die, putrefy and contaminate the air, but would move it rapidly through the gutters and sewers by throwing into them a sufficient quantity of Mississippi river water, the purest water in the world. This can readily be done, as the surface of the river is at all times nearly as high, and about half the year higher than the surface of the soil, and there is in the rear of the city sufficient hydraulic power to pump it into the canals leading into Lake Pontchartrain.

It is not to be expected that the world will immediately receive all the truths revealed by the microscope, and act upon them. An authority older than the microscope declared 3350 years ago, the existence of a tree, "which when cast into the bitter waters of Marah, the waters were made sweet." Moses does not say that the tree grows every where, or that in and of itself it sweetened the waters. He

simply announces, as a truth, the existence in nature of such a tree growing at Marah. No one seems to have believed him literally, or to have taken the trouble to look abroad into nature to find it ; yet, virtually, such a tree, though branchless and rootless, but not leafless, has been discovered by Professor J. L. Riddell, as really existing in nature, and growing abundantly in New Orleans. Professor Riddell does not say that the tree which he discovered, and named *oscillaria*, grows in all putrescent places, or that in and of itself it sweetens and purifies the foul and bitter waters of the sewers and gutters ; but he does say that it grows in New Orleans, and the waters are made sweet and pure, in a short time, in all instances where they come in contact with the *oscillaria* and the animals feeding on it. In the hot season of the year very dense forests of the microscopic tree, called *oscillaria*, are found growing in the sewers and gutters of New Orleans. Numberless animals of different species, herbivorous and carnivorous, are seen roaming through those forests. So close together do the trees stand, that the larger animals have difficulty in penetrating the forests. The trees themselves, instead of being dry and dead, actually seem to possess exquisite sensibility, exceeding that of the sensitive plant ; and, what is still more wonderful, they manifest some degree of *intelligence* ! Thus, they quiver and wince at the approach and touch of the herbivorous animals which feed upon them, while they manifest little or no sensitiveness at all towards the fiercer carnivora inhabiting the forests, the latter often climbing up the trees to avoid their foes, or simply for the pleasure of taking a ride in the tree tops, which are constantly waving from side to side.

The *Oscillaria*, which is a genus pertaining to the natural order Algæ, largely predominates in the microscopic forests of the gutters and sewers of New Orleans ; but it would not be safe to say that that alone sweetens and purifies the waters, to the exclusion of other vegetable growth and the microscopic faunæ. The latter may have inhabited the trees at Marah, and have been the principal agents in sweetening its bitter waters. Nor is it material to know whether the virtue to purify water resides in the *oscillaria* itself, in other flora associated with it, or the animals feeding upon it ; in either case its forests should be spared. Those who do not believe in Moses or the microscope have had time to learn in the dear school of experience that the destruction of the forests of *oscillariæ*, with all the animals



contained therein, under the filth ordinance, cannot prevent, but *may have caused the yellow fever*.\*

The influence of floral, hydro and terrene dynamics upon the health of man, beast, insect and vegetation, was typified by the plagues of Egypt. What is a crevasse but a smiting of the waters and turning them to blood, as far as many species, families and tribes of plants and animals are concerned? Any physical cause which greatly disturbs the equilibrium among plants and animals of any locality may breed a plague of some kind or other. Geologists have discovered, in the fresh water portion of Louisiana, many flora and testacea common to salt water. They flourish there from the saline particles in the earth. A crevasse, by pouring upon them for months in succession a large body of fresh water, causes great numbers of them to become sickly or to perish. The rich and fresh deposit left by the waters of the crevasse calls into being myriads of purely fresh water worms, insects, and infusoriæ and cryptogamic organisms, doomed to inevitable destruction as soon as terrene dynamics imparts to the earth its former saline properties—causing plagues from their putrefaction, until the healthful equilibrium among the plants and animals, natural to the locality, can be restored.

Since a succession of crevasses have occurred in the vicinity of New Orleans, and filth and trash have been used to fill up swamp lots, thereby obstructing the natural current of the rain water, the yellow fever has put on wings. To clip them and drive pestilence out of the land, it will be necessary to ascertain what works are required to give a good drainage, and to secure a permanent equilibrium in organized life in such a locality as New Orleans and its vicinity, between fresh water and salt, and on a plateau of high land and swamp. The Sauv  Crevasse, by converting the brackish water of the Lakes Pontchartrain and Borgne into fresh, and those of the Mississippi Sound into brackish, sickened the fish and oysters, and made them very unwholesome articles of food—causing cholera and yellow fever in many of those who ate them. A fish dinner, in summer, was apt to result in a severe attack of yellow fever, and an oyster supper in the fall and winter in cholera.

Disease requires a nidus for its incubation in the animal economy.

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\* All the facts stated in regard to the microscopic observations in this paper have been submitted to Professor Riddell, and have been corrected by him, and approved as according entirely with his observations.

Unwholesome food and drinks taken into the stomach, and the respiration of a vitiated atmosphere, throw into the economy the materials out of which it constructs its nidus, the zymotic germs of some diseases or other, will seldom be wanting to supply the nest when once formed. The yellow fever in New Orleans is generated, with but few exceptions, in the following manner: Unacclimated persons, just from a long sea voyage, mostly from Ireland and Germany, with scorbutic blood, loosening their teeth and corroding their gums, and with the contagion of European typhus or ship-fever adhering to their garments, crowd around the ships and steamboats entering the port, to hire themselves to load and unload the vessels, and to perform all manner of drudgery work on the levee, and in other parts of the city. They do so because they can get higher wages for work done in the hot sun on the levee, or in the filthy holds of ships, than for any other kind of labor, where they would not be exposed to the sun or to a putrescent atmosphere. It is quite immaterial whether the ships come from the West Indies, Rio Janeiro, France, Germany or Great Britain, whether the steamboats are from Galveston in Texas, or Pittsburgh in Pennsylvania; or whether the vessels be the common flat-boats from the upper country, laden with corn or other western produce in a damaged condition; in either case the wages of labor is *death* to the unacclimated. Because such labor, performed by such unacclimated individuals, generates the yellow fever in their systems. The first cases in 1853 were generated by immigrants on board of ships from Europe, which had not touched at any infected port. The zymotic germs, when once generated, are not slow to seek a nidus in other unacclimated persons, which is found in the families and associates of those laborers who have been employed on the levee and the shipping. To save rents, this class of people huddle together in dilapidated tenements, mostly sleeping on the floor, which they literally cover at night. In some seasons, as in that of 1857, the disease is entirely confined to this class of persons in some particular district. That year it killed ninety-nine of them, and did not extend to other classes, or go beyond a small district in the neighborhood of Tchoupitoulas Market. Such has been its character at most of its visitations—a local malady of a very limited duration—confined chiefly to some particular district of the city, and to particular classes of individuals. But in 1853, 1855, and 1858, its germs seem to have put on wings, because it flew in every direction, sparing no other class of indivi-

duals than the well acclimated. What gave its zymotic germs wings? Was it a pestilential state of the atmosphere in the city itself by the dead animals and filth carted out on the back lots, obstructing the exit of the rain water to the swamp? or a more general vitiation of the air caused by the numerous crevasses, which have occurred of late years, producing a loss of balance among the members of organized life in the country on the lower stem of the Mississippi river? Was it the wanton destruction of the Oseillaria, and the death and putrefaction of the immense and inconceivable number of animals inhabiting its aquatic microscopic forests? Was it some terrene poison forced into the atmosphere by terrene dynamics, or brought up by the electricity of the earth by deeply wounding the earth with mattock and spade in the hot season of the year? Or was it owing to the engrafting of the communicable jail or ship-fever of Europe upon the yellow typhus of America by the foreign immigrants? The solution of the above questions is fortunately not of paramount importance, because the yellow fever, in its fearful spreading form, can be, with as much certainty, prevented as small pox, *and by means as simple and as cheap.*

PREVENTION OF YELLOW FEVER.—Its own laws suggest the preventive measures. Like small pox, its own laws forbid it from attacking the same individual twice. The exceptions are so few as to form the general rule. By the operation of its own laws it spares the negro population, or if it attacks them at all, it handles them so gently as seldom to kill. It can be prevented from spreading with as much certainty as small pox by *insulating* it, that is, by surrounding it with unsusceptible persons. The materials for insulating the diseases are most abundant in New Orleans. More than half the white population, and nearly all the negroes have free papers already made out, exempting them from its attacks. They can go where they please and do as they please, into the holds of the foulest ships, handle goods saturated with black vomit, or sleep in the same room or bed with yellow fever patients, without incurring any risk at all, simply because they are not susceptible to it, being exempted by the laws of the disease itself. All that is required to prevent the disease from being introduced into New Orleans by the shipping, or generated on ship-board after their arrival, would be simply *to insulate the shipping and the levee in front of the city, during the hot season of the year, with acclimated persons, and to let none others act as stevedores, or work on the levee, but those who are thoroughly acclimated,*

under pain of being sent to the work-house or penitentiary until cold weather. It would be humanity to the unacclimated foreigners, seeking employment on the ships, boats, and the levee, in the hot season of the year, to send them to prison, because experiences proves that incarceration is sure to save their lives, while labor in the hot sun on the levee is equally sure to kill them.

The method of insulation to prevent yellow fever, requires no previous preparation, involves no expenditure of money, and throws no obstructions in the way of commerce. It does not require expenditures for hospitals. Every ship and steamboat could provide for its own sick, and keep them on board until they recovered or died. With a physician on board, the disease would be attacked in its forming stage and *aborted*, instead of remaining in its nidus until it is fully developed, when it is very apt to kill, even before the unfortunate patients can be removed to hospitals or elsewhere. The plan of insulation might at least be adopted until the city is better drained.

**DRAINAGE.**—There is not a planter who could grow a crop of cotton or sugar on the ground New Orleans stands upon, with its present drainage, if the houses were all removed. There is not a planter on the Mississippi river who has got a better plateau of ground for planting purposes than that on which the city of New Orleans stands. On a few rare occasions the city next to the swamp has been overflowed by the breaking of the levees, or by some violent and long-continued storm, driving the water of Lake Pontchartrain upon it in the rear. But, with these exceptions, it is not affected by either the river, the swamp or the Lake water. Yet the largest half of the built up portion of the city, is overflowed from half a leg to waist deep, at least twenty times a year by the rain water and the river water alone. Four-fifths of Canal street, running at a right angle from the levee, in the very heart of the city, where the river makes its beautiful Crescent bend, is often overflowed for hours together sufficiently deep to float a flat-boat. There is plenty of fall, but the drainage is not sufficient to carry the water off fast enough. The plateau, through which the streets leading from the river to the swamp, pass is not a dead level but an inclined plain. The first quarter of a mile has a fall of seven feet, beyond which the fall is from two to three feet per mile to the swamp, or what was formerly a deep morass before the water was pumped out by draining machines. The draining of that fen does not drain the city in any reasonable time ; because

the capacity of the ditches leading to it is not sufficient to discharge the water. If such a plateau were on the top of the Alleghany Mountains it would be inundated just as New Orleans is by very hard rain. The city is inundated by the rains oftener and deeper now than it was before the fen behind it was drained; because the back lots next to the morass have, in many instances, been filled up, while the capacity of the ditches, carrying the water through the artificially elevated ground, is no greater now than anterior to the draining of the morass. Instead of intersecting the land next to the fen with deeper and wider ditches, vast expenditures have been incurred to elevate it with batture mud and city filth. So little attention has been paid to hydrodynamics, that it was supposed that the fen behind the city had only to be drained to make the city dry. That error still exists although the city is not drained after the draining machines have been in operation nearly twenty years. About seventeen years ago G. T. Dunbar, Engineer of the State of Louisiana, in his Report to the New Orleans Draining Company, mathematically demonstrated that "*the true cause of the inundation of Canal street after the every hard rain, and all that part of the city above the Basin west of Bourbon street, was in the small size of the ditches.*" Although the experience of many years has proved that draining the deep swamp in the rear of the city, is not the same thing as draining the city itself, as was originally supposed, but rather tends by keeping the hydraulic police asleep, to convert New Orleans into a rain water lake, it is now proposed to spend the eighty thousand dollars appropriated by the Legislature last year, not in giving greater capacity to the ditches, which Mr. Dunbar demonstrated to be *too small*, but in reclaiming a larger body of swamp land. Unless those ditches be enlarged the city will continue to be inundated by every hard rain for seventeen or any number of years to come, even if all the swamps in the State were made dry.

The first thing a planter would do, if the plateau on which New Orleans stands was cleared of the houses, in order to convert it into a sugar plantation, as the upper portion once was, would be to open the old ditches the old planters had, which have since been stopped up or dwindled down to petty gutters. One of these, of the dimension of a canal, run from the levee through the street where Poydras Market now stands, effectually draining the land on which a very populous part of the city stands, but which is now so deeply

overflowed by the rain water as to make it utterly unfit for farming or planting, if the houses were removed. The next thing the planter would do, would be to enlarge the capacity of the ditches, alluded to by Mr. Dunbar, which being done, he would have as dry a plantation as any other on the lower Mississippi. This he could do without the aid of any draining machines at all. With them he could make the entire swamp dry between the rear of the city and Lake Pontchartrain.

PLANTATION DRAINAGE.—Mr. Egana, twenty-seven miles below New Orleans, has, by the aid of draining machines, converted a considerable quantity of *shaking prairie* into arable land. Last year he grew as good corn upon it as grows on the prairies of Illinois. He is preparing this year to reclaim and put in cultivation an additional tract of prairie, measuring about a thousand acres. A great many planters are reclaiming their back or swamp lands with draining machines. While every planter, occupying a higher land than that on which New Orleans stands, contrives to keep it dry, and while a great many are reclaiming deeper swamps than any between the river and Lake Pontchartrain, the people of New Orleans, though heavily taxed to keep a number of draining machines in operation, are wading in water after every heavy rain. They never could have been mystified so long on the subject of drainage, if the *influence of floral, hydro and terrene dynamics* had received the attention it merits.

CAUSE OF ERRORS ON THE SUBJECT OF THE DRAINAGE OF NEW ORLEANS. As already said, New Orleans is built upon six cypress forests, one above the other, in layers—the stumps of each layer still existing in many places. The shaking of the houses, when heavy vehicles pass along the streets, and the rising up of water into any excavation of the soil only a foot or two deep, have led to the idea that the swamp in the *rear* of the city was the cause, and to the erroneous belief that if that could be drained thoroughly, the city would be dry. But the water in the swamp is not the cause of it. The same power which sunk the cypress forests brings the water to the surface. That power is *terrene dynamics*. The weight of the harder or upper crust of earth, particularly with a heavy growth of cypress on it, forces the softer and more watery part of the sedimentary deposits to rise upwards, thereby causing the upper crust to settle or go down. The slimy matter, thus forced to the surface, dries, and in its turn becomes an upper crust, pressing upon and forcing up the slimy, soft mud be-

neath—thus carrying down one forest and elevating a sufficient quantity of the soft mud to form the basis of another forest above the sunken one. The same law of terrene dynamics is still in operation. It forces the soft mud up through the cracks in the paving stones, where it dries and becomes dust, to be reconverted into mud again when it rains, and is washed into and fills up the gutters. It gives the city authorities much trouble in having it seraped out of the gutters and carried off.

HOW TO PREVENT THE MUD AND DUST OF THE STREETS.—Mud and dust could be prevented by a thick coat of cement under the pavement. There would be little or no dust in the city if it were not for the mud oozing up through the pavement. A coat of cement under the pavement would save the greater part of the expense of seraping and drayage, which is enormous.

THE PRAIRIE TREMBLANTE DUE TO THE SAME CAUSE.—Soft mud, like water, cannot be forced through cement ; nor can the pressure of the earth force it through an argillaceous soil. Under such soils, in the delta, it seeks a vent laterally in the looser or more porous geological formations, and forms in those places the *prairie tremblante*. The latter appears at first in the form of a shallow lake. On the top of the water a sort of moss floats. This moss I take to be the *Sphagnum Palustro* of Linnæus—the basis of peat beds. It belongs, no doubt, to the same natural family of plants which our long moss or *Tillandsia usnoides* does. Like the long moss of the trees, *it dies*, but does not decay. It dies when its seeds mature. On its own body its offspring grows. Each generator forms an additional layer, and in process of time a sufficient basis is formed for other aquatic plants to grow upon. These aquatic plants give the moss bed the appearance of roots, stalks, leaves, mould, bark, etc. When the pressure of the earth heaves up the soft mud to a level of the lake, it is what is called the soft *shaking prairie*. After it gets above the level of the water it is still a shaking prairie, and in dry weather its surface is sufficiently firm to bear cattle. In wet weather the cattle sink into it, but do not bog, as in quicksands. The deeper they sink, the firmer the mud becomes. In process of time it gets firm enough to bear a growth of cypress trees ; and the forest gradually sinks from its own weight, pressing up the soft mud beneath it. The ground on which New Orleans stands was formerly nothing but a *shaking prairie*, nor has it yet lost its trembling quality, because the lighter and softer part of

the soil is near the surface, under the upper crust. The deeper down, the firmer does the soil become, as is proved by the boring to try to get an artesian well on Canal street.

ARTESIAN WELLS—HOW FORMED.—The terrene dynamics of Louisiana will throw much light on the subject of artesian wells. It is commonly supposed that an artesian well is formed by tapping a body of water occupying a higher level than the point where the tapping auger enters the earth. When a rock or stratum of clay is penetrated by the auger, the pressure of the earth forces the water up through the tube ever so far from below the surface of the soil where the boring is made ; provided there be no other way for the subterraneous waters to obey the terrene dynamic forces acting upon them, than through the aperture made by the auger. But if there be other avenues for the waters to obey the force that terrene dynamics exert upon it, it will not spout out of the tube, or even rise in it ; and this must always be the case where the soil pierced by the auger is of a light, porous nature. No artesian well can be got in a porous soil.

A *prairie tremblante* is soft mud forced up by terrene dynamics, on the same principle that a *jet d'eau* is obtained by piercing a stratum of clay which has water beneath it, feeling the pressure of the earth upon it, but no other way open to obey that pressure than by rising through the tube ; so, also, the soft mud, feeling the pressure of the superincumbent earth, obeys that pressure by rising up in the nearest lake next to the heavy earth pressing upon it.

The influence of the floral, hydro and terrene dynamics of Louisiana, is a subject opening a vast field for original research. In that field is seen an intimate relationship between such dissimilar things as health, agriculture and commerce, and a still closer relationship of the *artesian well* to the *prairie tremblante*, both of which are formed on the same principle that causes the soft mud to rise up through the paving stones of the streets of New Orleans, to be metamorphosed into dust and again transformed by the rains into the substance called filth, obstructing the gutters and sewers of the city. In that field, we see some of the influences exercised by floral, hydro and terrene dynamics, in the wild or unbridled state. But, beyond that we have imperfect glimpses of another field, when a bridle is put upon those strong dynamic powers, (as has long been done with steam,) and the whole three harnessed, and made to do good service to the State.



**COLMATES.**—By means of colmates the Mississippi river could readily be made to send its turbid water to the particular places where its sediment is most needed to elevate the swamp, or to enrich the land which cultivation has exhausted. Colmates are artificial aqueducts, with contrivances to put water under the will of man, for the purpose of bearing on its surface vessels laden with agricultural products to any spot, or for transporting the water itself to any spot where its sediment is needed—to throw it on or to shut it off at will. Any particular field, which has been under cultivation until the soil is exhausted, can readily be enriched by a colmate throwing upon it the turbid water of the river, after the crop is off;—nothing more being necessary, besides the colmates, than to confine the water on the field by some plow furrows running around it to form a ridge or embankment to confine the water on the field until it settles; when the clear water could be let off into the ditches, and a fresh supply of turbid water thrown upon it by raising the lock of the colmate. But the great advantage of the colmate is the bridle it puts upon those tremendous forces, floral, hydro and terrene dynamics, bringing them up in harness to work together for the benefit of the health, agriculture and commerce, of the city and State. M. Thomassy, the Hydraulic Engineer, has come here to make a bridle for them. The swamp, in the rear of the city, could have been filled up and converted into high land with deposits of the river water, let in upon it by means of colmates, passing through the streets. By means of a large colmate or two, only from five to ten miles in length, the city could be put in possession of the immense benefits to its commerce which would result from an inland water communication with the deep and secure harbor of Ship Island.

**MALAY LOUISIANA.**—By means of other colmates, without the expense of locks, an immense shaking prairie, larger than the entire State of Delaware, beginning some twenty miles below New Orleans, on the same side of the river, extending to the Balize and spreading out from the river Eastward to Chandelier Bay, Lake Borgne, and the Mississippi Sound, could be reclaimed. For a distance of eighty miles, the Mississippi river runs within a few hundred yards of that immense prairie, passing by to deposit its sediment in the Gulf of Mexico, instead of on that prairie where it is so much needed. The prairie has already been intersected with numerous natural canals, from twenty to thirty feet deep, and about one hundred feet wide, with natural

embankments on their margins, which have been constructed, like the prairie itself, by the wild and unbridled forces of floral, hydro and terrene dynamics. Nothing more is required to reclaim that immense prairie than a few colmates to give it the benefit of the sediment in the turbid stream that rolls by it. It is covered with luxuriant grasses, which cattle, horses, mules and sheep, are very fond of. The embankments on its canals are a little above tide water, but the main body of the plateau is overflowed daily by the tides. I have ventured to name that extensive and almost unknown region of the State, *Malay Louisiana*, from its only inhabitants being Malays, except a few white fishermen. The original settlers were runaway Coolies from Jamaica and the West Indies. The Malays reside on the banks of the natural canals intersecting the prairie in every direction. Their houses are made of Palmetto leaves, spread out in fan form, and fastened together with strings to slender posts sunk in the ground. Some of the houses have a number of rooms. The natural canals abound in fish, and the bays and inlets with extensive oyster beds. A few clumps of trees are to be seen on some small spots of the plateau.

About thirty-six miles from New Orleans, there is a shell mound on the prairie, rising upwards of twenty feet high, and covering a surface of some eighty or one hundred acres, very heavily timbered with live oak trees. On visiting this almost inaccessible mound, I found Indian pottery ware in the soil, underneath the roots of some large live oak trees, which had been blown down by the storms.

Floral, hydro and terrene dynamics, are three powers more active in Louisiana than in any other known country, and have converted it into a richer field for the geologist, the chronologist, botanist, zoölogist and microscopist, than any other portion of the globe.

If what I have said shall have the effect of awakening some higher degree of ambition in my younger professional brethren of the South to rise above the monotonous level of European routine, recipe doctors, by studying the natural history of their own country, its diseases, and the topography of the particular localities where they reside, I shall be content.

ART. III.—*Surgical Memoranda* (continued from March number of this Journal, 1858, page 183): By J. J. McELRATH, M. D., of Camden, Arkansas.

AMPUTATIONS for *Progressing Gangrene—Recovery.*

DURING the last century the celebrated *Samuel Sharp*, and after him *Percival Pott*, insisted earnestly that in removing mortified limbs by amputation, we should always wait until the organism had arrested the progress of destruction, and formed the line of demarcation between the living and dead tissues; otherwise, as they contend, we run the risk, after inflicting upon the patient a painful operation, of seeing the gangrene invade the stump. As a general principle, this view of the subject is doubtless correct. The recorded experience of modern surgeons, however, clearly shows that it is a principle which admits of very numerous exceptions. In what has been denominated constitutional gangrene, and perhaps also in gangrene arising from "frost bite" and from burns, the principle is of pretty general applicability. In traumatic gangrene, however, the exceptions are sufficiently numerous, I infer, to reverse the principle—amputation becoming the rule, while to wait for the arrest of destruction, and the formation of the line of separation, before proceeding to amputate, is the exception.

In no case, however, is the cause or the extent of the injury alone a sufficient justification for our adherence to any particular rule or course of action. Circumstances and conditions may, and do occasionally, present themselves, which are of such a nature as to override the rules generally applicable in the treatment of a particular or special class of injuries. The following case of double amputation for "frost bite" will better exemplify the meaning intended to be expressed in this last sentence.

(5). *Double Amputation for Progressing Gangrene from Congelation. Recovery.*

Rachel, aged twenty years, a slave, the property of Miss Martha Bullock, of this city, was exposed in a "snow storm" during the day and night of the 18th of January, 1857, with the temperature ranging from 20° to 25° Fahrenheit, below the freezing point. On the morning of the 19th, finding her way to a "burning log heap" in an open field, she gave her feet—in fact, her whole body—a "proper roasting." When found, some hours afterwards, she was almost helpless.

Dr. R. M. Green had charge of the patient during the succeeding ten days. Consecutive reaction was feeble, or, perhaps, entirely wanting; the cuticle was detached on many parts of the body, particularly on the right side; the feet and legs, to, some distance above the ankles were apparently completely dead and cold.

On the twelfth day from the receipt of the injury, the mortified parts having become very offensive, the destruction being on the increase, and the vital powers evidently "giving way," it was resolved to remove the limbs by amputation. The points selected for the division were about two inches below the knee joints. The gangrene seeming to extend beyond the middle of the legs, and these, including the thighs, being much swollen, nothing better could be proposed. Stimulants and quinine were without avail—blisters and incisions not having been resorted to.

The patient being properly placed and fully etherized, the operation was performed by means of the circular incision, in quick succession, a few minutes only being required for the removal of both limbs.

The shock sustained by the system from the operation was both transient and trifling. Stimulants, combined with a nutritious diet, soon brought about healthy reaction, and the patient made a slow but good recovery. Sloughing of the flaps occurred to some extent—not sufficient, however, to prevent the formation of useful stumps, and a complete covering to the bones. The ligatures came away from the fifteenth to the fortieth days.

The very extensive serous infiltration of the subcutaneous and intermuscular cellular tissue, which occupied the entire limbs as high as the hips, was rapidly discharged—large quantities being poured out through the first incisions, and continued to drain away for many hours after the limbs were removed.

Without presuming to speculate upon the probable termination of this case under a different course of treatment, it is sufficient for our purpose to know that the daily augmenting constitutional irritation, together with the slow but constantly advancing gangrene, was promptly arrested, and a far more favorable condition of things produced by the removal of the offending members. Amputation being sooner or later inevitable, I could see nothing but cruelty in the practice of making further attempts, by constitutional means alone, to arrest the progress of destruction. Had it even been possible to effect this, days, and perhaps weeks, of suffering must have elapsed

before it could have been accomplished. We must claim, therefore, that the end accomplished justified the means adopted.

(6). *Amputation for Progressing Traumatic Gangrene—Recovery.*

Mr. Barnes, aged thirty-two-years, of sanguine temperament, and of robust constitution, while assisting in the management of a small steamboat on the Ouachita river, fifty miles above this place, had his leg caught in a loop of the boat-line, and his foot torn off at the ankle joint. The muscles of the leg retaining their tendinous attachments to the severed foot, were divided at various points from the knee downwards, and drawn out from beneath the skin.

On the 24th of March, 1858—seventy-two hours after the receipt of this severe injury, the patient was brought to this city for surgical treatment. The injured limb was completely gangrened to the knee joint—his general condition being that of extreme prostration.

After allowing liberal portions of stimulants, in the shape of brandy, etherization was effected, and the limb removed, by means of the circular incision, at the middle of the thigh. Reäction was prompt and complete, and the most favorable result was anticipated. At the expiration of about twenty-four hours, however, a small dark gangrenous spot, the size of a half dime, made its appearance on the edge of the inner aspect of the flap. In despite of the free administration of wine and sulph. quinine, and the application of the nitric acid lotion, the gangrene spread with great rapidity—embracing, within less than three days, one half the circumference of the stump, and extended upwards more than five inches; the stump, at the same time, being swollen to thrice its natural size. It was somewhat remarkable, however, that with this formidable condition of things, there was but little constitutional disturbance.

Deep and extensive incisions were now carried through the diseased parts—a vesicating plaster made to embrace the entire stump as high as the hips, and *the liquor sodæ chlorinatæ*, was directed to be freely and constantly applied. This treatment, with a liberal allowance of wine and sulph. quinia, very soon arrested the mortification. The line of separation formed slowly, but the sloughing process was both tedious and frightful, nearly two months elapsing before the stump assumed a healthy condition. Yet the patient recovered completely at the end of three months, with a tolerable stump, and without permanent exposure of the bone.

There was, in this case, a peculiarity which I have occasionally noticed in operations performed upon patients under the influence of chloroform; that is, the arterial blood presented, in appearance, the character of venous blood. My friend, Dr. A. W. Holson, who assisted in the operation, and who ligated the vessels, observed that there was no artery; or, at least, there was no arterial blood, and that he had ligated nothing but veins.

Whether this appearance of the blood resulted from some influence produced, either directly or indirectly, upon the proper function of the organ of respiration, by the chloroform; or whether, from some other cause or causes, can, for the present, only be matter of conjecture.

(7). *Lithotomy—Successful.*

This patient was a negro boy, five years of age, belonging to Mr. William Wyatt, formerly of this place, but now a resident of Magnolia, Columbia county, Arkansas. The operation was performed on the morning of the 8th December last (1858). The calculus, which proved to be *lithate of ammonia*, weighed four hundred and eleven grains, troy.

My friends, Drs. Pace and Clark, who assisted in the operation, informed me that the patient made a speedy and good recovery.

Having performed this operation but four times, and all by the same (lateral) method, it would be deemed the height of presumption in me, to speak in favor, or against any particular plan of incising the bladder. Yet, notwithstanding the cases recovered well, there was, nevertheless, a far greater amount of mental satisfaction experienced, and self-confidence inspired when operating with a simple instrument (Liston's lithotomy knife). Complicated instruments, it does seem to me, are only safe, useful and satisfactory, in the hands of men of large experience. Frontier surgeons, who, like myself, have had but little experience in the use of instruments, will, I think, perform the operation of lithotomy with this simple instrument, or some other like contrivance, with a far greater degree of conscientious repose to themselves, if not with a greater degree of safety to the patient, than with the gorget, or any other instrument now in use; presuming always, that no man will perform this operation who is not perfectly certain of his anatomy.

(8). *Double Traumatic Cataract.—Operation Successful.*

William McCrone, aged 18 years, of strumous diathesis (sanguine

variety), while riding "through the woods," received a blow upon the ball of the left eye from a small switch or twig, which gave some pain for a few days, and produced a slight degree of inflammation in the conjunctiva, and perhaps, also, in the deeper-seated structures. All symptoms and appearances of injury, however, passed away at the end of a week or ten days. Some months afterwards the patient observed a want of clear vision in this eye. As this dimness increased, the other eye began to fail in the same way. At the expiration of a year and a half he was almost blind. When I examined him first (February, 1857), two years after the receipt of the injury—which he supposed to be the cause of the loss of vision—he could very well distinguish day from night, in fact could distinguish the outlines of any bright object placed before him. Dilatation of the pupil produced a very perceptible improvement for the time in his powers of vision. The catoptric test showed two erect images, the deeper one very obscure.

After a short preparatory course of constitutional treatment, the left eye was subjected to an operation. The cataract proved to be lenticular, and completely fluid. After breaking up the capsule pretty thoroughly, the lens was left to be absorbed *in situ*. A considerable portion of it very soon floated forward, and settled down to the bottom of the anterior chamber. The patient being enjoined quietness, and a dark room, was directed to take, every night, a pill containing three grs. of the proto-iodide of mercury.

At the end of twenty days, there was no evidence of improvement, when the needle was again introduced, and the cataract subjected to the same breaking up process as before. This operation was repeated four several times, at like intervals, and still no improvement; and what was most remarkable—not a speck of inflammation was induced by any one of the operations—nor was the constitution affected in the least by the medicine.

Being thus completely discouraged, I resolved to try the right eye. Here, the same condition of this existed, as had been found in the left eye. Nor was I in this more successful. After breaking up the cataract in this eye, five consecutive times, at intervals of twelve days, I discovered that the absorbents were acting with rapidity upon the cataract of the left eye, and very soon (within ten days) a very decided improvement was perceptible in both eyes. At the end of thirty or forty days from the last operation, sight was completely restored.

This young gentleman, who is living twelve miles from this town, visits my office occasionally; can read large print very well; kill squirrels with a shot gun; in fact, can see sufficiently well to attend to all the duties which appertain to his station.

I frequently operate for cataract, but have never witnessed a case similar to this, nor have I read of any such case in books. Why was there no inflammation excited by the repeated introduction of the needle? And what, finally, caused the absorbents to remove the cataract?

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ART. IV. *Fracture of the Skull*: By J. M. OWENS, M. D.

SAMUEL S—, æt. seven years and six months, of nervous temperament, was thrown from his horse on the 28th of January last, (1858) his head coming in contact with a stump, a sharp corner of which inflicted a wound about two inches in length, a little above the right temple, at the junction of the coronal with the squamous suture.

The patient was brought to town, and I was called to see him in half an hour after the accident occurred. Found patient looking very pale, seeming as though he was almost exsanguinous; pulse about 60 per minute, and very feeble; clothes completely saturated with blood, and still bleeding profusely, some branches of the frontal vein and of the temporal artery having been divided; no symptoms present that would indicate compression or concussion of the brain, and indeed no untoward symptoms save those occasioned from excessive loss of blood.

The hæmorrhage in this case proved a very valuable remedy; for, as it afterwards turned out, there must have been compression to a very considerable extent, which being relieved by the hæmorrhage, did not present the symptoms common to compression; integuments extensively swollen and very tender; hair completely matted.

Owing to the swollen condition of the scalp and irritable temperament of the patient, I was unable to make a minute examination of the wound, and necessarily had to content myself with cleansing the wound and arresting hæmorrhage, which was very easily accomplished by the application of compress and bandage. Gave brandy



and ammonia to counteract the tendency to syncope from loss of blood.

On the morning of the 29th, found patient up, walking about the room, seeming as if nothing had happened.

I deemed it prudent at my second visit to attempt an examination, to discover, if possible, whether or not there was fracture of the skull. On removing compress and bandage the blood began again to flow profusely, rendering the patient perfectly unmanageable, so I was again necessitated to abandon the idea of an examination, and resorted to the compress and bandage, to arrest hæmorrhage. Ordered compress and bandage to remain twenty-four hours; the patient to be kept perfectly quiet.

30th.—Removed compress and bandage; no hæmorrhage, scalp still extensively swollen and seemingly very painful to the touch. The patient continuing so obstinate, I did not make a third attempt to examine the skull, for I had concluded from attending to the symptoms that there was no fracture with this compression; I cut off the hair, cleansed and dressed the wound, ordered the wound to be washed and dressed once per day with simple cerate.

I did not see the patient again until the 5th of February, when Mr. S. came to say that in dressing the wound he had removed a piece of bone. I called immediately to see little boy Sammy, and judge of my astonishment, when, on removing the dressing, I discovered protruding through a fracture in the skull not less than an ounce of brain—a veritable *fungus cerebri*, an accidental encephalocele. I was seriously alarmed, for I had been in the practice but a short time, and although I had read of cases similar to this, I had never seen, much less been called to treat, a case of the kind, and I naturally supposed, as I imagine the majority of young practitioners would, that it was all up with my little patient, unless a superhuman power interfered in his behalf.

The patient had by this time become very familiar, and being intelligent beyond his years, remarked, “Doctor, you can do as you please with my head, and I’ll let you.”

I removed a portion of the protruded cerebral mass, and on examination found an opening in the skull sufficiently large to admit the ends of two fingers—not from fracture altogether, but from a separation of the bones at the coronal and squamous sutures.

The parents were very much alarmed and exceedingly solicitous to

know my opinion of the case ; but having failed to discover the fracture when first called to treat the case, and having had but little experience in practice, and entertaining some doubt as to the probable result of the case, I was very cautious in giving a prognosis, and requested a consultation. My brother (Dr. J. A. Owens, of Monticello, Arkansas) was called in, and after examining the case, gave it as his opinion that the best plan of treatment would be to watch the case closely, and combat the symptoms as they presented ; the antiphlogistic treatment to be strictly pursued in the event that inflammation supervened. I cut off the hair, washed and dressed the wound with simple cerate, applied compress and bandage moderately tight, so as to produce supuration of the protruded brain ; ordered saline aperients, to keep the bowels in a soluble state, the patient to be kept perfectly quiet in a recumbent position.

I visited the patient, examined and dressed the wound once per day for two weeks, (the dressings consisting mainly of simple cerate) during which time the case progressed slowly towards a favorable termination, seeming, as appeared to my inexperienced eye, to suppurate too freely. The granulations presenting an unhealthy appearance. I touched the granulations with caustic, and washed with a weak solution of chloride of soda.

Every thing seemed to go on finely under the treatment, except the wound would not entirely heal. At one visit I would find the wound almost closed ; at the next visit, in two or three days, there would appear a large granulation in the centre of the cicatrix. I removed the granulation with caustic and with the knife, time and again, and still it came. Examined with probe frequently ; could discover no loose fragments of bone, and I supposed that an irritation was being kept up by a rough surface at the seat of fracture, which I supposed would be remedied in time by the deposition of lymph, and the formation of new bone.

The patient started to school, called at my office three or four times per week, all my endeavors to cause the wound to heal proving abortive.

Thus the case continued up to the 1st of June, when I thought proper to make an incision, to discover, if possible, the cause why the wound would not heal. I found and removed a piece of bone about as large as a five-cent piece ; brought the edges of the wound together, and retained them with adhesive strips. In three days little

Sammy was well, and there remains now no visible trace to show that the patient is minus an ounce of brain.\* S. is at school, learning remarkably well, preparing himself, as he says, for the medical profession.

This case, among other important features in a surgical point of view, proves that remarkable power in nature, the *vis medicatrix naturæ*, to overcome disease and substitute a healthy for a diseased action.

HAMBURG, ARK., NOV. 13, 1858.

ART. V. *Asphyxia.—Resuscitation from Apparent Drowning.*

DR. DOWLER—*My Dear Confrère:* In compliance with your request, during the conversation this afternoon upon the subject of asphyxia, and the various means to be brought to bear against it, in which I alluded to the fact of having many years since succeeded in restoring respiration, and finally life and health, to a black boy, about two years old, who had fallen into a cistern on the then country seat of the late Commodore John Stevens, of yacht celebrity, and other sterling acquirements, I send you the following hasty sketch.

Very many years since I did give a short account of the occurrence to a Medical Journal, but as you had not seen it, and I had not a copy, I will give you an outline of the matter from memory. So long a time has elapsed that the year is not accurately remembered, though I believe it to have been in 1819; that, however, is of no great consequence.

In the month of August, passing my summer vacation with the family of Mr. S., my cousin, shortly after dinner, at two o'clock, P. M., my attention was aroused by cries of the little black boy having disappeared; and as he had been last seen in the neighborhood of a large cistern, near the house, extending under ground, and only partially covered, leaving an opening of about three square feet, through which the water was drawn in a pail, by means of a long pole, into this cistern it was supposed the boy had fallen, and great was the hue and cry. Mr. S. was absent; Mrs. S. had retired to her room, after an early dinner, and, with the exception of myself, but two

black women were about. I had not then commenced the study of medicine ; I knew, consequently, nothing of what was proper to be done in the case, even if the child was before me, which he was not. The pole had to be hunted for, and, as usual when a particular object is especially wanted, it can not be found. Found it was at last, when I went at once to grope about the bottom of a large cistern. After some time I succeeded in fixing the hook at the end of the pole, brought the boy up, and landed him on the ground. What could I do then ? I told them to take off all the clothing, ordered them to go for hot water, and begin to rub him. Recollecting that in the library there was a copy of the Encyclopædia, the large edition, I went to work to find out in a hurry what was proper to be done. I gained what I wanted, and then sent for hot ashes and more hot water. The boy was well bathed, and rubbed with the hot ashes, just from the wood fire. Having noticed in a hurried glance in the book that I must try and reëstablish respiration, I would stop the bathing, though I ordered that the rubbing with hot ashes should be continued ; and putting my mouth to one nostril, closing the other and the mouth, I breathed into his lungs, after filling my own by a fresh inspiration. In these three things, aided by exposure to the hot sun in the middle of August, were all my means of cure ; but as I had no idea as to how long I should go on, and did not know when to cease efforts, I went on for about two hours, when I saw the child gasp. To be sure, I kept at work, and so continued until nearly dark, say for four hours, when I had the satisfaction to find the little fellow safe. But what a state he was in ! The skin of nearly one-third of the body and limbs was scorched or boiled, or rubbed off, and a pretty mess I had made of it. So much for meddling with matters not at all familiar to me. But I saved the boy's life ; and although he suffered not a little, by proper care from a regular M. D., ointments, time and youth, he got over it all, and only died some six years since, from a disease totally unconnected with the drowning or the remedies used, as I heard at that time from the family. That is the only case of asphyxia from drowning that I ever met with, and as the course pursued, harsh though it was, did succeed, I should most certainly, if occasion offered, resort to the same means.

I have read the article you handed me, and have noted the comparison of the effects, by the two methods of Dr. Hall and Dr. Silvester, and to the last I give the preference, for I never could greatly

fancy, or exactly realize, the *modus operandi* of that turning the body on the side and a little over. I confess my inability to see the precise manner in which by that manœuvre the respiration could be reproduced; yet it may be so, nevertheless. I can not imagine any injury possible to result from fully inflating the lungs of one asphyxiated by as full a blowing through the nostril into the lungs as possible, and I know it can be done. The bellows, to accomplish the same object, is, in my opinion, a very clumsy and most uncertain mode.

In cases of emergency, as in asphyxia from drowning, the very best mode, and the only proper one, is that at hand, for time is precious and will not wait for systematic processes, and I much question if any will be found superior to friction, heat, and inflation of the lungs, with occasional pressure on the breast.

As regards the entrance of water into the lungs, in cases of drowning, quite a number of experiments made by my friend Dr. Kitchen, of Philadelphia, and myself, convinced us that none was admitted. We tied cats in a bag, and drowned some twenty in water containing articles which could be detected by chemical tests. Not being able to perceive the least effect, the conclusion we had to arrive at was that water did not enter the lungs in cases of drowning. Experiments, conclusions, etc., were published in one of the Philadelphia Medical Journals of 1825 or 1826.

Sincerely yours,

EDWARD JENNER COXE, M. D.

No. 266 CAMP STREET.

ART. VI.—*Intestinal Wounds.*

CAMDEN, ARKANSAS, 12th January, 1859.

BENNET DOWLER, M. D., *Editor New Orleans Medical and Surgical Journal.*—Dear Sir: In the *Gazette Hebdomadaire de Médecine de Paris*, of 16th July last (1858), is an article on the subject of intestinal wounds, translated from the *New Orleans Medical and Surgical Journal*, of March, 1858.

There being a slight omission in the original communication, it affords me pleasure to state, for the information of the Paris editor, that the wounded intestine was returned, unreservedly, into the abdominal cavity.

Experimental observation, on a very limited scale, however, has induced me to believe that intestinal wounds are not, generally, closed, as is supposed, by what is termed, adhesive inflammation between the contiguous surfaces. Yet there is, certainly, an advantage gained by changing the order of version, and placing the serous surfaces in contact, as, by this method, the sutures and included parts are placed in the most favorable position for being speedily thrown off, and discharged through the alimentary canal, instead of being left in the peritoneal cavity, where their presence, if too long retained, might, possibly, produce mischief.

Nature, always prolific in her resources, is doubly so when one of the strongholds of her citadel is threatened with danger. Where an intestine is opened or wounded, a supply of "plastic lymph," more or less abundant, is quickly thrown out for the protection of the serous lining of the abdomen, by preventing the escape of fecal matter through the opening. And were it possible to procure absolute rest and quietness, for a number of days, a majority of these concealed wounds of the intestines might be cured, and the patients preserved from destruction.

It is too often the case, however, that the circumstances, under which injuries of this character are inflicted, make it impossible to institute a proper and rigid course of treatment, before the parts have been spoiled, and rendered insusceptible of self-opposition, by the escape of fecal matter through the opening.

In addition to the usual means adopted for procuring rest, and combating inflammation, where the intestines are supposed to be wounded, I have, on two occasions, thought that much benefit was derived from the application of a compressing bandage, applied so as to force the abdominal viscera to occupy—or rather to obliterate, as near as possible, the peritoneal cavity, by bringing the parts in direct contact throughout. There being no positive evidence, in these cases, that the bowel was injured—the patients recovering well—the benefit derived from the use of the bandage is only a conjecture. Yet, I think the plan a good one, for reasons that will readily suggest themselves to any surgeon.

I had hoped that the present year would afford me an opportunity of renewing my experiments in this department of surgery. The increasing demand upon my professional services, however, forbids the anticipation of that hope.

Respectfully and sincerely yours,

J. J. McELRATH.

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ART. VII.—*Illustrations of Fever*: By BENNET DOWLER, M. D.

THE term *congestion*, as applied to fevers, is sufficiently vague and latitudinarian. *Hyperæmia* is in the same category, though it generally is used in a sense more nearly approximating that of inflammation, and, indeed, often as identical with the latter. Both congestion and hyperæmia indicate a preternatural organic accumulation of blood; both are usually referred to by writers as being sthenic and asthenic, active and passive, but in neither case are found the morbid alterations proper to inflammation, as suppuration, gangrene, and other less marked changes which might be enumerated. Broussais, however, maintained that "it is impossible to draw a line of distinction between congestion and inflammation, the one passing into the other by insensible degrees."

Unfortunately, neither congestion nor hyperæmia can be accepted in place of the old but theoretical term, inflammation. Congestion (which conveys a clear idea,) is almost always an antecedent or an accompaniment of inflammation, but not its sole fundamental characteristic, being on the contrary, in a practical point of view, considered as the antithesis of the latter. Thus, if the class of therapeutic agents called stimulants be curative in congestion, they are usually deemed injurious in inflammation, as the latter, according to the received theory, requires antiphlogistics.

This nomenclatural confusion is referred to merely to show that however desirable it may be to set aside a term so ambiguous as that of inflammation, it cannot be fully accomplished by substituting for it either congestion or hyperæmia. In pathological anatomy, or post mortem histories, none of these terms should be substituted for physical descriptions; at least, the term inflammation should be rejected.

The organs should be described as natural, vascular, injected, anæmic, infiltrated, enlarged, atrophied, indurated, softened, colored, and the like, as the case may be. This method, which is restricted to physical description or facts, leaves pathological logicians to wrangle as they choose, as to whether an exudation, effusion, induration, softening, and so on, are due to sthenia or asthenia, debility or inflammation, dynamical or structural change, innervation or sanguineous action, etc.

On the present occasion, I do not propose to write an essay, but merely to copy one or more recorded cases of congestive or some other fever, as my leisure may permit.

1. *Congestive Fever—Fatal Case—Post-mortem Examination.*

1848. Sept. 22; 1, P. M.—F. H., born in Germany, aged 28: for eleven years last past, a resident near this city, upon the river coast; is a sugar manufacturer; was conveyed to the city to-day and admitted into hospital in a state of insensibility. His wife says that he was seized thirty hours since, soon after breakfast, with violent convulsive fits, and became immediately speechless and totally insensible. She says that he never had any kind of fit before; that he had no premonitory symptoms, etc. From her account of his primary seizure as well as from present appearances, his case may be regarded as an apoplectic form of congestive fever often called "pernicious fever." His medical attendant diagnosticated his disease, "congestion of the brain."

The convulsions are intermitting; they begin with muscular spasms in the extremities, especially in the right arm, and soon extend to the whole body; the eyes become distorted and lose their parallelism; he foams in the mouth; the respiration and pulse are irregular; the skin, particularly that of the face and neck, cyanosed; mouth distorted. During the convulsive paroxysm the pupils are enormously dilated; in the interval, they are natural. His person is plump and fleshy, but apparently exsanguineous; hands 91°; bend of the arm 98°; axilla 100°. Percussional sounds very dull over the chest.

Sept. 23d.—Skin pale and bloodless; pulse 130; respiration either uneasy or stertorous; persistent insensibility; intermitting convulsions.

Treatment: An enema, which brought away dark-colored fæces; blood-letting twenty ounces; extensive sinapisms; assafoetida, valerian, etc.



Died at 10, P. M., sixty-four hours after the primary seizure. Post-mortem examination thirteen hours after death; the air dry, 66°; cadaver cold, pale; surface exsanguineous; abdomen concave; cadaveric rigidity moderate; muscular and adipose tissues natural.

*Head.*—Slight sub-arachnoid redness over the left hemisphere; the vessels of the hemispheres much developed, but collapsed and empty; serosity about three ounces in the arachnoidal sack; cohesion of the brain natural. *Chest.*—Right lung congested at its base and centre where its cohesion is diminished; the costal pleura, towards the spine, injected; the heart flaccid and rather pale; the serous tissue of the right auricle and ventricle, dark red [perhaps post-mortem dyeing only]. *Abdomen.*—General anæmia of the omenta, mesentery, serous membrane, etc. The bowels throughout pale and bloodless; the mucous tissue of the stomach, pale, being without rugosities; flaccid, somewhat softened; variegated with greenish, yellow, and reddish tints, forming marbled spots. *Liver.*—This is of a pale or cork-color; enlarged, about one-third; cohesion natural; the hypertrophy is chiefly upon the convex aspect of the right lobe which has pushed up the diaphragm against the base of the right lung; the gall-bladder contains very liquid, yellow bile. *Kidneys.*—Right kidney enlarged, one-fourth; engorged; injected; parenchyma of a dark red. Left kidney anæmic. *Spleen.*—Enlarged, double size; softened, its parenchyma broken down and easily washed away from its areolar septa or cellular tissue. Other organs natural, excepting a prevalent anæmic condition both externally and internally; scarcely any blood flowed during the dissection of the head, chest, or abdomen.

## 2. *Congestive Fever—Recovery.*

November 3.—J. S., aged 37, lake-waterman, resident in and near the city for nine years; sick four days; was taken with severe chills, debility, and watery stools, which latter continue. Has no acute pain; thirsty; tongue tumid; load or oppression of the stomach, with nausea; partial apnœa or “smothering about the heart,” as he terms it; respiration somewhat hurried; pulse quick, small, soft; countenance dejected; senses natural; but he says he feels “a little lost in his head”; conversation fatigues him; skin cold, dusky, cyanosed; urinates. Has taken twenty grains of calomel.

Nov. 4.—Convalescent. In four or five days after, he regained his strength and health.

3.—*Congestive Fever—Death.*

J. C., born in Virginia, aged 37, resident four weeks; sick about one week; was treated chiefly by cupping, revulsions, blisters, etc.; his physician found him in a collapsed state, with severe vomiting, purging, stupor, and prostration, with little or no reaction.

Post-mortem examination two hours after death: Extremities cold; trunk quite warm; abdomen slightly concave; free from emaciation; muscles natural; fat abundant; jaws rigid.

*Abdomen.*—The whole venous system distended with claret-colored blood, which coagulates feebly; liver of a dark red mahogany color, its central portion diminished in cohesion; bile of the gall-bladder, molasses-like in consistence; stomach small, containing a yellow mucilaginous water, its mucous coat reddish, but of natural consistence; the ileum contains a dark bile-like fluid; its mucous coat near the cæcum red, vascular, and injected; the mucous coat of the colon in its arch, and near its sigmoid flexure, ulcerated in two places as large as the quarter of a dollar, nearly circular, with raised edges, excavated, without involving the muscular or serous tissues; several of the mesenteric glands are enlarged, some of which are softened, having a white, cheesy or tuberculous appearance and consistence. *Head.*—Brain firm, having many bloody points, on cutting its substance; pia mater very vascular, forming a close net work, with moderate turgidity; serosity three or four ounces in the arachnoidal sack and ventricular spaces.

4. *Congestive Fever.*

Sept. 9.—J. B. R., born in France, a gardener, about 25 years old, sick three days; retained his senses; answered distinctly until his voice failed him, a few minutes before death. In a quarter of an hour the dissection began. Body hot, except the insteps and backs of the hands; no rigidity except of the recti muscles; the abdomen concave; the skin has a faint tinge of yellow; the muscles pale; the blood of a pink color.

Brain moderately vascular; the consistence slightly diminished; the external hue of the brain a dusky drab. *Chest.*—The left ventricle of the heart slightly thickened; the whole texture brittle, containing polypi partly blood, and the residue a yellow concretion of moderate tenacity. Lungs natural, but collapsed to one-fourth the size of the cavities. *Abdomen.*—All of the intestines contracted in calibre, con-

taining yellow bile, but not otherwise changed. The stomach contains about one pint of diluted bile which becomes thicker and thicker towards the termination of the large intestine. The spleen enlarged to double size, is inelastic, breaking, if bent, or denuded of its coats; is of a black color, excessively friable. The liver enlarged two-thirds; inelastic, cutting in glossy slices and uniformly of a color almost inky; coats adherent; the gall-bladder greatly distended with bile, three to three and a half ounces; thicker than the albumen of the egg, and so tenacious that it can be drawn out into a rope a yard long. The urinary bladder empty and contracted to the size of a hen's egg; the large veins, as the portæ and cavæ, greatly distended and so dilated as to admit four fingers; blood of a dark pink color and diffuent.

Without proceeding further to enumerate cases illustrative of symptoms of the various stages, modes of treatment, recoveries, deaths, and anatomical appearances, in congestive fever, I will here conclude with a few general remarks and critical reflections, although contrary to the purpose already expressed as above, and already printed off in the preceding sheet of this journal.

The existence or reality of congestive fever has been denied, and spoken of even with derision as altogether fabulous, chiefly in Northern latitudes. The late Dr. Parrish of Philadelphia, about fifteen years ago, opposed, in the Medical Society of that city, the admissibility of the term *congestive* as applied to the malarial fevers of the South and West, formerly called malignant, which he called adynamic, not being, according to him, congestive, and which he ascribes to nervous prostration, etc.

Others, in like manner, have denied the reality of this fever, or have sought to identify it with other fevers. During the great epidemic of 1858, a long newspaper discussion, in French, appeared in this city, concerning the liability of city creoles to contract yellow fever, which turned chiefly upon the identity or rather differentiations between pernicious and fellow fevers, and in which, among other diagnostics, a therapeutic criterion was appealed to, namely, quinine, which it was asserted is the sole and infallible remedy for pernicious, but not for yellow fever. It was maintained that the natives of the city, who were said to have had yellow fever, in 1858, had suffered nothing more than attacks of a marked form of pernicious fever, curable by quinine.

According to the United States Mortality Statistics (as recorded in

the general tables) for 1850, among 323,023 deaths, no case of death from congestive or typhus fever is enumerated, while under the head of *fever*, are recorded 18,108 deaths; also, intermittent, 964; remittent, 148; scarlatina, 9,584; ship, 240; typhoid, 18,099; yellow, 785. (Concerning these blunders, a few remarks will be made in the sequel.) Indeed, Dr. Jarvis of Massachusetts, whom Prof. De Bow, Superintendent of the Census, officially engaged upon the classification of the diseases, announces with great brevity, that "congestive fever is merely a form or phase of *typhus*" (Mort. Stat. U. S. art. 22, page 47.) In a previous page, art. 14, he avers that, "*congestion* is unintelligible." According to these official laconics, there was, of course, no death from congestion, as there was no death from typhus, anno, 1849-50! Now, according to the official reports of the Board of Health of New Orleans, for 1849, 164 persons died of typhus; 41 of pernicious, and 78 of congestive, not to name other fevers, as bilious, malignant, yellow, ataxic, adynamic, nervous and typhoid fevers—the last mentioned amounted to 178. Typhus, in this city, is almost exclusively restricted to ship-immigrants, recently from Northern climates. Congestive fever and *la fièvre pernicieuse*, are, it is believed, reported by the present Board of Health, under one name, without regard to the words of the actual certificates of physicians, pernicious fever being considered as identical with congestive. Whether this classification is approved by French physicians, who are most apt to adopt the former nomenclature, is not known. It is, at least, inconvenient to use two names for the same thing. It cannot be said with truth, that the *fièvre pernicieuse* is in its symptoms, tendencies, and nature, an ordinary intermittent. Without now alluding to its differential diagnosis, or its points of contact and agreement with intermittent proper, it is sufficient to say, that the latter differs greatly in its tendency to a spontaneous cure, while pernicious fever tends to a mortal result, if left to run its course, without medication.

The assumed identity of congestive and typhus fevers is not admissible, at least, in Southern latitudes. Although they may coincide, blend, unite, and mutually mask each other, yet these relations are not essential but accidental. The agild fever of Louisiana is no more the burning typhus gravior of Massachusetts, than the sugar and cotton of the former are the granite and shoes of the latter.

Congestive fever, or some of its synonymes, as agild fever, congestive chills, adynamic, malignant fever, cold fever, cold plague, per-

nieious fever, etc., have been long and firmly planted in the South, particularly in rural districts. That it may be considered as much an idiopathic fever, as most other fevers so called—that it is neither pure paroxysmal intermittent, nor a mere accidental phase of typhus—that it often resembles the severer form of remittent and the collapsed stage of cholera, are propositions more or less tenable, which not a few practical observers are willing to affirm as altogether true, and which cannot be invalidated by the simple denial of persons who never have met with this most fatal malady; a malady which, a country member of the Louisiana Legislature, a few years ago, quaintly described by saying, “it made the patient as cold as an iron wedge.”

Nevertheless, the patient during life is distressed, as in cholera, with a sensation of thirst and internal heat, and after death, often becomes very hot. A few years ago, Prof. Stone, of this city, informed me, that in consequence of this post-mortem heat, in such cases, he had several times been sent for in order to determine whether the body was really dead.

The topography of this disease, presents little explanatory of its origin. The marshes, rivers, and depressed plains of Louisiana are, it is believed, more exempt from its ravages, than the elevated plateaux, terraces, and hilly regions, and, among equal numbers, cities appear to suffer less than rural districts.

Congestive or pernicious fever has been recognized as the desolating plague of the *campagna di Roma* which, ever since the days of Lancisi, has furnished writers on malaria, with the staple commodity for their theories of marsh poison. A few remarks on this ancient classical marsh, (*pompina paludes*) almost within sight of the immortal turrets of Rome, may be allowable, though without derogation to the shaking prairies, interminable swamps and annually inundated lands of Louisiana, five times larger in area than the whole *campagna* including its morasses.

Lady Morgan describes the *campagna di Roma*, as being a pestilential marsh; of which, however, later travelers give a very different description: thus, Carter in his letters, says of this district of utter desolation, that it “lies in a circular form, and is something more than one hundred miles in circumference, to the sea itself, to which its uniform expanse bears a striking resemblance. Its surface is not a dead level and marshy, as the remarks of some travelers have led me to believe, but is varied by undulations of about the size of heavy

swells of the ocean in a gale. In the whole distance of fourteen miles, I did not observe a drop of stagnant water, or nuisances of any kind, which would be likely to breed pestilence. The soil is a light sandy loam—the last in the world to which we would look for bad air and noxious exhalations. The campagna is by no means so barren, as many tourists have represented. It is universally covered with a lively green sod, like ordinary American pasture land; appearing to be fed close by the cattle, sheep and goats, which are forever grazing upon it, both winter and summer. The whole region lies unfenced, though the herdsmen and shepherds of the proprietors have their prescribed limits. There are no trees of any kind. Scarcely a single acre of this immense tract, twelve or fifteen hundred square miles, is under cultivation. A colony of Yankees, in less than half a century would make the campagna di Roma, one of the finest countries in the world. Such have been my remarks in crossing this region, six times on four different routes. Dark ruins are scattered over this waste in shapeless masses. Whatever may have been its former condition, this extensive tract at present much more nearly resembles a meadow than a bog, and in aspect is not unlike the borders of some of the lakes in the western part of the State of New York. A considerable portion of it is arable. Very little stagnant water was observed; the land was gay with a profusion of flowers; the air breathed the fragrance of spring, instead of poisonous malaria; the carols of birds are more frequently heard than the croaking of Horace's frogs. The road built upon the foundations of the old Appian Way, constructed by Appius Claudius, three hundred years before the Christian era, is magnificent," etc. (Letters, 2d ed. ii. 152, *et seq.*)

This realm of malaria and death, is described by Forsyth as "a climate where Heaven's breath smells sweet and wooingly; having a vigorous and luxurious nature, unparalleled in its productions; once the fairy land of poets; the favorite retreat of great men. Even the tyrants of creation loved this alluring region; spared it; adorned it; lived in it; and died in it." Such is the topography of one of the haunts of pernicious or congestive fever, which frequently visits the eternal city itself.

For more than two centuries pernicious or congestive fever has been noticed and elaborately described, by European writers, under various names, as *febris syncopalis*, *febris perniciosa*, *fièvre perniciouse*, etc. It prevailed at Madrid (1639); at Copenhagen (1652); at London

(1657); at Leyden (1667, 1669, killing two-thirds of the principal inhabitants); in Germany and Holland (1691, 1720); at Rome (1695, 1703, 1705); at Turin (1722); at Breslau, in Silesia (1737); at Luxembourg (1759, 1761); at Manheim (1761); at Fano (1764); and, also, in many towns and rural districts of France, in the last quarter of the eighteenth century, as well as in Italy. (See *Hist. Méd. par OZANAM*, T. ii; also Burserius' Latin work, *Institutionum Medicinæ Practicæ, volumen secundum*. Venice, 1782.)

In 1773, it prevailed at a village opposite Avignon, where an epidemic had never before been seen—out of 224 inhabitants, 208 were attacked, and in two months 75 died. The fever soon spread to Avignon, Arles, and many other places. In 1804 it prevailed at Breno, in Italy; in 1806 at Ercole, five miles from Naples, in a village of 550 souls. The disease lasted nine months, killing 115; that is to say, 50 children, 38 men, and 37 women. This account is given in Sédillot's *Med. Jour.* by M. C. Daudebert. (*Ozanam. Hist. Méd.* ii. 77.) In Bordeaux the previous year, this disease attacked 12,000, destroying, in five months, 3,000 inhabitants of that city. The laborers, brandy-drinkers, upon the canal were, above all others, exempt from its ravages. Of the two forms of this disease, in that epidemic, one was *without intermissions*.

It would be too tedious to give a chronological list of the more modern epidemics. The readers of this journal will have seen numerous papers upon this disease, the existence of which is now denied in two recent and important official works published by the authority of the United States, namely, the *Medical Statistics of the Army*, and the *Mortality Statistics*. Hence the question—have we a congestive fever among us? Although in the official army statistics, for five years, 1840–4, *febris congestiva* gives a far higher number of mortal cases than all the forms of intermittent united, namely, *febris intermittens quotidiana*, *febris intermittens tertiana*, *febris intermittens quartana*, and nearly twice as many as *febris continua communis* (page 488). Yet, in a subsequent page, this classification is repudiated, and the reader is gravely told that "*febris congestiva*," is to be "regarded as congestive terminations of *intermittent*." The term congestive fever is retained in preference to *pernicious intermittent*, because it is *universally* employed by the medical officers in that sense, as also throughout the Southern and South-western States." This quotation is given to show the existing ambiguity and inaccuracy of the nomenclature concerning congestive fever, without, now, fully controverting its justness.

This dictum is sufficiently invalidated in the special reports of the work itself, which may be found in the appendix, not to name other evidence. In these reports, congestive and intermittent fevers are referred to separately: thus, Surgeon McCormick says (page 662), "I have found opium and quinine to have a similar action when given in *congestive fever*, particularly in the cold stage. They are the remedies I especially rely on in this disease. Like quinine, opium, when given in a full dose, seldom fails to arrest the paroxysm of an *intermittent*." Again: Surgeon Baily (p. 670-1), describes, as prevalent in the South and Southwest, two forms of "*real congestive fever*, one of which is intermittent—though unlike, named the same. The second, a *local congestive fever*, caused dreadful mortality among the troops," etc. Surgeon DeLeon describes the fevers of the Peninsula of Florida as "intermittent, remittent, and congestive. The congestive fevers were the most malignant seen by me (him) while in that region." (673.) In speaking of the Southern States, he avers that "congestive fever is endemic to various parts of the Southern country. Congestive fever is characterized in its onset by strongly marked symptoms of congestion of some of the viscera most important to life, and, of consequence, by great oppression of the nervous system simulating debility—suddenly overwhelming all the viscera of organic life, producing fatal congestions." (675.)

Statements of similar import, some of which are more definite and definitive relating to the independent individuality of congestive fever, are given in a majority of these special reports, as contained in the appendix of this enormous quarto.

Although, as above noted, the United States Mortality Statistics of the Seventh Census, abolishes congestive fever, as seen in the general tables, yet, fortunately, in the tables for the individual States, the usual nomenclature and common classifications as originally returned in the official documents, the only reliable data, are reproduced. From these sources, making every reasonable allowance for incidental error—which it is well-known, conformably to the doctrine of probabilities, is virtually corrected where the observations are very numerous, as in the premises—I say, after all reasonable concessions in this regard, the work above mentioned affords the strongest presumptive, nay, positive proof of the prevalence of an idiopathic congestive fever, and that it has a distinctive character, notwithstanding its real or reputed alliance with intermittent.



The first series of tables in the U. S. Mortality Statistics, as already mentioned, not only ignores congestive, but typhus fever, while in New Orleans at the very time when the census was taken, the deaths from this latter disease, reported by the Board of Health, reached 119 ! The census statement of deaths, in this series, is : fever, 18,108; intermittent, 964; remittent, 148; ship fever, 240; typhoid, 13,099; yellow, 785; scarlet, 9,584; typhus and congestive, none.

In order, however, to ascertain the reality and geographical distribution of congestive fever in the United States, it is proposed to examine the entire official returns of the mortality statistics of the individual States, omitting *congestion not specified*; also, *congestions of the brain, lungs, etc*, as well as all other fevers not specified as congestive, such as bilious, inflammatory, intermittent, remittent, typhus, typhoid, etc.

Deaths from congestive fever in 1849-50: (U. S. Mort. Stat.)

Maine.....	0	Illinois.....	242
Massachusetts.....	0	Michigan.....	68
New Hampshire.....	3	Missouri.....	170
Connecticut.....	5	Wisconsin.....	15
Rhode Island.....	0	Iowa.....	49
Delaware.....	2	Tennessee.....	131
Vermont.....	1	Arkansas.....	128
New Jersey.....	26	Mississippi.....	240
California.....	1	Alabama.....	243
New York.....	73	Georgia.....	61
Maryland.....	1	North Carolina.....	83
Pennsylvania.....	37	South Carolina.....	153
Virginia.....	70	Florida.....	21
Kentucky.....	82	Texas.....	138
Ohio.....	112	Louisiana.....	186
Indiana.....	231		

It may be here added, that the New Orleans Board of Health reported 106 deaths from congestive fever for the year ending with April, 1856—for the seven following months, 94.

New, is not still a colony of old England. If English physicians do not meet with congestive fever, nor New Englanders either, this is no valid reason why the general Government, through its medical agents, should virtually, or rather overtly veto the official documents of Southern and Western physicians, who, unfortunately lose, as they themselves say, hundreds, nay, thousands, yearly, by congestive fever, but far less than do Northerners by typhus. The medical authorities of

the army and of the census, say there is no congestive fever, that even the term is unintelligible, and that, at the utmost, congestive is but "a mere phase of typhus fever."

Book builders, annotators, lecturers, and colleges in the North, however competent to instruct the rising generation in all that concerns diseases of their own climates, sometimes derisively called "sectional medicine," may possibly not be able to do so as it regards diseases of hot climates. At least, the residents of the latter ought to know the maladies of their climate better than others.

According to the Mortality Statistics of the United States for the year ending June 1, 1850, the deaths from typhus in the Gulf States, from the Peninsula of Florida to the Rio Grande, amounted to the following numbers:

Florida .....	9
Alabama .....	0
Louisiana .....	290
Texas .....	66
	365

Now, by the same authority, in a like number of the Northern States, the mortality of the disease is set down, for

Maine .....	282
New Hampshire.....	124
Vermont .....	74
Massachusetts .....	558
	1038

According to the seventh census, the total population of the four Gulf States above mentioned, is, in round numbers, 1,617,000, which divided by the whole number of deaths from typhus, gives one death from this malady to 4,430 inhabitants; while the population of these four New England States amounts to 2,209,000, which, divided by the aggregate mortality from typhus, gives one death to 2,127 souls; that is, more than twice as many deaths as equal numbers of the population in the Southern States.

Upon the whole, it may be provisionally admitted, until further and

more reliable data shall be adduced, that in the North-Eastern States, typhus is most predominant; that congestive or pernicious fever is extremely rare, and that the latter malady prevails to a great extent in the Southern and Western States of this Republic.

The Report of the N. O. Board of Health just received gives the following mortality statistics for 1858: *Congestive fever* 328, not to name other congestive diseases, as congestion of the brain 156—of the lungs 36, etc.

I will conclude this paper with remarks from one of the latest foreign writers on pernicious fever, which appears nearly allied with the congestive fever in the valley of the Mississippi. M. Haspel's work on the Pernicious Fever of Oran, in Algeria, which is ably reviewed in *The British and Foreign Medico-Chirurgical Review*, gives the following plan of treatment for curing this disease, as it appears in Africa. He gives at once from thirty to seventy-five grains of quinine. "This dose in a case of imminent danger, may be given for several days in succession. The belief that quinine only exerted beneficial power over the periodicity of the disease, is exploded, it being now proved that its efficacy is just as great, whatever form the paludal disease may assume. M. Haspel has never seen any ill effects whatever, result from these doses. In several patients great epigastric tenderness, anorexia, and an abnormal condition of the tongue, have been considered erroneously, to contra-indicate its administration; but, in fact, these conditions disappear with the fever, under the influence of the quinine. Advantage is sometimes derived by giving opiates an hour previous to taking the quinine. We must not be content with cutting short the attack; for the apparent amelioration will be followed with cruel deception. Smaller doses may now be substituted. M. Haspel speaks disparagingly of other remedies for the fever itself. *Pernicious fever*, if left to itself, infallibly kills in the first to the fourth paroxysm—rarely being prolonged to the fifth. Its entire duration is from twelve to seventy-two hours in adults, and from twenty-four to forty-eight in children, having *slight* remissions, which seem like a halting in the struggle, always occurring. Although the symptoms known as those of *gastro-enterite*, are of so common an occurrence during the whole course of the diseases met with in Algeria, *post-mortem* examination exhibits no proof whatever of such an affection having been present."

ART. III.—*Observations and Reflections on Yellow Fever*: By M. MORTON  
DOWLER, M. D.

WE hear it sometimes asserted, even in this community, that yellow fever as a subject of discussion is a worn out and exhausted theme, and that, as journalists, if we would secure the attention of our readers, we would do better to embrace some other topic. It must indeed be admitted that no medical subject in modern times has attracted to itself a greater number of writers, lecturers and observers. Both the professional and the non-professional have entered the field. The literature of yellow fever is formidable to contemplate. The disease has engaged the medical mind in every nation in Christendom, and has been investigated by the master spirits of our profession. Dr. La Roche, while himself contributing in the form of his late work, 1,400 large octavo pages on this disease, devotes 45 of his own ample pages to his bibliographical catalogue—referring to works which, if duly consigned to the shelves, would, of themselves, form an enormous library. Did the extent of our positive knowledge in any manner correspond with the magnitude of our yellow fever literature, we might perhaps, with some show of reason, abandon our subject as a theme worn out and exhausted.

But all the world will bear melancholy testimony, that should such equipoise between the known and the written, has no existence. In relation to many of the mere elementary principles, which are in themselves absolutely necessary to positive knowledge in various departments of this subject, it is no injustice to say that the profession are no wiser than their fellow-citizens. The medical philosopher must still “learn to labor and to wait.” Notwithstanding the homage we justly pay to our medical literature, the universal public does not fail to discern in the midst of a yellow fever epidemic, the fallacies of our ponderous volumes, and to duly recognize our elaborately learned methods of teaching nothing. Not an epidemic occurs which does not set aside the dogmas of the highest authorities, and that too in the very eyes of the multitude—dogmas put forth and received in all mathematical confidence. We hope that no over-scrupulous brother *in Æsculapio*, will be offended because of these remarks. What the public have declared; what the newspaper press did not fail to cast in our teeth during the late epidemic; and what we have moreover been wholly unable to controvert, may surely be recounted in the pages of a medical journal. It is a fatal delusion, therefore, to suppose that

the subject of yellow fever is a worn out or exhausted one, or that so far as our positive knowledge is concerned, notwithstanding the magnitude of our yellow fever literature, we are otherwise than in the very infancy of our knowledge touching a pestilence which, since its first appearance in this city, has consigned more than a hundred thousand of its inhabitants to the tomb.

When we enter into an appreciative view of the premises, it is perfectly obvious that a very great portion of our medical book-makers are, of themselves and of their own personal knowledge, very little conversant with the subjects on which they lay claim to authorship; and that this remark will in an especial manner hold good in relation to the subject of yellow fever. Our larger treatises—mostly emanating from the North, and quite “out of harm’s way,” are little else than the histories of conflicting opinions; diluted citation of authorities; the weighing of probabilities against probabilities, and possibilities against possibilities. This species of book-making requires no high order of qualification, and is no difficult art. The possession of ordinary literary abilities, an ample library and supply of the current journals, and the necessary leisure, are the only pre-requisites. Thus fortified in his position, it is even easier for the author to write a large book than a small one; and he has of course an excellent opportunity of excogitating with great eclat, numerous opinions which are quite his own, but drawn from the airy regions of imagination. And without contributing a solitary additional idea to our stock of medical knowledge, we are too often told through the unappreciative *Sarsaparilla puffs* of the reviewer, that the author has erected a permanent monument to his genius, and that his book will endure for centuries. Such is the not unfrequent inauguration of what has been called high *authority*. At the hands of such high authority New Orleans has been a sufferer, as has also our honorable and useful profession, and the cause of truth; and until we cease to receive at the hands of these over-ambitious *authorities* in the profession, such pretentious claims as they have put forth from the closet, to a knowledge of what in reality as yet is wholly unknown in relation to yellow fever, we may look in vain for real progress.

In a former article published in this journal, I have endeavored to give “satisfying reasons” why the subject of yellow fever, so far from being considered an exhausted theme, should be the grand engrossing medical topic of the day with the profession in New Orleans.

If ever a satisfactory light beams forth on the mysteries of this pestilence, it must radiate from some such place as New Orleans, where it has a local habitation and a name. No one will have the effrontery to deny that we have in this city a large body of physicians composed of men who are equal in every respect to those of any other city in the Republic. It is nothing to the disadvantage of our medical men that they are comparatively but little identified with the prevailing shams of book-making, to which we have alluded, and by which men of no authority in anything, become the oracles of the profession. It cannot be supposed, that all that is really known of yellow fever is not familiar to the profession in this city. The veneration in the profession for those who write books, on the part of those who write not, is, it is true, too great throughout the realm, and doubtless has its influence here; but as I have alluded to what has been called high authority, let us ask ourselves who, in reality, are the high authorities? Why, none other than those, who, like numerous physicians of this community, have labored in the midst of the disease for fifteen, twenty, thirty years. These are the true oracles, if any exist; but the great mass of these have remained silent. It is high time they were awakened to a sense of their responsibility.

With every facility here for the prosecution of inquiry in relation to our subject—a subject of the deepest importance to the whole human race—a malign influence, too plainly to be observed, has retarded our progress, blasted our energies, and has brought about retrograde action. The establishment of a Quarantine here, and the organization of a Board of Health, will serve as an exemplification in point, and will show, that notwithstanding all the additional light which the later epidemics have thrown on the subject of yellow fever, retrogression has taken the place of progress. Let us, for a moment, inquire into the remarkable circumstances under which the present Quarantine was brought into existence, and examine how far the action therein taken can be reconciled with a due regard to medical truth, to say nothing of the best interests of this community.

Quarantine, in relation to yellow fever, had been in this city an exploded idea for the space of thirty-three years. It was inaugurated here by legislative enactment 1821, and abolished 1825; and it was even at the time of its adoption an exploded system. The events occurring during the four years of its existence, and especially, the increased malignity of the sweeping epidemics of 1822 and 1824, not-

withstanding the "rigid enforcement of the law," led to its abolition. For the thirteen years next succeeding the abolition of Quarantine, there were but four epidemics, being in the ratio of time, less than one-third the number that occurred during the four years of its existence. These thirteen years extend up to the healthy season of 1838. Between the years 1839 and 1853 inclusive, extending through a period of fifteen years, we may be said to have had five epidemics, being a ratio on the time, of a little more than one-quarter of that of the period of quarantine. After, therefore, the long period of thirty-three years, in which quarantine had scarcely been mentioned save in that derision which a few years' experience of expense, and delusion merited—after this long period in which New Orleans had stood forth as an example and beacon for the guidance of other cities, and as the champion of her own best interests, the memorable epidemic of 1853 is ushered in. This has been called the *great epidemic*, and the numerical prevalence, and the number of deaths that occurred in this city, and the coëxistence of the disease in six States of the Union, justly entitle it to that distinction. I have no hesitation, however, in coming to the conclusion, after extensive observation in every epidemic here, during the last twenty-two years, that though in sections of the country which had never before been visited by the pestilence, the mortality in that season was wholly unparalleled, so far as this city is concerned, the mortality of 1853, considered in ratio of the number attacked by the disease, was lower than in any epidemic that has occurred since 1837. But reserving this conclusion for further discussion, as also the fact, that since the establishment of the present quarantine, three malignant epidemics have visited the city, including the remarkably deadly and protracted one of 1858, let us return to the great epidemic and the quarantine mystery to which it gave rise.

Two or three of the circumstances attending the epidemic of 1853, could not have failed to arrest the attention of all, namely: its early appearance, its unusual season of disappearance, and its *unequivocal local origin*. On the disappearance of this epidemic, a Sanitary Commission, composed of five physicians, selected for ability and learning, were appointed by the Common Council of New Orleans, with a view of determining what progress, if any, could be made, in relation to the disease, in the investigation of the great epidemic. Amongst the various subjects referred to the Commission, was the following instruction: "To inquire into the origin and mode of transmission or

propagation of the late epidemic." Drs. A. F. Axson and S. D. McNeil, the gentlemen to whom the solution of this question was assigned, after a thorough inquiry into the subject, submitted an elaborate report, setting forth, as, indeed, might well have been anticipated, that the epidemic was clearly and unequivocally of local origin. "We have thus shown, we trust," say these gentlemen, at the winding up of their report, "that there is no just or tenable ground for the opinion that our fever was imported," and the majority of the Commission make the following emphatic declaration: "This Commission has not remained satisfied with theoretical presumptive evidence. Most careful scrutiny into the actual occurrences of the first eruption of the fever, its spread, the character of its localizations, the persons most liable and suffering from whatever class and country, have converted presumptive proof into positive certainty, that the fever originated with us, etc." Drs. Axson and McNeil, moreover, in their report, announce with the neatest brevity, "that the disease has not been derived from abroad, but is of local origin."

The learned Commission here gave utterance to a broad truth, however clearly (as we shall maintain hereafter) they dogmatized quite out of the depths of our actual knowledge, when they coupled, as they unanimously did, the fact with the asserted discovery, "that the fatal malignity and spread of the disease, was justly attributable to a *very remarkable concurrence and combination of the atmospheric and terrene causes, always peculiarly fatal to human health and life,*" and that "these have been most amply explained and fully pointed out."

But to proceed, two members of the Commission, it is true, namely: Professor Riddell and Dr. Simonds, do not subscribe to this "positive certainty." Something must be conceded to Professor Riddell for his known devotion to his own botanic and chemical theory, and to his conviction that the epidemic was owing to the importation "of the perfected spores of cryptogamic plants from a source further South, into an atmosphere deficient in ozone." But even he was merely in favor of "*some kind of quarantine,*" which, he told us, "it was proper and expedient we should have."

Dr. Simonds, whose report in favor of quarantine is made up, not of any special local deductions from the medical history of New Orleans epidemics, but mainly of extracts from an article from another quarter of the country, by Professor Hume, had some time previously published in Dr. Fenner's Southren Medical Reports, a highly valuable



statistical article in relation to yellow fever, as it appeared in the Charity Hospital, which is of itself sufficient to demonstrate the local and indigenous origin of yellow fever, and to overthrow the whole fabric of quarantine, which he advocates. He has therein shown, from the records of that institution, that for thirty consecutive years, setting out from 1820, there is no one year in which there has not been cases of yellow fever in the Charity Hospital; and I can myself aver that in no year, for the last twenty-two years, have I not attended to cases in private practice.

That anything, therefore, connected with the epidemic of 1853, afforded the least *public* reason for the revival of the exploded system of 1821, is what cannot be readily conceived. The present quarantine was established in defiance of the following adverse antecedents, being the accumulated counter-testimony of thirty-three years, confirmed, as we have seen, by the crowning evidence of the very epidemic on which the system was managed into existence. Let us here set down the population of this city according to the national census, beginning at the quarantine era, and descending to the present time, which stands thus: in 1820, 27,126; in 1830, 46,310; in 1840, 102,193; in 1850, 126,375. According to the local census of 1843, the population was 145,449; so that, taking the latest ratio of increase, our population, at the present writing, is somewhere about 175,000. New Orleans, therefore, now stands with a population more than six times the number she contained when she abolished her first quarantine, with a commerce increased in an infinitely greater ratio—exporting eighty millions in value—with arrivals in her ports—then few and far between—amounting now, in all kinds of water craft, salt and fresh, to eight or ten thousand visits annually.

During the thirty-three years' absence of quarantine, New Orleans arose from the condition of a tenth-rate city, to that of the queen of American exporting cities. With the rapid increase of her population, the number of yellow fever subjects during this period proportionably increased. Her intercourse with those parts in which yellow fever never fails to annually appear, was augmented, say a hundred fold, during the absence of quarantine. It must follow, therefore, that if quarantine be necessary to prevent the importation of yellow fever epidemics—all the elements of reputed danger, thus existing in this accumulated form—the corresponding degree of danger ought to have been verified by actual experience during its

thirty-three years' absence, and New Orleans have been left at once to the mercy of annual epidemics. But the very reverse is true. The malignant epidemic of 1837 made its appearance in the midst of a financial crisis, which depressed commerce and commercial enterprise, which more or less affected New Orleans up to the year 1847, in which there was a violent epidemic. During the ten years that elapsed between these dates, there were four yellow fever epidemics. On the contrary, between the years 1847 and 1853, a period of five years, no yellow fever epidemic appeared, and in which a great portion of our people fondly indulged the hope that the pestilence had taken its final flight. A period as will be seen by the census, in which our population expanded in an unprecedented ratio—a period in which we closed our war with Mexico, and withdrew our troops from a yellow fever district—a period in which we were in constant communication with the pestilential port of Vera Cruz—a period in which the acquisition of California and its gold had aroused commercial enterprise throughout the Republic—in short, a period in which there were more arrivals from infected ports at our wharves, than ever before had been known in the history of New Orleans. Had the commanders of vessels, believers in quarantine, entered into a conspiracy with malice of forethought, to bring about an epidemic of yellow fever annually in New Orleans, during the five years that succeeded the epidemic of 1847, they could scarcely have done more towards this object than they did in their innocence of such design. To bring cases of yellow fever into New Orleans after the close of the Mexican war, from Havana or Vera Cruz, as was repeatedly done, was deemed a matter of small moment, and attracted no attention. Here, then, we have five years of previously unexampled expansion in commercial intercourse and in population, while the danger, so far from being augmented, is reduced to nullity ; and when

The terrible, unknown, and indistinct,  
Yet awful thing of shadows

overtook us at last, in 1853, we had the satisfaction of knowing from official authority, as well as from our own convictions, that none of the preceding causes had brought about the calamity, and that in the language of the present President of the Board of Health, "we felt warranted in stating, that the epidemic was not derived from abroad, but was of spontaneous origin."

During this five years' respite from epidemic yellow fever, the disease appeared, as is well known, in a great number of places, with which we are in frequent communication, in which it was never before known to occur. Under these circumstances it ravaged a great portion of the eastern coast of South America, making its appearance for the first time in 1849-50, along the whole coast of Brazil. The populous cities of Baiha, Pernambuco, Parà and Rio de Janeiro, were smitten with great violence, and it is asserted by some authorities that Rio lost 13,000 of her inhabitants by this visitation, the disease reappearing in 1851-2-3. During the four years between the appearance of the disease in Rio (1849), and the great epidemic in New Orleans (1853), North American intercourse with Rio, as indicated by the value of the coffee importation, stands for these two dates as seven to eleven millions of dollars. Surely if there be any utility in quarantine, we were incurring also greatly augmented danger from these wide-spread extensions of yellow fever. Without quarantine we could scarcely have hoped to escape the fatal importation. But it is clear, in regard to the pestilence, that it cannot be said we have had our coffee charged

.. With some mixture powerful o'er the blood,  
Or with some dracm conjured to that effect,"

for we are told in the lucid report of the Committee of the Sanitary Commission, "that from none of the vessels, either from Rio or Jamaica, have we been able to find a case of fever or death of all those occurring in May, or up to the period when it commenced to prevail as an epidemic, either among the crew or among those who worked on these vessels. If then, the fever originated from other than local causes, it must have done so on the Northampton, an emigrant ship from Liverpool direct, a conclusion rendered the more inevitable by all the antecedent facts." "The first case of fever that died came from the Northampton."

The truth is, from the very beginning the importation doctrine fails of proof. That yellow fever was not originally imported from Africa, but was found by Columbus and his men to their sorrow, to be local and indigenous in San Domingo, does not admit of rational doubt. It is but too evident that the existence of the disease in the New World has proven itself to be coëval with the landing of Columbus on its

shores. We extract the following from *Vol. II., Cycloped. Pract. Med., Art., Yellow Fever*, which refers to authorities, which are calculated to remove all doubt of the fact that the yellow fever, so far from having been originally imported into the West Indies in 1793, raged amongst the companions of Christopher Columbus in San Domingo in 1494.

“At page 337 of Dr. Bancroft’s Essay on Yellow Fever, we have evidence of the existence of the disease at St. Domingo in the year 1731, and in subsequent pages, of its having prevailed there epidemically in 1733–4–9–40–1–3. The insalubrity of that island was manifested soon after its discovery ; for it appears that the sickness among his men, gave Columbus great anxiety. It could have scarcely been expected that anything very precise as to the nature of the disease, from which they suffered, should have been transmitted to us. The latest historian, Washington Irving, merely informs us that “when they fell ill their case soon became hopcless.” \* \* \* \* Oveido, in his “*Historia General de las Indias*,” (ed. in folio, 1547, book ii., chap. 13,) speaks of a general mortality among Columbus’s people in 1494, which he attributes to the humidity of the island. He states that those who returned to Spain were of a yellow or ‘saffron color,’ that finding the country so unhealthy they objected to go there (book iii., chap. 4) ; and that in consequence three hundred convicts were at one time sent to St. Domingo. He adds, that if the king offered him the Indies he would not go there.” \* \* \* \* “Further details are given by Herrera (*Madrid*, 1601), as to the violence and suddenness of attack, etc., of the disease, which carried off so many of Columbus’s men at the time in question at St. Domingo ; and he refers to a letter (book iii., chap. 15), written in 1498 by Columbus to the King of Spain, attributing the sickness of his men on their first arrival, to peculiarities in the air and water.”

The worst said of the climate of San Domingo by subsequent writers is, that it is unhealthy to Europeans, owing to the violent heats and heavy rains ; though the heat is mitigated by the regularity of the sea-breeze, and the contiguity of the mountains. On the plains the thermometer rises to 96°, sometimes to 99° ; but in the mountainous tracts it seldom rises above 78° ; and in the most elevated parts, a fire is frequently necessary, etc.” (*Homans’s Cyclop. of Commerce, art. Hayti.*) No doubt the general insalubrity of San Domingo has been much exaggerated ; and though it is doubtless true that the country is *unhealthy* to *Europeans*, there is nothing whatever to show that in

this fine country Europeans are perennially attacked with anything resembling the rapidly-hopeless and fatal malady, which assailed the Columbian Argonauts. The "violence" and "suddenness" with which men were "carried off," the "yellow or saffron color" and the "sudden hopelessness," must have been something out of the *ordinary* standard of insalubrity of the climate. In fact, the conclusion, that the yellow fever prevailed there epidemically 365 years ago is inevitable.

Authorities will show that the most awful yellow fever epidemics occurred in various quarters, when commercial intercourse was comparatively in its infancy ; and Old Spain, since the year 1800, in the midst of her national and commercial decadence and comparative isolation, has been visited by this pestilence in eighty-nine different localities, notwithstanding her adhesion to quarantine regulations, with a rigidity that absolute governments alone can exercise.

But this is not all. The possibility of importing the conditions necessary to a yellow fever epidemic into the city of New Orleans, and of avoiding them by quarantine, which has been seen is without foundation—implies of course, also the possibility of *exporting* the same calamity to other communities in which we are not quarantined against, and here the same facts govern the question which apply to importation. In proportion to the numerical prevalence and malignity of the disease, and the frequency of intercourse, would be the danger and probability that New Orleans, during an epidemic, might create an epidemic in other localities, if the doctrine of imported yellow fever epidemics be true. Our population has increased ; our intercourse has increased ; and though the frequency of our yellow fever epidemics have not increased, yet when they do occur, they are almost sure to occur with an augmented numerical prevalence. Now, if the quarantinists are in the right, all communities with which we have frequent and increasing intercourse, and that do not quarantine against us, during our yellow fever epidemics, ought to be considered as subject to frequent and increasing danger. There is no proof, however, that the danger ever existed, and of course no proof that it has increased. Numerous Southern ports have not quarantined against us, and the same may be said of all the towns and cities of the whole Mississippi Valley, with transient and rare exceptions. The experience of thirty-three years conclusively proved that the yellow fever could not be rendered epidemic, or be in any manner propagated in

any part of the country, by carrying the disease out of New Orleans on board of steamers or otherwise, and, when in 1853, the yellow fever made its appearance in town and country, in six States of the Union, this experience was all that was necessary to satisfactorily prove that the yellow fever, wherever it that season appeared, originated, as it did in New Orleans, and as we are told in the expressive words of Dr. Axson, "*locally and spontaneously.*" Enough has been seen here, during twenty yellow fever epidemics, to show that the disease cannot be propagated by contagion, *for a single mile*, beyond certain local determined city boundaries, much less for thousands of miles, and during one epidemic after another, cases have been freighted in every direction into the interior, without effect.

Now, what is the position in which the great body of the medical profession in this city is placed, in the eyes of the wide world, by this establishment of quarantine? Nothing more nor less than that opinions and doctrines utterly obsolete here for the better part of half a century, have suddenly been embodied in our general medical creed; that all the results of our enlarged, accumulated, and painful experience vanished before the events of 1853, as illuminated by the radiant light of the Sanitary Commission, and that, in their investigation of an epidemic, which they found to be "of local and spontaneous origin," "not imported," and (introduc. p. x.) "not personally contagious," we have found satisfactory reasons for embracing the doctrines of importation, contagion, and quarantine. The quarantine law is very naturally supposed elsewhere to be "the very echo" of professional opinion here. Such is our *prima facie* position in the face of Christendom, while the picture it sets forth is the very reverse of the truth. Quarantine has been established here in defiance of medical opinion, and at war, as we have seen, with the whole category of possibilities and probabilities, that pertain to our medical history, under the simple force of an argument of a thirty-five thousand dollar potency. Medical opinion here has not undergone this retrograde metamorphosis; and it is high time the profession should come forward and announce the fact, and repudiate any such imputation.

Having spoken of the quarantine law in its antagonism to that progress which is justly expected to grow out of our locality and familiarity with yellow fever, we shall here introduce a few general reflections on the subject. And here, let me once for all, define my position. I have not written this article for the purpose of reflecting on the

official conduct of any place-man under quarantine law. I bear them all good will and personal friendship, which I am desirous to maintain. I shall hold no controversy with any of them, nor notice any replies, should my remarks call them forth. I assume the ground, which I indeed truly hold, that the stipendiaries of the Quarantine and Board of Health, are both personally and professionally, *bein fort comme il faut*.\* I write simply to give utterance to my own impressions on a subject with which I have been familiar, for well nigh a quarter of a century; and I have good reason to believe that the readers of the journal will, as on former occasions, be content to hear what I have to say.

A quarantine law, enacted thus in the very face of the antecedents to which I have adverted, is essentially vicious, and must necessarily come to an end, or endure only by being regularly defended, at *all hazards*, before the Legislature, after each epidemic, by an elaborate one-sided argument—especially as we have so many epidemics of “local and spontaneous origin,” and “not at all imported.” The case requires special pleading, or rather apology. The party for the defence has a direct interest in perverting and distorting facts, failing to see at all what others see clearly; condemning the authority of his *confrères*, and substituting dogmatism for truth—imagination for reality—importation for localization. Who is there in New Orleans bold enough to fight such a battle? None.

“ I dare do all that may become a man ;  
Who dares do more is none.”

The introduction of quarantine into England must be accredited to the celebrated Dr. Meade, who suggested it as a measure against the introduction of the plague, which, in his day, raged at Marseilles, and which he regarded as contagious. He brought forward the subject in 1720, in a work entitled “*A Short Discourse concerning Pestilential Contagion, and the Method to be used to prevent it.*” (*Pettigrew Med. Biog.*

\* We are bappy in the conviction, that we are not so terribly bad off in this respect as the people of Great Britain. Witness the following emphatic declaration of the Edinburgh Medical and Surgical for April, 1852, p. 498: “The administration of the laws relating to quarantine is entrusted to a set of persons whose ignorance can be equaled by nothing but their obstinacy. To these persons, offices of rank, authority, and considerable emolument are assigned; and, indeed, their authority rests upon, and their emoluments are derived from, maintaining the opinion and the principle that yellow fever is a contagious disease, and can be imported from the West Indies and South of Spain. So long as the gentlemen are in receipt of large sums of money for maintaining these opinions, it is altogether unreasonable to expect that they will ever relinquish these opinions, or be made to admit that yellow fever is not contagious, and cannot be imported. To expect such a concession, is to require too much from such frail materials as those of which men and physicians are composed.”

vol. 1 Art. 1. Meade.) The fiction and delusion attending this origin of quarantine, is quite in keeping with its subsequent history. It is known that the celebrated Defoe possessed, in a greater degree than any other writer that can be named, the power of arraying the most stupendous and incredible fictions, with all the semblance and circumstances of truth. In a work written by him, entitled, "History of the Plague," he gave ample scope to his busy and exuberant imagination, with all the gravity of doctor of medicine. The man who could wield time, place and circumstance, with such an air of candor and *naïveté*, as to satisfy the public that the ghost of Mrs. Veal had actually appeared to Mrs. Bargrau, could not fail to give every embellishment to his miraculous "History of the Plague," and it appears that the worthy doctor was most egregiously hoaxed by the very "man-Friday" on whom he had relied for a knowledge of that pestilence. We quote the following from the Edinburgh Review, July, 1853. The allusion to the Copland School of Philosophers is peculiarly just and appropriate.

"From very early times—from the days of Sir Thomas Brown, upon whose evidence that natural diseases 'were heightened, to a great excess, by the subtlety of the devil coöperating with the malice of those we term witches, at whose instance he doth these villanies,' Sir Matthew Hale convicted two poor women of witchcraft—from the days of Dr. Meade, upon whose advice quarantine was established in Great Britain, and who, as we learn from Sir Henry Ellis, was deceived by Defoe's fabulous 'Journal of the Plague;' the medical profession has betrayed a remarkable tendency to credulity on the subject of the origin and spread of epidemics, and has from time to time allowed itself to be deluded by follies much less interesting than the novel which beguiled the sagacity of Dr. Meade, and not more truthful than the 'great discovery of witches in Denmark,' upon which the philosophic inquirer into vulgar errors unwarily founded his opinion when giving, before the devout Lord Hale, that evidence which sent two helpless women to the gallows. Chronologically arranged, the tales of yellow fever epidemics would alone supply materials for an interesting chapter in the history of fiction. The curious in such literature will find them collected with great care in 'Copland's Dictionary of Medicine,' titles, 'Pestilence, Hæmagastric,' 'Protection from Foreign Pestilence.'"



We subjoin a few more remarks from the same article—an article in which the reviewer shows up the celebrated “Boa Vista” and “Eclair” question, so familiar to every reading member of the profession, rendering the conclusion irresistible, that the whole question, on its real merits, resolved itself into a pretence for *pecuniary indemnity, or rather a scheme of beggary*, before the British Parliament, which was successful, in obtaining from that august body, “a grant of money and supplies of different kinds,” for the Boa Vistans; and we are told that they were thereby “encouraged to repeat an experiment, which, on its first trial, had proved so successful.”

“Yellow fever, being a disease alien to our close soil, *our sole knowledge of it must be obtained from other climes*. But before giving credence to narratives of the rise and progress of yellow fever epidemics, we must be satisfied not only that our informants had the desire, the perseverance, the leisure, and the ability to elicit, but that they actually elicited the whole truth, and nothing but the truth. In a well educated, highly civilized community, with *free discussion*, and a numerous body of scientific men to observe and compare facts, and the time, order, and manner of their occurrence, this security may be obtained, but it *cannot be obtained elsewhere*. We must have all the facts before us; not the inferences or presumptions of a partisan *expecting preference*, if he can by fortuitous coincidences in the guise of cause and effect, fabricate a plausible case to prop a falling superstition, by whom, what is hoped, is confidently believed, and what is believed is confidently told. What he asserts confidently he is bound to prove clearly; if the evidence be defective, the deficiency cannot be supplied by conjecture, the failure is fatal.” \* \* \* \*

“This doctrine, of the spread of epidemics by contagion, though apparently originating with the vulgar, gradually insinuated itself into medical literature in a dark and superstitious age. But no one in our time has ventured to assert that he has ever personally traced the cause of an epidemic from its source by contagion; for no man has himself personally seen the introduction of an epidemic, and traced its progress from person to person, through an entire locality; and this supposed personal observation of the rise and progress of an epidemic, resolves itself, when closely examined, into the mere adoption, at second hand, of the reports, or rather gossip of others—the ignorant, the prejudiced, and the credulous, in the time of a panic.”

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"The uncertainty of the *period of incubation*, is the favorite instrument for tracing an epidemic to importation. When a swarm of cases nearly simultaneous, announces the outbreak of an epidemic, if any one of them can be connected, however remotely, with a low lodging, frequented by seamen, or with a bag of foul clothes, or an old jacket from a ship, this convenient *period of incubation* will remove all difficulties. Though the disease may not have manifested itself in this chosen instance, till some days after it had appeared in several others, this single case is antedated by such a prolongation of the incubation period, as may be necessary to make it precede its predecessors. This is accomplished by the process of self-deception, in the following manner: The epidemic was derived from a foreign source by contagion; this is the only case which can be connected with a foreign source; this, therefore, *must* have been the first case of actual seizure; but through some peculiarity in the constitution of the patient, which we must assume to have existed, though it cannot be discovered or explained, and which gave him the power of retarding the development of the disease, the period of incubation was, in this instance, some days longer than in those in which the disease was first manifested, and in which, moreover, the symptoms do not appear to have been *so satisfactorily marked*."

In establishing quarantine here, we are thus falling back into the darker epochs of science, commerce, and civilization, out of which the whole of Christendom has emerged with such rapid strides. If we cannot learn by experience and observation, we would do well to look for light to the "mother country." A city in which the yellow fever agency shows itself either epidemically or sporadically, every year, has assumed the solemnly ludicrous position that there is *danger from importation!* The city of Manchester, that ought to have everything to fear in this behalf, and that has never been visited by yellow fever, is so far in advance of ourselves in the investigation of the subject, that through her Chamber of Commerce, she has boldly proclaimed "*that the quarantine laws of Great Britain are an irksome incumbrance, obstructing national intercourse, perilling life, fostering and engendering disease, and squandering large sums of public revenue.*" When we announce, that during the five years' reign of our New Orleans quarantine, we have been afflicted with THREE deadly epidemics; we are told, in reply, that these visitations had their sole origin in a want of *enlarged powers and authority*, on the part of the quarantine administra-

tion. Such is the manner in which this wretched incubus on commerce gasps for life. The "increased-powers-argument," is one which admits of no solution, as we travel upwards towards the infinite. Omnipotent power, is the only power which may prevent epidemics; but no temporal power ever yet delegated to any sanitary body, in the most absolute government, has succeeded any better than our present quarantine, with all its yearnings for power. If more power is to be granted, let it be extended to life and limb at once; for all powers inferior to this have been weighed in the balance and found wanting. Indeed, I believe the "Ottomites" can bear witness to the inefficiency of powers thus enlarged and amplified. I shall attempt to show, when I undertake to discuss the aetiology of yellow fever, that as little as we truly know of the material agent by which the phenomena of yellow fever is excited, we know quite enough to determine: 1st, that the disease is not propagated here by importation; 2d, and admitting the wholly untenable ground, that the disease may be imported, and that it may spread by contagion, that said agent has none of those chemical, physical, or other properties known to agents that may be decomposed, neutralized, or annihilated by ventilation or fumigation; and that the said agent, as it manifests itself here, has none of those qualities which enable us to refer its origin to the terrene and skiey influences to which it has been attributed.

(*To be continued.*)

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## PROGRESS OF MEDICINE.

### ART. I. *Lithotriety.*

[THE following Report comes from a high source, and, like everything else from Mr. Brodie, it bears the impress of candor, truth, and an unbiassed judgment.

There can be no doubt that this method of operating for stone in the bladder has been too little practised in this country, and that many lives are sacrificed by a resort to the knife, that would be saved under the judicious use of the forceps. Mr. Brodie, who gave me a

copy of this Report, was very earnest in his expressions in favor of this method of operating in adults, and felt sure that with proper care in the use of the forceps, and a little patient perseverance, relief can be afforded with a great degree of certainty. I do not think, however, that it is necessary to follow Mr. Brodie's directions in all cases, for manipulating to get the stone between the blades of the forceps. The object is to do as little violence to the prostate and bladder as possible; and, having witnessed the operation in a number of cases by M. Civiale, I am satisfied that the stone may be seized more readily, and with as little, if not less, violence to the prostate, in many cases, by turning the beak of the forceps downward, behind the prostate and towards the base of the bladder, than by the method of Mr. Brodie. M. Civiale has improved his forceps. The sliding blade has a shield upon its screw, so that it moves without noise or jar, which in the ordinary instrument with screw power tends to obscure the nice sense of touch, so necessary in feeling for a small or soft stone. As soon as the stone is caught between the blades, by turning the handle it is at once fixed, and the screw is brought to bear.

W. S.]

*Notes on Lithotrity, with an Account of the Results of the Operation in the Author's Practice:* By SIR BENJAMIN C. BRODIE, BART., D. C. L., V. P. R. S., Corresponding Member of the Academy of Sciences of the Imperial Institute of France, Serjeant-Surgeon to the Queen, Surgeon to His Royal Highness Prince Albert, formerly Surgeon to St. George's Hospital, etc.

ALTHOUGH the operation of lithotrity has been for many years extensively and successfully practised in a neighboring country, it has hitherto been adopted only to a limited extent by British surgeons; yet, if I may be allowed to judge from the results of my own experience of it, it must be regarded as a method of relieving those who suffer from calculus of the bladder, which is attended with much less uncertainty and danger, being at the same time much less formidable to the patient, than the operation of lithotomy.

\* \* \* I propose, first, to communicate, in a series of notes, some observations as to those circumstances which are especially deserving of attention, as affecting the success of the operation; secondly, to give a brief but accurate account of the actual results which I have met with in my own practice, from the time when I first performed the operation, down to the present period; and, lastly, to add some observations as to the amount of danger involved in the removal of calculi by crushing, compared with that which belongs to lithotomy. I can have no better opportunity than the present of offering such a communication, as, having for some time declined the operative part of my profession, it is not probable that any further

delay will enable me to make any very important additions to whatever knowledge of the subject I now possess.

§I. 1. I have never performed the operation in young persons below the age of puberty. The operation of lithotomy in the cases of children is so generally successful that it has not appeared to me that, as to them, any deviation from the old method of treatment is required; while, on the other hand, the small capacity of the urethra, and the difficulty which must attend the introduction of a forceps sufficiently large to crush a calculus of any considerable size seems to render them unfit subjects for lithotrity.

There is another very sufficient objection to the last-mentioned operation in children; namely, that instead of being completed at once, as in the instance of lithotomy, it is for the most part necessary that it should be repeated several times, and that such a repetition requires a degree of patience and self-control, and thought of the future, which children can scarcely be expected to possess.

2. I have not myself performed the operation of lithotrity on females. That I have not done so has arisen partly from the circumstance, that in them cases of calculus of the bladder are rare, as compared with those which occur in the other sex—partly from the facility with which the female urethra may be dilated so as to admit of the extraction of a calculus of moderate size, and partly from the belief that the bladder would not easily retain the quantity of water necessary to the safety of the operation. I am, however, inclined to believe that as regards the last-mentioned objection I have been in an error, as Mr. Fergusson informs me that he has crushed calculi in the female bladder without difficulty, and with perfect success.

3. The forceps which I have been in the habit of using is that made by Weiss, with a handle sloping downwards and a simple screw. For ordinary cases a forceps of very moderate size is sufficient; for cases of very large calculi it is prudent to use a larger and stronger instrument. I have never met with a single instance in which, however large it might be, a calculus once seized by the forceps did not yield readily to the pressure of the screw, or in which any harm happened from the bending or breaking of the instrument. I have lately seen a forceps in which the force intended for crushing the calculus is applied by means of a short cylindrical rod, introduced laterally, with some projecting teeth acting on a rack on the anterior part of the sliding blade. Such an instrument may afford some small advantage in the way of greater expedition, and so far may not be objectionable in cases of very small calculi; but it is evident that, as compared with the forceps with a screw, it must be very deficient in power, and that it would be very unsafe to have recourse to it in cases of calculi beyond certain dimensions.

4. The peculiar shape of the extremity of the forceps (without which it would not answer the intended purpose) renders it less easy to be introduced into the bladder than an ordinary sound or catheter, and a certain degree of force is generally required to enable it to pass through the urethra, where it is surrounded by the prostate gland,

such as is not at all necessary in using either of the last-mentioned instruments. The manœuvre necessary for this purpose can not well be described in words, but it is easily acquired, and it may be learned by practising on the dead quite as well as by practising on the living body. It is only in cases of considerable enlargement of the prostate gland that any real difficulty in the introduction of the forceps exists; but even in such cases I have never met with an instance in which the difficulty was not overcome by a cautious and gentle manipulation, nor with any in which any injury was done to the neck of the bladder in this part of the operation. In using the forceps it is important that no attempt should be made to open the blades until the sudden cessation of the resistance offered by the prostate gland shows that they have fairly entered the bladder. The blades may be partially separated while a portion of the curved extremity is still in the urethra; but a certain effect of this is, that the patient is subjected to much unnecessary pain, at the same time that there is great danger that the injury thus done to the verumontanum and orifices of the seminal ducts may induce inflammation of one, or even of both of the testicles.

5. No prudent surgeon would willingly undertake the operation unless the bladder admitted of the injection and retention of from at least four to six ounces of tepid water. In cases in which the bladder has lost its power of retention, it may generally be restored by keeping the patient for a week or fortnight in the recumbent posture, and at the same time injecting some tepid water once daily into it. This method has failed with me only in a single instance. In consequence of the failure I had recourse to lithotomy, and extracted a very large calculus, composed of the oxalate of lime, with angular projections on its surface, which sufficiently explained the irritable state of the bladder. The patient died in a few days after the operation.

Although, as has been just observed, the operation should never be attempted without the bladder having been previously distended with tepid water, it is worthy of notice that it has occurred to me sometimes to find that during the operation the whole of the water which had been injected had escaped by the urethra, without my being at the time aware of the circumstance, so that many fragments must have been crushed in an otherwise empty bladder. On such occasions I have always been apprehensive lest some ill consequences should ensue. It is true that this did not happen in any one instance; still it is a thing that ought to be carefully avoided, as it is plain that any but the most careful manipulation with the forceps would be dangerous under these circumstances.

6. The most important part of the operation is undoubtedly that of seizing the calculus with the forceps; and this may be dangerous and difficult, or safe and easy, accordingly as the surgeon is or is not acquainted with the right method of proceeding. The rule should be to *move the forceps in the bladder as little as possible*, never using it as a sound for the purpose of exploring the bladder or ascertaining the position of the calculus. Such an examination does not assist the surgeon in seizing the calculus afterwards: it gives pain to the

patient, excites the bladder to contract and expel the water which had been previously injected ; and I know that instances have occurred, though not in my own practice, in which a rough handling of the forceps has caused great injury to the bladder, ending in the death of the patient. The rule for seizing the calculus (which I must acknowledge to have first learned from witnessing the very dexterous operations of M. Heurteloup,) is as simple as possible. The patient lying on his back, the handle of the forceps is elevated, which, of course, brings the convex part of the curved extremity of it in contact with the posterior surface of the bladder, where it is contiguous to the rectum. The forceps is then to be opened, by withdrawing the sliding blade to a greater or less extent, according to the probable size of the calculus, the fixed blade being at the same time pressed gently downwards, in the direction of the rectum. The object of this manipulation is, that the forceps, being below the level of the other parts of the bladder, the calculus may fall into it by its own weight, and it is generally successful. If it should not do so, the forceps, without being moved from its situation, may be gently struck with the hand on one side, or on its anterior part, and the slight concussion thus communicated to the bladder will probably be sufficient to dislodge the calculus, and bring it within the grasp of the instrument. If it should be otherwise, the forceps being closed, may be very gently and cautiously turned to one side or the other, so that the curved extremity of it may make an angle of twenty-five or even thirty degrees with the vertical line of the body, then opened, and pressed in the direction of the rectum in the manner already described. When the prostate gland is much enlarged, there is sometimes a difficulty in seizing the calculus, arising either from it lying under that part of the gland which projects into the bladder, or from the impediment which it offers to the elevation of the handle of the instrument. For such cases the operating table invented by M. Heurteloup, which enables the patient's shoulders to be suddenly lowered, is very convenient ; or the same purpose will be answered sufficiently well if the patient be placed on a light sofa, the end of which may be raised by an assistant. The calculus is then seized, not in that part of the bladder which adjoins the rectum, but in the fundus, this being rendered the lowest point by the elevation of the pelvis. If the calculus does not at once drop into the blades of the forceps, the patient may be directed to walk round the room, or to change his position as he lies, by turning first on one side and then on the other, the bladder being afterwards emptied by the catheter, and then again injected with tepid water. By these means the calculus is made to change its position, and may often be readily seized, though it could not be so previously.

7. In the early part of my practice, I was accustomed to use a forceps so constructed that a greater or smaller portion of the crushed calculus was always retained between the blades. In this way a considerable quantity of calculous matter may be removed at each operation, and the patient may, to a great extent, be saved from the trouble of passing it afterwards with the urine. There are, however, some very grave objections to this mode of proceeding. The

withdrawing of the forceps, if much loaded with calculous matter, stretches the urethra beyond its natural diameter, and in so doing, not only gives the patient much pain at the time, but renders him liable to rigors afterwards; secondly, in four instances in which I had adopted this practice, the urethra was torn, and an infiltration of urine into the surrounding tissues, followed by urinary abscess, was the consequence. Two of these patients, in whom the mischief produced was deep in the perineum, died, notwithstanding the abscesses having been freely opened as soon as they were detected. The experience of these cases led me some years since to discontinue the use of the forceps already referred to, or, at least, to have recourse to it very rarely, and only under some special circumstances, and to substitute for it a forceps made by Mr. Weiss, in which there is a longitudinal opening in the curved part of the fixed blade, with a corresponding projection in the opposite or sliding blade. The effect of this instrument is to crush a calculus very completely, and in such a way that no part of it remains between the blades, the whole being left to be passed with the urine afterwards. The ultimate cure of the patient may, in some instances, be thus a little (but not greatly) protracted; but this inconvenience is more than compensated by the smaller amount of pain which the patient suffers, the smaller liability to rigors, and the complete absence of danger from the infiltration of urine and perineal abscess.

8. An important question arises, to what extent may a calculus be crushed at one operation. In the dead body the largest calculus may at once be reduced to atoms. But it would be very unsafe to attempt to do this in the living person; first, the time during which the bladder will retain the water injected into it is limited—and, for the most part, it begins to escape in a few minutes after the introduction of the forceps; secondly, the only part of the operation during which the patient much complains of pain is the instant of the attempt being made to seize the calculus, the pain then evidently arising from the pressure of the forceps on the neck of the bladder. Now, I have observed, that where the crushing of the calculus had been, during one operation, very frequently repeated, more or less inflammation has always followed, extending from the neck of the bladder to the mucous membrane, causing a great mucous secretion, and preventing a repetition of the operation for a considerable time afterwards; and hence I have come to the conclusion that it is a more prudent, and in the end a more expeditious mode of proceeding, to be content, on any one occasion, with crushing the calculus only to a moderate extent. At the same time, it is plain that no general rule can be laid down as to how much can be prudently accomplished. The surgeon must use his own discretion, being guided chiefly by the amount of inconvenience which the patient suffers. In one case, twelve or fifteen fragments may be safely crushed, in another not more than six or seven. The consideration of what has now been mentioned leads me, on the whole, to prefer not putting the patient under the influence of chloroform, as it prevents the patient from describing those sensations, which may help to direct the surgeon in his practice.

9. There is another reason for not being anxious to do all that can



possibly be done at any one operation, namely, the greater liability that this occasions of the operation being followed by a rigor. For the same reason, it is important that the urethra should not be stretched beyond its natural diameter, either by the use of very large instruments, or by the withdrawing of the forceps loaded with the fragments of the calculus. It may be necessary in a few cases that a very narrow urethra should be dilated, but this should be gradually accomplished by the introduction of bougies, the operation itself being deferred until the dilatation is completed. In all cases, however, there is more or less risk of the occurrence of a rigor unless due precautions are taken to prevent it. With this view, I always advise that the patient should go to bed afterwards, with an additional blanket over him, the bed itself being previously warmed in cold weather. I was accustomed formerly to prescribe a dose of the *Pulvis Ipecacuanha Compositus*, or some other opiate, to be administered after the operation. For the last few years, however, I have preferred giving the patient a tumbler of hot wine and water, or brandy and water. It seems to me that this is more effectual in preventing a rigor than the opiate, at the same time that it has the advantage of not inducing costiveness or headache afterwards. I need scarcely add, that it is important on all occasions that nothing should be omitted which may tend to lessen the liability to rigor. Even in the most healthy persons, it is followed by more or less depression of the vital powers, and prolongs the interval which must elapse before the operation can be repeated; while, under certain circumstances, when calculus of the bladder exists, in combination with other disease, it may be actually dangerous to life. I shall have occasion to refer to this subject in reference to the complication of renal disease hereafter.

10. When a calculus has been crushed of so small a size that two or three applications of the screw were sufficient for that purpose, it may be that the fragments will be immediately passed with the urine, so that the bladder may be quite free from them, and the cure of the patient completed in the course of two or three days. When, however, it has been necessary at any one operation to repeat the crushing process several times, for the most part very few of the fragments, and sometimes none at all, are passed during the first three or four days, or it may be even for a week. The explanation of this I apprehend to be, that the pressure of the forceps on the neck of the bladder, which I have already mentioned as the principal inconvenience to the patient at the time of the operation, occasions some degree of inflammation and tumefaction of the prostate gland afterwards, sufficient to prevent the escape of the fragments in the first instance. That nothing may interfere with the neck of the bladder recovering its natural condition, it is desirable that the patient should remain quiet for the first two or three days on a sofa. Afterwards, with a view to the more free discharge of the fragments, I have generally advised him to walk gently about the house, or even a short distance out of doors, during a part of the day, and to drink plentifully of barley-water or some other diluting liquor. At the same time, all violent exercise, such as rapid walking or running, should be avoided, as it might occasion the urethra to be blocked up by the too rapid escape of the frag-

ments from the bladder. The patient, in some instances, suffers a good deal of inconvenience from a fragment of the calculus being lodged in the urethra, especially in the membranous part of the canal. Various kinds of forceps have been contrived for the purpose of removing the fragments under these circumstances, and I have in two or three instances used them with advantage where a calculus had been lodged in the space between the glans and the scrotum. I cannot say that the forceps may never be required where a fragment is lodged in the posterior part of the urethra, or even that it may not be necessary to extract it by an incision in the perineum. Such a case, however, has not occurred in my own practice. It so happens that I have always succeeded in dislodging the fragment by two or three times introducing a rather small elastic gum catheter, and thus altering the position which it occupied in the urethra. I may take this opportunity of observing, that it is very important to avoid cutting down on the urethra immediately in front of the scrotum. In two instances in which I thus removed a calculus, not after crushing, but where it had been extracted from the bladder entire, by the method proposed formerly by Sir Astley Cooper, notwithstanding the immediate introduction of an elastic gum catheter, and the retention of it in the bladder, a small quantity of urine found its way into the cellular tissue of the penis and scrotum, causing a series of troublesome abscesses afterwards.

In an elderly person, in whom there is usually more or less enlargement of the prostate gland, the fragments do not come away so readily as in those who are younger. This especially happens where the patient has lost the power of emptying the bladder by his own efforts. It would be a mistake, however, to suppose that the incapability of expelling the whole of the urine prevents them from coming away altogether: still the process is more tedious, and requires some assistance beyond that which is required under ordinary circumstances. Firstly; the patient should be directed to void his urine stooping forward, or even in the recumbent posture, lying with his face downwards. Secondly; tepid water should be injected by means of a syringe, or elastic gum bottle, through a large silver catheter, having a wide aperture near its extremity on the concave side, by which means fragments below a certain size may be washed out of the bladder. This may be done daily, the injection being repeated on each occasion three or four times. In one instance, in which a complete retention of urine followed the crushing of a large calculus, in the course of two or three weeks the whole of the fragments were thus brought away, the patient regaining the power of emptying the bladder afterwards.

11. Where the presence of a calculus has excited inflammation of the mucous membrane of the bladder, indicated by excessive irritability of that organ, and a copious secretion of mucus from its membranous surface, I have generally deemed it prudent to defer the operation for a time, the patient remaining at rest in the recumbent posture until the inflammation has in some degree subsided. This state of things, however, unless the inflammation is excessive, does not form an absolute objection to the operation, though it may be a reason for proceeding more cautiously than if the bladder were in a perfectly

healthy state. Indeed it often happens that the first crushing of the calculus by distributing its weight over a larger surface of the bladder, is followed by a marked improvement of the symptoms, the pain which the patient suffers being immediately diminished, and the mucous secretion reduced in quantity.

In the great majority of instances the crushing of a calculus may be repeated at moderate intervals, without any signs of inflammation, or any very large secretion of mucous being induced. In other instances it is quite otherwise. This may depend on circumstances altogether beyond our control, some individuals being more and others less susceptible of inflammation. Much, however, depends on the operator. I have already observed that if, from an over anxiety to obtain a speedy cure, the operation on any occasion be much prolonged, the probability of such ill consequences is much increased: and I need scarcely add, that the same result is to be expected from any rough manipulation with the forceps.

12. It has been objected to this mode of removing calculi that there is always a risk of a fragment remaining in the bladder, and causing a recurrence of the disease: and it is quite true that this may well happen without great caution and vigilance on the part of the operator. A single small fragment may escape detection, however carefully the examination of the bladder may be conducted, especially where there is any considerable enlargement of the prostate gland. Still it may be detected at last, and the patient should not be finally dismissed as cured while any of the symptoms of calculus of the bladder remain. It has happened to me, in three different instances, to make more than one most careful examination of the bladder, without discovering a fragment which has at once fallen within the grasp of the forceps, when the examination has been repeated a few weeks afterwards. I may take this opportunity of observing, that where the calculus, or fragment of a calculus, is of a small size, the forceps affords a much more certain means of exploring the bladder than the common sound or catheter, the latter instruments being, in fact, very inefficient for this purpose.

In a case of diseased and sacculated bladder, it is plain that a fragment lodged in one of the sacculi, not only cannot be crushed, but may escape detection altogether. One such case has occurred in my own practice. A gentleman labored under a calculus of the bladder, complicated with an enlargement of the prostate gland, which prevented his emptying the bladder by his own efforts, and made the constant introduction of the catheter necessary. The calculus had been crushed, but much irritation of the bladder remained, which I was led to attribute to the disease of the prostate gland. After some months the patient died, and the body was examined by Mr. Bowling, of Hammer-smith, who found the prostate gland much enlarged; abscesses in the kidneys; a fasciculated and sacculated bladder, and a fragment of the calculus impacted in one of the sacculi.

13. There is some difference in the operation in different cases, depending on the chemical composition of the calculus. A calculus composed of oxalate of lime requires a greater force to crush it than one of the same size composed of lithic acid. It is important that one of

the former kind should be crushed as completely as possible, as otherwise the urethra is liable to be irritated by the sharp angles presented on the surface. A calculus composed of the triple phosphate, or of the mixed phosphates, or more especially of the pure phosphate of lime, is more easily crushed than one of lithic acid; so that if of a small size the mere pressure of the thumb will often be sufficient, without the application of the screw. When there is disease of the bladder, with a constant exudation of adhesive alkaline mucus from the lining membrane, phosphate of lime is continually being deposited in small masses like a soft mortar, which, if allowed to remain become aggregated, and form the nucleus of a large calculus. It is important that such a result should, if possible, be prevented. I have elsewhere published an account of a case of this description,\* in which a very large accumulation of deposits of this soft phosphate of lime had taken place, causing great suffering to the patient, but which I was enabled to remove by means of a forceps having the fixed blade made concave, so as to answer the purpose of a scoop, without the use of the screw; the bladder being regularly washed out afterwards to prevent any further accumulation from taking place.

14. That which I have just mentioned belongs to a class of cases in which a cure of the disease is out of the question; and in which the operation of lithotomy would be attended with so much danger that no prudent surgeon would undertake it; but in which, nevertheless, great advantage may be obtained by the use of the lithotripsy forceps. In the cases to which I refer there is a permanent and incurable disease of the bladder, induced originally by a tumor of the prostate gland projecting into it, or in some other way, the calculous matter being deposited, not by the urine, but by the secretion of the mucous membrane. When collected in a certain quantity, by irritating an inflamed and highly sensitive organ, it becomes a source of most intense suffering far beyond what occurs in ordinary cases of calculous disease. We have, for the most part, no means at our disposal, by which, in such cases, the deposit of the phosphate of lime can be altogether arrested; but, by watching the patient carefully, and removing from time to time the small calculi as they are generated, the sufferings which they would otherwise produce may be mitigated or prevented. Two or three such cases have occurred in my own practice, in which I was enabled, not ultimately to preserve, but to lengthen the patient's life, and so that he passed the remainder of his days not in a state of absolute ease and comfort, but with a far less amount of suffering than would have been the case otherwise.

15. It is not uncommon for the operation to be productive of a slight hemorrhage, sufficient to discolor the water drawn off from the bladder, or even the urine, for two or three days afterwards. The blood seems to proceed from the neck of the bladder, and probably arises from the pressure made by the forceps on the prostate gland in the act of seizing the calculus. I have never, in my own practice, met with hemorrhage to any considerable extent, or such as to interfere with the progress of the cure. It is plain, however, that in a

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\* "Lectures on the Diseases of the Urinary Organs," 4th edition. p. 301.

ease of enlarged and very vascular prostate gland a greater hemorrhage might occur under the most careful treatment : otherwise its occurrence must be regarded as evidence of the forceps having been too roughly used, without that degree of caution and lightness of hand which I have already mentioned as being absolutely necessary to the success of the operation.

16. The advantages of lithotripsy are certainly most conspicuous in cases of small calculi, where a cure may be effected by one or two operations, so that the patient, in the course of four or five days, finds himself free from the pain and anxiety belonging to a most formidable disease, and able to resume his usual occupation. There are undoubtedly cases in which the magnitude of the calculus is such that it would be impossible to seize it with the lithotripsy forceps, or to crush it if seized, and in which the patient is reduced to the alternative of a miserable death from the disease, or of taking the very small chance, which, under such circumstances, lithotomy affords him of a cure. Such cases, however, are, according to my experience, rare exceptions to the general rule ; calculi of very considerable dimensions may be crushed with ease and safety. I have, in some instances, when the calculus was seized, found that the blades of the forceps have been separated to the extent of an inch and a half, yet have found no difficulty in the operation, nor any other inconvenience than that of having to repeat it oftener than when the calculus was of smaller diameter. In the year 1844, in a case which the late Mr. Aston Key attended with me in consultation, from the length of time during which the symptoms had existed, and from such examination as we were able to make, we were led to apprehend that the calculus might be of so large a size that crushing it would be impracticable. However, we determined that the operation should be attempted. I had a forceps made for the purpose, larger and of greater power than those which I had previously used, and we were agreeably disappointed to find that it yielded at once to the force which I was thus able to apply, the fragments being afterwards gradually reduced by means of the ordinary instruments.

It must be observed, however, that where we have recourse to this operation it is difficult to determine the exact size of the calculus. We have no means of knowing whether it be seized in its longest or in its shortest diameter. The fragments which come away may be preserved and weighed, but these are no measure of what is actually destroyed, as many of the smaller fragments may be entangled and lost in the mucus, and so much of the calculus (especially where it consists of the phosphatic salts) is always reduced to a state of impalpable powder, which renders the urine opaque, but is otherwise imperceptible to the eye, though it may be detected by chemical analysis.

§ II. Having made these observations as to the points which principally demand the attention of the surgeon, in connection with the operation of lithotripsy, and having offered those suggestions which, according to my experience, are most important with a view to its favorable termination, I shall endeavor to communicate to the Society, as nearly as I can, an exact account of the actual results o

my own practice. In doing so it will be my especial object to describe, however briefly, the cases in which the operation, in my hands, has proved to be unsuccessful, and the causes which have led to its failure. I have the means of doing this with sufficient accuracy, as I have the advantage of referring not only to my own notes, but to those of my friend and former pupil Mr. Charles Hawkins, who has assisted me for many years past in the great majority of cases which have come under my care, and has preserved notes of every one of them.

From these two sources I obtain an account of 111 cases. There are a few others of which I have no written memoranda; and these being scattered over a period of twenty years, I cannot charge my memory with an accurate recollection of them. They are probably as many as nine or ten; but that I may not incur the risk of magnifying the extent of my experience, I will set down the whole number of cases under my care as being no more than 115. I am aware that this is a small number compared with those which have been treated by my friend M. Civiale, and some other surgeons in Paris, who devote themselves especially to this branch of our profession. It is, however, I apprehend, quite sufficient to enable any one who is a careful observer to make himself acquainted with all the more important points of practice.

When I speak of having performed the operation in 115 different cases, I do not mean to say that these occurred in the same number of individuals. One great advantage which this operation has over that of lithotomy, is that, being less formidable to the patient, he is not unwilling to undergo it again and again if there should be such a liability to calculus disease as to render it necessary. There was a gentleman whom I attended, in conjunction with the late Dr. Prout, in whom the tendency to the deposit of lithic acid was so great that none of the methods commonly in use, nor any others which we were able to devise, had any sensible influence over it. Originally the deposit had been in the form of lithate of ammonia, the surface of the vessel being always stained of a red color. Afterwards it assumed that of renal calculi. Several of these were voided from time to time, while others, being of greater magnitude, remained in the bladder, and required to be crushed. The operation was performed on this patient as many as eight times in the course of the same number of years, always with complete success. He died at last the victim of cerebral disease;—a not uncommon termination of those cases in which there is so great and uncontrollable a disposition to the production of lithic acid in the blood. In another patient, who was also under the joint care of Dr. Prout and myself, there was a similar liability to the formation of renal calculi; but they were generally of a small size. I removed at various times, probably more than one hundred calculi of the size of hemp-seeds, by means of the scoop-forceps; but on two or three occasions, the calculi being of a larger size, it was necessary to crush them. In three other cases I had to perform the operation a second time. It is to be observed that I refer here to cases in which there could be no doubt that the calculi were altogether fresh formations, and that I carefully exclude the

very few instances that have occurred to me, in which a recurrence of the symptoms might be reasonably attributed to some fragment or fragments having been overlooked in the first instance.

It would be needless to enter into the details of the very large majority of cases which have been under my care, the patients having obtained complete and permanent relief without any dangerous symptoms or other ill consequences. Those which form an exception to this general rule, may be distributed under the following heads: first, cases in which there was a fatal result, arising solely from the operation itself; secondly, cases in which a similar result was to be attributed to the coëxistence of some other disease, brought into an active state by the shock of the operation, but which disease would in itself have been sooner or later the cause of death, even if the operation had not been performed.

The cases which belong to the first of these divisions are five in number. I have already referred to four cases, which occurred in the early part of my practice, in which the urethra had been lacerated in withdrawing the forceps loaded with calculous matter. In two of these the patients died. In one of them, urinous effusion took place behind the triangular ligament of the perineum, causing extensive sloughing of the cellular membrane in the immediate vicinity of the prostate gland and neck of the bladder. In another case, the effusion took place in the perineum behind the scrotum. As soon as it was discovered the perineum was freely divided; but great constitutional disturbance followed, attended with acute inflammation of the lining membrane of the bladder, and an immense secretion of adhesive mucus, under which the patient sank and died. In a third case, a calculus of very moderate size was crushed without the smallest difficulty. The operation was followed by a rigor, which, not being relieved, as is usual, by a perspiration, terminated in an attack of fever, and ultimately in the patient's death. There were no local symptoms whatever; the bladder being free from irritation; the urine remaining acid, free from mucus and pus, and there being no indication of renal disease. It certainly may have been otherwise, but it seems to me that the circumstances are best explained by supposing that the febrile disturbance of the system, of which the rigor was the precursor, was the actual cause of death. Unfortunately, the objections of the patient's friends to a *post-mortem* examination, prevented my having the opportunity of obtaining any positive information on the subject. In a fourth case, a very small calculus was crushed with great ease by a single operation. The operation was succeeded by a rigor, which terminated, as is usual, in a perspiration. A disturbed state of the system followed, marked by a frequent pulse, a furred tongue, and much prostration of strength; and attended with a deposit of adhesive mucus, but not in large quantity, from the urine. These symptoms continued, and after some time an abscess presented itself in one groin. The abscess having been opened, a considerable discharge of matter took place, and was followed by great relief as to the general symptoms. The patient seemed to be in an improving state, when, between four and five weeks after the operation, he suddenly expired. On examining the body, no remains of the calculus

were discovered in the bladder. The mucous membrane of the bladder bore marks of inflammation. There was an abscess of the pelvis occupying the space between the bladder and rectum, and extending in the direction of the abdomen as high as the groin, in which the puncture had been made. The parts were carefully dissected and examined by the late Mr. Vincent (who had attended the patient with me in consultation), by Mr. Charles Hawkins, the Curator of the Museum of St. George's Hospital, and myself, but no lesion could be detected of the bladder, nor any kind of communication between the bladder or urethra, and the abscess. Still I cannot doubt that the abscess was somehow the result of the operation. Probably a very small splinter of the calculus might have penetrated the coats of the bladder, allowing the escape of a very minute quantity of urine into the cellular membrane. This would be quite sufficient to account for an extensive suppuration, at the same time that it is easy to suppose that so small a puncture might at once have closed, so as to be invisible afterwards.

The fifth case was that of an elderly gentleman, in whom, after a calculus had been twice or three times crushed, inflammation of the mucous membrane of the bladder ensued, attended with little pain, but with a very large secretion of mucus, and great prostration of his bodily powers, under which he gradually sank and died.

In the three cases which follow, there seems to be no reason to doubt that the fatal result, though it immediately followed the operation, is more to be attributed to the existence of renal disease than to anything in the operation itself. Still, as in each of them the patient might have lived longer if it had not been had recourse to, we must admit them as examples of the operation being unsuccessful.

In one of these cases the patient, though apparently in good health otherwise, had been for some time liable to attacks of severe pain in the loins. A calculus, of a moderate size, was crushed without any difficulty, some of the fragments being withdrawn with the forceps afterwards. The operation was followed by a rigor. On the following day there was an attack of most intense agonizing pain in the loins, attended with a very frequent pulse, which lasted for several hours, and could be relieved only by opium. Several attacks of the same kind succeeded each other at various intervals, one of them being ushered in by another severe rigor. At first, on the pain subsiding, the pulse returned to its natural standard; afterwards there was no remission of the febrile symptoms; the patient was alternately drowsy and delirious. He gradually sank, and died twelve days after the operation. The body was examined, with the assistance of Mr. Henry James Johnson, who attended the patient with me. Some fragments of the calculus which had been crushed, with three or four other calculi of a small size, were found in the bladder. The bladder and urethra, and parts immediately in connection with these, were in a perfectly healthy state. The left kidney was enlarged to nearly double its natural size, and of a dark red color, from excessive vascularity. The investing membrane adhered more closely to the surrounding adeps than to the kidney itself. When it had been removed, numerous small deposits of straw-colored lymph were found interposed



between it and the glandular structure. The mucous membrane of the pelvis and infundibula was inflamed, and contained a considerable quantity of muceo-purulent fluid. There was no disease of the right kidney.

Another case was that of a gentleman who, besides the usual symptoms of calculus of the bladder, suffered from the effects of an enlargement of the prostate gland, which prevented him from being able to empty the bladder by his own efforts. The urine, when drawn off by the catheter, was turbid, and deposited some mucus afterwards.

After the first crushing of the calculus the patient had a rigor, which, however, was followed by no ill consequence. The bladder was emptied at regular intervals by means of a catheter having a large aperture, at the same time being washed out daily with tepid water. In this manner a large quantity of the broken down calculus, consisting of the mixed phosphates, was brought away.

The operation was not repeated until after the expiration of a fortnight. It was again followed by a rigor, and by another on the succeeding day. From the effects of this last rigor the patient never recovered; and he died on the third day after the second operation. On examining the body after death, the prostate gland was found enlarged, and forming a tumor, projecting into the bladder, of the size of an ordinary plum. The mucous membrane was rather more vascular than under ordinary circumstances, and the muscular tunic was somewhat increased in thickness, but not more so than in most other cases of enlargement of the prostate gland. In other respects the bladder and urethra were in a sound state. The kidneys were of a dark red color, from excessive vascularity; and, on separating the investing membrane, numerous small deposits of pus were found beneath it, imbedded in the glandular structure.

In the following case, as long a period as six weeks elapsed between the last operation and the death of the patient; nevertheless, I cannot do otherwise than refer it to the same class as the two preceding; as, although he labored under a disease from which he could not have ultimately recovered, we must suppose that the fatal termination of it was hastened by the operation. The patient was under my care in St. George's Hospital, laboring under calculus of the bladder, complicated with general ill health, but at first having no symptoms indicating any immediate danger. The calculus was crushed by five operations, at intervals varying from five to thirteen days. During this process he had several severe rigors. From the time of the last operation he was free from pain, but the urine was alkaline, depositing a good deal of adhesive mucus, with great general debility, increasing until he died.

On examining the body, one kidney was found to be enlarged to double its natural size, with numerous abscesses, from the size of a pin's head to that of a pea, imbedded in the glandular structure. The mucous membrane of the pelvis and infundibula bore marks of inflammation, and was encrusted with phosphate of lime. The opposite kidney exhibited similar marks of disease, but to a less extent. The prostate gland was a good deal enlarged; the mucous membrane of the bladder was inflamed, and the muscular tunic thickened.

As it is my wish to give an exact account of the results of my own practice, I shall add to this list of casualties a notice of another case in which the death of the patient took place shortly after the operation ; but under circumstances which make it at the least very doubtful whether the operation had in any way contributed to it. The patient was an elderly gentleman, and the calculus was of a large size. It was crushed without any difficulty, but several operations were required, occupying altogether a period of three or four weeks. No unfavorable symptoms presented themselves, except that the patient, who was naturally of what is called a nervous temperament, was on the whole in a desponding state of mind, taking an unfavorable view of his own condition. However, he seemed to have perfectly recovered, and had begun to resume his usual habits, when he was seized with a severe attack of diarrhœa, which did not yield to the usual treatment, and after a few days proved fatal. Mr. Keate had seen the patient with me in consultation, and Mr. Charles H. Johnson had assisted me in the various operations, and in my absence from London, they examined the body without finding any morbid appearance except that one kidney was somewhat larger and more vascular than under ordinary circumstances.

I am not aware that among the cases which have been under my care, there have been any besides those which have been now enumerated, in which the operation can be considered as having either caused or accelerated the patient's death. In by far the greater number of instances the entire destruction of the calculus is immediately followed by relief from all the former symptoms, the bladder being no longer irritable, and the urine being free from mucus. It occasionally happens, however, that after the cure is completed, or supposed to be completed, the mucous membrane of the bladder remains in a more or less inflamed state, indicated by a frequent desire to void the urine, and a deposit from the latter of adhesive mucus. This may arise from a fragment of the calculus having been overlooked, in which case the symptoms will continue until such fragment has been detected and crushed. It may arise also from the calculous disease having been complicated with disease of the prostate gland, or of the bladder itself ; and it is, moreover, one of the numerous ill consequences of a too rough manipulation on the part of the surgeon, or too long protracted operations. The symptoms here referred to, if merely the result of the operation, generally subside in the course of a fortnight, with little other treatment than that of keeping the patient in a state of repose, and chiefly in a recumbent position on a sofa.

§ III. It would be unreasonable to expect that any method of treatment for the relief of a disease so certainly fatal if left to itself, and productive of so much misery, as calculus of the bladder, should be constantly and uniformly successful. If lithotomy has its dangers, lithotripsy has its dangers also ; and the only question for the practical surgeon to consider, is, which is the least dangerous of the two. Of the nine cases which I have enumerated it may well be doubted as to one of them whether the attack which was the immediate cause

of the patient's death was really connected with the operation ; while in two others, the fatal result was to be attributed to a mode of performing the operation which my later experience has led me to abandon. But, even if we admit the whole nine cases as a fair example of the average failure of the operation, the proportion of deaths to recoveries is somewhat less than 1 in  $12\frac{1}{2}$ .

In order that I might compare this with the proportion of deaths from lithotomy, I have referred to a paper by the late Mr. R. Smith, of Bristol, entitled "*An Inquiry into the Statistics of Stone in the Bladder,*" and I there find it stated, that in the Bristol hospital, during a series of many years, the average of deaths after lithotomy was 1 in  $4\frac{1}{2}$  ; in the hospital at Leeds, 1 in 5 ; and in the hospital at Norwich, 1 in  $7\frac{1}{4}$ .

Thirty-five years have elapsed since the publication of these statements, but there is no reason to believe that the success of lithotomy is greater now than it was when Mr. Smith collected his observations. The editor of a weekly journal (the Medical Times and Gazette), has for some time past published the statistics of the various operations performed in the London hospitals, including lithotomy. The facts seem to have been collected with some care, and are probably a near approximation to the truth. Mr. Charles Hawkins has been at the pains to collect from the various numbers of that journal published during the year 1854, the facts relating to lithotomy, and it appears that of 59 patients who underwent that operation as many as 10 died, being in the proportion of rather more than 1 in 6.

But here two other facts must not be overlooked, without which no just comparison can be made of the results of the two operations. First, while cases of vesical calculus in children under the age of puberty, in private practice, and among the more affluent classes of society are of rare occurrence, they form the very great majority of those which are admitted into hospitals ; and, secondly, the proportion of deaths after lithotomy among children is very much less than it is among adults. Both these facts are sufficiently obvious to those who have had the opportunity of witnessing the practice of our larger hospitals. From the data furnished by the medical journal to which I have already referred, it appears in the London hospitals during the last year children formed a small fraction more than three fourths of the whole number of those who underwent lithotomy ; that among them the deaths were in the proportion of 1 to 14 recoveries ; while among adults the deaths and recoveries were equal. That in this instance the large proportion of deaths among adults was beyond the average, and depending on accidental circumstances, cannot well be a matter of doubt, and indeed it is plain that no general rule can be drawn from the limited number of cases which occur in the space of a single year. Still, as even in the Norwich hospital, where there is reason to believe that lithotomy has been on the whole more successful than in any other public institution, the proportion of deaths among adults is reported to have been four times as large as that among children\* it is evident that the difference in the degree

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\* "Medico-Chirurgical Transactions," vol. xi., p. 32.

of danger at these two periods of life is very great ; and it must always be borne in mind that, in estimating the comparative value of the two operations, it is only the results met with in adults that should enter into our calculations.

My own experience has certainly led me to the conclusion, that lithotomy, *if prudently and carefully performed, with a due attention to minute circumstances*, is liable to a smaller objection than almost any other of the capital operations of surgery. The cases to which it is not applicable are very few indeed, and they are chiefly those in which, from the calculus having attained an unusual size, the danger and difficulty of lithotomy are so great, that no surgeon would willingly, nor otherwise than as a matter of duty, undertake it.

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#### ART. II.—*The Ecraseur.*

*The Dublin Hospital Gazette* for December, 1858, quotes from Mr. Skey's *Surgery* (2d ed.) the description and estimate which that able surgeon gives of the *Ecraseur* of M. Chassaignac, as follows:

i. The *Ecraseur* (crusher) is an instrument of modern invention, and is derived from the French school of surgery within the period of a few years only. At the present date it only claims the indisputable charm of novelty, and its real merits possibly have yet to be ascertained. The *Ecraseur* consists of a strong steel chain, forming a loop at the end of two metal grooves, which is drawn tight by a wheel and axle, or other mechanical power, employed with sufficient force, to tear asunder by slow degrees, any textures embraced within the circle of the loop. The force of the *Ecraseur* is such as to be irresistible to any structure of the human body, bone alone excepted; and the advantages claimed for such an instrument are unquestionable, provided the morbid structure to be removed present the attributes suitable to the development of its peculiar powers. On such conditions, but on such only, the *Ecraseur* is an instrument of some utility, which may retain its place among the implements of the surgeon. When these conditions are neglected, its defects will so largely predominate over the merits of the instrument, that it will be rejected as a coarse and mischievous appendage to the table of the operator. The *Ecraseur* purposes to separate growths from the surface of the body, the removal of which, by a cutting edge, would probably be attended by objectionable bleeding. By such powerful constriction the integuments are drawn together at the point of junction of the tumor with that of the body, and thus the exposed surface left behind is reduced to an indefinitely small and almost nominal area, the healing process of which would be apparently perfected in the course of a few days. The conditions required for exhibiting its powers consist in a growth,

whether skin or other morbid product, indicating probable loss of blood on removal by the knife, and attached to the body by a narrow neck. Without the liability to hæmorrhage, the offices of the Ecraseur are misapplied, because the growth may be more readily removed by the knife.

The merit of the Ecraseur consists in the exercise of a slowly applied crushing influence, by which the blood vessels are disorganized by severe pressure, and the textures brought within the chain-loop are compressed, blended, and rent asunder. On the first introduction of the instrument into this country, certain rules were recommended for its employment, by which intervals of time were required, or supposed to be required, during its application, for the coagulation of the blood within the vessels leading to the growth. But coagulation of the blood would appear to be by no means an indispensable condition to its use; because it is obvious, that so great a degree of pressure is employed, that the vessels are themselves disorganized, as the general mass and their orifices are blended with the tissues around; escape of blood, therefore, is impossible, and although coagulation of the blood may not occur during the act of separating the tumor, it will, doubtless, quickly follow the operation. The second condition consists in the appropriate form of the structure to be removed. The circle which is to be subjected to the pressure of the chain must be clear and definite. The line of junction of the tumor should be naturally constricted, because the chain involves in its pressure the textures on both sides, and this appears to be the most serious of the objections to the employment of the instrument, viz.: its destructive influence on the parts around; for however complete the operation, however small may be the surface exposed on the separation of the growth, yet, such is the fatal influence of the violence done to the parts around, that separation almost inevitably follows. I have at this moment three patients in St. Bartholomew's Hospital, from whom I have removed diseased growths, by means of the Ecraseur; and in each case the exposed surface is at the present time greater than it would have been, had the operation been undertaken with the knife. From two of those persons I removed large hypertrophied growths from the prepuce. A weighty objection to the Ecraseur is founded on the fact, that we cannot define the exact line of separation it may accomplish between the morbid and the healthy structure. The drag on both during the early part of the operation, or until the division is partly completed, is immense; the introduction of one or more needles can scarcely protect the tissues beyond them from being involved in the increasing force of the traction, as the loop is reduced in size. The consequence is, that when the operation is completed, by the entire separation of the morbid mass, we find a want of nicety and exactness in the line which would have been more perfectly effected by the knife. And assuredly, when the instrument is employed on a growth having no contracted neck—in other words, when the substance included in the loop is large, the integuments around, when freed from the powerful agency by which they are drawn together, will gradually retract and leave a larger surface exposed, which the immediate introduction of sutures will fail to maintain in apposi-

tion, by reason of the destructive power that has been exerted both on the integuments and on the textures beneath them. No doubt, cases may be found—rare examples of disease—whose form and nature may appeal to the *Ecraseur* as a warranted implement of surgery, but it will not frequently succeed in carrying out the intentions of the operating surgeon, if those intentions embrace a clear wound, limited in size, and of healthy character. The time consumed in the operation is longer than removal by other means; the pain without chloroform is great; and the principle as much allied to the period of barbaric surgery, as any invention of the olden time.

ii. The following example of the successful application of the *Ecraseur* in Uterine Polypus is cited in *Brailhwaile's Retrospect* (Jan., 1859), from the *Dublin Quart. Jour.*, having been reported by Dr. ROBERT JOHNS, Member of the Council of the Surgical Society of Ireland, etc.

[A lady, ten years married, but never pregnant, consulted the author last year, stating that during the last two years she had been subject to severe menorrhagia, often passing large clots; and also suffering from severe uterine pain, diarrhœa, sickness, and other symptoms arising from loss of blood. She had a very anæmic, even malignant aspect.]

On making a vaginal digital examination, I found a fibrous polypus about the size of a chesnut, projecting from the os uteri, which, by very slight traction, was brought down into the vagina; it was attached to the inner and anterior surface of the cervix, about an inch from the os, by a pedicle of about two inches long, and half an inch thick; the os was patulous, flaccid, and dilatable. Having determined to remove the tumor by "*écrasement linéaire*," I explained to my patient what I proposed doing, to which she willingly consented, at the same time saying that she would submit to any treatment to be again restored to health. As she expected to be unwell on the day or so following, I deferred any interference until after the period had passed over, but I ordered a tonic mixture, containing the ammonio-tartrate of iron, etc.

September 20th. The catamenia have ceased for three days. Ordered to have the bowels well freed, and to repeat the tonic iron mixture.

23rd. This morning I found my patient in a state of great excitement, and very hysterical. Not deeming her a fit subject for chloroform, I gave her some wine, which had the desired effect. Having then placed her in the position for lithotomy—which I consider preferable to any other in such cases, when operating on virgins, on females like Mrs. D., whose vagina is not much dilated nor very dilatable without inflicting much unnecessary pain, or on those whose os uteri is very high up—I then proceeded thus: Having gradually and gently introduced the first two fingers of my right hand into the vagina, and having found the polypus in the position before described, I passed a finger on each side of its pedicle, a little above its insertion into the tumor, and drew it down as near as possible to the peri-

neum; having now replaced my fingers by the chain of the *écraseur* (which then surrounded the tumor), I shortened it until constriction was produced, and removed the polypus *very slowly* and steadily. There was not a drop of blood shed either during or subsequent to the *écrasement*, nor did my patient experience the least pain. A very trifling vaginal, discolored discharge, caused by the debris of the pedicle, set in that evening, and continued for three days. Cold water vaginal injections having been daily employed, on the fifth day she was up and about her house, and not a vestige of the pedicle could be discovered by the "toucher," but as the os was still very patulous, and felt rough, an examination with the speculum was instituted, when an ulcer was seen extending about two-thirds around the os uteri, which yielded very quickly to a few applications of solid nitrate of silver, when the os closed up to its normal state. This lady called to visit me on the 3d December, when in appearance she was greatly changed for the better, as she had regained her natural healthy color, and had lost the malignant aspect. She stated that her periods had become perfectly regular and painless; she had no vaginal discharge of any description; and, in fact, that she was in better health than she had enjoyed for years.

The following facts, I think, are fairly deducible:

1. That vaginal hemorrhage, continuing for any length of time, being accompanied by clots of blood, assuming particular shapes, and having been preceded by an increase of flow at the catamenial periods, is strong presumptive evidence of the existence of polypus of the uterus.
2. That the situation of the tumor, whether in the uterus or descended into the vagina, does not seem to exercise any effect in increasing or decreasing the hemorrhage.
3. That the amount of blood lost in this disease is not in proportion to the magnitude of the tumor.
4. That ulceration of the os or cervix uteri is a very fruitful source of hemorrhage in polypial disease.
5. That it is not by any means necessary to draw the tumor externally to the vagina, in order to remove it by linear *écrasement*.
6. That there is no advantage derivable from *écrasing* the pedicle high up.
7. That the *écraseur* is a valuable instrument if properly employed; that is, very *SLOWLY* and *steadily*; but, if not, its use is very doubtful, if not hazardous.

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#### ART. III.—*Irrigation in the Treatment of Lacerated Wounds.*

TO THE EDITOR OF THE LANCET.—Sir: M. Broca, at the Hôtel Dieu, has revived of late two methods of treatment, as simple in detail as efficacious in action, but, nevertheless, nearly obsolete.

The one, originally proposed by M. Josse, of Amiens, in 1833, is that of continued irrigation in the treatment of lacerated wounds of the extremities; the other, suggested by M. Gerdy, of La Charité, consists in prolonged elevation of the limb, at an angle of 45°, in dealing

with diffused phlegmon. As in none of the cases of either class which I witnessed was any other remedy applied, the credit of the more than usually rapid cure may be fairly ascribed to the means above mentioned.

Amongst the several instances in which the "irrigation continué" was employed, two struck me particularly as illustrations of its power in safely tiding over the period so fraught with danger to the patient—namely, the first ten days after the accident. One case was a crushed hand and arm; the other a compound fracture of both bones of the leg, with extensive laceration. These were admitted into the Salle St. Côme, a ward rarely free from some form of erysipelas, phlebitis, or purulent infection. In the first, the irrigation was persevered in for fifteen days, during which time the process of reparation went on healthily and rapidly; in the second instance, it has been in operation for ten days as yet, and with the same excellent results, although there are two cases of erysipelas in the same ward. This immunity may be accidental, but I have noticed it in the nine or ten cases which I have seen treated by irrigation. Its mode of application is the following: The limb, lightly bandaged, is set on a splint made water-proof by a lining of oiled silk, (a gutta percha splint would simplify matters,) and a little outlet, or gutter, is contrived in the most dependent part of the apparatus for the water to drain off. The supply is obtained from a pail hung above the bed, by means of a syphon, fitted with a stop-cock to regulate the flow, and the stream trickles down a twisted band cut into strips below to distribute the fluid equally over the limb. The water in the pail is kept at an unvarying temperature, and somewhat tepid, so that these cases require constant attendance. One point to be observed is, that the treatment should be commenced as soon after the occurrence of the accident as possible, and always before (*never* after) the inflammatory reaction has set in.

As regards the cure of diffused phlegmon by position, in one case in which I saw the plan of elevation adopted, the affection was already far advanced (the sixth day) when the patient was admitted. She was a girl of eighteen; her leg was swollen to twice its natural size, of a dusky red color, hard and brawny to the touch, with throbbing pain, and considerable constitutional derangement. M. Broca hesitated before using the bistoury, and at the patient's earnest entreaty gave her twenty-four hours' respite, raising the limb at an angle of  $45^{\circ}$ , by means of a chair, inverted and placed lengthways under the mattress; the girth of the leg was accurately measured and the patient put on low diet, with no medicine. Next morning, at the visit, the circumferential measurement had diminished an inch and a half, and the other symptoms had improved in like proportion. During the ensuing forty-eight hours, the amelioration was so considerable, that at the expiration of that period all treatment was suspended, and the patient pronounced convalescent. In this instance the cellular tissue was on the eve of sloughing, and as early incisions are very generally practised here, the poor girl would inevitably have had two or three parallel furrows, "scored deep and long," into a limb which is now as whole and sound as ever it was.



One of the latest novelties in Parisian hospital practice is M. Chas-saignac's successful application of the Ecraseur to the radical cure of fistula in ano. He passes the chain along the fistulous track into the rectum, and back through the anus. The wound made in opening up the false passage with this instrument being of an irregular lacerated character, it cannot heal by the first intention, and granulates up from the bottom without much trouble.

I am, Sir, your obedient servant,

J. D. CHEPMELL, M. R. C. S.

Faubourg St. Honore, Oct., 1858.

—*Lancet*, December, 1858.

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ART. IV.—*On the Theory of the Production of Hernia.* By Professor ROSER. (*Arch. für Physiol. Heilk.*, 1858, pp. 60–70.)

THE views here advanced have already been published by Professor Roser, seventeen years since ; but as they have not excited the attention he believes they deserve, he reproduces them with the advantage of being able to add, that all subsequent investigation during so long a period of an active career, has only confirmed his conviction of their truth.

Debating with a celebrated professor of surgery, he declared that a sudden production of hernia was impossible, according to the laws of mechanics. The professor maintained the possibility, inasmuch as he had examined persons who had shown no signs of hernia, and yet, after violent exertion, its presence had become manifest. This, Roser regards not as observation, but as a *post-hoc* conclusion. It is well known how difficult (or when small, impossible) it is to detect an empty hernial sac. We only recognise it when the intestine has entered it, and we can feel the impulse on coughing, etc. ; but when such entrance is prevented by various circumstances, the most we can say is, not that no hernial sac, but that no hernia is present. Why is it not possible, as Scarpa and Cloquet have shown, to produce a hernial sac on the dead body by the use of violent compressing power, aided by apertures made in the tendinous walls of the abdomen? First, because the peritoneum is not sufficiently elastic to bear the necessary extention ; and, secondly, because compression of the soft and fluid contents of the abdomen acts by hydrostatic law ; and although the peritoneum may be stretched, it is not thrust out as it is found to be in hernia, and as it may be to some extent by the finger locally applied.

Professor Roser advances these two propositions, that femoral hernia arises from the dragging out (*Herauszerung*) of the peritoneum, and that external inguinal hernia, or more properly its sac, is almost always congenital. First, with respect to *femoral hernia*, the dragging out of the peritoneum is brought about by nodules of fat, which, apper-

taining to the subserous tissue, are firmly attached to the peritoneum. These nodules slide between the fibres of the *septum crurale*, thrust them asunder, and lead to their disappearance. The anterior part of a nodule passes out under the plica, covered only by *fascia superficialis*, and increases in size. Its movements are favored by its pyriform shape, and by the motions of the body, and the peritoneum following it, a sac is gradually formed. In all the instances of commencing hernia the author has had the opportunity of examining, he has found such fatty nodule at the apex of the sac. We cannot expect to find this in old or large herniæ, as it would disappear under pressure. Professor Linhart, of Würzburg, the only anatomist who has of late years investigated the subject of hernia, has confirmed these views, stating that he believes that traction exerted on the peritoneum exerts far more influence in the formation of hernia than the pressure exerted by the contents of the abdomen.

*External Inguinal Hernia.*—The valvular character and oblique course of the inguinal canal, should have formed a sufficient reason for rejecting the ordinary theory of the formation of this hernia. All subsequent investigation has convinced Professor Roser that his statement made long ago, that this hernia is almost always congenital, is correct. He has found that the *hernia vaginalis funiculi*—i.e., an open state of the upper part of the vaginal process—occurs much oftener than is supposed. It has been found in almost all the children the subjects of inguinal hernia, whom he has examined; and he has frequently met in adults hernial sacs so long and narrow, that they could only be regarded as incompletely-developed vaginal processes of the peritoneum, into which intestine had not passed. He had also found the anatomical signs, detailed in his former treatise, exhibiting the congenital nature of the affection. Other co-existing anomalies of the peritoneal formation are often met with. Beside the descent of the testis, there is a descent of the cæcum and sigmoid flexure, and disturbances of these often occur at the same time.\* As the author has found in almost all the outer inguinal herniæ that he has examined, such grounds for considering them congenital, he has come to the conclusion that the bulk of cases regarded as accidental do not merit the appellation, inasmuch as the sac has been in existence prior to birth. He refers in confirmation of his views to Camper's statement, that of 63 full-timed children in whom the testis had descended, the vaginal canal was obliterated only in 7, it being open on both sides in 34, on the right side only in 14, and on the left side in 8. So likewise Professor Engel, whose investigations are now published in the *Wien Wochenschrift*, states that in children at birth, or during the first fourteen days afterwards, the vaginal canal is found oftener obliterated, or at least considerably shorter on the left than on the right side—a fact agreeing with the preponderance of hernia on the right side. He found the canal entirely closed at birth in ten per cent. After fourteen days no trace of it could be found on the left side in 30 per cent., while it remained open on both sides at the end of fourteen days in 60 per cent. In the adult the presence or the remains of the vaginal canal was observed in 31 per cent. of the bodies examined, on both sides in 37·5 of these, and on the right side alone in 62·5.

*Outer Inguinal Hernia in the Female.*—The author long since proposed the question to Professor Meyer of Zurich, whether women were not liable to a similar descent of the peritoncum as men ; and that writer, in a paper in Müller's Archiv, has shown that in the female as well as the male fœtus, a projection of the peritoneum into the inguinal canal docs take place. Its metamorphosis, however, occurs much earlier, as soon as the fifth month ; and it is also less considerable, and therefore less liable to disturbance than in the male sex.

*Internal Inguinal Hernia.*—This, Professor Roser observes, may, in some cases, have a fatty origin, like femoral hernia ; but he believes that other cases arise from a local protrusion of the peritoneum, in aged and relaxed subjects, in whom partial atrophy of the *fascia transversalis* has taken place. This is a very different thing, however, to the sudden production of hernia usually admitted. He believes with respect to internal inguinal hernia, that the fact that it occurs almost exclusively in aged men is not generally known. It takes place indeed much oftener than most authors admit ; and may be almost said to be as frequent in old men as femoral hernia is in old women. The much greater narrowness of the ring is the reason it does not occur often in old women.

*Umbilical Hernia.*—This is the only hernia which is produced according to the old theory, viz., by a *vis a tergo*, and even here the author suspects that in some cases it may arise from a congenital protrusion of the peritoncum, remaining from the fœtal state. As this hernia differs in the mechanism of its formation from other kinds of hernia, so does it in the remarkable fact of its being generally spontaneously curable, as may be seen from the small number of cases met with in the adult, compared with the large number occurring in infancy. In aged and fat individuals, accidental hernia of this form is, however, frequent.

Originally commencing his investigation with a purely scientific object, Professor Roser has since found that it has an application to legal medicine, the question not unfrequently arising, whether a hernia has been produced in consequence of acts of violence in scuffles, assaults, etc., and the culprit risking to be dealt with too harshly in consequence of the off-hand way in which the affirmative is pronounced.—*Brit. and For. Med. Chir. Rev.*

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ART. V.—*Wounds of the Chest—The new Bullet.*

DR. G. H. B. MACLEOD, in his *Notes on the Surgery of the War in the Crimea*, published in 1858, says concerning the treatment of penetrating wounds of the chest involving the lungs, that venesection is required to arrest hæmorrhage, etc.: The collapse which follows penetrating wounds of the lung, though dangerous, is yet, if not very profound or prolonged, the best guarantee for the patient's safety.

To such cases the observation of Hewson is peculiarly applicable: Langor and faintness being favorable to the congelation of the blood, and to the contraction of the bleeding orifices, should not be counteracted by stimulating medicines, but, on the contrary, should be encouraged. With our modern notions on bleeding, it is often difficult to reconcile the necessity, which experience shows there is, for energetic depletion when reaction sets in. The majority of our patients were certainly not subjects in which this remedy could be pushed so far as Guthrie and Hennen would appear to recommend; but I think it was very generally observed that those cases did best in which early, active, and repeated bleedings were had recourse to. It is well known, that in sieges generally, soldiers do not show their usual tolerance of bleeding, and when their health is so much undermined as it was at Sebastopol, the surgeon is often placed in a most unpleasant dilemma. That many most excellent recoveries were made without having recourse to the lancet is undoubtedly true; but not a few, I fear, died from want of it. When the loss of blood by expectoration and by the wound has been very free, of course the necessity for abstracting it otherwise will be much less. The system is then far more easily reduced to that point which favors the formation of the "caillot tulaire." We must, in cases where venesection is required, be especially careful to bleed by a large orifice, and be guided by effects. This, with perfect rest, the lowest diet, cooling drinks, and possibly digitalis, must form our means of managing the early stage. Any return of the oppression will show the necessity for further depletion.

Of the newly invented conical bullet, Dr. Macleod says: It never rests in a bone, channels, or perforates its substance, without splitting it, like a wedge; nor does it ever come to mark a bone with any touch more gentle than what occasions its utter destruction. In the Crimea we had many opportunities of observing the action of both kinds of ball, and so far as I could judge, their effects were so dissimilar, as almost to justify a classification of injuries founded on the kind of ball giving rise to them. The longitudinal splitting of the bone is so dexterously and extensively accomplished by these balls that while but a small opening may lead to the seat of fracture, the whole shaft may be rent from end to end. I have repeatedly seen the greater part of the femur so split. Stromeyer has shown that this longitudinal splitting seldom transgressed the line of the epiphysis, an observation which I can most decidedly affirm; for though the injury has at times been sufficiently severe to implicate both, yet the rule has been just as he says.

The reviewer adds: This is the most instructive part of the whole book, for here we have not only arguments and deductions, as elsewhere, founded on those most delusive data, statistical returns, but a record of some observations which the author had an opportunity of making while residing for some time at Scutari in 1854. Taking advantage of the occasion, he was present at many post-mortem examinations, and he tells us that, in most of the cases of compound fracture, it was found that the bone was split up, and comminuted to an extent which rendered union next to impossible, the splintered fragments crossing the fracture in every direction.

This was probably *the* peculiarity in the surgery, or rather pathology of the Crimean campaign, and the only point in which it differs from those of former times. But no difference in treatment resulted from the more extensive shattering of the bone; the Crimean surgeons began with attempts to save the apparently hopeful cases, but were soon compelled to acknowledge the justice of the oft-repeated assertions of military surgeons, who had formerly served in British and foreign armies, that in attempting to save limbs many lives are lost. Nevertheless, inasmuch as amputation in the upper third of the thigh is shown by the statistical results of the Crimean and other campaigns to be almost necessarily fatal, an exception must be made in this instance; so that the rule of practice with regard to the thigh may be accepted as settled. From a review of the opinions of the most trustworthy authorities on military surgery, and the experience of the late campaign, the author states:

Finally, then, let me repeat the conclusion;—that under circumstances of war similar to those which occurred in the East, we ought to try to save compound comminuted fractures of the thigh when situated in the upper third; but that immediate amputation should be had recourse to in the case of a like accident occurring in the middle or lower third.

Compound fractures of other bones admit of treatment according to the principles which regulate the management of similar injuries in civil practice.—*Glasgow Med. Jour*

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ART. VI.—*On Hospital Gangrene.* By M. MAUPIN. (Mémoires de Médecine Militaire, tome xx., p. 368.)

THIS paper is the result of M. Maupin's observations upon hospital gangrene as it appeared in the French army of the East, in which it committed frightful ravages. In reference to the conditions under which it may become developed, M. Maupin observes that overcrowding of patients has usually been considered a necessary condition of the production of the disease; but that at the hospital into which the Russian Embassy was converted at Pera, and which united every condition of salubrity—and among these ample space—gangrene broke out amongst the wounded officers seven days after the arrival of eight others who had been severely wounded, the wounds of the preceding occupants having been slight. Still, the gravity of the disease will generally be found to be proportionate to the amount of vitiation the air has undergone. The disease may indeed arise even in the open air when there is a considerable agglomeration of wounded soldiers, examples of which occurred to the author in Algeria. Overcrowding may be only relative, and a given number of patients that in the time of peace may be advantageously treated in a hospital,

will in the period of war give rise to hospital gangrene. Not only do the numbers of the patients, but the gravity of the cases and the constant succession of such cases, increase the hygienic exigencies. The rule is, then, that the wounded soldier requires space and air in proportion to the gravity of his wound ; and when hospital gangrene is once set up in a ward, the dispersion of the subjects of it is a measure alike beneficial to themselves and the other patients. As long as the medium remains unchanged, the treatment is but tentative, and the results are uncertain.

In order properly to appreciate the instability of the results of treatment, we should bear in mind that, if epidemic hospital gangrene may be an essentially local affection, it is frequently during a campaign, but the expression of a general modification of the economy, of a true intoxication, the energy of which, intimately dependent upon the salubrity of the locality, and the number and nature of the wounds treated therein, is increased or diminished, revived or extinguished, with the increase or diminution of the number of the patients, with their agglomeration or dispersion. Means which in isolated cases of gangrene may act heroically—as the actual cautery, sulphuric acid, perchloride of iron, and in milder cases, citric acid, carbon, iodine, etc., are in the epidemic form either powerless or only of temporary benefit. It is this which explains the differences of opinion that prevail on the treatment of this disease.

In respect to the local affection considered separately, we must recognize an acute and a chronic form, a distinction of importance as regards treatment. The acute form is denoted by the conversion of the tissues into a pulpous or putrilaginous magma, and which, as soon as the destructive process is arrested, becomes detached through a series of eliminatory processes. The ulcerative is the chronic form of the disease. The putrilaginous form is almost always primary, and is found almost exclusively in recent wounds ; while the pulpous form, though often also primary, is also often met with in old wounds, and it does not pursue the same rapid and destructive course as the putrilaginous form. The ulcerative form is almost always consecutive to the two other forms, appearing especially in the case of old wounds, or after amputations these have necessitated. In the putrilaginous and pulpous forms the most appropriate measures are those which hasten the separation of the parts affected, and stimulate the reparative process. Of these, the actual cautery, sulphuric acid, and the perchloride of iron occupy the first rank. In an hospital in which there are at least 300 cases of hospital gangrene at the same time, the application of the actual cautery is made with difficulty, while, although it is just as painful as the other means, it does not adapt itself so readily to all the situations, forms, and depths. The sulphuric acid has been found by the author preferable to it and to the nitric acid, while other practitioners give the preference to the perchloride of iron. The eschars once separated, styrax and aromatic wine proved the best of topical applications ; while, when cicatrization was delayed, or there was a tendency to substitute the ulcerative form, citric acid, camphor, cinchona and carbon, tincture of iodine or nitrate of silver, and if necessary, nitric acid, imparted a new and favorable

impulse to the cicatricial process. The formation of a crust or magma on the surface of the sore by means of some of the above powders has been found to encourage the healing process, and to diminish the patient's sufferings. For the chronic form the perchloride of iron is better adapted than the sulphuric acid. But let the local measures employed be what they will, success will be impossible or transient, unless the general conditions and the necessity of removing the patient from the infected medium be borne in mind.—*Br. & For. Chir. Rev.*

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ART. VII.—*Scott on the Treatment of Joints and Ulcers, and Chronic Inflammations.* Edited by WILLIAM H. SMITH, Esq.

At a moment when the tide of conservative surgery seems to be somewhat on the turn, and Scott's plaster, with various orthopædic contrivances in diseases of the joints, appear to be taking the place of resections of the articulations of the knee; at a time when the prætubercular epoch of "morbus coxæ," in engaging the attention of our chief surgeons, and absolute rest of joints is again inculcated in hospitals, Mr. Wm. H. Smith has done good service to surgery by publishing the work of John Scott. We need scarcely say that though not openly recognized in our more straight-laced schools, yet Scott's plan is constantly adopted in nearly every one of our London hospitals.

We believe we shall be doing a service, therefore, to some of our provincial readers by detailing the steps of the plan of treating disease of the joints, so long recognized in our London institutions. We will take a case of common "white swelling."

"In the first place, the surface of the joint, suppose the knee, is to be carefully cleaned by a sponge, soft brown soap, and water, and then thoroughly dried; next, the surface is to be rubbed by a sponge soaked in camphorated spirit of wine, and this is continued a minute or two until it begins to feel warm, smarts and looks somewhat red. It is now covered with a soft cerate, made with equal parts of the ceratum saponis and the unguentum hydrargyri fort. cum camphora. This is thickly spread on large square pieces of lint, and applied entirely around the joint, extending for at least six inches above and below the point at which the condyles of the femur are opposed to the head of the tibia. Over this, to the same extent, the limb is to be uniformly supported by strips of calico spread with emplastr. plumbi. These strips are about one inch and a half broad, and vary in length, some are fifteen inches, others a foot, others half these two lengths, the shorter or longer of course selected according to the size of the part around which they are applied. This adhesive bandage ought to be so applied as to preclude the motion of the joint, and prevent the feeble coats of the bloodvessels from being distended by the gra-

vation of their contents. Over this adhesive bandage, thus applied, comes an additional covering of emplastrum saponis, spread on thick leather, and cut into four broad pieces, one for the front, the other for the back, and the two others for the sides of the joint. Lastly, the whole is secured by means of a calico bandage which is put on very gently, and rather for the purpose of securing the plaster and giving greater thickness and security to the whole than for the purpose of compressing the joint."

In case of chronic synovitis, as well as in those innumerable joint affections included under the term "white swelling," this method of Scott's is in constant use at St. Bartholomew's Hospital, the London Hospital, and other institutions. Without committing ourselves, however, to the opinion that there is anything in the "Scott plan" that might not be as readily attained by starch or gypsum bandages, yet, from all we hear, we believe it is very worthy of a trial. Patients, when sick, who are laid up with troublesome old disease of joints, are ever wishing for something new; if, therefore, legitimate practitioners are not ready to obey the apostolic command, to "prove all things," holding fast by whatever is useful or good, if practitioners, in the provinces especially, be not made aware what is going on in our large hospital institutions, their practice passes away into the hands of quacks.

It is now discovered that what Professor Syme instinctively discovered several years ago is true, that resections of the knee-joint are a mistake, and more might be done by temporising methods, such as this one under review.

The pathology of John Scott is occasionally of a rather too original kind, it must be confessed, as when he speaks of the flattening of the nates in "morbus coxæ," but then we do not look for modern pathology so much as for practical hints in a book first published in 1828. The batch of cases, also, we would rather pass over, as with our advanced knowledge of treatment now in 1858, what with cod-liver oil, steel, rest, sea air, etc., all such cases are more scientifically treated than they were thirty years previously.—*Med. Cir. Braithwaite Retrospect.*

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ART. VIII.—*Treatment of Phthisis Pulmonalis by Caustics.* By DR. ROU-  
AULT. Translated from the French, by J. P. Barbot, M.D.

[*L'Union Médicale* for November, 1858, contains a series of papers by Dr. Rouault, of Reimes, on the treatment and cure of *Phthisis Pulmonalis* by the application of caustic to the chest, of which the following is a synopsis. Dr. Rouault's views and reasoning are certainly ingenious;



but of the benefits he claims for his mode of treating consumption, though supported by the publication of a number of cases in which it was tried with success, we are only partially convinced. We are afraid the Dr. has fallen into the error of many writers, of forming hasty conclusions, and attempting to generalize certain modes of treatment, which, however beneficial in isolated cases, will not stand the test of a general application. The Dr. says truly enough, that most of the *cures* for consumption have not withstood the test of time and experience, though highly extolled at first. It remains to be seen whether *his* method will be more fortunate.—TRANSLATOR.]

Dr. Rouault considers that phthisis pulmonalis is invariably produced by the union of two elements: one, tubercular, the other phlegmonous. He does not wish to imply that there exists a necessary connection between tuberculosis and some previous inflammation, which had led to congestion, and, subsequently, to the formation of tubercles. But, he believes tubercles do not undergo all the phases of their development, except under the influence of a certain degree of phlogosis or local hyperæmia, and that it is only when the influence of this latter condition is brought to bear upon them, that they become a morbid or pathological product. In fact, it has been shown that the tuberculous element, when simple and isolated, that is, when entirely free from any inflammatory complication, may remain imbedded for years in the lungs, without producing any symptoms of pulmonary disease. Tubercles may also exist unsuspected, in other organs, such as the liver, kidneys, spleen, the bony structure, a large portion of the lymphatic glands, without in any manner affecting their functions and general appearance, and only to be revealed by microscopic observation. When no inflammatory action is brought to bear upon them, these tubercles are almost totally innocuous, and give rise to no morbid symptoms, except in a limited number of cases, and then, only act by mechanically disturbing the functions of the organ in which they have been developed.

Dr. Rouault quotes Andral's authority to show, that in most cases, there is a period in which pulmonary tubercles do not reveal their existence by any pathological symptom. But whenever a certain degree of inflammation acts upon this tubercular element, the latter ceases to be latent, and the pathognomonic symptoms of phthisis are rapidly developed.

This fact is of the highest importance in therapeutics. For, if in-

flammation or hyperæmia—which is as necessary to the development of tubercle as heat and moisture are to the growth of seeds—be arrested, all the pulmonary symptoms which it had produced will diminish or disappear. The tubercles will still exist in the lung, but they will cease to be a source of constitutional disturbance, and the patient will return to the normal state of health in which he was before the supervention of the inflammation. Doubtless, this inflammation is sometimes produced by the tubercles themselves, which act upon the system as foreign bodies will. But, in these cases, *effect* has become *cause*, which cause reacts upon this foreign body, hastening its growth and its softening. But whether cause or effect, it is all important to remove this inflammation, and this removal of the inflammation, Dr. Rouault considers to be the first indication in the treatment of phthisis. No doubt, it would be preferable to combat the proximate cause of tuberculosis; but, unfortunately, all the methods and therapeutical agents that have been recommended and used hitherto, have not answered our expectations.

Dr. Rouault considers the revulsive treatment, by caustic applications to the chest, to be the most powerful and successful way of curing phthisis. He does not believe that these applications have any direct influence on the tubercular element; they only counteract its attending inflammation. They are, nevertheless, of undoubted benefit. By removing the local inflammation, and restoring the lungs to their natural condition, by modifying any over excitement, they check the progress of tuberculosis.

Dr. Rouault uses the Vienna caustic, which is made by mixing together into an homogeneous powder, in a heated mortar, fifty parts of caustic potassa, and sixty parts of quicklime. Sufficient of this caustic powder is made into a paste of proper consistence with alcohol, and two disks of the diameter and double the thickness of a *five centimes* (four cent) piece, is applied over the anterior and lateral portions of the chest, about three inches below each clavicle, and two inches from the outer side of the sternum. As much as possible, try to apply them between the third and fourth, and fourth and fifth ribs. The patient should be lying on his back, of course, during their application. When the caustic is fresh and of good quality, from eight to ten minutes are sufficient to form an eschar. It is known that this result is obtained, when a dark circle or ring appears outside of the paste, and the patient declares that the pain occasioned by the caustic

has considerably diminished. The paste is then removed, and there is left a circular and perfectly circumscribed eschar of a greyish color, which must be dressed with adhesive plaster.

Dr. Rouault selected the parts of the chest pointed out above for the application of the caustic, as they are those nearest the large bronchi and their sub-divisions; and as phthisis is always attended by more or less inflammation of the bronchial mucous membrane, which inflammation exerts a great influence over the progress of the disease; and also, because it is a good rule in therapeutics, always to apply counter-irritants as near as possible to the seat of inflammation.

The Vienna paste is the mildest, most easily applied and promptest caustic that he knows of: it never produces eczema, erysipelas or furunculi; and even if converted into an issue, it does not inconvenience the patient or prevent him from attending to his usual avocations. It occasions but little pain, which ceases as soon as the caustic is removed.

Its good effects are usually felt in six weeks after its application. It is sometimes advisable, nay necessary, after that period to renew the applications at some point near the first. In timid or nervous persons it might be preferable to convert the first applications into issues at once, keeping up the suppuration by means of peas, until the cough and difficult breathing have ceased entirely.

These caustic applications are beneficial in all stages of the disease, but they are truly curative only in its incipency, or shortly after. If used in its first and second stages, they will completely arrest its progress; the cough and difficult breathing will disappear in a surprisingly rapid manner; and the patients soon recover strength, color and flesh.

When softening of the tubercles has occurred, and cavities of greater or lesser size are formed, these caustic applications will prevent new crops of tubercles, and protect the portions of the lung left sound. Thus, if from having seen them too late, patients cannot be cured, at any rate their lives can be prolonged and their sufferings alleviated.

In connection with the caustic, patients should take large quantities of Iceland moss jelly, and if they be troubled by a short, dry, and convulsive cough, small doses of the extract of belladonna, night and morning. The patient should, if possible, use animal food exclusively, and exercise as much as possible, in the open air, in fine weather.

Dr. Rouault says, he has by these means, succeeded in curing patients who had, before placing themselves under his treatment, tried the usual methods of curing consumption, without any success.

It is but justice to Dr. Rouault to mention that at the close of his memoir, he declares that he himself had found that his system is not infallible.

We omit his illustrative cases.

ART. IX.—*On Consumption.*—By Dr. RICHARD PAYNE COTTON, Physician to the Hospital for Consumption and Diseases of the Chest.

[AMONGST other causes influencing this disease, Dr. Cotton says—] *Hereditary transmission* appears to have a smaller share in producing phthisis than is generally supposed. In a thousand cases, three hundred and sixty-seven, or rather more than one-third, were members of consumptive families; whilst, in the remaining six hundred and thirty-three, or in somewhat less than two-thirds, the disease could not be shown to have proceeded from hereditary causes.

It is certain, however, that this scarcely exhibits the full extent of hereditary taint, since it embraces only the preceding generation (statements referring anterior to this, being too vague to be depended upon); and it is well known that phthisis, like other diseases so propagated, may remain dormant, for one, two, or even more generations, and then show itself again at a remote period, when, perhaps, its original existence in a family has been long lost sight of. The above, therefore, must be considered only as an approximation to the truth, or as exhibiting the frequency with which hereditary influence can be demonstrated. Consumption is more likely to be acquired by males than females. As a general rule, *hereditary* consumption is less controllable than *acquired* consumption; and those cases which prove the most rapidly fatal, will be usually found to proceed from, or to be accompanied by, hereditary taint. Inactivity is one of the most fruitful causes of delicate health; and I have seen many cases of threatened phthisis entirely recover, from nothing else than the use of regular and moderate exercise.

When hectic fever becomes fully established, the patient is seldom free from diarrhœa, perspirations, or profuse expectoration, which seem to exhibit towards each other a certain degree of antagonism. In proportion to the increase of one, the others have a tendency to diminish; and as one becomes checked by treatment, another too often makes its appearance. The issue of nearly every fatal case of phthisis is immediately attributable to the exhausting effect of one or other of

these three symptoms ; hence it becomes a matter of practical value to ascertain their comparative influence upon the patient.

Diarrhœa is the most rapidly destructive ; perspirations the next ; and expectoration the least so. [Under the subject of the preventive treatment of phthisis, the author observes, that when a parent, even although competent to the office, is yet possessed of a marked hereditary predisposition, it is better for her offspring, as well as herself, that her duties should be transferred to another.]

The diet should be *moderate*, simple, and nutritious, and consist of *a full amount of animal food*. Regular exercise should be made imperative ; and where it is possible, the residence should be fixed in some healthy district, far removed from the depressing influences of a town ; whilst the cultivation of the mind should neither be commenced too early, nor be pursued too rigorously. The consequence of a disregard to these matters is often painfully exhibited in weakly children, who have been injudiciously sent to the schools of our metropolis.

[The union of iron with iodine in the form of *syrupus ferri iodidi*, seems peculiarly adapted to tubercular affections.—*Braithwaite's Retros.*

#### ART. X.—*Electro-Galvanic Anæsthesia*

[THE following papers from *The Lancet* for December 1858, and January, 1859, will serve to show that the claims of Electricity, as an Anæsthetic, have not been fully settled, at least in the British Metropolis.]

*Electro-Dentistry.*—*To the Editor of The Lancet.* Sir : I shall feel much obliged if you will insert the following facts in your journal :

A few years prior to the Great Exhibition of 1851, Mr. Laxton registered for me the right to patent the electro-galvanic current for surgical purposes, its usefulness having struck me when asked by patients to allow them to hold chains, consisting of the metallic combination of various galvanic elements, when undergoing dental operations. Subsequently, at the Great Exhibition, I exhibited a battery, with the chain and wires, ready for the dentist's use. This I have, and it can be seen at any time.

I feel justified in saying, that no one but myself can claim the invention of the battery, whether as an anæsthetic agent or otherwise for surgical purposes ; and I beg further to say, that I have notified this to the patent office, but at the same time members of the dental profession are quite at liberty to use it.

Delveil, of France, exhibited in 1851, also an electrical machine for medical purposes ; and Meinig also showed another at the Great Exhibition.

I am, Sir, your obedient servant,

G. WAITE.

Old Burlington street, Sept., 1858.

*To the Editor of The Lancet.*—Sir: Having recently read, in the columns of *The Times*, several letters upon electricity in its relation to tooth extraction, in which the authors have endeavored to prove that teeth can be painlessly extracted under the influence of that important element, allow us to offer a few remarks deduced from direct experiment upon ourselves, which tend to throw considerable doubt upon its efficacy. There is not a more satisfactory method of testing the efficiency of a remedy than by a personal trial; and, adopting that idea, we have unpleasantly convinced ourselves that to try it upon another would be anything but fair. Being in possession of a powerful Smee's battery, we alternately sat down to experience the pain-lulling effect supposed to be produced by contact with its wires. A pair of forceps were firmly secured to one pole, the other being embraced by the hand. Upon the instrument being applied to the tooth, and a mild current of electricity passed through it, a sensation not altogether agreeable, but yet not absolutely painful, was produced; the current was then gradually increased, but before the plates were two-thirds immersed in the trough, the feeling was intolerable, not only pervading the tooth, but all the surrounding structures, and remaining for some considerable time afterwards. The pain caused by thrusting the instrument beneath the gum was not in any way ameliorated by the passage of the current, but could be as distinctly felt, and in fact more acutely, than if that current had been altogether absent.

It is true, there are some persons to whom, under any circumstances, the shock of extraction is comparatively painless, whilst others suffer most acutely. The absence of pain in the former class might be readily attributed to the action of galvanism, had that been employed, but the effect would have been precisely similar without it; whereas, the application of a current, unless extremely mild (in which case we presume it would be useless), to a nerve already racked by pain, must be positively excruciating.

If, by the aid of electricity, such an advantage could be obtained as that which *The Times* correspondents have so lately asserted, the boon would be an invaluable one both to the operator and the patient; the deadly vapors of chloroform would give place to the harmless current of the battery, and patients would no longer shrink from an operation which, though a minor one, is nevertheless rife with extreme agony; but we are convinced that no patient who had born extraction under the influence of galvanism, and without it, would give preference to the former mode of operating, which, judging from our own feelings, only augments the misery. It may be urged that we were unable to arrive at a correct conclusion, not having submitted to the final ordeal; but we are of opinion that our test was the more satisfactory, for directly the nerve is ruptured, which must necessarily be one of the first effects of extraction, the electric current is cut off; and it is immaterial whether it be continued during the latter part of the operation or not.

We are, Sir, your obedient servants,

A. PRINCE, M. R. C. S.

F. FENNEL, Dentist.

Harrow-road, Upper Berkley street,  
September, 1858.

*The use of Galvanism in Toothache.*—To the Editor of *The Lancet*—Sir: As the anæsthetic power of galvanism, as employed in dental surgery, is at present attracting much attention, it may be interesting to some of your readers to learn that its application, in cases of toothache, will, as far as I am at present able to judge, instantly and effectually remove pain.

I have lately used this remedy in a bad case: the patient suffered excruciating agony, lost her rest, and was unable to attend to her domestic duties in consequence. The tooth was so much excavated, and decayed, that I do not suppose any dentist would have attempted the operation of plugging. A slight touch of the conducting-wire instantly removed the pain; and its subsequent application for a few seconds was attended with complete relief, which appears to be permanent, as there has been no return of suffering for the period of five weeks which has elapsed since the operation.

I am, Sir, your obedient servant,

CHARLES TAYLOR, M. D.

Walton Lodge, near Liverpool, Oct., 1858.

*Galvanism in Dentistry.*—To the Editor of *The Lancet*—Sir: Instead of leaving London this autumn, I have been extensively engaged in experiments with the electro-galvanic current for dental operations. The idiosyncrasies of patients being very different, the electro-galvanic shock varies in its intensity; and I do not recommend those acted upon with difficulty to test the strength of shock necessary, in their peculiar cases, to anæsthetic purposes. From statistics, I am not far wrong in averaging adults sensible of the pain of extraction under the galvanic current at three to one; while in the cases of children up to twelve years of age, the numbers may be reversed,—viz: one to three.

The following hints may not be considered useless at this moment, viz: Operators should consider the constitutions of their patients, and operate on none subject to epilepsy, or to disease of the heart or brain. 2nd. Medical sanction should be obtained prior to undergoing the double shock of electro-galvanism and extraction. 3rd. The intensity of the shock should be measured by the electrometer. Lastly: Practitioners should not draw and put forth hasty conclusions from a few isolated cases, but wait till such time as electro-galvanism shall have been assiduously tested by themselves, and experimented also by others fully capable of giving its merits and its failures their full and separate consideration.

I am, Sir, your obedient servant,

GEORGE WAITE.

Old Burlington street, Nov., 1858.

ART. XI.—*Anæsthesia and Anæsthetics.* (Original and Selected.)

[THE great event which illuminated the close of the fifth decennium of the XIX century, before which the transactions of humanity whether civil, military or physical, “pale their ineffectual fires,” is that of anæsthesia in operative surgery, obstetrics, and practical medicine. The use of anæsthetics, both as it regards the facilities which they afford in the diognostication and cure of certain diseases, is constantly extending. Nevertheless, there is a danger in the administration of these remedies which, as yet, no one, however skillful, can foresee or prevent—a danger which would be far more evident if the fatal cases of anæsthetization were all published—a danger which is fully proved by deaths which are known to have occurred—a danger which has already created distrust and alarm.

Is there as much danger to life from the mere pain of a surgical operation as from the anæsthetic which prevents pain? That the mortal effects of anæsthetics are witnessed only in cases of præexisting disease of the lungs, heart, etc., can no longer be maintained, as post-mortem examination has proven. Experience shows that man and beast inevitably perish from the inhalation of these agents, the precise amounts of which, not being yet known and fixed, are for that reason the more dangerous. Those who escape death, escape because they inhale less than the uncertain fatal dose, but there is always more or less difficulty, not to say impossibility, in guessing at what is safe, and what is unsafe in amount, etc. Does not this inherent uncertainty, from which numbers suddenly die, outweigh the mere element of pain, at least, in all such cases as are known never to die from the mere pain of an operation, as in the extraction of a tooth, and a multitude of other operations.

It is believed that a formidable list of deaths from anæsthetics administered to prevent pain during the extraction of teeth might be enumerated, independently of the *suppressed or unpublished cases*. The many narrow escapes from apparently impending death, and the after effects sometimes due to these agents should not be passed by without serious consideration. The precursors of impending death, often cannot be detected, or come too late to be of use.

The popular biases (little short of mania) in favor of anæsthetization for the removal of pain during normal parturition, and many trifling surgical operations, if not arrested by candid and public



avowals of danger, through medical men and medical journals, may call for legislative interference, and a further extension of the penal code, more particularly, if the reported and suspected cases of death from this cause in certain parts of the world, beyond the limits of Louisiana, be not exaggerated.

It is reasonable to suppose that surgeons should be somewhat biased in favor of anæsthetics, seeing that by their influence, the patient lies passive, unresisting, noiseless, senseless, and inert, like a corpse; and, therefore, in a most favorable condition for any operative process which may be required by the exigencies of the particular case. But it is worthy of inquiry, whether in some cases of operative surgery and obstetrics, pain may not be useful as a guide to the operator. It is not intended, however, now to examine this question.

In a preceding article on Lithotrity, Sir Benjamin Brodie, a high surgical authority, incidentally says, in regard to that operation: "The surgeon must use his own discretion, being *guided chiefly by the amount of inconvenience, which the patient suffers*. In one case, twelve or fifteen fragments may be safely crushed, in another not more than six or seven. The consideration of what has now been mentioned leads me, on the whole, to prefer not putting the patient under the influence of chloroform, as it prevents the patient from describing those sensations which may help to *direct the surgeon in his practice*."

Of all pains, those of parturition are among the most agonizing; yet, experience shows, that they are, on the whole, more safe for both mother and child (with, perhaps, rare exceptions) than artificial anæsthetization, the fine words of its advocates to the contrary notwithstanding.

Professor Carpenter, in his work on *Human Physiology*, justly remarks: "That the final cause or purpose of painful feelings is to stimulate the individual to remove himself from what would be injurious in its effect. Pain gives warning;" etc. He adds: "The following case, recorded in the 'Journal of a Naturalist,' affords a remarkable instance of this general fact—the correctness of the statement having been called in question, it was fully confirmed by Mr. Richard Smith, the late senior surgeon of the Bristol Infirmary, under whose care the sufferer had been. 'A traveling man, one winter's evening, laid himself down upon the platform of a lime-kiln, placing his feet, probably benumbed with cold, upon the heap of stones, newly put on to burn through the night. Sleep overcame him in this situation; the fire

gradually rising and increasing until it ignited the stones on which his feet were placed. Lulled by the warmth, the man slept on; the fire increased until it burned one foot (which probably was extended over a vent hole) and part of the leg above the ankle entirely off, consuming that part so effectually, that a cinder-like fragment was alone remaining—and still the wretch slept on! and in this state was found by the kiln-man in the morning. Insensible to any pain and ignorant of his misfortune, he attempted to rise and pursue his journey, but missing his shoe, requested to have it found; and when he was raised, putting his burnt leg to the ground to support his body, the extremity of the leg-bone, the tibia, crumbled into fragments, having been calcined into lime. Still he expressed no sense of pain, and probably experienced none, from the gradual operation of the fire, and his own torpidity during the hours his foot was consuming. This poor drover survived his misfortunes in the hospital about a fortnight; but the fire having extended to other parts of his body, recovery was hopeless." 549.

This case, though a very extraordinary one, is not so extraordinary as the physiological explanations of it as given by that eminent physiologist, Dr. Carpenter. Instead of adopting an impossible explanation, the circumstances of the case point out, not only a possible, but an easy solution. *The poor drover was anæstheticised by the carbonic acid gas (one of the acknowledged anæsthetics) from the lime-kiln, which he had both in the sleeping and waking states inhaled in the open air, yet so much diluted with the latter as not to asphyxiate himself altogether.*

MM. Ozanam, Faure, Fontaine and others, have recently been experimenting upon animals with carbonic acid as an anæsthetic, from which it appears that this deadly gas, when duly diluted with atmospheric air, produces perfect anæsthetization which may be prolonged to an extent, to which the other anæsthetics now in use, furnish no parallel. This *spiritus lethalis* or *mephitic choke damp* of the lime-kiln, not natural sleep, caused the drover's insensibility while the fire was consuming his leg, and calcinating his bones. Eternal anæsthesia, as suicides have long known, is easily effected with a little ignited charcoal in a closed apartment. For more than two decennia carbonic acid has been known as a *local anæsthetic*; there is, perhaps, sufficient evidence extant, showing that it was used with this view, nearly a century ago.

Such a "deep sleep" once fell upon Adam by a special miracle, dur-

ing which, the dissection and extirpation of his rib was safely accomplished by the All-knowing Physician, the Almighty Anæstheticist.

The question is not simply can pain be prevented? But the question for the philosophical surgeon is, which, on the whole, is the least dangerous to life, the pain or the anæsthetic? Is not the chance of death as between pain and anæsthesia, a hundredfold greater from the latter? Is it not here, if anywhere, that the greatest-good-principle should predominate? Death from an anæsthetic is not at all analogous to death from lightning, explosions, earthquakes, disease or other unavoidable accidents; its administration is wholly voluntary, and its risk is voluntarily incurred for the almost miraculous good it generally accomplishes.

The apotheosis of anæsthesia is now apparently complete. "From the rivers to the ends of the earth," the civilized world, with loud acclaim, pronounces that "pain is conquered."

Whether the future shall reëcho these acclamations down the corridors of all coming time, may be doubted. Without further discovery and greater safeguards, the day of revulsion will probably soon arrive.

As the matter now stands, however, the chance of death from anæsthesia is slight, and at the worst, it guarantees as actual the long desiderated ideal of an euthanasia,

"Which o'er the sense lethean dews distill."

The great results which remain for the science of anæsthetics to achieve, are, a safer mode of administration; more exact information concerning the quantity of these agents to be used; their immediate effects consistent with safety to the patient; the maladies, constitutions, and conditions in which they are safe or otherwise; their consecutive effects, including the exact statistical results or comparisons between patients who have recovered or died after operations, with or without having been anæstheticized, etc. Here, as in physiology, experiments upon the inferior animals, will doubtlessly serve to throw coruscations along the pathways of science which otherwise would be dark and dangerous labyrinths.

For my own part, without having the remotest claim to be considered an authority in the matter of anæsthetization, I have always

been skeptical as to its safety and perpetuity. I can sustain my opinion by that of Jack, a slave belonging to the Commercial Bank, on whom I lately performed an operation, assisted by Dr. Barbot. I had, some days in advance, fixed the time for extirpating a tumor which had annoyed him all his life. I had not, of course, hinted anything about anæstheticizing him ; but in the meantime, others had attempted to persuade him thereunto ; but Jack declared, in effect, all the world could not convince him that he could be put to sleep with safety, so as not to feel the operation. He preferred the pain, which he bore without flinching or complaining, though the operation was prolonged by arterial bleedings. His synthetic logic has too often been confirmed by the logic of events.

An universal toxic, intoxicating, narcotizing, asphyxiating, blood-contaminating agent, which, under the most favorable circumstances, for a time annihilates intellection, sensation and voluntary motion, and which, also, either suspends or greatly embarrasses the involuntary actions and essential functions of animal and organic life, appears at the first view to be the fundamental antithesis of life and physiological well-being. From no point of view can be traced the exact boundary line which separates complete anæsthetization from the realm of death. In fact, an anæstheticized individual, presents a repulsive object in the eyes of physiology. The bright chain of vitality is suddenly strained to an unnatural extent, or falls into a confused, shapeless mass, which the flattering eulogies of surgeons cannot altogether disguise, though generally fortified by a happy resuscitation from apparent death. If it be a human maxim in courts of justice, that it is better a thousand guilty men should escape, rather than a single innocent one should be punished, ought not a parallel maxim to obtain in the court of surgery, namely, a thousand persons should suffer the pains of "medicable wounds," rather than jeopardize a single life unnecessarily ?

B. DOWLER.]

i. *The Introduction of Chloroform into the Arteries.*—When a few grammes of chloroform are injected into the abdominal aorta of a rabbit, an instantaneous rigidity of the muscles of the inferior extremities is observed, which resembles the stiffness of death. Various essential oils produce an effect similar in character, and limited to the parts injected. The muscles seem to acquire the hardness of wood, when the experiment is made upon a living animal; whilst the rigidity is less apparent in proportion as the muscle is

deficient in contractile power, and the organism in vitality. When the cadaveric rigidity, natural to the tissue, has disappeared, there is no effect produced, whatever the nature of the substance thrown into the arteries.

The rigidity resulting from the chloroform is the effect neither of a coagulation of blood in the vessels, nor of the albuminous fluid imbibed by the muscles; but of a positive change in the contractile element which enters into the composition of the tissue. Under these circumstances, the muscular fibre is less elastic and more fragile, manifesting either partially or completely, a want of normal contractility, and an interruption of the electric currents peculiar to it, and presenting in fact the same characteristics as in the rigidity of death, or that produced by an unnatural temperature. This condition presents no point of analogy with an ordinary tetanic contraction, and MM. Cage and Flourens, who have so considered it, have been deceived into this belief by the partial development of mobility in the affected part. For though the stiffness disappears always in a few moments when the experiment has been attempted upon a living animal, yet the movements which are subsequently observed, are invariably produced by those muscles which did not participate in the primary rigidity. Those which were really thus impressed lose their vitality at once, and soon putrefy as well in the living as the dead animal, and in fact more rapidly in the one than in the other, since, though all remnant of vitality be destroyed in the ejected tissue, it will remain hard and intact for weeks when the whole body has decayed.

It is remarkable that chloroform, in combining with muscular tissue, should destroy its vitality, and at the same time endow it with the power of resisting putrefaction for a long period. The explanation of the seeming difference in this regard, in the living and the dead animal, is this, when the animal survives the experiment, the blood continues to circulate—leaves the muscle, and, by carrying chloroform from it continually, destroys this protective combination, whilst, at the same time, it moistens the tissues, impregnates them with oxygen, and maintains their temperature.

When gangrene has succeeded that condition of relaxation which follows rigidity, the blood coagulates in the vessels, entering into the seat of the decomposing process; thus exhibiting one of those cases in which the obliteration of the vessels is the effect and not the cause of mortification. It is probable that the absorption of putrid matter produces this coagulation; and that the general system participates in the disturbance, because of the taking up of poisonous elements by the vessels surrounding the gangrenous part without penetrating into it. Coagulation certainly takes place very slowly in these vessels; and M. Kressmaul has established, by a conclusive experiment, that even when circulation has ceased in a gangrenous member, certain absorbable matters, as the iodide of potassium, can still penetrate into the general circulation, even when they are applied very far from the line of demarcation between the living and dead tissue, a discovery, by the way, of great practical utility. This fact proves that in cases of wet gangrene, there is always a certain exchange

between the part completely destroyed and the circulation which is prejudicial to health, and that the gravity of this absorption depends upon the extent of the gangrenous surface, and not alone upon the thickness of the member affected. The final deduction from all this is, that it is a matter of importance not to leave the diseased extremity in contact with the healthy tissues of the body.

The gangrene resulting from chloroform should really be classed with that which is produced by the absorption of certain poisons; but it proves at the same time that these poisons do not necessarily partake of the nature of ferments, for the extremity is really dead from the effect of the chloroform upon it, before putrefaction takes place.

The different phases in the progress of general gangrene may be thus divided.

1. The tissues (muscles, nerves, etc.,) lose their vital properties, die and become rigid. This is the *cadavérism* which often precedes gangrene properly speaking.

2. Gangrene—Softening, commencement of putrefaction, blood continues to circulate, at least its coagulation has not yet been the cause of gangrene.

3. Sphacelus—Putrefaction affects the blood penetrating the gangrenous tissues: it coagulates and arrests the circulation. Often currents foreign to the vessels continue to circulate for some time in the interior of the tissues. Development of gas, destruction of tissue.

4. Mummification—The mortified tissues lose the liquids which impregnated them, recover them no more and wither up.

Translated from the *Archives Générales de Médecine*, *Septembre*, 1858.

[*Med. Jour. North Carolina*.]

ii. *Mode of Administering Chloroform—Its Use in Delirium Tremens.*—Henry Potter, Esq., M. R. C. S., in a paper giving cautions, as to the mode of administering chloroform, says: It has been my object of late to try and ascertain the least amount of anæsthesia necessary for surgical operations, and I am quite confident that a much less quantity of chloroform is required, especially in minor operations, to produce the effect than is usually supposed to be necessary, provided it be given slowly, and is sufficiently diluted with atmospheric air, as in Dr. Snow's apparatus; I mean that state of anæsthesia necessary for the abolition of all recollection of pain and the production of perfect unconsciousness of anything having happened.

The patient very often appears to feel, and will flinch when the knife is first applied; but this I consider to be the effect of reflex action, and not an indication to press the chloroform. In some operations, especially those about the rectum, though the patient be well under the influence, we always meet with some inconvenience from these reflex actions. A case which I had the other day in dental surgery is a good illustration of sensation returning before the brain becomes conscious of what is going on. I was called to give chloroform to a lady for the extraction of ten teeth. I got her slowly but thoroughly under its influence, knowing it must take some time to

remove so many teeth, and that the inhalation could not be renewed on account of the mouth becoming full of blood. The first five were extracted without the slightest movement, but as the operation proceeded, sensation returned, and I was obliged to use considerable force to keep her in the chair during the extraction of the last tooth. She came to herself very shortly after, and was delighted to find that she had got over all her trouble—to use her own words—“without having felt it the least in the world.”

Now, it appears to me that, if the operation is a very slight one, the minimum amount of anæsthesia necessary to produce the effect ought to be the great aim of the chloroformist. Dr. Snow used to consider that, with his apparatus, from three to four minutes was sufficient to bring the patient under the influence of chloroform, but I think six or seven is much nearer the mark. I attribute the comparative safety of the inhaler, over a sponge or handkerchief, to the much less rapid introduction of the poisonous vapor into the blood, in consequence of its large admixture with common air; whereas, with the sponge or handkerchief the effect is much quicker, and the dilution of the vapor a matter of great uncertainty. Much more congestion is produced, from the circulation not having had time to accustom itself to a foreign vapor, and the danger to life is greatly increased. We must all consider the introduction of chloroform as one of the most important discoveries of our age and a great blessing to suffering humanity, by removing all the horrors of operative surgery; but at the same time there is some risk, and a vast uncertainty in the effects produced on different constitutions, of which there are no means of judging beforehand. It is only by constant practice and experience that we become familiar with the varied symptoms that occur, and know how far we may press chloroform, etc.

Mr. Potter proceeds to illustrate his views by cases, from which he concludes :

These cases will represent the principal dangers I have met with in the administration of chloroform—a branch of practice to which I have paid great attention ever since its introduction. To sum up, they may be divided into three heads :

First. Errors in previous treatment, principally in respect of food; where the patient has been kept too long fasting; or, on the other hand, has been allowed to take food just before the operation.

2d. Injudicious management of the chloroform—that is, introducing it too rapidly, or pushing it too far.

3d. That state of system in the patient which is found associated with a weak, flabby heart.

The first error, in all ordinary cases, may be avoided by common care.

For the judicious management of chloroform, some previous practical acquaintance with its administration is essential, especially in

operations which threaten to be protracted. If space permitted, I could give many cases in which sufficient anæsthesia has been maintained for very long periods without any bad effects.

As to the third class of cases, I will only remark, that in the present uncertainty which exists as to the physical signs of those conditions of the heart, we cannot expect always to diagnose them; but, I believe, by habitual care in the use of the chloroform, combined with that facility in appreciating danger which is acquired by practice, and by the prompt use of stimulants when danger does occur, the ratio of accidents may in future be greatly diminished.—*Lancet*, Sept. 1858.

Brook-street, July, 1858.

iii. *The Great Value of Chloroform in Delirium Tremens.*—(Under the care of Mr. Curling, London Hospital.)—On one occasion we remember seeing a large muscular man, a laborer, brought into an hospital, with symptoms of delirium tremens of the most violent character. The most powerful anodynes, frequently given had no effect; he was strapped down in his bed, and had gloves put on his hands. He continued to struggle and rave, without intermission, from the time he was admitted, and died from exhaustion on the same evening. This astonished those under whom the poor man was placed, and a regret was expressed that any restraint at all had been practised. Had chloroform been administered in this case, it is very probable that the man would have been saved. We saw an example of delirium tremens, on the 7th instant, in the London Hospital, the patient being a stout, healthy man, twenty-seven years of age, under Mr. Curling's care. He was a clerk out of situation, and affected with this malady for the first time, from drinking rum to excess. The symptoms were very violent, but not to extreme degree. Drachm doses of laudanum were administered three different times in succession, without the slightest effect; then twenty-five grains of extract of hyoscyamus, with the same result. At last, it was determined to try chloroform. This had been inhaled scarcely five minutes, when he fell asleep, and continued sleeping for six hours, when he awoke, and the symptoms of his trembling delirium had vanished. Several other cases have been successfully treated by the administration of chloroform in the same hospital.

This form of practice in delirium tremens is by no means new, having been employed since the year 1852. Many practitioners prefer giving chloroform internally in this affection, sometimes combined with sulphuric ether; but when it is desirable speedily to obtain quietude and repose, lest the violence of the delirium should exhaust the patient and render the case hopeless, the best and safest results may be anticipated from the inhalation of a moderate amount.—*Ibid*, Jan., 1859.



ART. XII.—*Anæsthetic Discovery—A Grain of Wheat from a Bushel of Chaff.*

THERE has been a constant but unequal contest by man against the curse imposed upon him for the first fault, a constant endeavor to strike the word *pain* from the vocabulary. \* \* \* Although ages have rolled on, and millions upon millions have lived, pondered, experimented, and suffered, so meagre was the result, that even so late as 1839 the celebrated French surgeon, Velpeau, despairing of any solution to the problem, declared that "to escape pain is a chimera which we are not permitted to look for in our day; that the cutting instrument and pain in operative medicine are two words which never present themselves the one without the other." And yet he has lived to see his opinion changed. Only seven years after, as one of a commission appointed to examine the merits of a new means of relief proposed, he heard and acquiesced in the glad announcement made to the whole world: "We have conquered pain."

How was this glorious victory gained? Who won it? Was it a mere spontaneous, suddenly-imagined suggestion which luckily found a corroborative solution at once in experiment? Or was it a result from a series of trials and failures—a fact settled by a slow process of reasoning on a certain amount of given information?

Such are the questions often proposed—let us see if all of them can be answered satisfactorily. One, certainly the last, can be at once replied to affirmatively, for the facts to prove it are sufficiently numerous and well substantiated to satisfy even the most obdurate Mr. Gradgrind. Our object must be therefore to see at what period the first attempts were made to relieve pain, and in what they consisted.

Throwing aside, as doubtful, the story of the sleepy action of the nepenthe upon Ulysses and his companions; disputing with the Biblical commentators the theory that narcotics were given to the unfortunates about to be crucified; disbelieving entirely the assertion of Herodotus that a narcotic intoxication was common in his day; we reach at last a tangible fact and fixed date.

A worthy gentleman of Naples, by the name of Pliny, who, it is known, was living in the first years of our era, has written us a letter in most excellent Latin, in which he declares that it was the custom to give a certain decoction of herbs "before cuttings and puncturings, lest they should be felt." One would suppose this so satisfactory, that no doubt could be harbored as regards his integrity. But apparently not satisfied with the probable effect of so simple an assertion, he instantly proceeds to perpetrate a most abominable Munchausenism about a stone which he calls Memphites, declaring that it will produce the same effect. But even this admits of some explanation, for, as by his direction, it was necessary to apply it in order to stupefy the part, it is highly probable it was to be used as Mr. Montgomery intended to use his brick. That omnipresent race, however, the Chinese, who seem inclined to cheat us out of all claim to priority of invention, have taken issue with all the rest of the world, by declaring that from the remotest ages it has been the custom of their medical men to give patients a narcotic powder, so that no

pain need be felt. In a curious book in the Imperial Library at Paris, called Kou-kin-i-tong, we find the name Ma-yo given to this powder, which was probably no more than the Indian hemp now so extensively used throughout the East, under the name of Bhang, to produce a temporary intoxication, and the same drug which in the form of an extract is the bane of the hasheesh-eater.

Constantly through the years succeeding the death of Pliny, from Dioscorides, from Matthiolus, from the spirited narrative of Marco Polo, and all the chronicles of the crusades, from the old historians of the East, from the records of the Inquisition, and the published cases of the long series of illustrious surgeons down to this very year, we find mention made of attempts to relieve pain, while equally often proof is given of their inefficiency and failure.

The reason of this is obvious: with few exceptions, their experiments were directed toward the effects of solid narcotic substances upon the system. It was the substances used, and not the method, which caused the failures. Opium, which is one and perhaps the best of all narcotics, if given in sufficient amount to wholly deaden pain, (which can be done), possesses the most disagreeable property of deadening the recipient so utterly, that he rises no more in this world in a bodily form; consequently the user is always placed in the disagreeable dilemma of inflicting a certain amount of pain and keeping on the safe side, or of risking a coroner's inquest and a verdict of manslaughter and mal-practice. The trials, however, demonstrated one valuable fact, that as when swallowed they produced slowly a much more continued and excessive stupefaction than was needed, it was important to substitute some article which should produce the effect more quickly—more safely, even if it were used a little carelessly; and above all, one which should not cause a persistent condition when the administration had been stopped.

This was the first step toward our present state of knowledge. When the men of science had arrived at this conclusion, they all fell to experimenting and suggesting year after year. Old Baptista Porta proposed what he called his sleeping-apple, 'the smelling of which binds the eyes with a deep sleep,' which was a ball formed of some narcotic drugs, that was to be kept from the air, and when wanted for use, was to be held under the nose. Cold was another means, as it was found that when a man was nearly frozen to death, his sensibility to pain was much blunted. Pressing upon the nerves until there was no sensation in the limb; choking and bleeding the patient until he was all but insensible, were other plans. Mesmer advanced the theory of animal magnetism, and made many converts; but whether before or after they were operated upon, has never been decided. Finally, some ingenious man, whose name is wholly unknown to fame, suggested the use of alcohol—that is, the patient should be made so drunk that he could feel nothing. This being a pleasant form, met with much success, and was the second step onward. But still with this there were some faults. It was found that it required too long to produce the effect, that it was not caused equally in different persons, that it lasted too long, and lastly, that it was somewhat expensive and dangerous. So the wits of the chemists were set to work to devise something better than alcohol.

There is now existing near Naples, and records concerning it date back as far as Pliny, a cave called 'Grotta del Cane,' which is probably one of the outlets through the volcanic crust of which the whole vicinity of Vesuvius is composed, as from it constantly exhales a steam which is found to contain large quantities of carbonic-acid gas. This gas, which is heavier than common air, is totally destructive to animal life, if sufficient time be given for its effects; and any animal placed in it will die, unless quickly removed from it into the pure air. Taking advantage of this known fact, it was proposed to administer in such quantity as to produce its insensible but not deadly effect, and that instantly on its appearance, the administration should be suspended, and the operation be performed during the interval before perfect recovery.

This was the third step. But unfortunately its use was found to be too dangerous; and although it was advocated by intelligent men, and it was stated that some operations were performed under its influence, it fell into disfavor, and has never since been resuscitated. But it gave the right direction to investigation. In 1799, Humphry Davy, (not then Sir) who was an assistant in an institution in England for the treatment of disease by the inhalation of various substances, commenced a series of experiments with nitrous oxyd, or, as it is often called laughing-gas. These he published, and although he does not seem to have rendered himself by its use, at any time, wholly insensible, he must have caused some decided effect, for he has written: "As it appears capable of destroying physical pains, it may probably be used with advantage during surgical operations." This was the fourth important step.

But his was only a suggestion—a proposition which was never put by him to the test of experimentation; its death was coincident with its birth as far as any real benefit accrued from it to mankind. Nearly fifty years afterward, in the winter of 1844, a public lecture was given in the city of Hartford, to illustrate the effects of this very agent, the laughing-gas. Among the audience was a person by the name of Horace Wells, who, struck by its effects upon one of the persons who had inhaled it, made the casual remark: "That he believed that a person (under its influence) could undergo a severe surgical operation without feeling any pain." He offered to inhale the gas himself, and allow one of his teeth to be extracted. He did inhale it that very evening: a tooth was extracted, and as he asserted, without the slightest sensation of pain. This was the fifth great step, the demonstration of an invaluable principle—in fact, the discovery.

A number of experiments were subsequently made with the gas both by himself and others, and as is shown by the affidavits of many good and reliable men, with an eminent degree of success; for many dangerous and ordinarily painful operations were performed upon persons who took oath they had experienced no pain whatever. But two objections were found with it; its preparation was somewhat troublesome, and it was rather too bulky for transportation. He accordingly searched for some other agent which should more fully and perfectly fill the needful indications. There was then sold in every druggist's shop an article of common use in medicine, the effects and

method of managing of which had been perfectly well known for over five hundred years. This medicine was called an ether, the name of the chemical acid used in its manufacture being prefixed to it by way of designation. As there are many kinds of acids, there are consequently many kinds of ethers; of these the most common is sulphuric ether, sulphuric acid being used in its fabrication. It is a clear liquid, like water, highly volatile, intoxicating in effect, but in a much more rapid and excessive degree than alcohol. For over fifty years it had been recommended by medical men for inhalation in certain diseases of the lungs; and from individual cases where it had been used in this way, it was known that a certain amount of intoxicating effect would be produced. Reasoning on the fact which had been before experimented upon, that alcohol would produce a suspension of personal suffering, and the similarity of effect known to be caused by the inhalation of ether, Mr. Wells determined to try if it would supply the deficiency. This was the sixth, and the step which has left us at our present state of knowledge.

But although much was anticipated by him from the ether, he did not consider it wholly satisfactory, for he ultimately returned to the use of his first agent, the nitrous oxyd gas. During a visit made by him to Boston that same winter, he communicated his discovery to an old friend and partner, named Morton. On the sixteenth day of October, 1846, this same Morton made his appearance at the Massachusetts General Hospital in Boston and there, in the presence of a large number of surgeons and spectators, administered this ether to a man from whom was then removed a large tumor, without his having experienced the slightest pain. As from that day the surgeon has been able to gauge the amount of suffering he will inflict in any operation, as accurately as the corner-grocer can weigh out a pound of sugar; as narcotics, for three thousand years the sole champions against inflicted pain, unconditionally vacated the arena on the approach of the new-comer, it would seem the easiest thing in the world to tell the time of the discovery and the name of the man who really conferred it.

Three men have stood before the world as claimants for the honor. Horace Wells for his acknowledged use of nitrous oxyd gas in 1844; William T. G. Morton upon the undisputed ground of his public exhibition in 1846; and lastly, Dr. Charles T. Jackson, who makes the positive personal assertions that he, in 1842, by the accident of inhaling an excessive amount of ether, made the discovery that it would produce a perfect insensibility, and that it was from his information, and at his instigation, that Morton performed the conclusive experiment at the hospital.

By priority of date, it is obvious that the credit should be awarded Dr. Jackson, provided it were perfectly proved, first, that he did discover in 1842 what he asserts, and, second, that he reduced what was at first but a theory to a certainty, by the test of actual experiment. But here is the dilemma. Nothing was heard of his discovery and claim, until subsequent to its verification by another, while out of his own mouth, it is proved that he never experimented upon what he considered so invaluable a discovery. Whether he induced another to experiment for him, is a simple question of veracity, in which the pub-

lic have little interest; but as he kept his secret so well for four years, it is allowable to suppose that humanity might have been none the wiser at the end of forty.

Morton, who evidently considers the pen as mightier than the sword, and makes up by multiplicity of documents for weakness of proof, makes a direct denial that he ever received his information from Wells. Yet it is allowed that two years before his public appearance, he knew that Wells was experimenting with nitrous oxyd, and that he conversed with him concerning it. Some corroborative testimony is evidently needed to show when he formed and experimented upon the theory. The claim of Horace Wells rests upon testimony showing, that from 1844 to 1846, he used both ether and nitrous oxyd gas, to produce anæsthesia; upon testimony showing that he communicated his knowledge directly to Morton, and probably indirectly to Jackson. Could more be required to establish any demand? Should not this grain of truth, picked from the bushel of chaff with which the antagonism of others has enveloped it, be sufficient, under the benign influences of honest investigation, to produce a harvest of honor to the memory of that man, who died unnoticed and unrewarded, after bestowing one of the greatest blessings ever conferred on suffering man?—*The Knickerbecker, or New York Monthly Magazine, Feb., 1859.*

ART. XIII.—*Hints to Craniographers, upon the Importance and Feasibility of establishing some Uniform System by which the Collection and Promulgation of Craniological Statistics and the Exchange of Duplicate Crania may be promoted:* By J. AITKEN MEIGS, M.D., Professor of the Institutes of Medicine in the Philadelphia College of Medicine; Corresponding Secretary of the Philadelphia County Medical Society, etc. [From the Proceedings of the Academy of Natural Sciences of Philadelphia, August, 1858.]

EVERYWHERE Ethnology, the youngest and most ambitious of the sciences, is at length beginning to receive that earnest attention which the grandeur, importance and acknowledged complexity of its problems demand. Treated at first as an eminently speculative and metaphysical study, it has come at length to be considered as amenable chiefly, if not entirely, to the purely scientific methods of research employed by the naturalist. Long ago, the Ethnology of the so-called philosophical school, was simply a meagre Anthropology, made up almost wholly of certain national, psychologic phenomena of uncertain value and unknown relations. In the metaphysical systems of this school, physical characters and the working formulæ of the naturalist found no place. But the attempt to separate the phenomena of mind from the physical conditions with which they are constantly associated in nature, proved as barren in results, as it was irrational in conception.

Anterior to the time of Linnæus, the *philosophy* of man was everything, the *science* nothing. Of the latter, the great Swedish naturalist was himself the founder and first exponent. After Linnæus, came Buffon, Daubenton, Camper, Sœmering, Blumenbach, Zimmerman, and others, who, by regarding, and therefore treating man, as falling legitimately within the scope of the zoological method, attracted serious attention to the study of Anthropology as one of the natural sciences. The labors of these savants formed the foundation of this study, and gave it an impetus which is felt even at the present day. It must be confessed, however, that since the days of Blumenbach, in whose hands it received its more exact and scientific form, the progress of the study has been both slow and irregular; and the facts collected neither so extensive, nor for the most part so thoroughly and satisfactorily established as could be desired. Buffon, Cuvier, Lawrence, Péron, Kohnst, Davis, Wilson, and others, have all, at different times, taken occasion to deplore and comment upon this evident neglect of an important investigation.

Closely examined, the cause of this manifest neglect, and consequent slow progress of the Ethnological branch of Anthropology, will be found to be two-fold. In the first place and for a long time, the study of man was by many entirely separated from that of the rest of creation. Barbançois, Marcel de Serres, Ray, Brisson, Pennant, Vic d'Azyr, and other naturalists following Aristotle, the illustrious founder of philosophical natural history, have all in succession ignored the physical character of man, by excluding him in their classifications from the animal kingdom. Thus effectually isolated, he has been treated from a mental point of view only, and his zoological affinities and analogies completely overlooked. Another class of observers, however, having a more comprehensive conception of nature, and the unity of design which pervades it, have at length recognized the animality of man, and placed him accordingly among, and at the head of, the Mammalia.

In the second place, the study has thus far been one of divided effort, undertaken by individuals who, at different times and in different places, widely separated from each other, have worked at and attempted to solve with varying success, each his own favorite problem, with little or no aid from, or correspondence with others interested in, and studying the same specialty. In a word, there has been little or no combination, or regularly systemized effort among the students of Ethnology.

The human skull is so positively distinctive of race, that it claims at the hands of the student of Anthropology the most minute examination. The receptacle of the brain, of the organs of the senses and the masticatory apparatus, it exhibits race-characters more striking and distinguishing than those presented by any other part of the bony system. The pelvis, perhaps, comes next to it in ethnographic importance. The configuration of the skull influences to a considerable extent the characters of the countenance and shape of the features. "Hence, our zoological study of man," says Lawrence, writing in 1819, "will be greatly assisted by carefully examining genuine specimens of the skulls of different nations, which are easily

prepared and preserved, may be conveniently handled and surveyed, considered in various points of view, and compared to each other." Just twenty-one years afterwards, Wilde, of Dublin, wrote: "It is now universally admitted by the first authorities in this science, that to the form and character of the head can we alone refer in order to determine the varieties of man, either existing or extinct." (*Narrative of a Voyage to Madeira, Teneriffe, and along the Shores of the Mediterranean*: By R. W. Wilde, Dublin, 1840, vol. 2.)

Craniography is in truth destined to constitute one, perhaps the most important of the corner stones upon which the great edifice of the Natural History of Man is to be hereafter erected. To become a sure and solid foundation it must be composed of numerous and well-established facts upon which the student may unhesitatingly build up, until he can overlook and successfully grapple with the higher problems of the science. Like its elder sisters, Astronomy, Geology and Palæontology in former times, Ethnology is at present passing through what Comte calls the metaphysical or speculative phase of its career. Obscure writers and lecturers, impatient of that careful and laborious research which leads to correct results, and unacquainted even with what has already been done by Blumenbach, Retzius, Morton, Huschke, Davis, Virchow, and others, are daily bringing disrepute upon the whole study, by unwisely discussing questions for the solution of which the data have not yet been developed. That these controversialists of an hour may be silenced, and the science positively advanced every effort should be made to multiply and classify facts. But the multiplication and classification of facts must in great measure keep pace with and be dependent upon the establishment of cranial collections, which constitute, so to speak, the store-houses of the raw material ready to be elaborated into a science.

These collections and the important studies which they facilitate, are daily attracting more and more the attention of scientific men both in Europe and America; and the conviction is constantly gathering strength, that the zealous cultivation of Craniography is capable of yielding facts of the highest importance not only in a purely scientific, but also in a political point of view.

In the "summer of 1830, Dr. S. G. Morton delivered a lecture, introductory to a course of anatomy, 'on the different forms of the skull, as exhibited in the five races of men.' Strange to say, he could neither buy nor borrow a cranium of each of these races; and he finished his discourse without showing either the Mongolian or Malay. Forcibly impressed with this great deficiency in a most important branch of science, he at once resolved to make a collection for himself; and after a lapse of sixteen years, deposited in the Academy of Natural Sciences of Philadelphia a series embracing upwards of 700 human crania, and an equal number of inferior animals." This collection, now in the possession of the Academy, contains at the present time about 1100 crania, representing more than 170 different races and tribes of the human family.

The establishment of this large and varied collection, and the important investigations to which it has given rise, have served to

stimulate sensibly the advance of Craniography in Europe. Some years ago, "the Emperor of Russia was induced to found at St. Petersburg a national museum, exclusively dedicated to craniology, to contain the skulls of all the ancient and modern races of his vast dominions," (See Squire's *American Ethnology*, p. 3). All over Europe, Craniography is now being cultivated with considerable activity, and with highly valuable results, by such men as Retzius, Nilsson and Eschricht, of Scandinavia; Gosse, of Geneva; Dumontier, Blanchard and Serres, of France; Engel, Jeune, Carus, Virchow Huschke, Luceæ, Fitzinger and others, of Germany; and by Davis, Thurnam, Williamson, Minchin, and others, in Great Britain.

Many cranial collections are to be found in Europe and America, differing in the number and ethnic variety of their specimens. Precise information, however, as to their location, extent, variety and proprietorship is not easily obtained. From the writings of the craniographers above mentioned, and from my correspondents—especially Dr. J. Barnard Davis, of Shelton, England—I have become acquainted with a few of these collections. The largest and most diversified, as far as I know, is that contained in the Museum of the Army Medical Department, at Fort Pitt, Chatham, England. Of the existence of this collection I was not aware, until a descriptive catalogue of it appeared in the *Dublin Quarterly Journal of Medical Science*, for May and August, 1857. Through the kindness of Dr. George Williamson, the author, I have since received a copy of this catalogue, containing valuable photographs of many of the skulls.

The skulls embraced in the Chatham collection number about 600, and are arranged in four classes; 1st, oval-shaped skulls, including Europeans, Egyptians, Afghans, Hindoos, Singalese, New Zealanders, Otaheitans, etc.; 2d, skulls with projecting alveolar processes, or with the nasal bones in the same plane, comprising West African Negroes, Kaffirs, Hottentots, Bushmen, inhabitants of Mozambique and Madagascar, and the black natives of Mauritius, New South Wales and Van Diemen's-land; 3d, skulls with very prominent superciliary ridges, containing Sandwich Islanders, and 4th, skulls with broad and flat face, including Burmese, Malays, Chinese, Eskimos and North American Indians. The collection appears to be quite diversified. It contains specimens of about seventy different tribes and nations. Among these are many not represented in the Academy's collection: such as Albanians, Maltese, Spanish, Baluchi, Pariahs and Singalese among the oval-headed; and Mandingos, Joloffs, Timmanni, Sossoos, Kassos, Hausas, Hanti, Attans or Oppas, Pappas or Mahais, Barconkas, Ashantis and others of the prognathous African form.

Dr. Williamson's Catalogue is a very acceptable addition, not only to the literature of Craniography, but also to the means by which this science might be advanced. It contains short but important descriptions of 547 crania, and seven skeletons of different races of men, with extensive measurements of the same. The appendix embraces a number of anatomical measurements, estimates of the internal capacity, and a valuable table showing the relative frequency with which the ossa triquetra or Wormian bones appear in the occipital suture in the different varieties of men; the number of instances in which the



sphenoid is cut off from the parietal bone by a process of the temporal; the number of instances in which the lachrymal groove is formed entirely on the nasal process of the superior maxillary bone; the frequency of a suture in the centre of the frontal bone; and the size of the occipital foramen in the various races. The author also calls attention to the difference in the form of the anterior nasal openings in different classes of skulls, and illustrates his remarks with a number of outline engravings.

Of the present location and condition of the collections made by the earlier craniologists—those industrious pioneers of the science—I know nothing. Soemmering, writing in 1785, speaks of having examined the collections of Camper at Klein-hankum; of Hovius at Amsterdam; of Walter at Berlin; and of Blumenbach at Göttingen, (*Ueber die Körperliche Verschiedenheit des Negers vom Europäer.*) In the course of my reading, I have often found allusion to the cranial cabinets of Rau, (mentioned in Sandifort's *Museum Anatomicum*), Albinus, Gualtherus von Doeveren, Munro, Kokilansky of Vienna, and many others. Derwent Conway, in a work on Switzerland, published in 1830, says that "at the site of the cemetery of Zug, is a Golgotha, where are thousands of skulls piled upon one another." Of Gall's famous collection, a catalogue, translated from the French of Dr. Dauncey, his pupil and friend, appeared some years ago, in the 6th Vol. of the *Edinburgh Phrenological Journal and Miscellany*. In 1824 or 5, Mad. Becker, the niece of Gall's first wife, presented a portion of this collection to Dr. Roulett of Baden, near Vienna. Vimont's collection, in 1828, amounted to more than 1200 skulls and casts of man and animals. The celebrated Deville collection, at the Strand, in London, numbered, in 1830, more than 1800 casts and skulls. The Hunterian Museum, according to Prof. Owen's Catalogue published in 1853 contains a number of human crania. Judging from the numerous donations recorded, from time to time, in the different volumes of the *Edinburgh Phrenological Journal*, the Museum of the Society of that name must contain quite a large cranial collection. The collection of the Society of Antiquaries of Scotland was catalogued by Prof. D. Wilson, while acting as Secretary of that Society. Twenty-five years ago, when Phrenology was exciting such attention, numerous Phrenological Societies were founded in various parts of Great Britain. Busts, casts and skulls of men and animals were industriously collected and deposited in the Museums of these Societies. But many of the latter having ceased to exist, after a few years of activity, it is not now easy, though very desirable, to obtain any information concerning the disposal made of their collections.

Cranial collections are also contained in the Cabinets and Museums of many scientific associations, colleges, universities, etc., throughout Europe, such as the Royal College of Surgeons, in London and Dublin, Guy's Hospital, the Yorkshire Philosophical Society, the Bristol Infirmary, the Senckenberg Natural History Society, the Josephum in Vienna, the University of Dorpat, the Fremley Museum at Utrecht, Rijks Museum and the Anatomical Cabinet at Leyden, etc.

In a letter dated Shelton, 25th Dec., 1857, my friend, Mr. J. Barnard Davis, informs me that his private collection, at present, "exceeds

450 specimens." In the Museum of Thos. Bateman, Esq., at Lomberdale House, Derbyshire, England, are numerous "skeletons, skulls, and separate bones, exhumed from tumuli chiefly of the Celtic period in Derbyshire, Staffordshire and Yorkshire," amounting at the time of the publication of his excellent antiquarian catalogue (1855) to 224 specimens. Among other private collections may be mentioned those of Retzius, Nilsson, Eschricht, Van der Hoeven, Thurnam, etc.

The Mortonian collection, in this city, is by far the largest and most diversified in the United States, and, as far as I can learn, in the world. The Smithsonian Institution is in possession of several hundred crania, chiefly of American Indians. I have examined a few, also, in the Patent Office, in Washington. Most of the cranial collections in this country are small and are principally contained in the Museums of Scientific Associations, Medical Colleges, Phrenological Societies, etc. In this city (Philadelphia) the Museum of the University of Pennsylvania contains 169 skulls; that of the Jefferson College 72; that of the Pennsylvania College 125; and that of the Philadelphia College of Medicine 80.

Now, from the foregoing remarks, the members of the Academy will perceive that to the student of craniology, catalogues of all such collections, whether large or small—especially if they be descriptive—are of very considerable value. They make known to him the existence of other collections besides his own, and inform him what races and tribes of men are represented therein, or, in other words, precisely how much, and what available material has been collected for the furtherance of his scientific speciality. He may be desirous of studying the cranial characters of a particular race, of which the specimens in the only collection to which he has access are few in number or of doubtful origin. Having exact catalogues of the various cranial collections which have been made, from time to time, and deposited in different parts of the world, he turns to these and at once learns how many specimens of this race, besides his own, have been collected and where they are located. He at once opens a correspondence with the proprietors of these collections, and is soon put in possession of any information which he may desire.

Moreover, these catalogues would form an admirable basis for the interchange of duplicate crania, for the owners of them would know exactly where to apply to make up their deficiencies. The correspondence, also, to which the interchange of catalogues and duplicate crania would give rise, would of itself greatly facilitate the progress of Craniography, by making the students of this science acquainted with each other, and enabling them by a private interchange of opinions to verify their conclusions or examine them in different points of view before publication. There can hardly be a doubt that the different collections would be respectively increased by the extensive distribution of such catalogues in the hands of army, navy, and other government officials, officers of merchantmen, travelers, naturalists connected with exploring expeditions, and others whose opportunities might be favorable to making such collections, and who would cheerfully do so were their attention once explicitly directed to this matter.

Again, it appears to me that the progress of craniography might be very much and very readily facilitated by some such plan or system of coöperation as the following. Let all those actually engaged or interested in the study, in any particular country, notify the secretary, or appropriate officer, of the most prominent and best known scientific institution in that country, of the existence and precise location of any collection of crania with which they may be acquainted, no matter how small or imperfect such a collection may be, stating carefully the name and address of the Society or individual owning the collection, the number of skulls contained, and the different races of men represented therein. Let the secretary or other officer receiving such communications cause them to be published from time to time in the printed journal, transactions or proceedings of the Society. These being sent in exchange or otherwise, to scientific associations and individuals in other countries, would thus become the vehicle for the transmission of this information to the craniographers of the latter places. In the United States, for example, the Academy of Natural Sciences of Philadelphia, might, with great propriety, be selected as the recipient of all such craniological statistics, and the *Proceedings* of the Academy distributed to 200 associations and individuals in Europe, and to 210 in America, would then become the channel through which these statistics might be very generally made known. In Canada, the Canadian Institute, and its literary and scientific organ, the *Canadian Journal*, might be selected for a similar purpose. In Great Britain, the London Ethnological Society, might be enlisted in carrying out the objects under consideration by this or some other similar plan. French Ethnology being represented at present by the *Société de Géographie de Paris*, as I am informed by its learned secretary, M. Alfred Maury, in a letter dated June 24th, 1858, it appears to me this Society, through its *Bulletin*, might coöperate very successfully in the furtherance of this plan, with the craniologists of Scandinavia, Holland, Germany, Russia, Southern Europe, etc.

The editors of scientific, medical and literary magazines, journals, reviews, etc., have it in their power greatly to promote craniographic science by inserting in their pages from time to time, and thus disseminating the information obtained in the manner indicated above. This statement particularly applies to medical journals, inasmuch as most of those cultivating Craniography are physicians, not a few of whom are in the active public or private practice of their profession.

These crude suggestions are offered to the Academy in the hope that, being embodied in its proceedings, they may attract the attention and active support of those who are interested in, and who are able and willing to advance so important a branch of the *Science of Man*.

Finally, all Societies, colleges and individuals in this country, owning human cranial collections, and under whose notice this communication may fall, are respectfully requested to notify the undersigned of the existence of such collections, the whole number of skulls they contain, the races or tribes of men represented, and the number of skulls appertaining to each race; and to print descriptive, or if this cannot be done, simple catalogues of the same, and distribute them to all the craniologists and scientific associations.

The importance of this whole subject is urged upon the attention of foreign craniologists also, and their coöperation solicited by

JAS. AITKEN MEIGS, M. D.,

*Librarian of the Academy on Natural Sciences of Philadelphia,*

N. W. corner of Broad and George sts., Philadelphia.

July 31st, 1858.

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ART. XIV.—*Radical Cure of Hernia.*

GENTLEMEN: Allow me to call the attention of your readers to an article by Mr. Wood, of King's College Hospital, London, describing a new operation for the radical cure of inguinal hernia, published originally in the *Lancet*, of May 20, 1858, and copied into the last (January) Number of Braithwaite's *Retrospect*. Mr. Wood's procedure seems to me to come nearer fulfilling all the requirements necessary for the permanent cure of inguinal hernia than any heretofore brought before the profession. It is doubtless true, as Prof. Stone, of the University of Louisiana contends, that there can be no truly radical cure of the affection in question, except where the natural obliquity or valvular arrangement of the inguinal passage is preserved; that in all cases of the direct variety, and in those cases of the oblique in which the internal abdominal ring has been dragged down so as to lie opposite the external, the most perfect mode of treatment can effect only an approximative cure. Now, it is very well-known that the condition just mentioned as being the only one favorable for a radical cure, is found only in young subjects, and in adults in whom the rupture has taken place rapidly; and, on the contrary, that in the great majority of patients applying to the surgeon for relief, when the variety of the affection is not direct, the inguinal canal is virtually obliterated, the two rings lying directly opposite to each other. It is equally well-known that in the former set of cases a cure may be produced by a well-adjusted truss, or by Wutzer's operation, the indication here being to bring about adhesion of the opposite walls of the canal throughout their whole length. In the other cases every method that has been heretofore employed may be considered as only palliative or positively detrimental. From this general assertion I

do not exclude the operation of Wutzer, for, notwithstanding the success ascribed to this procedure, we are just now beginning to realize the truth of the unfavorable predictions made for it by the practical thinking surgeons of Great Britain, predictions based upon the fact that an operation which has for its object the substitution of a soft cicatricial tissue for the strong, unyielding natural structures of the abdominal walls, must, of necessity, prove a failure. Indeed, even in the most favorable cases, such as those referred to above, it appears to me that Wutzer's operation, the first effect of which is to distend the inguinal canal by a large deposit of lymph, must, in many instances, prove really injurious by pressing the parts asunder and thereby weakening their natural resistance. In the more common and troublesome cases, where the obliquity of the canal is lost, and its length reduced to the mere thickness of the abdominal walls around the external ring, and more especially in the direct variety of the affection, the most that such an operation can effect, is simply the retention of the protruded part, until the lymph has become in a great measure absorbed, or so loose and open in its texture as to offer no effectual resistance to pressure from within, a condition which is sure to take place in the course of a few months, or at farthest, a year or two. In proof of this, were it necessary, patients could be pointed to, who, operated upon by this method, less than a year ago, and discharged as cured, are now in as bad a condition as before the operation. I am inclined to think, therefore, that this procedure, so highly praised in Germany, its native country, and so eagerly seized upon by some of our own countrymen, is a decided failure.

It will be seen, by reference to Mr. Wood's paper, that this gentleman proposes to approximate the opposite borders of the abdominal opening, by a subcutaneous incision and suture—not to fill up the opening with lymph, but to reduce its size by bringing together the strong unyielding tissues composing the abdominal wall in this situation. The operation is, therefore, applicable to all cases of direct inguinal hernia, and to those of the oblique variety, in which the internal ring lies immediately behind the external, including, therefore, about four-fifths, or even a greater proportion of all the cases that apply for treatment. But, as described by Mr. Wood, the method, although very ingenious, is altogether too complicated for general practicability, and will doubtless undergo many modifications before coming into common use. But the principle is the true one, as all

must admit who are familiar with the anatomy of the parts concerned. And here permit me to say, that for some time past, I have myself been engaged in devising a method based upon the same, and had already matured a plan, which I was only awaiting a favorable opportunity to put into practice, when Mr. Wood's communication met my eye. As this plan is much more simple and entirely fulfills the main indication, I take the liberty of presenting a description of it, under the title of

*Sub-Cutaneous Suture of the Abdomino-Inguinal Opening, for the Cure of Inguinal Hernia.*—The special instruments necessary for its performance, are—1. A long needle, permanently fixed in a wooden handle, spear-pointed, with the eye and a slight curve near its pointed extremity—such a needle, for instance, as is sometimes used for carrying the ligature through the borders of a vesico-vaginal fistula; and—2. A flat, oval, metallic plate, about an inch in length, having a small stationary ring or knob fixed to the centre of one of its surfaces, and upon each side of this ring, above and below, and about one-third of an inch apart, a perforation sufficiently large to permit the easy passage of two thicknesses of small silver wire. One or two silk ligatures, and two or three feet of silver wire, such as is used in vesico-vaginal fistula, will complete the equipment.

The hernia having been reduced, a line should be drawn with ink or some other colored material over the long axis of the abdominal opening, *i. e.*, from above downward and inward, the object of which is to preserve a knowledge of the relative position of the skin to the opening in the introduction of the sutures. Next draw the skin thus indicated, beyond the outer margin of the opening, and hold it firmly there while the index finger of the left hand is pressed into the opening sufficiently far to hook the finger nail beneath the outer border of the latter. The needle, armed with a silk ligature, should now be introduced through the skin, upon the line previously drawn, and through the tendinous structures of the abdominal wall, entering the latter two or three lines below the level of the highest point of the opening, and nearly half an inch from its outer margin. When the point of the needle enters the abdomen, as can be ascertained by the forefinger of the left hand previously introduced as directed, which serves at the same time to prevent injury to the peritoneum and the contained organs, the instrument should be turned across the

upper part of the opening, and carried through the opposite border from within outward; to effect which the handle will of course require to be firmly depressed toward the surface of the thigh, and the finger partly retracted from the canal. Upon penetrating the inner border of the opening, it will be necessary also to push the needle on through the skin, when the ligature should be seized, and the instrument withdrawn entirely and detached from the ligature. The needle should now be reintroduced at the first point and carried again through the second, but this time in front of the abdominal opening, traversing the subcutaneous areolar tissue. When its point emerges at the orifice in the skin upon the inner side, the end of the ligature hanging from this puncture must be again passed through the eye and the instrument withdrawn. Both ends of the ligature will now be found hanging from the puncture made at the first introduction of the needle, which point, when the skin is allowed to return to its natural position, will lie over the long axis of the abdominal opening, and a few lines below the highest point of the latter. A silver wire may now be substituted for the silk one, by attaching it to one end of the latter and drawing it through the structures.

Another such suture should be inserted in the same manner, three or four lines below the first; and, where the abdominal opening is very large, a third may be required, but not often; occasionally, probably a single one might answer.

The wires having been lodged in their places, the two ends belonging to each suture should be approximated and carried through the respective perforations in the metallic plate, and drawn with considerable firmness, so as to bring the opposite sides of the abdominal opening closely in contact. They may then be secured by making a few turns through the ring on the plate—or, probably, a little knob in place of the ring would answer a better purpose.

The apparatus may remain in place almost any length of time without any serious results; but from two to four weeks will suffice in most cases, supposing that adhesion will have taken place in that time, and the parts firmly consolidated with the superjacent skin.

The mechanism of this suture, it will be observed, is precisely the same as that of Bozeman's button suture, now so generally adopted in the treatment of vesico-vaginal fistula, and the reason for substituting a fixed ring or knob, for the ordinary means of fastening by perforated shot, is simply to permit the tightening of the wires, if

this should become necessary, and to facilitate the removal of the apparatus.

The object of the operation is not to close up the abdominal opening entirely, except in females, where this might really be done with great advantage, but to narrow it as much as the spermatic cord will allow, and as there is little or no danger of constricting this latter, injuriously, partly in consequence of the presence of the neck of the hernial sac and partly on account of the peculiar characters of the cord itself, the lower suture may be made to cross it as closely as possible.

As to wounding the neck of the hernial sac, there is no necessity for this if proper manipulation be made with the forefinger of the left hand while in the opening. But in case this accident should occur, no danger will result, but probably an advantage, provided the bowel or omentum be not included. This is indeed the greatest danger to be apprehended, but may be avoided by proper care.

In some future number of your journal, I hope to be able to report some cases treated by the preceding method, and, at the same time, furnish a more explicit description, aided by appropriate woodcuts.

T. G. RICHARDSON, M. D.,

*Prof. Anat., Med. Dep. University of Louisiana.*

Eds. N. O. Med. and Surg. Jour.

*Alimentary Canal—An Operation for the Radical Cure of Inguinal Hernia.*

[During the last few years the operation for the cure of inguinal hernia, as performed by Wützer of Bonn, has been tried with success in this country. A very able article on this subject will be found at page 148 of our last volume, and others at pp. 101 and 103 of vol. xxxv.]

More recently, a new method of curing hernia radically, by a simple process, has been invented by Mr. Wood, of King's College Hospital; and from what we have seen of it, together with the state of the parts in the patient successfully submitted to it, we are inclined to think more favorably of it than of any other. The main features of this operation consist, 1st, in its being conducted subcutaneously, insuring a more ready and less painful healing; 2d, in the introduction into the canal of the two layers of superficial and intercolumnar fasciæ, which are unusually abundant and strong under the condition of hernia, and which are made to cohere into a solid plug by adhesion of their opposed surfaces, made raw by the subcutaneous separation from the skin, and are also supported and kept in position in the canal by the new adhesions contracted below by the skin from which they were separated; 3d, in the drawing together and close



union of the sides of the enlarged external ring by the lateral traction of the ligature upon them, caused by its passing through the same opening in the skin and in the compress upon it, and tying them down by consequent permanent adhesion to the invaginated plug of fasciæ behind them; and 4th, in the firm compression made upon the part during the formation of the adhesions, and their consequent greater extent and firmness, by the traction of the ligature upon the boxwood compress.

Mr. Wood is of opinion that the distinct evidence as to the position of the hernial sac of peritoneum and of the cord through the opening in the skin, and the great certainty that the feel of the tube gives, that its position at the extremity is close behind the aponeurosis of the external oblique, and has no intervening structure necessary to be avoided between it and the surface, remove almost entirely the danger of puncturing the sac in this operation; while he supposes that the succeeding pressure may operate after a while in producing adhesions of the opposed surfaces of the doubled-up sac. The operation leaves no dimple or deformity whatever; the cicatrices are very small and slight, and after a time will become nearly imperceptible.

John C—, aged twenty-five, a printer, applied at the hospital with a direct inguinal hernia on the right side, to which he had been subject eighteen months, with frequent obstruction and constipation of the bowels, and after meals had suffered from great pain and distress in the part. He has tried seven trusses, none of which had power to retain the bowel. The last he tried produced such irritation as to give rise to a series of abscesses in the groin, which was the direct cause of his application. The abscesses being healed, and the patient disposed to submit to an operation for permanent cure, the following was performed:

On examination, he was found to have a hernia projecting into the scrotum, producing a tumor, which he said sometimes reached the size of his two fists. On reducing it, the external ring and the internal opening were found to be enlarged, so as to admit the ends of three fingers, the margin being lax and loose. On the slightest cough or exertion, the bowel immediately dropped down into the scrotum.

*Description of the Instruments*—The instruments used in this operation consist of, 1st, a tube two inches and a half long, mounted on a strong handle, about three inches and a half in length, curved in a circle of an inch and a half radius, and flattened into an oval at one end, and forming a linear aperture a quarter of an inch wide at the point; 2d, a strong needle, having a corresponding curve, with a perforated point, projecting a full inch beyond the end of the tube when passed through it, and mounted on a strong handle; 3d, a boxwood pad or compress, two inches by one and a quarter, perforated by a hole at half an inch from one end, and crossed longitudinally by a bar of iron wire screwed on to the upper surface; 4th, a subcutaneous section knife, with a sharp point, a narrow blade, and an inch of cutting edge.

*Operation*.—The patient being laid on his back, with the legs a little drawn up, and the hernia returned, an incision, about three-eighths of an inch long, was made through the skin only, over the cord, about

an inch and a half below the external ring, with the subcutaneous knife, which was then carried close under the skin, so as to separate a circle of the superficial fascia around the opening of two inches in diameter. The detached fascia was pushed up into the inguinal canal by means of the curved tube, the end being placed through the opening in the skin. The extremity of the tube was then carried behind and close to Poupart's ligament, or the external pillar, to the extent of an inch and a half from the pubic spine. It was then felt, by depressing the handle, to raise the external pillar upon the extremity. The needle, carrying the thickest silk ligature, was then protruded through the tube, and pushed through the external pillar and the skin, the latter being previously drawn considerably downwards and outwards. The needle was then withdrawn, leaving one end of the ligature on the surface. The end of the tube was next shifted upwards and inwards, and made to protrude behind the internal pillar, as far as possible from its margin. Through this the needle was then passed, and the skin moved upwards and inwards till the point appeared at the opening previously made. The ligature was then freed, and the needle withdrawn, the tube being still held firmly in its position. The ends of the ligature were next passed through the hole in the boxwood compress, one on each side of the wire bar, over which they were then drawn close and tied firmly, so as to retain the pad close down to the end of the tube in the canal, which was then withdrawn. It will thus be seen that the first passing of the needle pierces not only through the external pillar of the external ring, but through the origin of the internal oblique and cremaster muscles, from Poupart's ligament behind it; and at the second passing, it goes through, not only the internal pillar, but also the conjoined tendon of the internal oblique and transversalis muscles, which is placed behind it, so that the sides of the inguinal canal are drawn together from end to end by the ligature. The wound in the skin of the scrotum was drawn firmly together by plaster, a fold of linen placed upon it, and the whole secured by a spica bandage; the patient being ordered to remain in bed, and to use no exertion whatever.

The bandage was removed on the third day, when the subcutaneous puncture was found completely healed by the first intention. The compressed ligatures were retained till the fifth day, some œdema and suppuration having by this time appeared around them, with considerable soreness and pain in the groin. The opening for the ligature looked red, healthy, and suppurating. It was dressed with wet lint and oil-silk, a large compress, and spica bandage. In a fortnight it was completely healed. No irritation nor swelling of the testicle occurred in the course of treatment, nor any symptom of peritonitis.

At present, three weeks after the operation, the external ring is felt by the finger, pushed up beside the cord, to be completely blocked up by a broad band of fascia passing across it and up along the canal, with the cord passing by the lower part. The sides of the canal are felt adherent and consolidated. There is no *ballotement* whatever felt on coughing; the groin of the side operated upon being more firm and resistant, in fact, than the opposite, which has always been a little weak. The patient was ordered to wear a truss for some months, to consolidate the part and protect the newly-formed

tissues. The skin is firmly adherent to the subjacent structures at the point of the punctures and subcutaneous separation. He has since undergone a severe test as to the efficacy of the cure in an attack of bronchitis, from which he has completely recovered, without in the least affecting the site of the hernia.—*Lancet*, May 29, 1858.

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ART. XV.—*The effects of certain substances upon the exposed hearts of animals.* By S. Wier Mitchell, M. D.

DR. MITCHELL (Dec. 20, 1858,) read an experimental paper before the *Biological Department of the Academy of Natural Sciences of Philadelphia*, of which the following is an abstract :

1st. That the hearts of the frog and turtle beat much less rapidly in vacuo, and sometimes cease to act until the air is readmitted. That the vacuum most probably retards the heart's action by the mechanical effects it induces, as well as by depriving it of oxygen, since the beat in vacuo is long and labored, and the accelerating influence of the readmitted atmosphere is almost instantaneous.

2. That mere isolation from the air, as by placing the heart in oil, does not alter the rate of the heart's movements for some time, but lessens their ultimate duration.

3d. That water, at aerial temperatures, stimulates the heart, and very soon causes it to cease to pulsate. That water at higher temperatures, as 100° F., and 113° F., produces much more rapidly the same results.\*

4th. That glycerine, at aerial temperatures, affects the heart but little, except as shortening the time during which it continues to pulsate. That glycerine at 32° F., depresses the heart's action, lessening the number of pulses per minute at least one half, and soon checking its movements altogether. That olive oil at 32° F., affected the heart very little at first, as to the number of beats per minute, but soon rendered them feeble, and finally stopped them; though at the close of a longer interval than was required by glycerine at the same temperature.

5th. That when the heart has ceased to respond to one stimulus, however violent, it will usually remain sensitive to others, apparently far less powerful.—*Proced. Acad. Nat. Sci.*

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ART. XVI.—*Académie de Médecine.* Award of Prizes for 1858.

THE Academy of Medicine (Paris), at the sitting December 14, 1858, awarded the prize of 3,000 francs, founded by Dr. Itard, to Dr. Duchenne, of Boulogne, for his Treatise on *Localized Electrization*

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\* I quote from memory, not having time to search for the record, an experiment showing that the expected heart of an alligator which had been regularly contracting for hours in the open air, suddenly ceased to move on being immersed in water at the aerial temperature.—B. DOWLER.

(*Traité de l'électrisation*), subject to a deduction of 500 francs as an encouragement to Dr. Foucart for his treatise on the Miliary sweating (*Traité de la suette Miliare*). The Itard Prize was founded for the purpose of rewarding the author of the best treatise or memoir on practical medicine or therapeutics within, at least, two years from its publication.

The Portal Prize, of 600 francs, for the best treatise on the Pathological Anatomy, Diagnosis, and treatment of Ovarian Cysts, was given to Dr. Louis Bauchet, surgeon of the hospitals of Paris.

The Barbier Prize, 2,000 francs, offered annually to such as may discover a perfect cure for diseases usually considered incurable, as hydrophobia, cancer, epilepsy, scrofula, typhus, cholera morbus, etc., etc., was not entirely appropriated, although the sum of 1500 fr. was accorded as an encouragement to Dr. Boinet for his work on Iodo-Therapy (*Traité de l'iodo thérapie*).

The D'Argenteuil, sexennial prize of 12,000 fr. to the author who shall make improvements of an important nature in the treatment of strictures of the urethra, or the allied diseases of the urinary passages, was not awarded in strict accordance with its original purpose. Among twenty-five memoirs submitted to the Academy, none was deemed worthy of the prize. Nevertheless, the judges, in view of the liberal aims of the founder, the Marquis d'Argenteuil, thought it expedient, on this occasion, to award recompenses: 1st, to Dr. Mercier, 4,000 fr.; to Dr. Gaillard, of Poitiers, and to Dr. Désormeaux, 2,000 fr. each; and, also, as encouragements, 1,000 fr. to M. Marquez, 1,000 fr. to Dr. Arnold, of London; and 1,000 fr. to M. Charrière (the father,) surgical instrument maker. At the same sitting Medals were awarded to many others.

A number of the standing prizes were not awarded at all, in consequence of the unworthiness of the essays presented, and are, therefore still open for competition during the year 1859.

B. DOWLER.

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ART. XVII.—*New Operation of Lithotomy.* The late Dr. MARSHALL HALL'S Proposition for a New Operation of Lithotomy.

*To the Editor of the Lancet.*—Sir: In the "Foreign Department" of your valuable journal,\* I noticed a proposition made by M. Valette, of Lyons, for a new operation for the removal of vesical calculi, which he called the hypogastric operation; and he narrated several cases in which it had been successfully performed by him. Whatever

may be the ultimate influence of this proposition of M. Valette on the present practice, I feel that an interest is now attached to the subject from the fact that my friend the late Dr. Marshall Hall held, in the month of June, 1855, as nearly as may be the same views as those lately advanced by M. Valette. A copy of the *lettre cachetée*, which Dr. Hall told me had been deposited in the Institute of France, has been kindly procured for me by Mrs. M. Hall from M. Flourens, of Paris, and I enclose it to you. I feel sure you will agree with me, that the document is of sufficient interest to be published in your journal.

I am, Sir, your obedient servant,

Brighton, Dec., 1858.

FREDERICK WILDBORE, F.R.C.S.

*Sealed Document.*—I propose to replace *lithotomy* and *lithotripsy* by *lithotomy*. 1. Before this operation is performed, the patient should be made to drink copiously. 2. A catheter, pierced with one hole only should be introduced, and so placed as constantly to leave in the bladder such a quantity of urine as seems desirable. 3. A canula, supplied with a sharp point, should be thrust into the bladder above the os pubis and below the peritoneum. 4. The point\* is then to be withdrawn, and the canula left, closed up by a little stop-cock, until a fistula, thoroughly surrounded by lymph, be well established. 5. This fistula should be properly dilated until the calculus can be extracted. 6. The fistula is afterwards allowed gradually to contract on an India rubber tube distended with air, of a proper size, which is gradually to diminish in calibre by the air being made to escape.

In the whole course of this operation no texture is divided; no blood flows; and the nervous system experiences no shock. A measure new in surgery is thus employed, viz. : dilatation instead of division of textures. This measure, when well conducted, is not dangerous to the patient, and offers no difficulty to the operator. It may also be applied in other circumstances; such as hydropericardium, empyema, and to replace tracheotomy, etc.

(Signed)

MARSHALL HALL."

Certified a true copy.

The Life Secretary for Natural Sciences.

FLOURENS,

Imperial Institute of France.

*The Operation of Lithotomy*—Proposed by Dr. MARSHALL HALL in 1855, and by M. VALETTE in 1858, suggested and performed with success by the late Mr. GEORGE BELL, of Edinburgh, in 1837.

*To the Editor of the Lancet.*—Sir: In the last issue of the *Lancet*, there is a letter by Mr. Wildbore, headed "The late Dr. Marshall Hall's Proposition for a new Operation of Lithotomy." The letter refers to a proposition by M. Valette, of Lyons, of a new operation for the removal of vesical calculi through an opening made above the pubis; and the object of Mr. Wildbore's communication is to prove that, in the year 1855, Dr. Marshall Hall held, "as nearly as may be, the same views as those lately advanced, by M. Valette." Mr. Wildbore has succeeded in establishing this fact; but the "proposal"

\* Or trocar.—ED. L.

itself is, in reality, an old story now, for, in the year "1837," a case was published in the *Edinburgh Surgical and Medical Journal* (volume xlvii., p. 412,) and is alluded to in Liston's "Practical Surgery" (p. 409, ed. of 1837,) which illustrates the principle on which the proposals of M. Valette and Dr. Hall are founded.

The case occurred in the practice of my father, the late Mr. George Bell, in the year 1829. The gentleman on whom he operated was, at that time, eighty years of age, and survived till the year 1836.

Having detailed the case, Mr. Bell concludes in the following words, which clearly and succinctly describe the operation he had successfully performed:

My proposal is—and it is my intention to follow it out as favorable opportunities occur—

1st. To inject the bladder, and puncture above the pubis, when the patient is in good health.

2d. *To allow the tube to remain in until the adhesive process has destroyed the chance of infiltration.*

3d. *To dilate the opening gradually as far as possible.*

4th. *If the size of the stone requires it, to enlarge the wound with the knife, in the course of the linea alba.*

This proposal is the result of experience, and I believe it to be thoroughly sound in principle. It is especially applicable to cases of stone, in which the prostate is considerably enlarged.

I am, Sir, your obedient servant,

G. BELL, M. D.

Atholl-place, Edinburgh, Jan., 1859.

[The residue of the article on the new operation, etc., has been unavoidably crowded out of this No.]

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## TO OUR SUBSCRIBERS.

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ON January 14th, 1859, *The New Orleans Medical and Surgical Journal* was purchased from the former proprietors, Drs. Stone, Jones and Chaillé, by Dr. S. E. Chaillé and Dr. Wm. C. Nichols (formerly of Alabama). Professors Stone and Jones will continue their connection with this journal as co-Editors, and both the editorial and business departments will remain as heretofore, the former under the management of Dr. B. Dowler, and the latter of Dr. S. E. Chaillé. In no respect, therefore, will the convenience or interests of our subscribers suffer by this change of proprietorship; on the contrary, there is every reason to hope that our columns will soon show evidence of an able and energetic laborer added to the editorial corps of the past eighteen months.

In the collaboration of this journal, every useful thought, sugges-

tion, fact or discovery, whether original or found floating on the broad stream of periodical literature, at all calculated to enlarge the horizon of medical science, will, as far as possible, be speedily communicated to its extensive and increasing number of readers throughout the vast expansions of the Southern and Western States of this Republic. It is desirable that this collaboration shall be coëxtensive and conterminous with these territorial expansions, whose variable climates and medical topographies blend with the tropical and temperate zones. A mere *New Orleans redaction* is too circumscribed for its aims. Its policy is expansion as well in collaboration as in subscription. Let every district be represented by scientific contributions.

In this way, this journal may hope to win the enviable name of a *home journal, an organ of the profession alone*; in this way its vitality will not be *parasitic*, but vigorous, and command the respect of foreign journalists, who cannot, however friendly, esteem a journal which is wholly a patchwork made out of theirs, having no originality beyond that which represents some pecuniary gain.

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## EDITOR'S OFFICE.—NOTICES.

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[Managing Editor's Office and Residence, 89 Constance, between Melpomene and Thalia streets; box 106 D, Post Office; box of the Journal at Mr. Morgan's book store, same place where communications, etc., may be received.]

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MARCH, 1859.

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Received, for this Journal, several Communications not yet considered.

## NEW MEDICAL JOURNALS.

- The Belmont Medical Journal*—Bridgeport, Ohio. Monthly. 16 pages.  
*Semi-Monthly Medical News*—Edited by S. M. Bemiss, M. D., Professor of Clinical Medicine in the University of Louisville; J. B. Benson, M. D., Professor of Descriptive and Surgical Anatomy in the University of Louisville. No. 1. Jan., 1859, Louisville, Ky.  
*The Louisville Medical Gazette*—Edited by L. J. Frazee, M. D. No. 1. Jan., 1859, Louisville, Ky.  
*The Marysville Medical and Surgical Reporter*—Edited and published by Lorenzo Hubbard, M. D., and H. W. Teed, M. D. Nov., 1858. San Francisco.  
 The obliging letter of Dr. G. Preyss, of Vienna, together with 38 numbers of his Journal, *Oesterreichische Zeitschrift Practische Heilkunde* (in exchange), has

been received, through the Smithsonian Institution. Also, through the same institution, a highly interesting MS. Letter (12 pages), from the Secretary of the Royal Society of Northern Antiquaries, under the Presidency of His Majesty Frederick VII, King of Denmark.

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## BOOKS AND PAMPHLETS RECEIVED :

*The Physicians Hand-book of Practice*, for 1859—By William Elmer, M. D. New York. W. A. Townsend & Co. 1859.

*The London Quarterly Review*—Jan., 1858. Am. Edit. L. Scott & Co., New York. From Messrs J. C. Morgan & Co., Booksellers, Exchange Place. N. O.

*Practical Dissections*—By Richard M. Hodges, M. D., Demonstrator of Anatomy in the Medical Department of Harvard University. Cambridge, Mass. John Bartlett. 1858. Pp. 254. From J. C. Morgan & Co., Exchange Alley, New Orleans.

*The Modern Practice of Midwifery. A Course of Lectures on Obstetrics, delivered at St. Mary's Hospital, London*—By Wm. Tyler Smith, M. D., Member of the Royal College of Physicians; with an Introductory Lecture upon the History of the Art of Midwifery, and copious Practical Annotations, by Augustus R. Gardner, A. M., M. D., late Instructor of Obstetrics in the New York Preparatory School of Medicine; Author of the "Causes and Curative Treatment of Sterility," etc. Illustrated by 212 engravings. Pp. 760. 8vo. New York. Robert M. De Witt. From Messrs. J. C. Morgan & Co., booksellers, Exchange Place, New Orleans.

*Annual Report of the Board of Regents of the Smithsonian Institution, showing the Operations, Expenditures and Condition of the Institution, for the Year 1857.* Washington. 1858. Pp. 438. 8vo. From the Institution.

*An Essay on Inflammation*—By J. H. Walters, M. D., Professor of Physiology in St. Louis Medical College. Pp. 32.

*Report of the Board of Health to the Legislature of the State of Louisiana.*—January, 1859. Pp. 62. Baton Rouge. 1859. From Dr. Baldwin, Secretary of the Board.

*Annual Report of the Legislature of South Carolina Relating to the Registration of Births, Deaths and Marriages*—For the year ending December 31, 1857. Pp. 98. Columbia, S. C. R. W. Gibbes. 1858.

*The Coast Survey*—Reply to the Official Defence of its Cost, Abuse, and Power. Pp. 36.

*The Retrospect of Practical Medicine and Surgery*—Being a Half-Yearly Journal, containing a Retrospective View of every Discovery and Practical Improvement in the Medical Sciences. Edited by W. Braithwaite, Lecturer on Obstetric Medicine, at the Leeds School of Medicine, etc. Part XXXVIII. January, 1859. Uniform American Edition. Pp. 366. 8vo. New York. W. A. Townsend & Co. 1859. From the Publishers.

*Introductory Lecture to the Female Medical College of Pennsylvania, for the Session of 1858-59*—By Mrs. E. H. Cleveland, M. D., Professor of Anatomy and Histology. Pp. 16. Philadelphia. 1858.

*Report on the Functions of the Cerebellum*—By E. Andrews, M. D. Chicago, Ill. Extracted from the Transactions of the American Medical Association. Pp. 19. Philadelphia. 1858.

*Annual Report of the Board of Administrators of the Insane Asylum at Jackson, La.*—Pp. 37. January, 1859. Baton Rouge. 1859.

*The Mosaic Account of the Creation*—By James C. Fisher, M. D., Member of the Academy of Natural Sciences. Pp. 8. Philadelphia. 1858.

*The Half-Yearly Abstract of the Medical Sciences*—Edited by W. H. Ranking, M. D., Cantab., Physician to the Norfolk and Norwich Hospital, and C. B. Radcliffe, M. D., Lond., Fellow of the Royal College of Physicians in London; Physician to the Westminster Hospital, and Lecturer on the Materia Medica at the Westminster School of Medicine. Pp. 316. No. XXVIII. July-December, 1858. Philadelphia. Lindsay & Blakiston. 1859. From the Publishers.



THE  
NEW ORLEANS  
MEDICAL AND SURGICAL JOURNAL.

MAY, 1859.

ORIGINAL COMMUNICATIONS.

ART. I.—*Observations and Reflections on Yellow Fever*: By M. MORTON  
DOWLER, M. D. (Continued from page 235.)

IN continuation of my observations and reflections on the subject of yellow fever, published in the March number of this Journal, I shall at present set out by offering a few remarks on the doctrine of importation, which now happens to be the official doctrine of this city. The legislation of the State is based on the assumption—which is, as we shall show, wholly disproven by experience—that there is proof from the history of the past, that New Orleans in the absence of quarantine restrictions, is every summer running the risk of epidemic yellow fever, through the medium of vessels arriving in her port from localities in which the yellow fever may be prevalent. It is to be regretted, that in the discussion of such questions as those of “*contagion*,” “*importation*,” etc., medical men, in their earnest endeavors in the development of truth, have not resorted to wider and more comprehensive principles. The discordant and contradictory deductions which almost always arise in the special investigation of any given epidemic, on the part of the physicians who may be appointed to inquire into its history, afford little or no guide to sound legislation. There is in general but little really sound philosophy infused into such inquiries. The *ignes fatui* of *contagion* and *importation*, are apt to be the most conspicuous lights. To this end an overwhelming

importance is always attached to *the first cases that occur, and the localities in which they occur*—circumstances which are, in all probability, really without any distinctive value, and throw no light whatever on the subsequent extension and progress of the epidemic. If there exist a salaried Board of Health and Quarantine—the utility of which annually becomes a subject of more than doubt in the legislative mind—the sanitary body will scarcely fail to take care to fortify itself by a special logic for the exigency. It will ignore the existence of an epidemic till its silence strikes the public with amazement, and then will proceed to fasten the mischief on the devoted hold of whatever ship it can be done with the greatest plausibility. The misfortune, according to the official ratiocination, is solely to be attributed to a want of stringency in the quarantine law, which might be easily remedied by an extension of authority; and the whole history of the epidemic previously to the alledged importation, is most vehemently discredited. Should the investigation of the subject be referred to an *unbiassed* medical commission, and upon inquiry there be found certain coincidences between the appearance of the “first cases” and personal intercourse with a certain ship which has arrived from a port in which yellow fever is prevailing, or on board of which the disease may be found to exist, there will generally be no hesitation on the part of the committee in proclaiming—as did the committee in view of the Norfolk epidemic of 1855—the supervening epidemic to be the result of importation. Nor will it avail when you remind the inquirers that their conclusion is environed by numerous palpable sources of fallacy: first, that the appearance of a ship at port and the inception of a perfectly independent and spontaneous epidemic at that port, may have been quite coincident with each other; second, that experience at the port of New Orleans affords proof, that not one time in a thousand would it so happen that an epidemic would follow such personal intercourse; third, that no existing quarantine has ever pretended to exclude the multifarious “fomites,” so called, from being directly sent into the very heart of our maritime cities throughout the Republic, that the so called “susceptible” articles of the United States mail, are never detained under any pretence to undergo the process of “fumigation;” that nail fumigation is no longer practised in Great Britain, but has fallen into utter contempt; that quarantine leaves the world virtually *in statu quo*, and that consequently the importation in question is merely a virtual

example of what is constantly occurring, both with and without the occurrence of epidemics; and fourthly, and generally that the importation is, under these circumstances, merely begging the whole question, and opens the whole controversy with the non-contagionists; for the positive evidence of yellow fever contagion, much less of its manner and form, is absolutely wanting, and the *modus operandi* of the material agent is unknown. The able and candid committee on the Norfolk epidemic of 1855, were perfectly satisfied that the conditions which gave rise to that terrible epidemic were imported into Norfolk from St. Thomas, on board of the steamer Ben Franklin; nor do they appear to consider that their conclusion does not exclude every source of error and fallacy. The report of that committee is characterized by a remarkable degree of clearness and good sense. The meteorological and terrene elements which have been absurdly proposed in this city to explain the aetiology of yellow fever, and claimed as a discovery, of which more will be seen in the sequel, is summarily set aside by the Norfolk Committee. I shall, however, maintain, when I come to speak of the material agent by which the phenomena of yellow fever is excited—an agent which in the present state of our knowledge possesses no sensible properties, and the very existence of which is as yet a purely rational and necessary deduction, *a posteriori*—that the explanation given on the part of the committee, of the origin and spread of the Norfolk epidemic is wholly untenable, and is irreconcilable with all the knowledge we possess of that agent, as tried by any recognized standard of either contagion or infection.

But let us here examine the legislative assumption which at present regulates our intercourse with the Gulf of Mexico. When medical reasoning either assumes the circular form, or when its conclusive teachings fail to carry conviction, it is often better to abandon such reasoning and appeal to the principles which regulate the workings of every-day life, and which are obvious to every man of business, and such kind of appeal is especially appropriate when communing with our legislators. In many instances in which medical men cannot agree, or in which it is not their good pleasure to agree, outside evidence fully settles the question. Let the doctrine of yellow fever imputation be admitted for the sake of argument, and let the chances that epidemics will or will not occur be relatively determined on the basis of the probability of that imputation; then ac-

ording to well-determined laws which are disclosed in the Theory of Probabilities, the results so far being in accordance with historical facts in this and all other cities will constitute, mathematically speaking, a *reductio ad absurdum*. If the doctrine of yellow fever importation be true, it follows that the probability that an epidemic will occur rather than that it will not, will exist in direct ratio to the antecedents which may tend to favor the importation. These antecedents are according to the quarantinists the arrivals of vessels direct from ports where the yellow fever is prevailing, or of vessels with yellow fever on board, or the introduction of what they are pleased to fancifully call "yellow fever fomites." Now according to one of our historical authorities, M. Debouchel, whose statement may not be *exactly* accurate, but which is sufficiently so for our present purpose, the yellow fever was epidemic in New Orleans in 1797, 1802, 1806, 1810, 1814, 1818, 1832, 1824, 1827, 1831, 1835, and it was so to my personal experience in 1837, 1839, 1841, 1843, 1847, 1853, 1854, 1855, and 1858.

The slightest glance at these figures will at once show, that it is utterly repugnant to common sense to attempt to establish the relation of cause and effect in any manner whatever between the reputed governing antecedents and these epidemic results. These reputed antecedents have multiplied at least a hundred fold since the year 1797, in the gigantic commercial progress of the city of New Orleans; but the succession of epidemics has *remained nearly stationary*. When her population was about 8,000, her shipping a few hundred tons, and she had comparatively neither imports nor exports, it will be seen that there was a non-epidemic interval of four years, but that between the years 1847 and 1853, she had a non-epidemic interval of *five years*, and that, too, at a period when her shipping had augmented to more than a quarter of a million of tons, her annual imports to twelve, and her annual exports to eighty millions of dollars—a period in which her incessant intercourse with the hotbeds of yellow fever is shown by the fact, that of the eight hundred thousand bags of coffee imported into the United States, valued at ten or eleven millions of dollars, one half is freighted into the city of New Orleans. Now we all know that where the governing antecedents to any series of consequents exist, these consequents must not only take place, but that they must take place in a regular, and not in an arbitrary or contingent manner. Should any contingency arise by which these antecedents are

rendered a hundred times more favorable to the certainty or probability of any series of consequents, the consequents themselves will inevitably show that their occurrence has been favored a hundred fold on the grounds of probability or certainty. The yellow fever epidemics of New Orleans and every other city pay no respect to importation in its application to these invariable laws, as may be readily shown. It follows, therefore, that in our investigation of the origin and propagation of yellow fever epidemics, *the importation element must be wholly excluded when viewed as the governing antecedent of yellow fever epidemics*, and that the legislative assumption which gave rise to our quarantine laws may be overturned with all the force of mathematical demonstration.

If the doctrine of yellow fever importation, as applied to the city of New Orleans, must thus necessarily be submitted to a test which fully sets aside the value of all the coincidences, "first cases," etc., on the strength of which learned committees, in their investigation of individual epidemics, have charged these visitations to the relation of ship and shore, it is not the less true that this test is equally applicable to all other cities, in the present state of our knowledge. Let us take for example the city of Charleston, and it will be found that the unfortunate frequency of her yellow fever epidemics must be referred to governing antecedents, of which we have not the slightest knowledge; and to refer these antecedents to any of the contingencies that are deducible from the importation hypothesis, is not only to assume to know what is unknown, but to assign causes which are radically falsified by their assumed effects. The population of Charleston in 1800, was 18,711; in 1810, 24,711; in 1820, 24,780; in 1830, 30,287; in 1840, 29,260; and in 1850, 42,985; so that she had but little more than doubled her original population in fifty years. Her history shows that increased frequency of epidemics has often been coincident with decline of her commerce. Her shipping was estimated in 1852 at about 42,000 tons, while that of New Orleans was quoted at about six times that amount. The imports and exports of New Orleans were also found to be at that time about six times the value of those of Charleston. Our city has at least ten times the amount of commercial intercourse with the perennial seats of yellow fever. European emigration, while it has mainly left Charleston untouched, has crowded the wharves of New Orleans, even in mid-summer, with the people of Germany, mostly in search of homes in the great West; and though the ancient city of the Huguenots, always distinguished

for her chivalry, refinement and patriotism, is now advancing with rapid strides, her history will show that she has been a city of tardy growth, and subject to many commercial reverses. Authorities may be cited to show that her yellow fever epidemics began in 1695, and that in 1732, 1739, 1745 and 1768, she was epidemically visited by this pestilence. During the last forty years she does not appear to have enjoyed the least advantage over New Orleans, as to the frequency and malignancy of yellow fever epidemics. The ruling antecedents of the pestilence appears to be no stronger in the one city than the other. We are told by Professor Dickson, that "*in Charleston an interval of a year or more is occasionally enjoyed.*" The editor in chief of this Journal says, most appositely to our present deductions, in his "Tableau of the yellow fever of 1853": "That yellow fever is produced by a cause or antecedent as invariable as the rising and setting sun is not the less certain because it is unknown. Even the games of chance, so called, happen in strict conformity to changeless law, as much as the winds, waves and eclipses."

If it be true that our yellow fever epidemics are imported, as alledged, it must follow that the contingency called quarantine, if it should not absolutely prevent the occurrence, it would diminish the probability that epidemics would occur. It will be seen, however, by the catalogue of New Orleans epidemics, that the longest non-epidemic interval, five years (or six years, if the epidemic of 1853 be reckoned "local and spontaneous"), was in the absence of quarantine, and at the maximum of exposure; and that amongst the shortest of these intervals occurred under the quarantine of 1821, when the exposure was *minus*. Under the present quarantine, indeed, between the years 1853 and 1855, it was remarkable that *there was no non-epidemic hiatus*—additional proof, were it at all necessary, not only of the non-importation of our epidemics, but of the utter futility of quarantine pretensions.

In proceeding to offer a few remarks on the aetiology of yellow fever, it may not be amiss to allude to some of the opinions which prevail and have prevailed here. I shall, therefore, in a special manner, refer to the views put forth by the Sanitary Commission appointed by our City Council, and to whom was confided the official duty of inquiring into this subject. To the influence of that Commission we are indebted for our present Board of Health and Quarantine, and as certain reviewers *at a distance* who are quite innocent of any knowledge of yellow fever, and who retain all their original innocence, have ex-

told the conclusions of the Commission to the skies, as more than Harveian and Jenncrian discoveries, and would send all dissentients in a very different direction, a candid and just appreciation of these views become the more necessary. And this is further all the more necessary at the present time, as some of the more obnoxious measures of that learned and estimable Body have been actually carried into effect, and are now a part of the law of the land, and as all of its measures which have hitherto failed of being carried into effect continue to be pressed upon the attention of our Municipal and State authorities. The "power-extension" still put forth a *project* of laws for creating "*Health Departments*," "*Health Police*," and for "*prohibiting the disturbance of the surface of the earth during the summer months*." That most learned Commission after all recommended and carried out the present quarantine,\* notwithstanding the clearly "local and spontaneous origin of the great epidemic," they were called on to investigate. The opinions of the Commission are now the official *opinions* of this community. They have been quoted and applauded by the book-makers and reviewers. They regulate our intercourse with the mouth of the Mississippi, and our commerce pays \$40,000 a year as the cost of their interference. But it appears that the cost is not yet sufficient, and the power is not sufficiently extended. Clothed thus with the authority of law, and seeking extended authority, the views of that most learned Body stand as *prima facie* evidence abroad that they are in accordance with the general views of the physicians of this city. Hence the importance of submitting these views to the ordeal of *free discussion*, in which should they prove sound, they will thereby acquire additional lustre. We shall examine such doctrines only as were unanimously concurred in by the learned Body in question.

We were told by the Report of the Sanitary Commission, with en-

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\* The following extract, in which are embodied the reasons of the Commission for their recommendation of quarantine, is taken from page x of the Sanitary Report, and embodies the unanimous opinion of that Body. It is doubtful if the annals of science can furnish a logical parallel—a more perfect *non-sequitur*: "In relation to the subject of quarantine, although the Commission is unanimous in belief that no system, however rigid or successfully carried out, can ever be a substitute for the sanitary or preventive measure we have recommended, and which if properly enforced, would be at once a protection against the spread and origination of yellow fever and cholera amongst us, yet in the imperfections that must attach to all such measures, we unite in recommending the establishment of a quarantine station, under the surveillance and control of the Health Department," etc. The logical chain runs thus: Quarantine cannot be substituted for local sanitary measures; the latter, "if properly enforced," would exclude yellow fever and cholera entirely from amongst us; but the imperfections which must attach to local sanitary measures, which if perfected would be sufficient of themselves to effect this exclusion, and for which measures quarantine cannot be substituted, must be remedied by the establishment of quarantine! But this is not all, the local sanitary recommendations and measures, and the local "Health Department," were never carried out at all; and admitting the *reductio ad absurdum*, that quarantine is capable of performing the extraordinary feat above ascribed to it, in the complete absence of the above local measures, according to the showing of the Commission itself, the present quarantine being in the position of a complete substitute, must be, so far as the public is concerned, utterly useless, though its cost be forty thousand dollars.

tire unanimity, when speaking of the epidemic of 1853, "that its fatal malignity and spread was justly attributable to a very remarkable concurrence and combination of the atmospheric with the terrene causes, always peculiarly fatal to human health and life," and further, that "these have been most amply explained and fully pointed out." (Introduction, page xi.) The sub-commission on the origin and spread of the epidemic proclaimed, "that there existed here, as attested by our records, very peculiar meteorological conditions, proven by general experience to be capable of producing, in coöperation with local causes, fatal and malignant forms of fever, and that these conditions were present in an exaggerated degree, and impressed upon the prevalent type of disease susceptibilities assimilating it to another and distinct form of fever."

The platform of the Commission is here specially laid down—the sky-cause and the ground-cause. Each of these agencies separately, as well as conjointly, have been upon insufficient proofs assigned by medical philosophers for hundreds of years as the causes of epidemics. Without, therefore, the discovery of some *new law*, determining the *modus operandi* of these agencies, there can be nothing satisfactory in proposing such an ætiology, much less in the declaration that the cause above and the cause below produced conditions in 1853, which, to use the language of the Report, "are always peculiarly fatal to human health and life," or which, "as attested by our records," and "proven by general experience," are capable of generating any such an epidemic as visited this city on that memorable year. The existence of such air-poison and ground-poison in 1853, in New Orleans, in form and manner here designated, is in fact disproven "by general experience," as well as "by our records."

Let us now proceed to offer a few remarks on the sky-cause, as set forth by the Sanitary Commission, and which has caused certain writers and reviewers to respond to the cry of *Eureka!* which emanated from that distinguished Body. In the Report published by them the words "meteorology," and "meteorological conditions," are of frequent occurrence; but with the exception of the detailed meteorology for three months in 1853, (July, August and September), given by one of the members of the Commission in another part of the Report, the following table, which has been extensively copied and eulogized, is the beginning and the end of all the meteorological conclusions given to the world by the Sanitary Commission.



Climatic or Meteorological Elements of Epidemic Cholera and Yellow Fever at New Orleans, and Comparison of the Meteorological Conditions which have prevailed during the three Epochs of Commencement, Acme or Maximum, and Declination, of the Epidemics of Cholera and Yellow Fever, on an average of Six of the First and Fifteen of the Second.

	No. of epidemics examined.	Average duration of epidemic influence, in days.	Average ratio of mortality for each epidemic, to the population per 1000.	TEMPERATURES IN THE SHADE.			TEMPERATURES IN THE SUN.			TEMPERATURES OF THE DEW POINT			BAROMETER.			WINDS.			HUMIDITY.			RAYING POWER.			
				General aver. at commencement.	Maximum.	Declination.	General aver. at commencement.	Maximum.	Declination.	General aver. at commencement.	Maximum.	Declination.	General aver. at commencement.	Maximum.	Declination.	General aver. at commencement.	Maximum.	Declination.	General aver. at commencement.	Maximum.	Declination.	General aver. at commencement.	Maximum.	Declination.	General aver. at commencement.
From Cholera,	6	37.68	21.99	72.84	66.95	69.73	81.36	84.59	80.31	66.71	56.79	61.14	30.109	30.060	30.075	S. E. & E.	S. E. & E.	N. E. & N. W.	.850	.796	.791	5.06	7.08	7.24	
From Yellow Fever,	15	58.33	18.30	80.14	79.60	65.72	104.91	117.07	99.11	75.82	74.34	62.12	30.108	30.054	30.074	N. E. & S. E.	N. E. & S. E.	N. E. & N. E.	.688	.865	.755	3.79	4.53	8.02	
Difference.	9	20.65	6.31	7.30	12.65	4.01	5.55	32.48	18.80	9.11	17.55	0.98	.001	.036	.001	m	little	little	.038	.069	.036	1.27	2.55	0.78	
				more.	less.	less.	more.	more.	more.	more.	more.	more.	less.	less.	less.				more.	more.	less.	less.	less.	less.	more.

That mistakes and discrepancies should frequently be found mixed up with the transactions of scientific bodies, is no more than may be expected, and due allowance may always be claimed. I know of no rule, however, which may legitimately claim this allowance in behalf of such bodies, when they discuss and entertain questions which are wholly and in themselves absurd. I can scarcely conceive of a greater absurdity than the attempt to deduce the meteorology of cholera from anything which is ever specially seen in, or known to New Orleans. There appears no necessity of constructing a meteorological table for this city, to prove the *notorious truism* discovered by the Sanitary Commission, "*that in comparison, cholera exists in a greater range of temperature and humidity than yellow fever.*" In 1849, cholera was epidemic here, in the midst of a humid atmosphere near freezing point, and in Jefferson City, it prevailed in 1854, at the average summer heat. The cholera appears at Moscow, in the *winter*, where the average temperature for the season is as low as  $23^{\circ}.18$ , and at Calcutta, in the *summer*, where the average for the season goes up to  $87^{\circ}.72$ . It appears at New Orleans, where the annual average fall of rain is 50.90 inches, and at San Francisco, where the annual average is only 16.62 inches, as also at places in New Mexico, where the rain does not exceed six inches annually, or are indeed rainless. While the disease was prevailing in the winter of 1848-9, in New Orleans, the average winter temperature of which is  $55^{\circ}.80$ , it was prevailing in northern localities of our country, the average winter temperature of which is as low as  $30^{\circ}$ . The average temperatures of New Orleans, noted for twenty years, is, for winter,  $55^{\circ}.80$ ; for spring,  $72^{\circ}.08$ ; for summer,  $82^{\circ}.04$ ; for autumn,  $69^{\circ}.28$ ; and for the whole year,  $69^{\circ}.80$ , while the corresponding averages for the city of Moscow are  $23^{\circ}.18$ ,  $40^{\circ}.08$ ,  $59^{\circ}.88$ ,  $42^{\circ}.62$ ,  $41^{\circ}.85$ . A great disparity of temperature is of course here presented. Now it is known that in the depth of the Russian winter of 1830-31, the cholera raged in an extraordinary manner in the latter city. In order to square these winter cholera epidemics with the temperature prescribed for cholera by the above table, the Sanitary Commission resort to the extraordinary expedient of assuming *that the Russian houses in which the cholera prevails in winter, contain filth, are greatly crowded, badly ventilated, and that though the exposed thermometer may be below zero, the*

temperature *inside of the houses is between 70° and 80°!*\* The violent meteorological deductions here made in relation to cholera, are indeed too absurd for serious examination.

But let us here examine this table as a proposed key to the meteorology of yellow fever, and it will be seen at a single glance, that owing to two fundamental defects the table is utterly without value, even admitting the untenable ground that the ætiology of that disease can be referred to any special known meteorological conditions, deducible from a comparison between epidemic and non epidemic seasons. The same remark is applicable to the table, in its relation to cholera. The first defect is, that it is a table of mere *averages* of temperature, barometer, dew-point, winds, duration of epidemics, etc., etc., and thus all of the individualities by which every epidemic season is characterized, are merged in the whole number of epidemics, and all means of instituting a comparison amongst the poisonous degrees of the thermometer, hygrometer, etc.; the degrees of meteorological crime chargeable to each epidemic season, are not exhibited. The second is, that neither the meteorology, either averaged or individualized in detail, is given of the twenty-two years in which the yellow fever did not epidemically prevail, between the years 1817 and the year 1854, and consequently we have no means of comparing the meteorological details of the healthy with the epidemic seasons, or the healthy seasons with each other; so that for all that is here shown, yellow fever might have often appeared here, as it has repeatedly done in one series of years, and been absent in another, under precisely the same meteorological conditions. Nay, we have the means of satisfactorily showing that accurate meteorological averages of the aforesaid fifteen yellow fever seasons placed side by side with those of the twenty-two years of exemption, would exhibit either perfect identity, or mere homœopathic difference. There are, of course, remarkable differences in the meteorology of some seasons when compared with others, taken in their individual capacity; but no known epidemiological deductions of any value have ever been drawn from the premises. Though we are never absolutely free from yellow

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\*<sup>11</sup> Statements are recorded of the prevalence of cholera when the *exposed* thermometer was near zero. This is not at all incompatible with an *inside* temperature of between 70° and 80°, with filth and peculiarly noxious effects of crowding and most defective ventilation (and of course a high dew point,) all of which we know exists in Russian dwellings, where this disease prevailed. The incongruity then no longer exists, for it is *the condition to which the individual is exposed that is to be estimated.*" (Sanitary Report, p. XVI.)

fever for twelve months at a time in this city, we have rarely ever had three more healthy months during the warm season, than the months of July, August and September, 1849. The cholera had taken its flight; we had only the usual number of sporadic cases of yellow fever, and nothing approaching an epidemic. Now, during the self same months for the year 1853, there was the greatest yellow fever mortality ever known in the city of New Orleans. It is very natural and interesting to inquire into the meteorological data in relation to *temperature and humidity* afforded by these two antagonistic seasons.

The following are the respective elements:

	1849.		1853.	
	AV. TEMPERATURE.	INCHES RAIN.	AV. TEMPERATURE.	INCHES RAIN.
July, .....	78°.74	14.741	79°.88	11.708
August .....	80°.96	4.473	81°.25	7.068
September.....	78°.44	2.600	76°.23	5.045
	TOTAL, 79°.38	TOTAL, 21.814	TOTAL, 79°.12	TOTAL, 23.769

These data may be found in Fenner's Medical Reports, pp. 100 and 101, and in the meteorological observations made for the space of three months by a member of the Sanitary Commission in 1853, and exhibit a most remarkable identity of meteorological conditions, upon the showing of that learned body itself. Their epidemiological deductions are disproven by their own figures:

“ Fair is foul and foul is fair.”

On examining the meteorological records for the three months next preceding the epidemic of 1853, and partially entering into that epidemic, it will be seen that the average temperature for the time, (April, May and June), was 74°.99, and that the corresponding average for the same months in the choleraic season of 1849, was 76°.26, being a difference of little over a degree. The same months for the healthy season of 1844, when compared in like manner with the three months next preceding the epidemic of 1853, present a difference of only 1°.93, the temperature of the former being *that much the higher*. The same three months for the years 1840 and 1841, have the average temperatures, 83°.14, and 83°.45, *almost exactly alike*. The former was an unusually healthy season, and in the latter there was a deadly epidemic of yellow fever. The quantity of rain that fell during the three months, April, May and June, immediately preceding the great epidemic, differs but *one-tenth of an inch*, from the quantity that fell for the corresponding time in the healthy season of

1844. The epidemics of 1841 and 1847 were very violent, and much resembled each other in malignity, notwithstanding the quantity of rain that fell during the epidemic of 1847 was about double the quantity which fell during the epidemic of 1841.

One of the formidable meteorological elements enumerated by the Sanitary Commission was the "*north wind*."

A reference, however, to the wind statistics of the year 1847, in which there was a protracted yellow fever epidemic, will show that the *southernly winds exceeded the northernly* to the extent of forty-five days, and that in the year 1848, which was very healthy, with the exception of about sixteen days of epidemic cholera at the close of the year, the northernly winds exceeded the southernly to the extent of twenty-one days. The cholera begun in southernly winds, which did not change for the space of seven days; the year closing with nine days of northernly winds, the disease being as malignant in the one wind as in the other.

With high altitude of the thermometer in the low country of the South, humidity and a high dew-point are always associated.

The Meteorological Register of Fort Moultrie (about six miles from Charleston, S. C.), gives the following mean dew-point, for the years and months below designated:

MONTHS.	1849.	1852.	1853.
July .....	76°.27	78°.70	77°.33
August .....	77°.69	75°.77	75°.32
September .....	71°.70	71°.74	71°.85
Average dew-point for the three months, respectively.....	74°.88	75°.39	74°.88

The mean dew-point given for the corresponding months in New Orleans, in 1849 and 1853, *by a member of the Sanitary Commission*, in the report of that body, and in the first volume of Dr. Fenner's Medical Reports, is as follows:

MONTHS.	1849.	1853.
July .....	75°.53	72°.13
August .....	76°.75	78°.05
September.....	73°.78	79°.93
Average dew-point for the three months, respectively.....	75°.35	73°.71

The Sanitary Commission unanimously identify yellow fever with the "*high dew-point*." Now, it will be seen by the foregoing summary,

that the dew-point in New Orleans, during the epidemic months of 1853, was *much lower* than it was here during the same months in the healthy corresponding months in 1849.\* There was general health and no yellow fever at Fort Moultrie in 1853, with a higher dew-point than there was in New Orleans, in the midst of the great epidemic. In the deadly epidemic that prevailed at Fort Moultrie, in 1852, it will be seen above that the mean dew-point was identical with the corresponding healthy months of 1849. In the latter year, with the same dew-point of course, for Charleston and Fort Moultrie, there was an epidemic yellow fever at the former place and not at the latter.

The atmosphere in no part of the globe is completely anhydrous. It everywhere contains moisture, not excepting the air of the Sahara itself, where it is not deposited, owing to the fact that the temperature of the desert never falls below its dew-point, under which circumstances the air never parts with its moisture. The very Sirocco itself has its dew-point. What is known to meteorologists as the DEW-POINT, is simply the *highest temperature* at which the moisture of the air will condense on any cold body. Even when the rays of our summer sun are fiercest, and dust deepens in our streets and is wafted into our dwellings, the air is strongly charged with watery vapor which is invisible, but which may be abundantly brought to view on the outside of a pitcher of iced water. The dew-point may be taken with tolerable accuracy, by the simple expedient of filling a thin silver goblet with water, and cooling the contents down with ice until the moisture *begins* to appear on the outside of the vessel, when, if the thermometer be thrust into the vessel, the former will declare the dew-point of the atmosphere. The higher the dew-point, the greater will be the quantity of vapor in the air, when compared to the air itself by weight. Thus, there is twice as much vapor in the air when the dew-point is  $52^{\circ}$ , as there is when it is  $32^{\circ}$ , and the quantity is doubled again when the dew-point is  $73^{\circ}$ , and is doubled again, should the dew-point be raised artificially to  $95^{\circ}$ ; for it seldom exceeds  $80^{\circ}$ , even in the torrid zone. (*See Espy's Fourth Official Meteorological Report, 1857; Maury's Physical Geography of the Sea; Army Medical Statistics, 1856.*)

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\* The Meteorological data reproduced in this paper, not otherwise designated, are taken from official sources, namely: The voluminous Reports of the Medical Officers of the U. S. Army, from all the Military posts, as published by Congress. These data are, perhaps, the most extensive, numerous, and reliable ever published by any government in Christendom. Their value is enhanced, not only by the number and competency of the observers, but by the fact that these data are not brought forward to sustain any individual's theory or biases.

The humidity of our atmosphere, both free and latent, that attends our summers, there is every reason to believe is a simple promoter of our health and well-being, moderating the otherwise disastrous effects of caloric. Indeed, the latent moisture as indicated by the dew-point in the midst of a long drought, saves us from prompt destruction. When the moisture ceases to be latent, and is set free in the form of dew or rain to be again converted to vapor, nature is struggling so far as we know, simply for our bodily good. At least there is, as we have seen, no reason to believe that she by preference makes use of certain definite measurable points in our summers for the concoction of the material cause of yellow fever.

The terrible yellow fever epidemic which prevailed in Norfolk, in 1855, came and went without the existence of any peculiar meteorological conditions, "as attested by our records," either "exaggerated" or otherwise. The able Committee, Drs. Seldon, Tunstall, Moore, Campbell and Gordon, who reported on that epidemic, discoursed like men who lay down a rigid distinction between the real and the imaginary, and their conclusions stand forth in bold contrast with the fanciful deductions of the New Orleans Sanitary Commission. What the former did not *know* they made *no pretense to knowing*. They simply tell us that so far as relates to Norfolk, the year 1855 "was not remarkable for any meteorological *or other conditions* foreshadowing the approach of the direful pestilence by which it has acquired so sad a celebrity." I have before me the U. S. Army Meteorological Register, on turning to which, I find that the amount of rain that fell at Fort Monroe, 12 miles distant from that place, for July, August, and September, was only 6.75 inches; the mean average for these three months, during the 18 years next preceding being 14.69, the rain being less than half the wonted average. The rain that fell from January to September (nine months), was 17.10 inches; and the annual average in inches, for the same months for eighteen years, was 34.24; the rain being half the latter mean. The mean temperature from April to September, inclusive, was 73°.02, while the mean for the same months for thirty years, was 70°.69. For July, August and September, mean temperature 79°.41; average temperature of these months for thirty years, 75°.87. Here we have the temperature *plus*, and the humidity greatly *minus*. The yellow fever season of 1852, at Norfolk, reverses these figures for July, August and September, and declares 10.28 inches for rain, and an average temperature

as low as  $74^{\circ}.36$ . In 1852, the yellow fever *made its appearance there in September*, with an average temperature for the month, of  $70^{\circ}.20$ , and a fall of only 1.55 inches for the same; and in 1855, it *originated in June*, in which month there was 3.80 inches of rain, and an average temperature for the month, of  $74^{\circ}.81$ ; the average for 30 years being  $74^{\circ}.22$  showing that fever had *originated* in Norfolk, in the midst of meteorological conditions, the most antagonistic to each other.

With the authorities before us we might pursue this part of our subject further, and exhibit further the utter untenability of the meteorological aetiology. The assertion, therefore, on the part of the Sanitary Commission "that there existed here in 1853, as attested by our records, very peculiar meteorological conditions," exhibiting themselves "in an exaggerated degree," conditions "proven by general experience" to have an agency in bringing about epidemic disease, is not only unsupported by a shadow of evidence, but is contradicted by general experience. That "there existed here" during that season "meteorological conditions always peculiarly fatal to human health and life," and that they "were fully explained and pointed out" by the Commission, is in like manner unsupported and contradicted.

The following is from the able report of the Norfolk committee on the epidemic of 1855, to which we have already referred, and which we here reproduce as an exemplification of the entirely different spirit which appears in the deliberations of the New Orleans and Norfolk Commissions. We need hardly say here, however, that we do not hold to the importation doctrine of the Norfolk committee.

"In 1855, we find that January was very mild, February and March cold, April very warm, May cool, June a fraction above the average, while July, August and September were very hot months. Whatever influence the heat in July, August and September may have had in increasing the disease, it could have had none in causing its outbreak, since, as will appear hereafter, the first cases of fever occurred in June. The heat of June was not so unexampled as has been asserted, for the foregoing table shows that it was barely above an average; and on examining the table of thirty years, we find in fifteen of them June was hotter than in 1855. In only one of these, viz: 1826, was there fever in Norfolk.

"The first five months of 1855 appear from the foregoing table to have been very dry, while June was very wet. There seems to be some doubt about the accuracy of the rain gauge at Old Point during the last five years, as the annual quantity of rain is far below the average; and unfortunately we have no access to any other kept for a series of years in this neighborhood. We feel sure from our own recollection, however, that the spring was a remarkably dry one. June must



surely have been very wet, since the rain guage at Old Point, which is suspected of marking less than the true quantity of rain, yet shows more than an average. The early part of the year was very healthy. With the exception of scarlet fever, which prevailed in March and April, there was less sickness than usual. In June and July there was some dysentery, but we think less than in ordinary years; certainly much less than in the preceding year, 1854. There were also in June and July a few cases of typhoid fever—a disease which prevails more or less every year. We are satisfied, however, that up to the middle of July the year was one of more than average healthiness; nor were there any febrile or other diseases to show an unhealthy state of atmosphere, or make us suspect the approach of a great epidemic.

“The condition of the city as regards cleanliness was not different from what it had usually been. The streets are generally kept cleaner than in most towns of the same size, but there was no doubt a great deal of dirt in the yards and houses of the lower classes.

“The statements that have frequently been published of meat putrefying with unusual rapidity, and the fruit mildewing and rotting on the trees, had certainly no foundation except in the distempered imagination of the writers. So far from it, the fruit was remarkably fine and perfect; and it was observed by the southern physicians, that they had rarely seen in Charleston or New Orleans figs so luscious and unblemished. Indeed, in the very height of the epidemic there was nothing that would have led a stranger to suspect that he was in the midst of pestilence, but for the frequent hearse hurrying at a trot through our deserted streets. The air was as balmy and pleasant and vegetation as healthy and luxuriant as in the most favored seasons.”

The New Orleans Commission did not thus lightly pass over the signs, wonders and prodigies, that flitted through the “distempered imaginations” of the excited public, in view of the epidemic of 1853; but on the contrary, instead of being discarded they were laid hold of by that learned body in support of their theoretical opinions. A mass of *non-professional testimony* is published to prove a position which soars above the powers of the most fervid imagination, namely, that the disease was spread from New Orleans by contagion. The prodigies above enumerated, and ten-fold more, were given to the world in honest sadness by the New Orleans Commission; and the blight, the cryptogamous mildew, the malign aspect of the elements, the “foul fiend,” contagion, stalking from door to door, the poison escaping from the “disturbance of the earth’s surface,” were all attested to by truthful but excited minds, goaded by the terrible realities of the epidemic as they broke in on a people unaccustomed

to the disease, and unprotected by what is erroneously called "acclimation." The Commission publish it all, and magnify it all.

*Lenox.* The night has been unruly; where we lay,  
Our chimneys were blown down; and as they say  
Lamentings heard i' the air; strange screams of death;  
And prophesying, with accents terrible,  
Of dire combustion, and confused events,  
New hatched to the woeful time. The obscure bird  
Clamored the livelong night; some say the earth  
Was feverous, and did shake.

\* \* \* \* \*

*Rosse.* By the clock, 'tis day,  
And yet dark night stragles the traveling lamp.  
Is't the night's predominance \* \*  
That darkness does the face of earth entomb,  
When living light should kiss it?

*Old Man.* 'Tis unnatural.  
On Tuesday last  
A falcon towering in her pride of place,  
Was by a mousing owl hawk'd at and killed.

*Rosse.* And Duncan's horses (a thing most strange and certain).  
Beauteous and swift, the minions of their race,  
Turned wild in nature, broke their stalls, flung out,  
Contending 'gainst obedience, as they would make  
War with mankind.

*Old Man.* 'Tis said they eat each other.

*Rosse.* They did so; to the amazement of mine eyes,  
That looked upon't.

Dr. Gillkriest, army surgeon, who made, in 1850, a most elaborate and able report on the yellow fever of Gibraltar, addressed to the London Board of Health, has examined the meteorological data afforded by that British stronghold, beginning as early as 1790, a period of sixty years up to the date of his report. His conclusion reads thus: "*I think it must appear, that the examination of meteorological variations furnishes no satisfactory information respecting the cause of yellow fever at Gibraltar.*"

The science of meteorology cannot be too highly appreciated, nor too extensively studied. The service which it has practically rendered to the cause of commerce, agriculture, navigation, etc., can never be over-estimated; and its relation to the whole circle of the physical sciences are most intimate and fundamental. It unfolds, with the most wonderful simplicity, the most sublime and important

phenomena in nature, and affords a key to many of her most mysterious operations. When we look on our mighty river, which constitutes one of the great features of the globe we inhabit, we behold a simple meteorological creation—a stream brought into existence, excavated, and kept in being, by simple and well-defined meteorological conditions. The ravine of the Mississippi is indebted for its contents solely to atmospheric humidity. Its rolling waters have all previously floated in the air. It requires but a comparative fraction of the atmospheric humidity to give food to an Amazon and a Mississippi. Reverse the meteorological laws that prevail in their valleys, and the latter would be transformed into saharas, and the water-channels into dusty ravines. The Federal Government is rendering important service to the cause of science by the valuable meteorological observations which have been brought to light from the different military stations throughout the Republic, in the published Medical Reports of the Army; and in thereby furnishing the world with reliable data on which duly to appreciate the *atiological fictions* which have been associated with yellow fever and cholera, and which have been authoritatively palmed on the credulity of the public and the profession, the meteorological observers “have done the State some service.”

In the next number of the Journal I shall enter into the consideration of *terrene causes* to which yellow fever has been referred in New Orleans, and in an especial manner to the highly fanciful views put forth by the New Orleans Sanitary Commission, in their collective capacity, and now urged before our Municipal and State legislative authorities. In due time I shall have somewhat to say on the pathology and therapeutics of yellow fever.

We close this communication with the following apposite quotation from an article from the pen of Professor Dickson, in the January number of the Charleston Medical Journal and Review. Speaking of the *ætiology* of yellow fever, he says: “Not heat, nor filth—vegetable or animal—nor moisture nor dryness; for these all have varied separately and in combination, without affecting the results. It has been seen where the surface was unbroken—absent when it was widely turned up. It has failed to show itself for consecutive years of a great variety of condition. It has shown itself in consecutive years of equally marked variety. Last year cold and dry as was the summer, it assailed a pleasant and neat village in the vicinity of Charleston, nor did the city altogether escape. This season,

while hot and dry, it invaded the city first, and has spared its neighbors. They are turning up no soil, laying no gas pipes, digging no drains. The streets are extensively paved, and the police are not accused of neglecting them or failing to keep them clean. Yet there we have the same intruder which forty years ago was ascribed to unpaved streets, undrained surfaces, and neglected scavenger's observances. It is plain that we have failed in this direction to indicate the cause, or by efforts suggested by these ancient views to weaken the force or arrest its effects."

ART. II.—*Observations on Iodo-Hydrargyrate of Potassium, and the Sub-Nitrate of Bismuth in Diseases of the Prostate Gland, in Gonorrhœa, etc.:*

By A. S. HELMICK, M. D., of Houma, Louisiana, late of Virginia.

IN the first number of this volume, I see a condensed translated extract (from a foreign Journal) of Dr. Caby's treatment of chronic gonorrhœa, etc., by the sub-nitrate of bismuth. While the subject is fresh before the profession, I have concluded to give my experience in the treatment of the same diseases by the same remedy, in connection with another medicine not mentioned in the extract referred to. In the fall of 1854, I saw an article in the Boston Medical and Surgical Journal, merely stating that Dr. Caby recommended a saturated solution of the sub-nitrate of bismuth, used as an injection in the treatment of acute and chronic cases of gonorrhœa. Since that time I have used it constantly with the happiest results, as the following cases will show.

More than two years prior to this time I had been prescribing the iodo-hydrargyrate of potassium in prostatitis, spermatorrhœa and other affections of the genital organs. I have used it in a great number of cases and a variety of different diseases, and have been seldom disappointed in its result. I believe the efficacy of this medicine is not generally known or appreciated in this section of the country, although it has been prominently before the profession, we might say, since the year 1834. Drs. Channing, of New York, and Hildreth, of Ohio, in the February and August numbers of the

American Journal of the Medical Sciences, for the years 1834 and 1840, describe it as an agent of no ordinary power, admitting of a wide range of applicability in the treatment of disease. Without doubt it is an all-pervading, universal alterative, and may be recommended in a variety of pathological conditions, and is also an excitant of particular organs and functions.

CASE I.—March 28th, 1852; A negro man aged 40 years came to me with the following symptoms: a constant desire to urinate; heat and pain in the perineum near the anus, with considerable tenderness there on pressure; pain greatest when the accelerator muscles exert themselves to expel the last drops; a sense of weight in the rectum. The catheter was introduced, and much pain was experienced by the patient as it passed the prostatic region. Considerable blood followed its withdrawal.

He was ordered the following R. deuto-iodide of merc. gr. iv; iodide of potassa ℥i; water fl. ℥i, five drops three times a day in a wine-glass of the cold infusion of the *prunus virginiana*, before each meal, and to be continued until its constitutional effects were manifest in the economy, which is known by a fullness or heavy pain about the frontal region, sick stomach or an active griping effect upon the bowels. He was also given as a diuretic, the infusion of tobacco, as prescribed by Dr. Fowler: R. tobaceo ℥i; boiling water fl. ℥ii; infuse 20 minutes, strain, and add alcohol fl. ℥ii; thirty drops four times a day in pine-top tea. This was continued two days, when the patient was relieved of all the distressing symptoms, the tenderness only remaining.

April 6th. To-day the patient reports himself much improved. On examination, tenderness is still found to exist at the bulb. He had resumed his work as miller. April 18, the symptoms have returned; was ordered the same treatment, and to be continued until its constitutional effects were again manifest.

On the 26th he reported himself entirely well and again went to work.

July 1st. I was again requested to see him, and found the disease had returned in an aggravated form. He had been exposing himself and lifting heavy timbers; indulging to excess in venery, etc. The prostate and neck were very tender, and much enlarged; blood passed at times with the urine; the catheter was introduced with difficulty, owing to the amount of pain produced. He was again

put upon the treatment above described, and ordered a strict diet and total abstinence, in the strictest sense of the term.

The medicine was taken for two days, when the symptoms were entirely removed, except the tenderness in the prostatic region. The deuto-iodide was discontinued on the approach of its constitutional effects, and resumed when those effects passed off. In a fortnight the tenderness had passed away, and he again expressed himself well. I saw him again in August, and found him entirely well. He remained well for four years afterwards, when I lost sight of him.

CASE II.—June, 1854. Was consulted by J. S. M., unmarried, aged 30 years, of leuco-phlegmatic temperament, blue eyes, very light hair, large brain, low stature, and slender form. He gave me the following account of himself: Was placed in a dry goods store when quite young; had indulged in the excess of venery from the earliest manhood up to the age of 22 years, at which time his virile powers became much impaired, almost to complete impotency; his digestive organs became also much disordered; became nervous, with a universal derangement of the entire system. He took exercise and used some tonic medicine, but from that time to the present he has had nocturnal emissions, more or less, always worse in the spring months and early part of summer. I should have stated above that about the age of 22 he said he had quit his excesses, and became a member of the church.

He presents the following symptoms and conditions on first coming to me for medical advice: His general appearance is that of debility and languor; has a consumptive cough; the worst form of dyspepsia; gait sluggish; is listless and careless; mind and memory impaired; shuns society; has nocturnal emissions as often as from three to eight times during a single night; bowels costive; tongue furred; nausea, vertigo, etc.; also at times a difficulty in voiding water. He denies every indulging in Onanism, but attributes the disease to his early excesses.

He was directed to use the iodo-hydrargyrate of potassa in the cold infusion of the wild cherry bark. I introduced the catheter, and found considerable tenderness at the neck of the bladder—blood followed the instrument; after this a large bougie was introduced, and the patient instructed how to introduce it himself, and was directed to introduce it every other day, if it did not occasion too much irritation. He was ordered a nourishing but not a stimulating diet, and

the medicine to be gradually increased to eight drops three times a day until its peculiar effects were produced, then to be discontinued until these effects passed off, which generally happens in some two or three days, then to be commenced again with five drops, as at first.

The next time the patient was seen (six days after) he appeared much improved; spirits greatly revived; had no emission since the first night—a positive evidence of the beautiful effect of this combination.

This course of treatment was pursued for about four weeks, when he expressed himself well; had gained considerable flesh, and was in every respect a new man. He remained free from the disease up to December, 1856, when I removed from the place. I have since learned that he has married a woman almost twice as large as himself.

CASE III.—August, 1855. A man aged 23 years; had been married about two months; had suffered several attacks of gonorrhœa before marriage, the last attack lasting several months; was taken with the usual symptoms of prostatitis, accompanied with those of diabetes; the desire to urinate was incessant, with tenderness about the perineum and bulb as in case No. 1. His countenance was haggard, and expressive of great anxiety. Two other physicians had attended him in former attacks, and had relieved him by diuretics, opiate enemas, purging, etc. He had occasion to drive eighteen miles one morning to attend court, and was taken soon after his arrival with a paroxysm more severe than any previous one. I was called to see him, and found him with the symptoms as above described. He was immediately directed to drink a saturated solution of the borate of soda, and to use an opiate enema *per rectum*. The borax water was continued during the day, with a relief of the urgent symptoms.

The next day he took the iodo-hydrarg. potass and the infusion of tobacco, as in case No. 1; also injections of the sub-nitrate of bismuth three times a day. The abnormal desire to urinate had entirely ceased in twenty-four hours. The tobacco infusion was discontinued in some three or four days. The other medicine was continued for two weeks. The injections were used only occasionally, when he considered himself well. He was directed to use an ounce of the *deuto-iodo. merc.* and potass mixture, taking from five to eight drop doses three times a day in the cherry bark water, and to discontinue at the approach of its unpleasant effects. He had one or two other slight attacks, but was soon relieved. Those attacks were brought

on by his marriage associations. Since then he has had no other attacks, and is permanently cured.

CASE IV.—October, 1855. A young man who had contracted gonorrhœa some two years previous, gave me the following account: Had been cured of the violent symptoms some three months after contracting the disease, but there still remained a mucous discharge from the urethra, easily excited or aggravated by exercise or excessive indulgences. Within the last year there was felt a tenderness about the neck of the bladder, with occasional attacks of strangury. He had tried many physicians, but the disease was only palliated, not cured. During the last three months he had been to see one of the most eminent physicians in Virginia; remained under his treatment two weeks, during which time the neck of the bladder and prostate gland were cauterized several times; he left, with a request from the doctor to return within two or three weeks, should he not get well. In three weeks he again went back and remained another week, and was again cauterized. He left with the disease as bad or worse than ever.

When I first saw him professionally, he was almost reckless of life, believing that he could never get well. He had a most distressing strangury.

I commenced treating him at noon; gave five drops of the deuto-iodide and injections of the sub-nitrate of bismuth, and allowed him to drink freely of a solution of borax during the afternoon and night. He took another dose of the deuto-iodide, and an injection of the bismuth at night. Next morning he was entirely free from the strangury. He continued the injections for about a week, and the iodide some three weeks longer, when the tenderness had entirely disappeared. He soon after left the place, and I saw nothing more of him for near a year, when he informed me that he had been entirely free from the disease since I last saw him.

There were several other cases treated in the same way, with like results, in the village and neighborhood, but these we will pass over.

CASE V.—January, 1857. Soon after I came to Louisiana, a young man came to me and stated that he had contracted gonorrhœa some ten months before, and had been treated by most of the physicians of his place (Thibodaux), and had also been to New Orleans, and tried several of the self-lauded "clap doctors" of that city; but none



did him any good. He had a constant discharge from the penis, at times greater than at others, which is the case with all these diseases which become chronic. There was also tenderness at the bulb and neck. The iodo-hydrarg. potass was prescribed, as in the other cases, being convinced that the disease was located at the neck and prostate gland. He also used the injection of bismuth. The discharge was arrested in less than two days; but the bismuth injections were continued for a week longer, and the iodide for about a fortnight, when the tenderness had altogether disappeared. All further treatment was discontinued. The young man soon after married, and to my certain knowledge has remained perfectly well and free from the disease to this day (January, 1859).

CASE VI.—August, 1857. This case was in Houma, Louisiana. A man aged 40 or 45 years; had never been married; had many attacks of gonorrhœa, the last attack had lasted fifteen months. He also had been treated by several doctors here and in New Orleans, but still the discharge continued. There was considerable tenderness at the neck of the bladder, on pressure. The discharge was painless; at times it stained the linen, as in recent cases of gonorrhœa. He was treated with the iodide in the usual dose, and the bismuth was given as an injection. The discharge was soon arrested, but would occasionally return. The tenderness continued for about a month. He was allowed a free use of demulcents and diuretics during the course of the treatment. In two months after he first began the treatment he got married; since then he has been free from all symptoms of the disease. This case was the most obstinate of any that I ever treated.

I have treated many cases of acute and chronic gonorrhœa, both in the male and female. In using the injection of subnitrate of bismuth in the acute form of the disease, I have found that it will arrest the discharge in a very large majority of cases by using only three or four injections; but the discharge will soon return unless other medicines are given in connection. When used to excess, say  $\mathfrak{z}$ i of the salt to  $\mathfrak{z}$ ij of water, the bismuth remains in the urethra and produces irritation, and if continued will aggravate rather than cure. I usually, as soon as the discharge is checked, advise the patient to pour off the supernatant liquor, and use that as an injection only, which always has a soothing and grateful effect.

I cannot rely upon the subnitrate for curing a recent case of gon-

orrhœa, but use it only to keep the discharge in check, until other remedies can assist in curing the disease. This, truly, is a great relief to the patient—nothing is more distressing and annoying to him than having his linen stained and bedaubed with the discharge.

The female patients that I have had in charge were treated with the bismuth injections three times a day, and a preparation of the balsams (no special one), and in a week, or at most a fortnight, all had reported themselves cured.

I am convinced that all chronic diseases of the male genital organs of this nature (gonorrhœal) are seated in the prostatic region, and if treated with medicines which act especially on this gland, and the parts adjacent, there would be no difficulty in curing every case. And my experience for several years in the treatment of the disease in question, has forced upon me the opinion that the above course of treatment laid down, "*mutatis mutandis*," will cure more cases than any other.

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ART. III.—*On Cod Liver Oil*: By E. P. GAINES, M. D. (Read before the Mobile Medical Society, January 7, 1859.)

THERE is a fashion in medicine, it seems, as in many other things. At one time cod liver oil was extolled to the skies. It served good for nearly all diseases; but for pulmonary affections no such remedy had ever been discovered. Consumption—that dread disease which destroys one-third of the human race—was said to yield, in all its stages, to the remedial action of this oil. Where too much is promised, too much will be expected, and in the disappointment that follows not as much is usually allowed as is due to real worth. Such appears to be the case with Cod Liver Oil, and lest its merits should be forgotten altogether, now that so many new remedies for pectoral complaints are brought before the profession, I will relate the experience I have had, and the conclusions I have drawn therefrom. I prescribed it, for the first time, in 1851, for a yellow woman, a slave, who had all the symptoms of phthisis. She seemed to be rapidly declining under the treatment of the physician who had had charge of

the case. He said she had consumption, and that he could do nothing more for her. I recommended the oil. Her cough became better, she gained flesh; and though every winter since she has been compelled to resort to this remedy, has so far recovered her health as to be a useful servant.

This ease made such an impression on my mind, that I had faith to try the remedy on myself, when a gradual decline, attended with cough, then hemorrhage, then night sweats—the cough getting worse, the weakness greater—showed too plainly that my inherited disease, consumption, had begun its work. So disgusting and nauseating, however, was the taste of the oil, that I could scarcely retain it; and when I did it would rise in my throat and sicken me hours afterwards. This would not do, for I vomited often enough through the day from the violence of the cough, without being made sick by medicine. So I had to give it up, until one day I read in a medical journal that Walshe recommended persons of an irritable stomach to take it with a mineral acid, and in small doses. I tried the elixir of vitriol, five drops added to a wine-glassful of water, with one teaspoonful of the oil, night and morning, and in one week's time so great was the amendment that, when as before I could not go to sleep without a draught of McMunn's elixir of opium to stop my cough, I had now no necessity for it, so much better had the cough become. My appetite improved, vomiting ceased, and strength so kept up, that I was able to get through with the softening and expulsion of that crop of tubercles, and have had no cough nor other symptoms of consumption for years, and am able, as you well know, to attend to a very fatiguing practice.

A gentleman, 45 years of age, consulted me for asthma, consequent upon a neglected bronchitis—and here I will say, that most cases of asthma that I have met with in this climate, have had for a cause a preceding attack of bronchitis. This gentleman had a constant troublesome cough, and was compelled to get out of his bed almost every night from paroxysms of dyspnoea. I put him on a mixture of senega, lobelia and iodide of potassium, with turpentine liniment, to the chest, from which he derived but partial benefit. I then recommended a trial of cod liver oil. He took a great many bottles of it, and he avers that it cured him. So strongly does he believe in it, that whenever he takes fresh cold he goes to his bottle of oil.

In 1856, was called to treat a lady who had a bad cough, pains in

the chest, profuse expectoration; had lost flesh; had night sweats, and all the rational signs of consumption. Auscultation and percussion made it clear, however, that it was only a severe case of chronic bronchitis, and with my faith in cod liver oil for its soothing and healing effects on the bronchial mucous membrane, I put her on the use of it; this, with quinine, blisters, and an occasional change of air, has restored her to health.

A young lady came to Mobile from the North for her health. She looked rosy and plump, but had a dry, irritative cough, and her cheeks would flush a little every day. She began to have fever, and begged me to examine her lungs. There was no dullness on percussion; the respiration was a little rude, and the expiratory sound somewhat prolonged. I advised her to take the oil. She took it with a little salt, which she said improved it much. Her cough left her, and she has been well ever since.

A near relative, at the advanced age of 76, had hooping cough, came near dying from the attack. It merged into a chronic bronchitis, with distressing paroxysms of dyspnoea. The oil acted wonderfully on her; it relieved her cough, restored her appetite, and improved her digestion. She took it at intervals for four years, and it always did her good. She died of old age.

Three or four years ago an old negro man came under my treatment for chronic bronchitis. His cough was so troublesome that I was compelled to give opiates, but the main reliance was cod liver oil, under the use of which he gained flesh and strength, and is now a useful servant, but still takes his oil whenever he has a cough.

My experience with this remedy in bronchitis is decided, and I think this deduction from my cases is reasonable, that inasmuch as persons who were so much troubled by their cough as to require the constant use of opiates to quiet it, have been enabled after using the oil for a short time to discontinue them, it must have a specific healing effect on the bronchial mucous membrane, independent of the good it does the constitution generally, by improving digestion, assimilation, etc., etc.

Far be it from me to overpraise this remedy. I have met with persons who never could get over the distate to it, and unfortunately have met with cases of consumption where they *could* take it, but derived no benefit. There is a family in which I practise, where the inheritance is so strong for all life to degenerate into tubercles, that I

have not been able yet to recommend any thing which will check this degeneration when once tuberculosis has set in. Three members have died with it, and now a fourth is affected.

It is unnecessary, gentlemen, to go on enumerating cases. I could speak of the great good it does children in the decline of whooping cough, stopping the cough, and building them up after they have gone through a long siege of this distressing disease—of the custom of my patients to give to their children for common colds and coughs. But I will not weary you, as I think I have said sufficient to set forth some of its true virtues.

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ART. IV.—*The Auxiliary Influence of Acids in Producing Salivation.*

BENNET DOWLER, M. D.: *Dear Sir*—In the November Number of the *N. O. Medical and Surgical Journal*, was published a paper of mine “On the Influence of Acids in conjunction with Mercurials.” Since its publication, I have received letters from several physicians, personally unknown to me, adding their testimony to the opinion advanced by me for fact. Among these, I may mention one whose name I have occasionally seen in the *Journal*, Dr. G. E. Elmer, of Marksville, Louisiana.

Some time after the publication of the paper alluded to, I procured the whole series of *Braithwaite's Retrospect*, in the first number of which, I found a paper advancing the same opinions as those advanced in mine. The paper in “Braithwaite” may be found in the first number, page 45, for 1840. It being a short one, I beg leave to copy it *verbatim*, in the hope that it may attract attention wherein mine, from the humbleness of its source, might not.

“SALIVATION BY SMALL DOSES OF MERCURY.

“In the discussion respecting the salivation of patients by minute doses of mercury, in the Westminster Medical Society, Mr. Snow offers an ingenious explanation, that he did not believe that the sali-

vation which was occasionally produced by a very small dose of mercury depended on any idiosyncrasy of constitution, which continued during the patient's life, but was the result of the presence of an excess of acid in the first passages. He had been led to this conclusion by having frequently seen patients salivated by a very small quantity of calomel or blue pill, taken at the same time with mixtures containing dilute sulphuric acid. Very lately, a man who was taking sulphuric acid freely for epistaxis, was severely salivated by two grains of calomel in a dose of cathartic pills. The acids naturally contained in the stomach were the muriatic and the acetic, and the mercury contained in a grain or two of calomel, or a few grains of blue pill, would, of course, be sufficient to produce the most serious consequences if changed into the bi-chloride. If the view he had suggested was correct, the means of preventing untoward effects would be to give corrosive sublimate at once, when a course of mercury was indicated, and to give it in suitably small doses.

“Mr. Streeter remarked that the fact of the action of blue pill being much more powerful at one time than another, might be accounted for by the conserve of roses with which the metallic mercury was triturated, being occasionally mixed with sulphuric acid, for the purpose of restoring its lost color. Hence, instead of the simple oxide, the sulphate of mercury was producing its effects upon the patient. He had no doubt, however, that some constitutions were remarkably susceptible of the influence of mercury in the mildest forms. He had seen severe salivation produced by the administration of nine grains of blue pill, although it had been given only in one grain doses three times a day, and its effects watched with the greatest care.”—*London Lancet*, Jan. 18, 1840, p. 625.

I refrain from any comment, and conclude by remarking that while the two papers are singularly corroborative of one another, Mr. Snow does not allude to the practical application of the fact, either in a therapeutic or prophylactic point of view, while these are the main points to which I wish to call attention.

Very respectfully, your obedient servant,

HUMPHREY PEAKE, M. D.

*Arkadelphia, Ark.*, February 18, 1859.

ART. V.—*Laryngotomy.*ROBSON'S LANDING, *Mississippi*, January 20th, 1859.

DOCTOR BENNET DOWLER: *Dear Sir*—The concluding paragraph in your "*Journalistic Musings*" for January, has emboldened me to give you a brief account of an operation of *laryngotomy*, which, if found to possess any points of interest, you can lay before your readers.

Very respectfully,

THOS. I. GOFF, M. D.

Philip, *at.* 12, slave of Col. John Wimbish, was sent to my office at six o'clock, P. M., December 22, 1858, suffering intensely from the presence of a cockle burr in his *throat*. The symptoms were very urgent; breathing loud and croup; cough violent and spasmodic, and attended with some expectoration of blood and phlegm.

I could obtain no accurate account of the circumstances attending the swallowing of the burr, except that it was blown down his throat at the mill. After a careful examination, I found that the burr, or whatever the foreign body was, occupied a portion about an inch above the *Pomum Adami*, and was permanently fixed in the body of the larynx. Being fully satisfied from the nature of the burr, its numerous points rapidly exciting inflammation and causing a narrowing of the tube, that a temporizing course would only add to the danger, I decided to operate immediately.

*The operation.*—The boy was placed upon a table, with his hands and feet well secured, and a small pillow under his neck, so as to throw the head slightly back. The first incision of an inch in length was made to correspond to the crico-thyroid ligament. The cellular tissue between the sterno-hyoid and sterno-thyroid muscles was next divided down to the ligament. The larynx was then fixed with a finger on each side, and with the forefinger of the left hand holding apart the edges of the wound, an ordinary thumb lancet was thrust in, making a cut of one-fourth of an inch in length. A probe slightly bent was introduced a short distance, which produced spasmodic fits of coughing, almost approaching to strangulation. The orifice was then enlarged, and the probe carried higher; after a few attempts the burr was dislodged, and thrown several feet from the table. A complete amelioration of all the symptoms took place, the loud breathing ceased, and the patient went off in a deep sleep.

In enlarging the opening, a small arterial branch was severed,

which bleeding internally, came near proving fatal. It being night and having very little assistance, no attempt was made to take it up; the hæmorrhage was arrested by the application of cold water, and afterwards a pledget flint was applied with a bandage, an opiate administered, and the patient put to bed.

On the next day no air passing through the wound, and natural breathing having been fully reëstablished, the edges were brought together with strips of adhesive plaster. At the end of the third day no bad symptoms occurring, the boy was sent home, and has since entirely recovered.

ART. VI.—*Removal of a Needle after a Sojourn in the Body for Twenty Years*: By W. J. HEDDENS, M. D., St. Joseph, Missouri.

AN intelligent and temperate colored boy came to me saying, "there was something in his hip—that he often had sharp pains in the part, and that he could feel something under the skin." I could distinctly feel a foreign body, and immediately cut down upon and removed a *needle*, which had the appearance of having been in the flesh for a length of time, as it was blackened and brittle; the point was broken off, and the fractured end rounded and smooth. He says that "it is impossible for the needle to have been introduced through the skin without his knowledge;" moreover, it was too much corroded and too brittle to have been recently introduced. About twenty years ago he swallowed a needle, which is the most probable way to account for its presence. Now, *did it work its own way out?* or, did the AUTOCRATEIA of STAHL expel it from his domains?

The motion of the different muscles might have caused it to move in some direction, and from the length of time in which it was making its exodus, it is probable that its journey was caused entirely by mechanical powers. It is true that there is a *power* in vital functions to remove morbid state and foreign bodies, often doing so by pouring out a secretion which *washes* or *lifts* the *materies morbi* from the economy; but here there was no suppuration. The needle appears to have *cut* its way through the tissues, and the breach of continuity was healed by the "immediate union" of surgeons,



I think there is too much power and too many actions assigned to this *anima medica*, now as well as formerly. The belief in the spontaneous cure of diseases, which has so great an advocate in the person of Sir John Forbes, is but an apology for the EXPECTANT, *vel* DO-NOTHING, *vel* HOMŒOPATHIC practice, which there is so great an inclination to adopt, by some members of the profession.

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ART. VII.—*Observations on the Anatomy, Pathology, and Treatment of the Spleen*: By GREENVILLE DOWELL, M. D.

ANATOMY OF THE SPLEEN.

THE spleen is a ductless gland, situated and lying in the left hypochondriac region. It is an oblong, flattened body, of dark blueish red color, about five inches in length, and weighs about six ounces in its natural and unexcited state; but varies a good deal in size during digestion, even in perfect health. It is spongy and vascular in texture. The external surface is convex, the internal slightly concave. It is indented along its middle line and pierced by numerous vessels, which form what is called its *hilum liens*, being irregular openings to admit its large vessels. Its posterior aspect is obtuse, its anterior sharp and notched. The upper extremity is larger than the lower, and somewhat round; the inferior is flattened. By its external or convex surface, it is in relation with the diaphragm, which lies over it and separates it from the ninth, tenth and eleventh ribs; by its concave surface with the greater end of the stomach, the external extremity of the pancreas, the gastro-splenic omentum and its vessels, the left kidney and supra-renal capsule, and the left crus of the diaphragm; by its upper end with the diaphragm, and sometimes touching the left lobe of the liver, and by its lower end with the transverse arch of the colon on its left extremity. It is connected with the stomach by the gastro-splenic omentum, and to the diaphragm by a fold of the peritoneum, called the *suspensory ligament*.

It is said by Gross and others, that there have been found from one to eight or ten small spleens, about the size of a hazelnut, and from that to a walnut, connected with the spleen proper by the

splenic artery to which they are directly attached. They are called by anatomists *liens succenturiati*. They are generally found near the greater end of the stomach. I have never seen any of these bodies myself.

The spleen is invested by the peritoneum and an elastic membrane peculiar to itself, which enables it to yield to greater or less extension by its blood vessels. This elastic tissue forms sheaths for all its vessels in their various ramifications. From these sheaths small fibrous bands are given off in all directions. These fibrous bands, called trabeculae, by their connections form its areolar tissue. That portion included by these sheaths is soft, granular, of a bright red color, and interspersed with malpighian bodies, small white bodies or corpuscles. These corpuscles are believed to be aggregations of cytoblasts, inclosed in a kind of capsule of capillary vessels. Separate cytoblasts are abundantly scattered through the red substance.

The spleen receives its blood from the cœliac axis by the splenic artery, which is of very large size in proportion to the spleen. The branches of the splenic artery are distributed to different sections of the spleen, and anastomose very sparingly with each other. The veins are very large, and by their various ramifications compose its principal bulk. The ramifications pour their contents into the splenic vein and help to form the *great portal vein*, which finally passes through the liver. The lymphatics are numerous and of large size; they terminate in the lumbar glands. The nerves are the splenic plexus, derived from the solar plexus.

#### PHYSIOLOGY OF THE SPLEEN.

The physiology of the spleen is not well understood, but from late investigations we begin to have more definite ideas of its uses and office in the human economy.

Like the other ductless glands, it does not seem to be essential to life. It may be removed or become perfectly atrophied without the patient's suffering material inconvenience. But in all such cases the lymphatics are much enlarged, and seem to perform its office in the economy of nature.

It is now known that the spleen does modify the blood that passes through it, but how the gland acts to produce this modification seems to be a matter of doubt. I believe the blood receives and parts with

some of its principles as it does in its passage through the lungs; for we know that the red corpuscles are diminished, and that worn out corpuscles are disintegrated, and in the colored portion of the spleen pulp, corpuscles are found in various stages of degeneration, and those corpuscles help to give it its dark color.

Kirke says (in his Physiology), that "the large size the organ obtains towards the termination of the digestive process, and the increase in the fine granular, albuminous plasma within its parenchyma, and the decrease of these materials, appear to show us, that the gland is concerned in elaborating the albuminous or formative material, and for a time storing them up to be gradually introduced into the blood according to the demands of the system." He also observes, that owing to the small amount of fatty matter, it is not supposed that it aids in the preparation of materials for the respiratory process. But it is quite probable that it, like the lymphatics, assists in forming germs of subsequent blood-corpuscles as there is a large amount of white corpuscles found in the splenic vein.

Besides these, its supposed functions, it acts, no doubt, as a *diverticulum to the portal circulation*. It is used to contain portal blood (after digestion) with its nutritive quality received from the food, and it gradually returns it to the liver, where it is further acted on before it passes to the lungs, etc. This is also made probable by its enlarging in affections of the liver and heart, tending to impede its passage through them, and by its decreasing when the portal circulation is depleted by discharges from the bowels and from the effusion of blood into the stomach.

#### PATHOLOGY OF THE SPLEEN.

The diseases and morbid anatomy of the spleen are better understood than its physiology. It is subject to all the various analogous and heterologous formations to which other parts of the body are liable. Inflammation, acute and chronic, abscess, gangrene, hypertrophy, atrophy, softening, suppuration, etc. In its hypertrophied state, it is also liable to be *dislocated* or *lacerated*. It is subject to *induration* and *hepatization*, or sometimes to become *fibro-cartilaginous*. It is liable to atrophy or even to entire destruction. Of all the *analogous* formations, the spleen is most subject to hydatids in its substance or on its capsule. It is also subject to fatty tumors and calcareous deposits, sometimes in small bodies like pin heads, one

case of which, I have seen accompanied with miliary tubercles in the lungs—which they much resemble, if they were not identical.

Serous cysts are found in and on the body of the spleen. These cysts are sometimes filled with mclcerous, atheromatous, or steatomotous substances.

There is no pathognomonic sign or symptom of these affections. The spleen is always more or less enlarged in these cases, but they cannot, during life, be distinguished from hypertrophy as a consequence of *fever*. Hydatids sometimes become very large and burst into the abdomen and cause death from peritoneal inflammation.

Of the *heterologous* formations, tubercles are the most common, and will be most generally found where there are tubercles in the lungs or mesentery. An interesting case of this kind, I saw in a subject I assisted to dissect at the Louisville University. In this case, the mesentery was completely studded with tubercles, so much so that Dr. Bayless kept the specimen, and it is now in the museum of that college. Melanosis is a rare disease of this organ, but all kinds of cancers are said to occasionally occur in it. None of these affections can be recognised during life. They may be inferred to some extent from the cachectic state of the system, and from being known to be present in other parts of the body.

Metastatic abscess of the spleen from wounds and surgical operations are said to be common, but not so often as in the lungs, liver, or brain.

Much the most common affection of the spleen is an acute inflammation of its parenchyma as an accompaniment or sequel to *fever*, dysenteries and diarrhœa. Acute inflammation of the spleen, if not promptly and judiciously treated, will most generally terminate in chronic inflammatory fibro-cartilaginous induration, suppuration, gangrene or apoplexy. Inflammation of the spleen is a common affection where I have practised. In the Mississippi and Brazos bottoms scarcely any one lives from three to five years without having been subject to this affection. I presume I have seen as much of this disease as any man of my professional age, and can speak positively on this subject. For the last twenty years I have lived either on the bank of the Mississippi or Brazos, or near their immediate vallies. Fourteen years on the Mississippi and six on the Brazos; twelve years in active practice, and eight years under my brother, then in full practice at Raleigh, Tennessee. Inflammation of the spleen, as I

said before, is common to every one living three or five years in these bottoms. Whites and mulattoes are much more liable to it than pure blacks. In blacks I have never seen a case of enlargement of the spleen except as a complication of typhoid or rather as a concomitant of typhoid fevers. Griffs are not as liable to it as mulattoes, nor mulattoes as whites. Blacks are partially exempt from intermitten fever, but are equally, I believe, liable to inflammation of the liver and remittent fever. As to affections of the liver, I have not noted any difference in persons of different colors; nor have I discovered any difference as it respects typhoid fever. I have thought, as a general thing, that blacks are more liable to typhoid fever than mulattoes or whites, but I attributed this to their want of cleanliness, which is observable to any one who is familiar with large negro plantations. I have found the spleen partially enlarged and rather indurated in all cases of typhoid fever, which I have examined after death even in a child two years old.

Acute inflammation, as I said before, is liable to terminate in chronic, and from chronic inflammation, we have hypertrophy as a most usual result, and from enlargement we have dropsy, a general anæmic state of the system, and an impoverished state of the blood.

If the active inflammation is not subdued or modified by treatment, the spleen will soften, suppurate, or terminate in gangrene. If the inflammation alone is attended to, and the patient is not treated for the primary cause, say malarial or typhoid fever, the spleen will become gradually enlarged, and often obtains an enormous size; many hundred of such cases have come under my observation.

Hypertrophy may come on gradually and even imperceptibly, but may be easily detected by pressure over the region of the spleen. There is always more or less tenderness on pressure, with flatness on percussion, even before the spleen can be felt. After it becomes large its outlines are easily traced, and from the small size we have described it in health, it enlarges until it covers the anterior cavity of the abdomen, even feeling as if the whole cavity were filled with a solid body. It now causes a protrusion of the abdomen similar to that of a pregnant woman in the ninth month. That the spleen is enlarged may be told also by the general appearance of the countenance and skin. The skin is of a sallow-white color, and may be easily recognized by those familiar with the disease. It may, nine times out of ten, be told by the general appearance of the skin alone,

without any tæctic examination. In cases of enlarged spleen caused by malarial fever, I have not so generally found the liver implicated, as we are led to suppose from reading the books; it is often found in almost a healthy condition, sometimes, I may say often, secreting bile too freely. Where the liver is not implicated but acting, I have not found mercury necessary to a cure of the spleen, while in those cases where the liver is diseased also, mercury is almost, if not absolutely necessary to perfect a cure. In intermittent and remittent paroxysms I have found a great tendency to *rigors*, if the spleen be enlarged, or enlarges during the paroxysm; but where the spleen was not enlarged, or did not enlarge during the paroxysm, the chill and fever generally came on together. Where the teeth are made to chatter, and there is a general shaking of the muscles, the spleen is always enlarged. This may be called a pathognomonic sign, for I do not remember a single exception. While the spleen is moderately enlarged, and before adhesions have taken place to the surrounding textures or organs, the spleen may be easily *dislocated*, several cases of which I have seen: the suspensory ligament is torn, and the spleen turns over, as it were, and falls forward on the colon and bladder, and here becomes fixed by adhesions, so that it cannot be replaced; yet it may be partially reduced, and the patient may live and even enjoy a moderate portion of health, but always have the sallow appearance which I have described. A case of this kind is now in Brazoria county, in a lad of fourteen years.

Suppuration of the spleen is rare in proportion to the number of cases of enlargement. By some, as the late Prof. Drake, it is supposed never to occur below thirty-five degrees of latitude. This is undoubtedly an error, for I have seen three cases in my county, south of Galveston. A Mr. Bradly had his spleen to suppurate in 1853, and it pointed through the abdominal wall below the ribs, and was punctured by my friend, Dr. F. M. Oatis, of Lordy Point, Texas. It discharged a large amount of dark grumous pus and blood. The orifice healed up; but he still has an indurated spleen of an enormous size, covering most of the abdominal wall. He is at times dropsical, and has a running ulcer on his leg, as a result of the chæstetic state of his system. He is able to ride about, and otherwise enjoys good health.

CASE 2. Mrs. P., of Brazoria county, was taken with intermittent fever in the spring of last year, 1858, and it continued to occur

during the early part of the summer, until finally the spleen suppurated and discharged its contents into the stomach. She was treated by myself and Dr. Porter of Columbia, with mercury, iodine, blisters, and quinine, or the citrate of quinine and iron, but nothing we could do would stop suppuration. There was a circumstance in Mrs. P.'s case worthy of further note: she was salivated with iodine used as an embrocation over the spleen, while there was no smell of mercury, or any ulcerations of the gums. The saliva poured out of the mouth, until she thought she would suffocate from it. By discontinuing the use of the iodine the salivation soon subsided, though the mercury was continued more or less for two weeks afterwards, and without any symptoms of ptyalism. She suffered greatly from retchings and sick stomach. The alvine discharges were frequent and of a flux-like character. She finally recovered, but her spleen is still very much enlarged.

CASE 3. Mr. Henry, a carpenter of Columbia, had had chills for nearly two years, at irregular periods, and finally was taken so bad that he could not work. He paid a visit to the Gulf of Mexico, for the purpose of trying sea-bathing, and while there his chills got worse on him, and he was brought home on a bed. I was called to see him, and used the usual remedies with energy, but could make no change for the better. I broke up his fever and stopped his regular paroxysms, but still he did not improve. I blistered over his spleen, which was much enlarged, but could not reduce it. About the fifth day of my attendance he had a hard shake, and thought he was dying. His chill lasted about one hour and a half, and went off with a profuse sweat. It came on him when he had about twenty grains of quinine in his system, which in most cases is sufficient to stop any chill: seeing this I immediately began to suspect suppuration, and I examined for a softening, but could not detect any. His liver was acting well; his stools were bilious; but I thought I could feel the liver lobulated, and it was tender on pressure. He had slight fever after his shake, with a hectic flush on his cheeks. These shakes continued at irregular periods for three days, in spite of all I could do, and he finally died delirious. There was no post-mortem, which I regret; but from all his symptoms, I thought there were several abscesses of small size. His symptoms resembled cases of suppuration of the mammary gland, etc.

## HICCOUGH.

I have seen severe and continuous hicough from an enlarged spleen. One ease, I well remember, occurred to me, while practising in Panola county, Mississippi. I was treating a young man for intermittent fever, attended with vomitings and sick stomach. His spleen was enlarged. I gave him some pills composed of blue mass and rhubarb, and followed it with large doses of quinine—ten grains every two hours until three doses were taken before his chill time. This broke his fevers and stopped his chills, and greatly reduced his spleen. But as the spleen contracted, he was taken with hicough, and it was more distressing than his former situation. I gave him all the antispasmodics I could think of—morphine, camphor, assafoetida, valerian, and warm baths; applied mustard to the stomach, etc., yet in spite of all this, his hicough continued even during sleep for eight days; at last, not knowing what to do, I salivated him, and as soon as the mercury took effect he began to improve, and with the salivation, he recovered entirely, and has never, as I know of, been subject to this symptom since. I thought in that case, the spleen had contracted adhesions to the diaphragm, and the medicines I gave him to cure his fever and ague, had cured his spleen and torn up the adhesions, causing the diaphragm to take a convulsive action. The calomel subdued the inflammation of the spleen and diaphragm, and thereby cured the hicough.

Gangrene of the spleen I have never seen. Softening is a frequent accompaniment of hypertrophy. Atrophy I have never seen, and think it very rare. Fibro-cartilaginous and indurated spleens are common. Where an enlarged spleen is reduced by medical treatment, it almost invariably is left indurated, and its coats become fibro-cartilaginous, and its trabeculae are enlarged and thickened. The spleen sometimes seems to lose its function, and is not easily enlarged by fevers afterwards. It is not an unusual occurrence for a boy, or child, or even for young men, to have enormously enlarged spleens, and to be sent out of the bottom lands to school, to recover entirely their health, and on their return they are not subject to malarial fevers, and especially are they exempt from enlarged spleens. But in these indurated cases the spleen is never found in its natural state or size in healthy persons who have never suffered from enlargement. It never returns to its normal size, and can generally be felt in its place. The spleen in such persons is found after death to be



fibro-cartilaginous. The spleen in its enlarged state is often ruptured or lacerated by external violence, by blows with the fist, falls from horses, etc. Several cases of death from such causes I have seen. A case of this kind is reported in the *New Orleans Medical and Surgical Journal*, by Dr. Chinn, of Brazoria, where an individual died from a blow over the spleen, in a fight. The homicide was tried for murder, but upon a post-mortem examination made by Dr. Chinn, the spleen was found softened and easily lacerated, and upon a statement of these facts the man was acquitted. I also knew a young man to be thrown from his horse, and to die in a few days from pain in the region of the spleen, caused, no doubt, from a laceration.

## THERAPEUTICS OF THE SPLEEN.

The means of subduing inflammation are as applicable to the spleen as any other organ, and should be resorted to immediately, as in all cases of inflammation. Where it is brought on with or by malarial fever, you must give in addition to mercury, quinine in large doses. Experience has taught us that the spleen can be reduced sooner by quinine in large doses given at once, than by small doses often repeated. Thirty grains given in three doses of ten grains each, are more efficient than thirty grains given in ten doses in twenty-four hours. If there be tenderness over the spleen after you have given an active dose of mercury and quinine in free doses, it is well to apply a blister, which will remove the tenderness and reduce the spleen, if enlarged.

In chronic inflammation of the spleen, with enlargement, I give but little mercury, and that generally combined with taraxacum. Blue mass, often repeated, I have found injurious. It is too slow in its action, and creates an irritation of the stomach; but if it be combined with some active purgative, as rhubarb or taraxacum, it answers every purpose and does well. I usually give one of the following pills, but much prefer the first where there is fever:

- |                                      |                                      |
|--------------------------------------|--------------------------------------|
| (1.) Calomel.....x. grs.             | (2.) Blue Mass.....xx. grs.          |
| Taraxacum.....v. “                   | or Rhubarb.....vi. “                 |
| Rhubarb.....vi. “                    | Make into four pills: to be given at |
| Make into four pills; to be given at | one dose.                            |
| one dose.                            |                                      |

If there be no fever, divide this into two doses, and give two pills every four hours. If there be diarrhoea with fever, combine an opiate with the pills, morphia or opium, the latter is best. If those pills do not operate in ten hours, give a dose of castor oil; if they act too

freely their action must be checked by opium: it is not well to let the bowels run off, as we say, in any case. After the action of these pills, or if the case be urgent, begin with the following pill: citrate of quinine and iron xxx. grs., extract of gentian or liquorice, sufficient to make into mass; divide into six pills; of these, give one every hour until all be taken, beginning in time to get them all taken before the expected chill. If the patient is disposed to be dropsical, I combine also with this pill some tonic. Oil of black pepper is the best, half a drop to each pill. This should be repeated from day to day, until the enlargement of the spleen cannot be felt, or until all signs of chills or fever are gone. The quinine and iron should be taken every cold or wet day, until all appearance of the disease is removed. There is a great tendency to relapse in these cases, which should be strictly guarded against. I have tried iodine in every form and every mode I could think of, but I cannot say it ever did any good in my hands; nor have I ever seen it do any good when applied to enlarged tonsils; the functions of the absorbents, or rather the lymphatics, are destroyed by the enlarged spleen, and we must therefore endeavor to cure by endosmosis instead of by intestinal absorption, hence, the great good which blisters do in this disease. Blisters should be small, and be often repeated. I have now abandoned the iodine treatment, and find I succeed much better without it. I never fail by blisters and citrate of quinine and iron. This preparation is the best of all the various compounds of quinine, in cases of enlarged spleen, or in anæmic conditions of the system. The bowels must, in all cases, be actively moved at least once a day, especially where there is a tendency to dropsy. In dropsical subjects, after giving the above pills, I often follow them with cream of tartar for several days or weeks. In cases of congestive fever where the spleen is enlarged, there will be no serious congestion of the liver or brain, but great congestion of the stomach and in the mesentery is evinced by continual vomitings, retching and diarrhœa, even almost like cholera. There will be great pallor of the surface, and the skin will assume its peculiar shrunken appearance, called *cutis asnerina*. These cases require stimulants of the most active kind—brandy, ammonia, sinapisms, with an opiate. But they will not bear much mercury without producing ptyalism, and it is not so necessary to a cure; but a moderate dose at first I have found generally necessary or beneficial. Our main reliance is upon quinine. It is in all cases a *sine qua non*. Where there is no enlargement of the spleen, the liver or brain is

sure to be seriously affected, and then mercury is our *sheet anchor*, and it must not be spared until you overcome the congestion, or affect the system with it. In these cases I give also the *sulphate of quinine*, for that seems to be the most efficient in all cases where the liver or brain is affected.

The best time to reduce an enlarged spleen in the bottom lands is in the fall, after frost, and by continuing your treatment through winter you can perfect a cure; at any other time relapses are sure to come, unless the patient is very prudent, and often no prudence will avail. Then the patient must leave the bottom lands, or take medicine all the time until the habit is broken up, and the spleen becomes quiet, as it were. I find irritation with Croton oil and sweet oil, xx. drops of Croton to the ounce of sweet oil, rubbed over the region of the spleen, or tartar emetic ointment, to answer a good purpose, where the patient is unwilling to blister. But I would never rely on iodine, as I said before, for I have found it inefficient. This seems strange, from its known and undoubted good effects in bronchocele and in enlargement of the lymphatic glands, but we must remember that the lymphatics are, as it were, destroyed, and therefore cannot act. The red globules must be increased, and also the fibrin, hence the good preparations of iron do in these cases. Unless the blood is enriched by iron, or some tonic, quinine will not stop a long-standing case of chills. It will always stop or check them for a while, but they will return. Port wine is an excellent tonic in these cases. Gin is even better where there is a tendency to dropsy. The Russian belt worn tightly around the body is good in all cases of enlarged spleen, whether it is from hydatids or fever, and where the spleen is dislocated, or the patient is dropsical. Any belt made of leather or cloth will answer, but the Morocco belt made wide is the best. It should be worn tight, and be kept so until the spleen is reduced. I have often cured cases by the following pill, which will be more readily taken by your patient than almost any other combination:

Sulphate of quinine.....	ʒ i.
Citrate of quinine and iron.....	ʒ ss.
Extract of Gentian.....	grs. x.
Hyoseyamus .....	grs. xii.

Make into twelve pills: give one every hour until six are taken. This pill, with an occasional dose of the calomel or blue mass pill mentioned before, will cure any case of enlarged spleen or chills, if persevered in a sufficient length of time.

ART. VIII.—*Memoir on the Nature of Miasm and Contagion*,\* Read before the New Orleans Academy of Sciences, 28th February, 1859. By J. L. RIDDELL, M. D., President of the New Orleans Academy of Sciences, and Professor of Chemistry of the Medical Department of the University of Louisiana.

THE present advanced state of the natural sciences, chemistry, natural history and physiology, warrant us in affirming that the essential matter of the virus of contagions, of miasms, and of the occult causes of zymotic diseases in general, must be of an organized and vital nature, for the following reasons:

1. They are subject to the same general laws which regulate the origin, increase, modes of existence and duration of animal and vegetable bodies.

2. Although they are regarded as poisons, noxious and to be dreaded, yet unlike arsenic, potassa, strychnine, and Prussic acid; and unlike all other poisons of definite chemical composition and unorganized nature, whether furnished by organic or inorganic chemistry, they do not lose their potency in acting.

3. Unlike most of the ordinary poisons they cannot be artificially produced, by compounding the elements composing them. They never originate by the fortuitous admixture of mineral ingredients.

4. Miasms and infections contaminate the air of the atmosphere. Air from healthy, as well as air from sickly regions, has been subjected to chemical analysis, repeatedly and satisfactorily. A variable trace of organized matter, in the form of microscopic spheres and filaments, and trans-microscopic particles; more abundant where zymotic diseases prevail, less abundant in healthy localities, is all of a suspicious nature that can be found in it. The other constant ingredients, nitrogen, oxygen, carbonic acid, and water; and the occasional ingredients, carburetted hydrogen, sulphuretted hydrogen,

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\* A paper bearing the same title, advocating the same views, and advancing the same arguments, was published by the author in the January number, 1836, of the Western Journal of the Medical and Physical Sciences, edited by Daniel Drake, M. D., vol. ix., pages 401 to 412, and 526 to 532. It was republished the same year in the Boston Medical and Surgical Journal. That miasms and contagions might be minute germs of vital organisms, had been vaguely suggested by various writers, at various times. Linnæus entertained that opinion. It was hinted at by the ancients. But the controlling reasons which give confirmation to such an opinion I have never seen adduced by any writer prior to the publication of my memoir. After the lapso of some years, several able American authors advocated these views in their writings. As among them, the names of Prof. J. K. Mitchell and Dr. W. P. Hort, both deceased; and Dr. J. C. Nott, Prof. James Jones, and Prof. Joseph Henry, may be mentioned.

ammonia, nitric acid, ozone, and mineral particles, cannot, by any possibility, be supposed to act as miasmatic, or infectious poisons. The amount of organized matters in the air of the atmosphere, seems from the analyses made, to vary in different places and at different times, from one to five parts, in two hundred thousand by weight.

5. Ordinary poisons introduced into the animal system, immediately begin to exert their specific influence; miasms, infections and contagions, lie seemingly dormant in the system after exposure, for periods more or less definite for each kind, called the latent period or period of incubation, which is analogous to the period required for the germination of vegetable seeds, and to the period required for the development of animal ova.

6. The history of epidemic diseases, and of zymotic diseases in general, shows that there must be a great number of different miasms, infections and contagions, just as there is a great number of different species and varieties of animals and plants; whereas, the known unorganized poisons, composed of dead elements, are in comparison very few in number.

7. Mineral compounds are definable with exactness, and being produced at intervals, no matter how remote, are found to possess an unchangeable nature; the corrosive sublimate of to-day being identical with the corrosive sublimate of two hundred years ago. Organized and living bodies present us certain types of form and structure, constituting species and varieties; and by lapse of time and with varying circumstances, the succeeding progeny of these species and varieties, often present us types of form and structure, more or less modified from the original. In a similar way we observe zymotic diseases to be generally somewhat modified, upon their recurrence after the lapse of time, from which may be inferred, that the type of the occult cause is liable to suffer modification, like organized and living species.

8. It has been established that nearly all the true diseases to which plants are liable, arise from the parasitic encroachment of fungi, algæ, lichens, animalcules, and insects. These diseases, are strictly analogous to the zymotic diseases of animals; and by parity of reason it may be concluded, that the zymotic diseases of animals must in like manner be due to the agency of parasitic living organisms.

MINERAL MATTER CONTRASTED WITH ORGANIZED AND LIVING BODIES. CHANCE  
GENERATION REPUDIATED.

In order that due weight may be assigned to some of the most important facts and arguments adducible in favor of the organized nature of miasms and contagions, I will here present the prominent points of contrast between organized and inorganic bodies.

*Form and Size.*—Organized beings, animals and plants, possess forms more or less rounded; and never exceed in size certain definite dimensions. Mineral or inorganic bodies, as a class, have no general determinate form, unless it be the angular one assumed by them in the crystalline condition; and in respect to size, there are seldom any assignable limits.

*Chemical Composition.*—Organized bodies are essentially made up of the ultimate elements carbon, hydrogen, oxygen, and nitrogen, in all cases united in a most complicated manner. The composition of individual minerals is always characterized by definiteness and comparative simplicity, though as a class, they embrace all known elementary bodies.

*Growth and Internal Structure.*—Minerals increase in size by the external addition of particles similar to their own, and the causes which elaborate and supply these particles are wholly independent of the bodies which they tend to augment. Animals and vegetables possess within themselves a power of elaboration; they consequently increase in size by the internal assimilation of foreign particles. In all organic structures of the more highly developed types, there exist harmonious and dependent series of vessels, tissues and organs, which concur in effecting this end; of this internal conformation minerals are wholly devoid. The same causes which develop and perfect the organic structure, ultimately put a limit to the duration of individual beings; whereas, all brute matter is endued with the negative attributes and conditions requisite to insure endless perpetuity.

Comparing mineral with organized matter, it will be seen that they are wholly unlike, that there is no gradation of one into the other; on the contrary, in no known instance do they seem in the smallest degree to approach each other in their essential characteristics. *Mineral matter* may be homogeneous, may generally be changed into the liquid or gaseous condition, without injury or destruction; may possess a crystalline form; has a definite, or more or less simple

chemical composition, which can be numerically expressed in combining proportions of its component elements; and mineral bodies increase or decrease in volume indefinitely, by addition to, or abstraction from, the surface—with no specific necessary limit to their duration in time. *Organized and living bodies* are never of homogeneous texture; never wholly liquid or gaseous, but always possessing solidity, with forms more or less rounded, never crystalline; increase in volume by an internal process of assimilation, to which their organized structure of cells, vessels and circulating fluids, is subservient; never exceeding in size a specific limit, nor enduring in time beyond a specific limited period. Organized bodies must be regarded as indefinite in chemical composition, although the elements composing them, when detached, are truly identical with the elements of mineral matter; and indeed, definite compounds, the proper subjects of Chemistry, exist componently in the organized structure, but so indefinitely combined with each other, as to be beyond the clear elucidating power of the Chemistry of the present day. All this is true, even of the most simple organic cell, whose existence has been revealed by the microscope.

Organized and living bodies can receive, absorb, elaborate and assimilate dead matter from the mineral kingdom; but no authentic instance is yet adduced, among the myriads of scrutinizing observations made, where mineral matter by itself has elaborated and exalted itself into the organized and living condition. The experiments of Crosse and Weeks upon the production of Aëri by Galvanism, and of Pouchet, on the appearance of *Aspergillus*, etc., in boiled water, deserve no weight in this question, for they were not conducted in a manner to exclude with certainty organic germs. We may, indeed, set it down as established, that organized matter has its inception with organized matter anteriorly existing; that all living beings are descended from living beings; just as motion is exclusively derived from preëxisting motion, and just as the present material combinations are derived from preëxisting matter.

The hypothesis of spontaneous or fortuitous generation, vaguely entertained by so many, is, therefore, an unnecessary and absurd assumption; unsupported by a single instance, among unnumbered well ascertained contrary instances; and not in harmony with the obvious laws of nature. This notion is apt to be entertained by superficial observers, to account for the appearance of mould, algæ, infusory

animalcules, and parasitic organisms, in situations, and under circumstances, where no preceding germs are easily observed; especially in the more obscure department of organic nature. But closer observation reveals the fact, that the earth, the waters and the air, swarm with exceedingly minute spores or organized particles, wonderfully tenacious of passive vitality, which are ever ready to develop themselves into visible life, where the requisite conditions obtain.

And thus appreciating the impassable barrier between animate and mineral nature, and which brute matter of itself can never surmount, we may boldly claim for the corpuscles causing malarious, infectious and contagious maladies, the distinction of possessing a share of vitality, humble though it may be, and located near the lower confines of organic nature; chiefly because, like the animals and plants which come under our observation, they manifest, as circumstances permit, the power of propagating and extending themselves indefinitely.

*Modification and Disappearance of Organic Species.*—Although a finite period of time is allotted to the existence of individual plants and animals, yet many eminent naturalists profess to believe that nature has no fixed bounds to the duration of species. Others equally learned—and with these I fully coincide in opinion—regard it as more consonant to the general ways of nature, and better in harmony with a multitude of established facts, to suppose that particular species or kinds lose their identity in process of time, by giving origin to modified kinds or varieties, which in turn may long wear the aspect of confirmed species, but which also in like manner give way to their successors.

ITEMS FROM THE NATURAL HISTORY OF THE MORE OBSCURE TRIBES OF  
ANIMALS AND PLANTS.

With the view of casting the light of analogy on the possible habits of unknown morbid molecules, it will be proper to cite certain established facts in the natural history of the Algæ, the Fungi, and the Protozoa.

*Pencilium Glaucum.*—Wherever the vinous fermentation goes on, the fermenting liquid is rendered turbid, by the presence of millions of simple, whitish, microscopic plants, known as the torula, or yeast plant. Its germs come from the air, and there is developed in the fermenting liquid, a wonderfully simple plant of a few concatenated



cells, which multiply by buds and offsets. It is a fungus, or an alga, just as you please; and in the liquid, produces mainly a development of what is called *mycelium*, mere vegetable subdivision and extension, without the production of any true germs, or spores as they are called, such as come from the atmosphere. A mass of this mycelium floating upon the surface of the liquid, or otherwise brought in contact with the air, will be likely to throw up delicate stems ornamented with tassels, the latter yielding the true aerial spores from which the original torula is derived. In this condition, it is a well characterized fungus, known as *Pencilium glaucum*.

*Euglenia Viridis*.—The green color of the water in the shallow street gutters of New Orleans, will be frequently found, upon examination, to be mainly due to millions of microscopic *Eugleniæ*, corresponding to the description of *Euglenia viridis*. It appears as an elongated, pointed, soft-bodied animalcule, moderately active, without external opening, and without any apparent internal organization except granules and disconnected cellules of various sizes, mostly of a green color, while some are colorless and one or more are red. It assumes, at will, a great diversity of shapes. When these animalcules have attained their full growth, which with a whole pool full occurs at the same time, they assume a globular form, and rise passively to the surface of the water, and form thereon a scum of intensely vivid green. Here they become dry and fall to pieces, and their contents of minute balloon cellules filled with air, ascend into the atmosphere mechanically, with the passing breeze and with the evaporating water. It is presumable that these balloon cellules are true aerial germs or spores. They may be detected and recognized in the atmosphere of New Orleans. They may possibly constitute a species of miasm.

I have been led to entertain the opinion, that when they develop themselves by germinating in water, they do not immediately reproduce the *Euglenia*, but instead thereof, a long green moving microscopic filament, a species of alga, known as *Oscillatoria*, with which our street gutters generally abound. When the *Oscillatoria* has performed its functions, it appears to break up into numerous transverse segments, which in time seem to develop themselves into the *Euglenia* condition. Difficulties attend these observations, and I do not feel fully assured that *Oscillatoria* and *Euglenia* are alternate forms

of each other. (Vide New Orleans Medical and Surgical Journal, September, 1852, pp. 182 to 184, tables xxi., xxii.)

*Distoma*.—In the lower orders of living nature, it excites wonder to observe the various transformations which particular species can undergo; successively presenting such dissimilar forms and habits, as to wholly mislead the casual observer as to their specific identity. Steenstrup, a Danish naturalist, made careful researches into the various transformations of a species of *Distoma*, a minute fresh-water worm. His results have been published as establishing an alternation of generations. Agassiz has extended the researches of Steenstrup (Sill. Jour., 1852, p. 425); and the various forms which this minute being assumes, may be thus enumerated:

1. In stagnant pools of fresh water, in which reside such fresh-water shells as *Lymnea* and *Paludina*, a spherule microscopically minute can occasionally be seen. This is the *egg* of *Distoma*.

2. This egg develops into an oblong ciliated animalcule, which is active and voracious, and corresponds to the genus *Opalina* of naturalists.

3. The *Opalina* being swallowed by *Lymnea*, is next transformed into the condition of a short worm, called the *grand nurse*.

4. The grand nurse gives viviparous birth to a numerous progeny of worms somewhat unlike itself, which still remain parasitic within the shell of *Lymnea*. These are called *nurses*.

5. Each nurse in like manner gives birth to a progeny of worms, very unlike itself, known to naturalists under the generic appellation *Cercaria*. These escape from the shell into the water, without further multiplication.

6. The *Cercaria* is considered as the larval condition of the *Distoma*, for it undergoes a transformation by which it assumes the *Distoma* form, totally unlike itself in shape. The *Distoma* exercises sexual functions, and reproduces the eggs before alluded to. The multiplication of individuals in the intermediate forms, is asexual, like the budding or gemmation of plants.

*Tænia*.—The different species of *Tænia*, or tape worm, which occasionally infest the intestinal cavity of animals that feed on flesh, including man, where they have been found twenty-four feet long, have been demonstrated to originate from other parasites, comparatively inconspicuous, which bear no general resemblance to them, belonging

to the genus *Cysticercus* of Zeder. The tape worm produces millions of minute eggs, that are expelled from the body with the faecal evacuations, and fortuitously finding their way to pools of water, are probably developed into active animalcules. This water drunk by swine, sheep and other herbivorous animals, gives origin to the *Cysticerci*, which occasionally infest them, by burrowing in the substance of the liver and other organs. Carnivorous animals, swallowing their infected, uncooked flesh, have tape worms developed in the intestinal tube.

*Polyporus Destructor*.—Several species of fungi have been observed to produce in timber the dry rot. Among them may be mentioned *Polyporus destructor*, *Merulius lacrymans*, *Merulius vastator*, etc. Developing extensively within the timber, they assume no characteristic form or outline, and consist wholly of mycelium or fungous structure, without ever producing true spores while thus burrowing. Occasionally they grow from the surface of the timber into the air, and then assume their specific forms and characters, and produce their microscopic spores, the analogue of seed in the higher plants.

#### DIFFICULTY OF RECOGNIZING AND DEFINING SPECIES AMONG THE LOWER CRYPTOGAMS AND PROTOZOA.

A vast number of instances, more or less similar to the foregoing, might be cited. From all which we may learn, that it is a work of great labor and difficulty to define truly, in all its phases of transformation, any living species among the lower cryptogams and protozoa. Uncertainty at present rests upon the natural history of infusory animalcules, and of the microscopic fungi and algæ. If from an unknown source, we behold a minute living and moving organism, of a spheroidal form, and without external openings into a cavity, we most likely cannot certainly refer it to any particular class or order of beings. To do this, its further development and transformation must be known; and to define it fully as a living species, we must acquire a knowledge of its successive conditions and appearances in all the stages of its cyclical progress, from the ovum, through transition, sexless forms, to the ultimate condition of a true oviparous being; successive conditions and appearances in general so unlike, as to afford the observer no *prima facie* clue to their specific identity.

CRYPTOGAMS AND PROTOZOA SOMETIMES ENDURE GREAT CHANGES OF HEAT, COLD, AND DROUGHT.

The red-snow, *Protococcus nivalis*, a fresh-water microscopic alga germinates in alpine and northern frigid regions, upon the surface of melting snow, presenting to the traveler sometimes immense fields of a scarlet color. *Ulva thermalis*, another alga, grows in Gastien thermal springs, where the constant temperature is 117° fahr. *Limneus pereger*, a fresh-water shell, inhabits the same thermal springs.

The dry spores of cryptogams, the dry ova of the protozoa, and the dry seeds of many plants, will endure, without fatal harm, the greatest degree of natural or artificial cold, and also a degree of heat much above the boiling point of water.

By rubbing up gamboge with water, a turbid yellow fluid is produced, in which may be discerned with the microscope, innumerable minute spheroidal particles incessantly oscillating backward, forward, and in every direction. This movement continues for many days. Any organized body treated the same way will exhibit similar molecular movements. They can hardly be considered vital movements, and are probably owing in general to reaction from chemical and osmotic forces. These movements cease, apparently, without the power of resumption, when the substance has been heated somewhat above 300° Fahr. It is not easy to determine in some cases, whether the oscillation of microscopic particles is due to these physical forces, or to the manifestation of vitality. Spallanzani long since observed, that some of the infusory animalcules can be repeatedly dried down to a condition of passive vitality, and then revived by the application of water. His experiments upon the *Rotifer redivivus* are most remarkable. He says it may be deprived of water, and reduced to perfect dryness, so that all the functions of life may be completely suspended, yet without the destruction of the vital principle; and that after remaining for years in a dry state, as an atom of dust, it may be revived in a few minutes, by being supplied with water containing a few particles of sand. This alternate suspension and restoration of life is said to have been effected on the same animalcule as many as seventeen successive times.

This is analogous to the preservation of dormant vitality by dry vegetable seeds, that in some authentic cases has continued for nearly forty years. I have myself made some experiments upon dif-

ferent species of animalcules, closely resembling the Rotifer redivivus, and upon other kinds, but without the remarkable results reported by Spallanzani. When I dried them thoroughly I failed to revive them; but leaving a little moisture, they would remain dormant, and would readily revive upon supplying water.

CRYPTOGAMS, PROTOZOA, ETC., MULTIPLY RAPIDLY.

The wonderful rapidity with which some of the cryptogams and protozoa have been observed to increase and multiply their numbers, is without any parallel in the higher departments of organic life. This multiplication of individuals is mostly due to spontaneous division, and to gemmiparous offsets, analogous to what we observe in many well known plants. At comparatively remote intervals, true ova are produced by the sexual fecundation of germs, thus giving origin to new and true generations of individuals, which afterwards go on to multiply beyond ready belief, by the vegetable process of fission and gemmation just alluded to.

Among insects, the different species of Aphis or plant lice are noted for their almost limitless increase in numbers by gemmation. From a single Aphis hatched from a fecundated egg, eleven successive viviparous sexless generations have been observed to follow, in the course of a single season. The rate of increase is so rapid, that at the fifth generation, should they all survive, the number of individual Aphides would amount to six thousand millions.

Algae, fungi and animalcules have occasionally been seen to multiply and increase in a still more incredible ratio. The *Vorticella ramosa*, an infusory animalcule, may be cited as affording an instance of seemingly precocious generation. Individuals a few hours old are said to be sufficiently mature to produce offspring, which in turn become parents to a multiplied progeny within a few hours afterwards.

HOW PARASITIC GERMS AND ZYMOTIC AGENTS MAY GAIN ACCESS TO THE INNER TISSUES AND FLUIDS: SHOWN BY THE WAY IN WHICH AMOEBA FEEDS.

The *Amœba princeps* (*Proteus diffluens* of Muller), occurring in all stagnant waters and fermented vegetable infusions, and therefore readily inspected by any one having a microscope, may be profitably studied, as throwing light on a hitherto obscure subject in physiology and pathology. The *Amœba* is one of several genera, including *Actinophrys*, *Arcellina*, *Difflugia*, *Pamphagus*, etc., making up an obscure

class of minute beings, the rhizopod infusoria. They are either or neither, animals or plants. The Amœba is a mere animate, moving, microscopic, shapeless bag, more or less filled with free or loose contents. It has no external opening, and yet it feeds on microscopically visible particles, diatoms, spores, and desmids. Meeting with a savory diatom on which it desires to feed, the Amœba, with its plastic bag, invests the diatom, and mechanically forces it within; extemporizing for the occasion an opening, which almost immediately heals. After digestion, the effete substance is in like manner cast forth, through any portion of the bag, indifferently, the passage way healing as before. In 1855, two European savants, Messrs. Dusche and Molleschott, if I remember aright, carefully observed, that although the absorbent villi of the intestines in the higher animals, including man, are enveloped in a continuous and apparently impervious membrane, normally permeable only to perfect fluids by diosmotic action, yet that solid particles, visible by the microscope, do nevertheless find entrance into the villi, by amœba-like perforations made at the time, and closing immediately afterwards.

Thus may we in a measure comprehend how the germs of parasites, the ova of animalcules, and the spores of cryptogams, gain access to the inner tissues and fluids of the human body, through mucous and other surfaces, that otherwise would present an impassable barrier.

#### ESTABLISHED INSTANCES OF DISEASE CAUSED BY PARASITISM.

The number of instances in which disease in living animals and plants has, up to this time, been demonstrated beyond cavil, to be the effect of parasitism, is much too great for ready enumeration. The different kinds of mould, blight and mildew, occasionally cutting off or lessening crops of cereal grains, fruits and esculent roots, are for the most part easily identified as obscure species of fungi. Different diseases affecting human hair, producing scald head, plica polonica, etc., have been clearly referred to the parasitic aggressions of obscure fungi belonging to the genera *Achorion*, *Microsporium*, etc. Aphthous sores in the human mouth, and many specific skin diseases, are in like manner produced by parasitic fungi. In all these cases the harm is done by the fungi in the myceloid condition. Catterpillers, moths and other insect larvæ, are sometimes seen to sicken and die from the growth of the mycelium of fungi within their tissues.

To demonstrate the presence of this mycelium, requires careful microscopic dissection. From the dead larva, the fungus may send stipes into the air, upon which will be produced a form of spore cases, with all the characters of shape, color and structure, by means of which the species is determinable and definable. The muscardine affecting the silk-worm, and the *Sphaeria* growing within and from the *Hepialus virescens*, are well known cases in exemplification. *Sarcina ventriculi* appears under the microscope like piles of cotton bales upon the New Orleans levee. It is an algoid vegetable, sometimes domiciliating itself in the human stomach, where it may multiply inordinately, giving rise to a distressing kind of chronic dyspepsia. The itch, a cutaneous disease to which the human race is liable, is produced by the burrowing of a minute insect (*Sarcoptes scabiei*) beneath the surface of the skin. About fifty different species of parasitic animals, worms and insects are known to inhabit man, internally and externally; and many of them, when existing in extraordinary numbers, become the immediate cause of disease. Parasitic animals, mostly the larvæ of insects, do also sometimes so affect plants, as to produce disease and death.

#### VIBRIO BICEPS, CONNECTED IN SOME WAY WITH CHOLERA.

On the 28th February, 1850 (vide New Orleans Medical and Surgical Journal, vol. vi., p. 793), I examined with the aid of a fine Spencer, one-eighteenth inch objective, some turbid, watery alvine evacuation, from a cholera patient in the Charity Hospital. It contained a diversity of living organisms, and among them, but more numerous, beyond numerical comparison, a most active little worm, about one-four-thousandth of an inch long, by one-forty-thousandth of an inch in thickness along the middle, with both ends alike rounded, and enlarged to one-twenty-five-thousandth of an inch in diameter. I estimated that there were nearly four thousand millions of these beings in every drop of the liquid; allowing two hundred and fifty drops to the cubic inch, they would move either by a series of tumblings over and over, or by serpentine swimming, apparently by lateral flexions of the body. Active and animal-like as they are, they should probably rank with algæ, being rather plants than animals, and may I think be referred to the genus *Vibrio*. Supposing this species undescribed except by myself, I venture to call it *Vibrio biceps*.

I thought it probable that the development of the *Vibrio biceps* might be specifically connected with cholera, either as a cause or effect; until after repeated examinations of the faecal discharges of persons in health, during the early part of March, 1850, while cholera was prevailing, I was induced to alter my opinion. I found myriads of the active *Vibrio biceps* in every examination; and although some days later I discovered less of them, I still supposed they existed normally in human faeces, and could have no special connection with cholera. I found them now and then during the month of March, in Mississippi water, coming into my laboratory from the river, through the water works, and therefore set them down as a very common sort of microscopic being. The cholera left the land, and I gave no further attention to the subject. Within a year past I have be-thought me to reëxamine human faeces, with the view to study the *Vibrio biceps* and other organisms more carefully. Judge of my astonishment, when after repeated and varied trials, I entirely failed to find any such organism as *Vibrio biceps* present. I cannot now find it in Mississippi water.

I now revert to my first conclusion of March, 1850, *Vibrio biceps* and cholera have left the country together. *Vibrio biceps* was connected with cholera, either as cause, or effect, or concurrent product. When its subtile invisible spores poisoned the air of New Orleans, they were perhaps inhaled or absorbed by all. These spores must have developed and multiplied, more or less, in the systems of all who were exposed to them. Why some should be taken down sick, and others not, it is not my purpose to explain. That during the prevalence of cholera in the Mississippi valley, the *Vibrio biceps* may be discovered in every teaspoonful of water flowing past New Orleans need not be marvelled at, considering the estimate of numbers in a drop before given, and the total amount of steamboat passengers that daily contribute their dejections to the turbid current of the Mississippi.

#### SPECIAL ORGANISMS DISCOVERABLE IN THE BLACK VOMIT OF YELLOW FEVER.

In the black vomit of yellow-fever patients, I have discovered no less than seventeen varieties or species of parasitic organisms, so minute as to require the high powers of a good microscope to make them visible. I think they may all be referred to the natural orders algæ and fungi. Only one of these organic forms appeared to be



special and peculiar to yellow fever (Vide Microscopic Observations pertaining to yellow fever, with four plates, by the author, October, 1854. Only one hundred copies were printed for distribution). And this one I found invariably present, with a degree of abundance vastly preponderating over the others. It consists of minute spherules of equal size, and as closely impacted together as is possible without altering their form; each spherule measures in diameter near one-fifty-thousandth of an inch. Now and then you meet with clumps of them, in which the spherules are all smaller; and occasionally with clumps in which they are all larger, never exceeding in diameter one-twenty-thousandth of an inch. Very frequently the epithelial cells of the stomach are packed absolutely full of them, the investing membrane of the epithelial cell remaining entire, and sometimes, though not generally, still exhibiting its nucleus. In these cases the spherules seem to be truly parasitic within the epithelial cell.

To prosecute such researches with success, so that reliable deductions can be made, as to whether certain appearances are due to parasitic growth or not, requires long experience and great aptitude in the use of the best microscopes of high magnifying powers, and a thorough knowledge of the normal and pathological histology of the structure under examination.

In my examinations I used an objective one-sixteenth of an inch focus, one of the finest ever made by Spencer. What connection these minute and simple organisms, two thousand times smaller than human blood corpuscles, in cubic dimensions, have with yellow fever, it is at present impossible to determine. They may be the gemmæ or buds, or possibly the myceloid development of some still more subtle parasitic organism, which in other less easily observable forms, vitiates the tissues or more likely the blood itself.

#### OCCASIONAL PREDOMINANCE OF INSECT RACES ANALOGOUS TO EPIDEMICS.

Our means of reasoning on these subjects by analogy, are amply reinforced by taking into account the temporary predominance of certain insect races, which every one has repeatedly observed, and of which there are many striking cases on record. The occasional cutting off of the cotton crop, over wide districts of country, by the aggression of numberless larval worms or caterpillars, cannot have escaped the notice of any one residing in the Southern United States. I think it was in the spring of 1822, that for a mile or two on both

sides of the Chenango river in New York, all the sugar maple trees abounding in that region were destroyed in the course of two or three weeks by the voracity of countless myriads of a large caterpillar, which had never been noticed there before, nor that I am aware of, since. In the spring of 1833 the leaves of the buckeye (*Æsculus ohioensis*) were infested and devoured to an incredible extent in Franklin county, Ohio, by the larvæ of a smaller yellow moth. In the summer of 1858, in Pike County, Mississippi, the persimmon trees (*Diospyros virginiana*), in like manner, generally suffered from parasitic caterpillars. Instances like these might be adduced without limit, all going to confirm the general conclusion, that such parasitic aggressions constitute real epiphytic diseases, analogous to epidemic diseases among men. No thinking mind, turned to the subject, will fail to discern a close analogy between the temporary prevalence and seemingly anomalous succession of epidemic diseases, and the occasional appearance, in vast and overwhelming numbers, of these races of hungry insects.

THE CHARACTER OF SPECIAL ZYMOTIC DISEASES, AND THE HYGIENIC INFLUENCE OF THE ATMOSPHERE; AS WELL AS THE TYPES OF ALL LIVING RACES BECOME MORE OR LESS CHANGED BY LAPSE OF TIME.

A great law seems to pervade and control all nature, as we rise above the lifeless aggregate, occult, indeed, and perhaps incomprehensible in its cause, but not the less obvious in its effects—the degeneration of living races. One race or variety will flourish and endure, perhaps through a long series of generations: at length it begins to degenerate, and is finally succeeded by other kindred off-set races or varieties, which in turn have their day, to be in like manner replaced by their successors. The mastodons, plesiosauri, and gigantic cycadææ of very ancient times, are now unknown upon the earth. They are extinct, and beings of modified structure and different habits have succeeded them. Within an ordinary life-time, illustrations of this law are presented for our contemplation, in the limited duration and decline of varieties of fruits and esculent roots. From the influence of this grand custom, even man himself is not exempt. The Indian is fast disappearing from the forests and plains of America, and where stood his rude wigwam, or where he made war upon the beasts of the wilderness, his successor, the white man,

builds his house, or taxes the fertile soil for the means of wealth and comfort.

Bearing in mind the modification which all living races are prone to undergo, by the lapse of time, and by the influence of altered circumstances; and also bearing in mind the brief hours sufficient to give full maturity to the minuter animalcules, algæ and fungi, revealed by the microscope, we can understand how it is possible that the living cause of an epidemic may not remain identically the same after an interval of years. Most recurring epidemics, nominally and nearly the same, are apt to assume somewhat different aspects at each return. At one time the disease produced will be quite mild, at another attended with great mortality, and certain trains of morbid symptoms appear in some years which are not observable in others. The air of the atmosphere which we inhale at every breath, is never devoid of invisible spores, buds and germs, possessed at least of passive vitality. The preponderance of organisms charging the air of one country, may differ in kind from what would be found in another; and thus a specific atmospheric influence, independent of the chemical composition of the air, which does not vary much; and independent of temperature, wind, drought and moisture, might be exercised upon human beings, predisposing them, as the expression is, to certain maladies. In the same region, after the lapse of months, years or ages, the preponderating organisms charging the air may differ in kind, corresponding to which difference the human diathesis may be observed to change. The influence of climate on the preservation of health, and on the production of disease, probably depends therefore more upon the minute organisms commingled with the air, than upon heat and cold, moisture and drought, electrical and barometrical variations, winds, meteoric vapors from above, terrene emanations from beneath, variations in the gaseous composition, and mineral contaminations of the atmosphere; in short, more than upon all proper climatic and physical causes put together.

ATMOSPHERIC AIR SHOWN TO CONTAIN ORGANIZED AND LIVING CORPUSCLES,  
INVISIBLY ADMIXED WITH IT.

To demonstrate the presence of minute organized particles in atmospheric air, is not difficult to accomplish by any one, with the requisite precautions. By means of a bellows, bag, aspirator or other contrivance, carry a small stream of air continuously for an

hour or two, through trebly-distilled, pure water, contained in a phial surrounded with ice. A portion of the water thus treated may be evaporated carefully in a clean watch glass. A light ash-colored stain will be left, which turns nearly black upon being heated short of redness, showing that the stain has been carbonized: or with a more decided effect, place a minute drop of pure oil of vitriol on some part of the ash-colored stain, apply heat, somewhat above the boiling point of water, and an intensely black stain will be produced, showing that organized matter has been carbonized. Heat to full redness, with exposure to the air, and the stains burn away and disappear, leaving an infinitesimal trace of mineral matter; keep the phial quiescent for some days, and a light-colored sediment appears, in which, by the microscope, very minute organized spherules and filaments, along with amorphous agglomerations, not resolvable to vision, may be satisfactorily demonstrated, possessing all the peculiar characters of organized matter, such as becoming the nidus of algaic growths, and finally, if the water be mostly decanted, and the phial closely corked, of entering into putrefaction. In experiments for collecting organized particles from the air, as before described, a much greater yield will be obtained if, before the air is passed into the phial, it be forced through a small metal tube kept heated to redness; because many of these aerial spores seem to repel water, until they are deprived of vitality.

The earliest experiments in this line of research that I have read of, were made by Moscati, a learned Italian, perhaps thirty years ago. He suspended a globular glass vessel filled with ice, over the rice grounds of Tuscany. An abundant deposit of dew took place upon the surface, which, when collected, appeared at first to be pure and limpid water. Little flakes afterwards appeared, "possessed of properties peculiar to animalized matters, and finally, at the end of some days the liquid putrefied completely."

In August, 1834, Mr. Bousingault published an account of similar experiments, with similar and still more satisfactory results, obtained by him upon the air near Cartago, South America. In February, 1836, I made a similar test upon the air of the city pest-house in Cincinnati, said pest-house at the time containing small-pox patients. As a result, I demonstrated the presence of organized matters in the air, in comparative abundance.

I have repeatedly tested the air of New Orleans for the presence of

organized matter, and have never failed to find it. The clearest rain-drop falling directly from the sky upon the cleanest glass surface will, upon evaporation, leave a faint trace of a stain, which can be rendered visible under a magnifier, by a peculiar oblique illumination, thus indicating the presence of non-volatile organized matter. In mid-winter of 1833, in Ohio, I carefully prepared several bottles of water, from clean, recently-fallen snow. They were tightly corked, and kept in the shaded corner of a room, where the water was not liable to be frozen. At the expiration of three or four months I observed that the lower portion of each bottle was traversed by myriads of delicate, dark-colored filaments, bearing a resemblance to some of the fresh-water algæ. Upon removing the cork a most unpleasant odor was exhaled, similar to that of animal putrefaction. The living germs of this singular growth must have come down from the high regions of the atmosphere, imbedded in the crystals of snow. The organized corpuscles admixed with air, are wholly invisible to common observation, on account of their extreme minuteness, and also on account of the extremely minute relative proportion in which they occur. The microscope is not adapted to make them visible, because air is not stable enough, to allow small particles floating in it, to be adjusted in the focus, and kept there. If, into a dark room, a narrow pencil of the sun's rays be admitted, and allowed to fall upon a piece of black cloth, so that the light may be absorbed, still leaving the room, as near as possible, in darkness, then will the path of the rays be made visible by a faint reflection of light, from the spheroidal surfaces of organized corpuscles, individually far too small to be seen.

#### HOW ORGANIZED CORPUSCLES ARE SUSTAINED IN THE AIR WITHOUT FALLING.

Admitting the presence of organized corpuscles in the air, as satisfactorily proven, there are those who would wonder how they could be sustained there without rapidly falling to the earth, considering that their specific gravity must be several hundred times greater than that of the air. When dry they are doubtless much lighter than water ; but, for argument's sake, we will suppose they have the same density.

Bodies of very small dimensions, no matter how dense their texture, encounter a degree of resistance in falling through material media, which essentially retards their velocity. Larger bodies do not experience this resistance in so great a degree, because they present less

surface in proportion to their weight. Very minute bodies, in falling through the atmosphere, soon acquire nearly a maximum or terminal velocity, which they do not exceed until they reach the earth. A spherical drop of water, one-tenth of an inch in diameter, has a maximum velocity of descent, through air near the level of the sea, of about ten and a half feet per second of time ; and by calculation in accordance with established physical principles, it will be found that a spherule of the same density, one twenty-four thousandth of an inch in diameter, would fall two and a half inches in one second. Were the diameter one hundred thousandth of an inch (which would allow of clear definition in a good microscope), it would fall one inch per second ; and were it one millionth of an inch in diameter, its terminal velocity would be one hundredth of an inch per second, which is at the rate of three feet in one hour's time. The ascent of heated air and the vapor of water, conjoined with the ordinary currents of wind, is doubtless sufficient to lift the miasmatic corpuscles into the air, and to sustain them for an indefinite time. Thus can we clearly see that wingless beings of transcendent minuteness may float securely on the subtile air, or be borne on the wings of the wind to remote regions of the earth.

#### BARON LIEBIG'S CHEMICAL HYPOTHESIS DISAPPROVED.

In the operation of miasmatic and contagious poisons in the animal system, Baron Liebig sees a close resemblance to the process of fermentation, which he regards as a phenomenon purely chemical. It so happens that although the main products, in all the different kinds of fermentation, are substances of definite chemical composition, yet the development and multiplication of microscopic living organisms always accompanies the fermentation, and keeps pace with it step by step. Each kind of fermentation is attended by specific organisms. We must therefore look upon the alcohol, the acids, etc., which make their appearance in fermenting solutions, as the concurrent result of chemical and vital forces.

The deservedly high standing of Liebig has induced the majority of physicians to give assent to his views above alluded to ; and by this means they passively entertain a respectable hypothesis, without the trouble of critically examining the grounds on which it rests. The immense diversity in the types of zymotic diseases implies corresponding diversity in the occult causes giving origin to them. This

does not comport with the simplicity of chemical laws. The liability, or rather the proneness, of the types of diseases to vary more or less by lapse of time or change of circumstance, implies a corresponding precedent variation in the occult causes. This does not comport with the never-varying operation of chemical forces.

#### ETIOLOGICAL CLASSIFICATION SUGGESTED.

The hygienic and pathological causes which affect or disturb the functions of health, may be set down under the following six general classes :

1. *Mental and Vital*—where the mind reacts upon the body, and where the vital influence may vary from deficiency to redundancy.

2. *Mechanical*—where the body is affected by ponderable matter acting in mass, as in the case of blows, wounds, compression, expansion, and other forms of mechanical force.

3. *Chemical*—where the tissues or fluids of the living frame are affected by the component molecules of bodies in the fluid condition, reacting chemically ; producing alteration, obstruction or dissolution of the part or parts acted upon.

4. *Diosmotic*—where the vital functions are affected by the diosmotic imbibition, or escape into or from the various organs and parts, of fluids that exert no proper chemical action.

5. *Imponderable*—where the vital functions are affected by heat, light, electricity or magnetism.

6. *Zymotic or Parasitic*—where the vital functions are affected, directly or indirectly, by living parasitic organisms.

To the last class of the foregoing general causes of disease, zymotic or parasitic causes, is referable nearly the whole catalogue of specific, well characterized, though generally recondite maladies that affect the human race.

#### DIFFERENCES IN THE CAUSATION OF CONTAGIOUS, INFECTIOUS AND MIASMATIC DISEASES, HYPOTHETICALLY EXPLAINED.

I will venture to conclude this memoir by showing that it is easy to frame rational conjectures, in accordance with the views advocated and the principles set forth, to account for the remarkably curious differences observable in the modes after which different zymotic diseases are communicated : in other words, to account for the etiological differences of contagions, infections and miasms.

*Contagion.*—The parasitic organism producing a contagious disease, within or upon the living body of man or beast, must be of such nature and habits that its true spores, ova or germs, are produced and perfected within or upon said living body, to that degree, that being introduced or applied to another similar living body, the same disease will be produced. These true spores, ova or germs, are not cast adrift into the atmosphere; and to reproduce the disease in another subject they require special or adventitious transportation, as exemplified by inoculation or actual contact.

*Infection.*—A non-contagious, infectious disease cannot be communicated by the application of any matter taken directly from the subject affected. The parasitic organism producing it is derived immediately from true spores, ova or germs, from without. These develop parasitically, perhaps, into some transmicroscopic myceloid or allotropic form, which may be indefinitely increased and multiplied by the process of fission or gemmation. The grave symptoms of the disease produced may perhaps be more owing to the cast off, dead and putrefying myceloid remains of the parasite, which the vital powers labor to expel from the system, than to its direct aggression in the living state. The excreta from the patient, though perhaps charged with the parasitic organism in one of its allotropic forms, contains no spores, ova or germs, which can immediately propagate the disease. A certain degree of aerial exposure, generally in damp, filthy air, is requisite for the development and perfection of the true spores or ova, which subsequently are commingled with, and vitiate the air, charging it with efficient corpuscles of infection. From air, and in some cases from water, thus poisoned, other subjects may acquire the same disease.

Some maladies are both contagious and infectious. In these cases the true spores or ova are produced upon or within the subject affected, and may either give rise to the same disease by contact, or may commingle with the air, and therein manifest the character of aerial infection.

*Miasms.*—Non-contagious, non-infectious malaria. The cause of malarious diseases may be referred to occult spores, ova or germs, which arise under certain conditions of temperature and season, from the protophyta, or protozoa, accompanying the decay of vegetable matter, mostly from standing pools of water, and their vicinity, and from marshes which, in warm weather, are alter-



nately flooded, and partly or wholly dried. They enter the animal system, and there take on a parasitic growth, without attaining to that degree of development, or that stage of transformation adequate to result in the perfection of spores or ova, either within the body of the subject affected, as in the case of contagion, or from any of his various excreta out of the body, as exemplified by infection. The ova or spores causing the disease, can only be generated in the malarious localities, so that we can form no probable opinion, as to whether the parasitic organism produced, abortively ends its existence within the body of the suffering patient, or whether it may not escape therefrom and still pursue its allotted path in an unknown cycle of allotropic generations.

THE VIRUS OF YELLOW FEVER A MIASM, RARELY INFECTIOUS, NEVER CONTAGIOUS.

Yellow fever, as it appears in New Orleans, is ordinarily a miasmatic disease, neither infectious nor contagious. Yet in 1853, originating, most likely, from freshly-imported virus, its energy was so increased that it became infectious, and was transported to most of the towns within hundreds of miles. It has never been known to become contagious. Similar variations have been observed in other diseases, which ordinarily are neither infectious nor contagious; but which, under certain unknown conditions, do now and then manifest an exaltation of virulent power, rendering them infectious, and even in rare instances probably contagious.

There is reason to believe that yellow fever was not originally indigenous to the delta of the Mississippi; its earliest germs must have been imported from abroad; but once being here, no reason can be shown why it might not winter here, and reappear the following season, as if it were really indigenous. Bearing in mind that all living races long kept through their successive generations, under the same continued circumstances, and under the same influences, tend to degenerate and run out, as is exemplified by the failure of the potato crop in Europe and America of late years; we may confidently anticipate that the yellow fever virus, naturalized with us, and reappearing from year to year, will gradually and surely become less and less malignant, and at last, possibly in ten or fifteen years, so lose its baleful and pestilential features, as to be no longer recognizable and no longer dreaded: *provided* there be, in the meantime, no fresh importation from abroad.

ART. IX.—*Synopsis of the "Report of the Board of Health to the Legislature of the State of Louisiana," for the year 1858.*

To the kindness of Dr. Baldwin, Secretary of the Board of Health, we owe our copy of its Report, which derives its chief interest from the contribution of its President, Dr. Axson.

The baleful effects of our epidemics upon the commerce and prosperity of our city are eloquently described, and quarantine boldly prescribed as the great remedy needed. The epidemic of 1858 is traced unhesitatingly to the Elizabeth Ellen, and the history of this vessel quoted at length from the November number of this Journal. That this vessel should have been infected with the yellow fever poison is regarded as all the more probable from the fact that she had visited St. Thomas (whence the fever is asserted to have been introduced into New Orleans) with a cargo of coal. For "the influence of ships freighted with coal, in concentrating the poison of yellow fever, and of coal in retaining and intensifying it, has been matter of observation" to those who have had ample opportunity to witness the necessary facts. Blair, of Demarara, Paul Candida, physician to the Emperor of Brazil, Dr. Schuyler, and others, testify to this deleterious influence of coal.

Beginning, then, with the importation of the fever by the Elizabeth Ellen, Dr. A. finds some facts which *seemingly* contravene this assumption, viz: the cases of Catharine Maxwell and of the Italian sailors from the Rosalie. Dr. A. confesses that the explanation of these *seemingly* contradictory facts "involves one of the nicest and most difficult problems in the whole domain of medical philosophy." To the elucidation of this perplexing problem seven pages are devoted, which may serve to confirm those already convinced, but fail to adduce either new facts or arguments calculated to satisfy the doubtful or convert the skeptical. From all the facts cited, Dr. Axson concludes that he has "shown that there is not only probable ground for believing that the fever of this year was of imported origin, but even adduced direct testimony, greatly strengthening that position, and converting a probability into a certainty; and having, moreover, demonstrated that the fever, once introduced into a community, under circumstances favorable to its diffusion, can and does spread by virtue of human intercourse, by currents of air, and in other less obvious modes, the question now occurs, why, if quarantine is an effective safeguard against the infection, did it not

prevent its introduction?" To this it is satisfactorily replied, that the leniency of the law (since the amendment of 1858) allows vessels fifteen days at sea, and free from sickness on their arrival at quarantine, to proceed at once to the city wharves. Such was the case with the infected Elizabeth Ellen, and thus legislative tinkering with the quarantine laws is made responsible for the five thousand yellow fever deaths of 1858.

"Half-way measures," says the President, "satisfy nobody, and involve much inconvenience and expense, without any proportioned advantages."

In this all must fully concur; but, after the Legislature has perfected *on their statute book* these measures, it is pertinent to inquire whether most excellent laws now extant, relative to other matters, are so rigidly enforced as to induce Dr. Axson to hope that a perfect system of quarantine laws would share a different fate? Yielding the question of the contagiousness of yellow fever, it remains still to be proved that quarantine can be rendered an efficient means of protecting us.

Most impotent and absurdly inefficient has it proved so far, and it is believed ever must, at least here, where too often notorious violators of the law are among our most popular lawgivers. It would be a safe wager that a quarantine nurse could, undetected, run the gauntlet of the quarantine officials, the municipal police, and the State militia to boot, for a bottle of whisky!

Until our people and institutions have undergone some great change, all quarantine measures must be "half-way measures;" and since these "satisfy nobody," we anticipate the pleasure of welcoming, at no distant day, the worthy President of the Board of Health to the society of the *disaffected*. He is now, however, still a victim of hope, already deferred, and destined to be deluded, and recommends the following amendment to the laws:

"1stly. To prohibit, from and after the first of May, and until the first of November, all vessels arriving from ports where yellow fever habitually prevails, to proceed higher up the river than the quarantine station, except with the sanction of the Board.

"2dly. To store all cargoes coming in such vessels in the United States warehouses, with the privilege to the consignees or owners of them to reship at their option, to go beyond the limits of the State. This, of course, to apply only to what are called susceptible goods, as hides, wool, etc.

"3dly. To compel all vessels thus detained at the station to police their own crews, etc., by muleting any vessel in damages which shall permit the escape from on board of any of the crew.

"4thly. To forbid, by suitable penalties, any towboat towing up such vessels to come to the city for the same period of time, unless they obligate themselves to carry only acclimated crews."

On examining the nature of the cargoes in vessels arriving last year, Dr. Axson says: "There occur but two or three instances of susceptible articles of merchandise, which it would be proper to exclude from reshipment to the city. All the rest, as specie, tobacco, coffee, fruit, etc., if perfectly sound, may be admitted without much risk."

These recommendations have not been adopted by the Legislature, and had they been it is more than probable that a few years' experience would have reduced them into the classification of "half-way measures."

Dr. Axson denounces our city authorities in forcible but well-deserved terms, for the filth and scandalous nuisances which infested the streets and public places during the past summer. These "accumulated nuisances," says he, "spring not from any necessity of situation, not from any unavoidable cause, but from tolerance, from indulgence, from actual connivance on the part of the city government." "Liebig once remarked, that of two countries, with an equal amount of population, we may declare with positive certainty that the wealthiest and most highly civilized is that which consumes the greatest amount of soap. 'This consumption,' says Liebig, 'does not subserve sensual gratification, nor depend on fashion, but upon the feeling of the beauty, comfort and welfare attendant upon cleanliness; and a regard to this feeling is coincident with wealth and civilization.' Were such a test applied to our city, as year by year our city fathers tolerate the accumulated heaps of filth in our highways and byways, it would confound the wisest of them to fix its precise position on the chart of civilization!"

These existing nuisances are regarded as "slow rather than inert poisons," and as greatly contributing to "a constant decrease in the expectations of life, and a gradual augmentation of sickness and death." It is proposed to remedy them by—1st, An efficient and comprehensive system of levee draining, from the swamp into the lake. 2d, A series of perfectly graded drains or sewers, by which

the fluid refuse of every yard, and certainly every street, shall flow into the swamp, or into reservoirs beyond the city limits, from which they can be drained into the lake. As subsidiary to this, and second to it only in importance, is the expedient of paving, as far and fast as circumstances will warrant, every street, to render them more cleanly. 3d, A more plentiful supply of water, to dilute and carry off the residue of any refuse, and to allow of sufficient washing of streets, yards, etc. 4th, An active corps of scavengers, who shall be something more than political place-holders or contractors, to remove regularly the rubbish and impurities which water will not or cannot carry away, and fit receptacles (something beside the gutters) for such rubbish, until carried away. 5th, A well-guarded system of regulations to prevent, and to remedy when prevention is impracticable, the overcrowding of the lodging houses of the poor."

We fervently pray that these reforms proposed by Dr. Axson may be accomplished before the hairs on the heads of his children are whiter than snow!

For the purpose of better protecting our citizens from small pox, the Board have, during the last year, expended a small amount of money every month, to purchase fresh and reliable vaccine, for "gratuitous distribution among the planters and citizens of the State."

In conclusion the Board renews its suggestion to the State Legislature, "of sending a request to the Smithsonian Institution, through their Delegates in Congress, for the supply of their apparatus used in making meteorological records."

The expenses of the quarantine in 1858 were about \$32,000, more than half of which was for salaries of officers and wages of employees. The receipts for 1858 were about \$34,000, which were collected from one hundred vessels for quarantine fees.

CHAILLÉ.

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ART. X.—*Report of the Board of Administrators of the Charity Hospital of New Orleans, to the Legislature of Louisiana.*

DR. W. G. AUSTIN, Treasurer of the Board, has our thanks for this document, which consists of financial and medical statistics, with a brief and business-like report from J. N. Lea, Vice-President. It is

stated that in consequence of the epidemic, 2,440 more patients were under treatment in 1858 than the year previous, the total for 1858 having been 11,337. But the actual current expenses for the year 1858, viz.: \$73,403 87 were less than in 1857 by \$2,400. The receipts, however, were far less than anticipated. The chief sources of revenue are from taxes on passengers, theatres, balls, concerts, etc., which have yielded only about \$30,000, instead of \$45,000, as estimated. The various receipts, together with the \$22,000 appropriation for the last year, will leave the hospital on February 1st, 1859, \$21,000 in debt. To meet this deficiency, as also to maintain the institution for 1859, "an appropriation of at least \$40,000 will be required." That the hospital may not "become a constant and heavy burden on the State Treasury," various suggestions are made to "create for it a permanent and reliable income by taxation." Among the expenses for 1858, we find over \$16,000 paid to officers, nurses, etc.; \$2,250 to the Sisters of Charity; for drugs nearly \$5,000; and some \$32,000 for meat, bread, milk, groceries, and marketing.

Judge Lea closes his report as follows: "We cannot commend too highly the conduct of the respective officers of the institution, and take pleasure in bearing testimony to the effective and regular performance of their duties. The internal economy is entrusted to the Sisters of Charity; their untiring devotion to suffering humanity is too well known to require commendation. We cannot, however, refrain from saying, that it is greatly to the management of the domestic affairs of the institution by these ladies, that the Charity Hospital has become the pride and boast of the State."

The statistics of the hospital for 1858 will be found elsewhere in this number.

CHAILLÉ.

ART. XI.—*Report of the Board of Administrators of the Insane Asylum at Jackson, to the Legislature of Louisiana.*

THIS Institution, says the Board, is now in a most prosperous condition, and its healthy location has tended greatly to the welfare of its inmates. So far its revenues have been derived almost exclusively from the State Treasury; last year, however, nearly \$4,000 were

received from pay-patients. The expenses for 1858 were over \$26,000, and the Board demands an appropriation for the year 1859 of \$21,000 for subsistence, and \$5,000 for repairs.

The administrators say, that "the appropriation for which we ask is based upon a simple calculation, made from the number of patients, and the cost of each to the State. That cost, by comparison, will be found to be much less than in other States." Elsewhere in the report it is asserted that "a statement has been made from the reports of fifteen similar institutions in sister States, for the present year, from which it appears that the average cost of supporting each patient is \$172 63 per annum."

The legislature is again urged to make such appropriations as may suffice to complete the out-buildings of the Asylum, in order that it may possess all that is requisite to promote the safety and comfort of the patients, and to induce the better classes to patronize it.

This Journal has already used its influence to persuade our legislature of the importance of carrying into execution these views. We regret to find that its members turn as deaf a ear to the solicitations of their own officers as to our representations. Legislators and constituents are both famous for grandiloquent *speeches* of "suffering humanity," "afflicted fellow-creatures," *et id omne genus ad nauseam*, but they leave us often to regret that the word is not father to the deed.

Dr. Barkdull, the Superintendent and Physician, assures the legislature "that, if the necessary means can even now be obtained to fully complete this institution within, say some two years, making it what it should and must become in order to insure the greatest success, we will pledge our reputation as a prophet, that (since the pay-patients' list under present arrangements has been increased, within less than two years, from *five to twenty-eight*) by the expiration of five years from this time, the institution can have from this source an annual fund of not less than *fifteen thousand dollars*."

Dr. Barkdull calls attention to the abuse to which the Insane Asylum is subject in being partially a receptacle for idiots as well as of "certain aged and infirm persons known as paupers." For each of these classes he correctly thinks it is the duty of the State to make some separate provision. Attention is drawn to another matter of immediate interest to our judges and juries, calculated to inspire them with that gratification which is derived from a sense of duty well per-

formed. Says Dr. B.: "during the past year, owing to the 'tricks of the law,' we have admitted no less than five criminals who had been indicted for the commission of various crimes of a grave character, acquitted on a plea of insanity, and ordered to the Insane Asylum. This practice is becoming a source of great annoyance to this place, which calls loudly for redress, and if continued, must in time prove a grievance of no small magnitude, in having these *scoundrels* turned loose among our inmates, with all their corrupting and dangerous propensities." In reference to this same subject the administrators assert, that "several criminals, acquitted on the ground of insanity, have been sent to the institution, and after their arrival here have improved and recovered with amazing celerity." Very gratifying this to you, Messrs. judges and juries, and to us the people!

By another "special report" from the "Committee on Charitable Institutions," we learn that they were instructed by the Senate to inquire into the truth of a report, "that the Insane Asylum at Jackson is improperly conducted." The result of the investigations of this committee will prove of interest to those of our readers who may at some time be called on for information and advice, in reference to this asylum.

"Your committee addressed themselves particularly to the administration and management of said institution, together with the treatment of the patients; and after having made as patient, full and fair an investigation as it was possible for them to make with the facilities afforded them, they unanimously came to the conclusion, first, that the administration, financially, was honest and economical. Second, that the buildings, rooms, bedding, clothing and food for the patients were comfortable, clean, healthy and plentiful. Third, that the treatment of the worthy and gentlemanly Superintendent and resident Physician, Dr. Barkdull, and that of his amiable and accomplished wife, who acts as Matron (though not officially known as such) towards the patients of every description, is kind and humane, and that they are constant in the faithful discharge of their duties, and untiring in their efforts to ameliorate the condition of the unfortunate inmates, for whose particular welfare the Institution was established.

"Your committee unhesitatingly state that, in their opinion, the superintendent and his wife, who acts as matron, faithfully discharge their respective duties, and that there is no just cause of complaint at their treatment of the inmates.

"Your committee particularly directed their investigation also to the treatment of and to the several amounts paid by that unfortunate class of patients denominated 'pay-patients,' and they are pleased to be able to state that in every instance they found them well cared



for, and occupying neat and comfortable rooms, and well attended by servants—it being the desire and object of the administrators and superintendent, so far as your committee could judge, to provide for these patients, so far as may be done in their present condition, according to the position they held and the circumstances by which they were surrounded in their brighter and happier days. Their food is prepared with that of the superintendent and his family, and in every case carried to and served in the room of each patient, only in cases where the superintendent judges that it would be best for their own condition that they should take their meals socially, and at the same table.

“The books of the institution show that at this time there are about twenty-eight of these patients, and that the rates of charges vary, according to the ability of each to pay, from ten to forty dollars per month—much the larger portion, say about three-fourths, paying twenty-five dollars per month. In no instance is a patient from another State admitted for less than twenty-five dollars per month. The owners of slaves, in every instance, are charged twelve dollars and fifty cents per month.

“Solitary confinement, the use of the strait jacket and corporeal punishment have been almost entirely abandoned, and a course of uniform kindness, gentleness and humanity adopted in their stead with great success.

“Your committee are unanimously of the opinion that most of the dissatisfaction and muttering manifested and heard against the present administration and management of the institution grew out of the action of a majority of the Board of Administrators, in raising the salary of the present superintendent and resident physician from the sum of fifteen hundred dollars to that of three thousand dollars.

“Your committee frankly admits that the amount of salaries paid to officers of the institution is a subject over which they have no legitimate control ; yet they, as tax payers and citizens of the State, claim the right to state that they fully concur in the opinion expressed by the majority of the Board of Administrators, ‘that the sum of fifteen hundred dollars is wholly inadequate as a compensation for the services of a superintendent and resident physician, who is worthy and well qualified for the unpleasant and arduous duties which the acceptance of such a trust imposes upon him. He should surely be fairly remunerated for the sacrifices that he makes for the duties that he performs.’ ”

In Dr. Barkdull’s report will be found full returns of the medical statistics of the asylum, from which we have made some selections, which have been transferred to another page.

CHAILLÉ.

ART. XII. — *Malum Egyptiacum, Cold Plague, Diphtheria, or Black Tongue*: By SAMUEL A. CARTWRIGHT, M. D.

BEFORE the time of Hippocrates, a malignant disease of the throat, called the Egyptian sore throat or plague, was known to the ancients. Its old name indicates its southern origin. It generally makes its first appearance in those localities where charbon and malign pustule have been most observed. The charbon and malign pustule are diseases of southern latitudes. Domestic animals are more obnoxious to charbon and man to the malign pustule ; or rather, the charbon in cattle precedes the appearance of the disease called malign pustule in the human species. This latter disease always occurs on some exposed part of the body, as the face, arms, neck and hands, and is supposed to be a veritable inoculation with matter from an animal which has died of charbon. Even the hair or skin of such an animal continues to retain its poisonous properties for an indefinite period of time. Charbon is most common in hot, humid localities, near the sea coast, or in rainy seasons and in places where the crops have failed or the productions of the earth damaged by natural or artificial causes. It is well known that the persons most liable to be afflicted with malign pustule are the herdsmen, tawers, tanners, retail grocers, butchers, veterinaries, shepherds, hostlers, cooks, and all those whose avocations bring them in contact with the lower animals while living, or the flesh and skins when dead. This is the class of persons most liable to be attacked with a malignant disease of the throat, proving rapidly fatal, bearing a variety of names, yet differing so essentially from every disease whose name it bears in nosological classifications as to have created a necessity for new names to identify it and to distinguish it from diseases bearing the same name in the locality in which it occurs. Thus, it has been called by the Faculty *cynanche maligna*, *angina gangrenosa*, *scarlatina maligna*, *pneumonia typhodes*, and such names, while in common language it has been called *spotted fever* in New England, *putrid sore throat* in the West Indies, *cold plague*, *typhoid pneumonia*, *malignant scarlet fever* and *black tongue* in the Middle and Southern States. Physicians, perceiving that it is not properly a *cynanche* or an *angina*, because in many cases the lesions of the throat are not sufficient to account for its rapid fatality ; not a *scarlet fever*, because, unlike scarlet fever, it is not contagious ; not a *pneumonia*, because the lungs are generally but little affected, have lately given it the new name, *diphtheria* or *diphtheritis*. It is so named

because a skin, membrane, web or aphthous crust, of a whitish, greyish, red or black color is perceived in some part of the throat—supposed to be the result of exudation matter from inflammation. But this exudation matter or false membranous substance is, in most cases, entirely insufficient to account for the speedy death by apparent strangulation. Children with croup often have ten times as much, and live for many days, and then recover, while this disease proves fatal in less than twenty-four hours, and at no time is the crust or membranous exudation sufficient to cause death by mechanical obstruction of the air passages. There would be no objection to the new name, diphtheria, if it did not lead to an erroneous theory and practice. It presupposes that there is an inflammation pouring out exudation matter, which forms the aphthous crust or membranous substance. Hence it leads to the indication that the inflammation ought to be subdued and the crust expelled by vomiting. A fatal error in practice. The name diphtheria, however, suggests the propriety of local applications to the fauces, and has been of some service in bringing the nitrate of silver (a useful remedy, though not the best of its class) to bear upon the disease. While it was regarded as a cynanche, an angina, a pneumonia or scarlet fever, the inflammation, supposed to exist in the throat, was further supposed to be owing to constitutional causes, and hence general remedies were relied on to subdue it, to the almost entire neglect of local applications. The practice was so unsuccessful that no disease in the catalogue has brought so much opprobrium on Medicine, as a science, as this has. It has done more than all other complaints combined to encourage empiricism; not because quacks of any school or no school are more successful in its treatment than the regular doctors, but because the latter are *unsuccessful*. It killed Gen. Washington in less than twenty-four hours from its onset, notwithstanding four copious bloodlettings, an abundance of calomel and tartar emetic, and many blistering plasters.

For more than a half century it has been bringing not only the regular scientific physicians of the United States into disrepute with the public, but the chief implements they use in their warfare against other diseases. Empiricism, in all its forms, taking advantage of the popular prejudice thus created against the lancet, calomel, tartar emetic and blisters, has heated the prejudice against all such remedial agents to so high a degree as to have driven near a moiety of the

population of the United States into the arms of the ignorant and to nostrums ; not because the popular mind prefers ignorance to knowledge, but because it fears the bugbears, *calomel* and *the lancet*, which have *not been* driven out of the sanctuary of knowledge as they have been driven out of the entire domains of empiricism.

Long ago I perceived, or thought I perceived, that the want of success in the treatment of this and some other diseases of the South was owing to the regular physicians not practising the regular system of medical science, built on the accumulated experience of all ages and countries for a period exceeding two thousand years, but a *reformed system* imported into the United States from Edinburgh. The science of medicine underwent a reform in Edinburgh about a century and a half ago, to adapt it to the diseases observed in a little corner of the north of Europe—a corner differing essentially in its cosmic meteorological and climatic conditions from any and from all of those countries from which all prior medical experience had been drawn. Hence a new nomenclature was there made to embrace the new order of diseases observed, new theories invented, and a new practice adopted to suit the diseases in that little hyperborean corner of the globe. This Young Physic or Reformed Medicine is entirely out of place when imported into the United States—a vast confederacy of empires, differing in almost every respect from the petty kingdoms in the north-west of Europe, but agreeing pretty closely in climate with those renowned countries in Europe, Asia and Africa from which the great mass of medical experience has been derived. That experience had been drawn from localities where bilious fevers prevail, similar to those observed in the United States ; but such fevers not being met with in the north-west of Europe, were even excluded from the reformed nomenclature of Edinburgh. American physicians saw bilious fever every day, but it was not in their books, and they found it very unmanageable when treated like a hyperborean synocha or synochus, for the plain reason that it is a different disease. They often met with pneumonia and pleurisy, and found them readily to yield to the antiphlogistic treatment imported from the north of Europe. It was generally, however, in the cold season of the year, and in elevated districts, that this treatment succeeded so well. But very often, in low, damp, miasmatic regions, they met with a disease which they found very difficult to squeeze into Cullen's Nosology. It was sometimes very much like pneumonia, and they called it pneu-

monia, and went to treating it like pneumonia, but their patients died very often in less than twenty-four hours. They then concluded it must be typhoid pneumonia, or *febris petechialis*, like that observed in the north of Ireland; but, unlike that disease, their patients died almost before they got through auscultating them, to find out what point in the lungs the mischief lay; others died in the same house or neighborhood, at the same time, in less than eighteen hours, without any pneumonic symptoms at all, but from apparent strangulation from a sore throat, with mouth open and tongue out. They changed the name to cynanche maligna, angina gangrenosa, scarlatina maligna or erysypelas phlegmonoides. The common people called it *black tongue*, cold plague, putrid sore throat. Physicians who had looked into French authors recognized the disease, which prevailed in various parts of Virginia, Maryland and Pennsylvania in 1813 and 1814, as the veritable *morbus strangulatorius*, which ravaged Montpelier in 1760, Rome in A. D. 380, Spain in 1600, and Naples in 1619, under the name of *malum Egyptiicum*. It appears in low, unhealthy districts, after there has been great mortality among domestic animals from eating mouldy food, damaged grain, etc.

In 1805 the crops were damaged by early frosts in the New England States, and a portion of the State of New York—not only that year, but for two or three succeeding years. During this period, we learn from Dr. Elisha North, and many other medical authors and writers, that an epizooty was very common among the domestic animals in a number of localities in the North-eastern States. Early in the spring of 1806, the disease under consideration made its first appearance at the small village of Medfield, twenty miles south-west of Boston, near the Charles and Stop rivers. It almost simultaneously appeared in various other localities, and for three or four years spread terror and destruction throughout the New England States. Its epizootic origin was not suspected. At that early day it was not known that low down, near the confines of organic nature, certain corpuscles or zymotic germs exist, possessing peculiar poisonous properties, and endowed with a sufficient amount of vitality, to propagate and extend themselves indefinitely, under a conjunction of circumstances, favorable to the development of their vitality.

It was not known that poisonous fungi, as these germs might be called, could exist in certain localities in America, and not in the north of Europe. Nor was it known, at that time, that plants and

animals have their peculiar zones, and that different localities stamp their diseases with a peculiar type. Then, as now, American diseases were named and treated by a large proportion of physicians of the United States precisely as if they belonged to the same type as those observed in Great Britain. Treating the negro as if he belonged to the same type as the white man invariably injures or destroys him. This fact has begun, of late years, to be seen and appreciated. But the irrationality of treating such diseases as black tongue and milk sickness, by formulas derived from British and French hospitals, where such complaints are unknown, is scarcely yet perceived in the North-western and North-eastern States of the Union. When the disease under consideration appeared in New England, the text-books were ransacked to find its name and proper treatment. It was doubtful whether to place it among the phlegmasiæ, exanthemata, or fevers. Dr. North called it *typhus petechialis malignus novus*. He acknowledged "it partook of the nature of three or four different diseases, and seemed to be a compound of cynanche maligna, scarlatina anginosa, the plague of Egypt, and the typhus fever of Great Britain." It was generally known as the *spotted fever*. Its pathognomonic symptoms were soreness of the throat, fœtid breath, sudden and great prostration of strength, weak pulse, laborious and accelerated respiration, cold surface, pain in the head, anxiety and oppression about the præcordia, often violent purging, and terminating in death, from apparent strangulation, in from eight to twenty four hours, or in recovery after a more or less protracted illness. Under all its various names—cold plague, typhoid pneumonia, phlegmonous erysipelas, putrid sore throat, malignant scarlet fever, black thrush or black tongue—the above mentioned symptoms, observed in New England, characterize the disease in all other places down to the present time.

Half a century ago it appeared in the parish of Wintonbury, Hartford, Connecticut, and killed four of the Shepherd family; all dying in from eight to twenty-one hours after the commencement of the disease. This aroused the physicians, and they had a meeting, to collect from one another the result of their experience. But it amounted to nothing, as they all looked at the disease through Edinburgh medical books. After the meeting three adult daughters of Jonathan Bull were attacked, and died in less than twenty-four hours. It next appeared in the family of the Postmaster of Hartford, John Dodd, and killed him and his wife and four children in a very short time.

I have just received a letter\* informing me that the disease has appeared in the parish of Concordia, in this present month of March, and has killed eighteen negroes on one plantation, notwithstanding the efforts of homœopathy to prevent it—all dying from twelve to twenty-four hours from the commencement of the attack, with symptoms, as described to me, exactly similar to the recorded symptoms of the disease as it occurred in New England upwards of fifty years ago. I likewise hear of it in Mississippi, Alabama, South Carolina and Georgia.

I had written thus far when Dr. S. S. Head, a sugar planter on Bayou Lafourche, called on me. I told him I was writing an article on Black Tongue, called also typhoid pneumonia, putrid sore throat, malignant scarlatina, or phlegmonous erysipelas, and that I regarded it as the same disease as the charbon or malignant pustule, which is common enough among the cattle, as well as among the inhabitants of his section of Louisiana. I called to his recollection a lady of his neighborhood, who left home with a pustule on her arm, but otherwise in apparent health, to come to New Orleans, but died before she got here. She was suddenly taken with all the symptoms of typhoid pneumonia, and died in a few hours. The doctor told me of two horses of his, each of which had a pustule on the shoulder, which did not seem to incommode them much; but in the course of two or three days both horses fell down, and died in great agony in a very few hours. At length he observed a similar pustule on the arm of one of his negro men, who was strong and healthy, and did not seem to regard it as anything but a common boil or sore, looking very much like the vaccine pustule. A few days afterwards this man was attacked with malignant symptoms, and died in a few hours.

In February, 1832, I was called to the plantation of the Rev. Benjamin Chase, about ten miles east of Natchez, and found about fifteen negroes with malign pustule. Three or four had died before I got there. Four others were apparently in the jaws of death. Skin cold,

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\* (Extract from the letter above mentioned, dated March 18th, 1859.) "There has existed for some time, at Mr. A. V. Davis's plantations, in Concordia, a disease which has been very fatal among his negroes. They are attacked with sore throat and fever, and in some instances die in twelve hours with violent purging. Mr. Davis had lost eighteen negroes when I left home. The disease seemed for some reason more violent in one quarter. Two somewhat similar cases occurred last week, on the second plantation below this. Both proved fatal in a very short time, but I hear they died of suffocation. It is said this disease has existed for some time in Florida, Georgia and South Carolina, where it has been very fatal,

respiration hurried and laborious, pulse feeble, and the whole body exhaling a most offensive cadaverous odor. I cauterized the pustules, and gave repeated doses of capsicum, quinine, camphor, aromatic ammonia, and also sulphuric ether combined with ammonia, camphor and oil of cajuput; mustard foot baths and hot applications to the body and limbs. They all recovered. About ten others had pustules on them, but they were walking about. The others had walked about until some three or four days after the appearance of the pustules, when they were suddenly taken with pain in the head, delirium and prostration of the vital powers. To prevent the occurrence of such untoward symptoms, I cauterized the pustules in the ten, and they suffered no further inconvenience from them. Observing that some of the negroes had pustules on their backs, where I had never seen them before, I inquired of them into the cause. They told me that they had been clearing new ground, burning brush and trees, and had got very hot, and worked with their shirts off, in order to be cool. I ascertained that the carcass of a horse, which had died of charbon, was near the new ground they had been clearing. After I had got through with the negroes, and was ready to return to Natchez, where I then resided, Dr. James Metcalf, who had also been sent for, arrived. Presently Parson Chase's little son came in and showed us a pustule on his arm. Dr. Metcalf cauterized it. He used nitrate of silver. My favorite remedy is the red-hot iron: it acts quicker and more effectually than nitrate of silver, and does not give as much pain; if brought to a white heat, and boldly used, it scarcely gives any pain at all. The pustule itself is insensible, though it gives pain. Col. Chotard has a negro man called the *Mason*, so named because, finding him with all the symptoms of the New England spotted fever upon him (throat free from any disease inside,) but discovering a malign pustule underneath his jaw, on the outside of his throat, I cauterized it three times with a red hot iron. After his recovery he received the *sobriquet* of Mason, supposing he had taken three degrees.

The spotted fever, the cold plague, the black tongue, and most of the cases reported typhoid pneumonia, putrid sore throat, and malignant scarlet fever, are nothing more nor less than a charbon or poisonous pustule inside of the throat, instead of out. Dr. Elisha North, in his "Treatise on the Spotted Fever of New England," says there was one symptom invariably present in every case of the disease, and that symptom was *sore throat*. Sore throat was regarded by Dr. North



and the New England Medical Society as a premonitory symptom, and was published as such. Only one case of spotted fever occurred in which the characteristic aphthæ in the throat was not present : that was the case of Polly Wadhams. After a careful examination no aphthæ could be discovered in her throat ; but Dr. North, however, discovered an aphthous patch on the inside of her lip. (See Medical Repository, vol. 15, page 66, New York, 1812, for history of this case.) Polly did a hard day's work at washing with this aphthous patch on her lip. It was a local disease, like the pustule on the arm of Dr. Head's negro man, until the poison from it gained the circulation ; then both were stricken down with the same constitutional symptoms.

The disease called typhoid pneumonia, or cold plague, so prevalent in the Southern States among negroes, would long since have been disarmed of its terrors if physicians had no ears ; for then they would have used their eyes in discovering the canker in the throat, their sense of touch in ascertaining the temperature, and their olfactories would have detected the cadaverous, putrid odor of the breath and the secretions. I am enthusiastically in favor of tartar emetic in pneumonia, properly so called ; but I never would think of giving it or using the lancet in pneumonia with sore throat, putrid breath, and a cadaverous odor emanating from the whole body.

When the vital powers are utterly prostrated, as they are in the disease called typhoid pneumonia, or cold plague, the patient gasping for breath, and but a few hours to live, unless speedily relieved, there is no time to be lost in vain attempts to ascertain what are called physical signs by auscultation. The patient is not in a condition for those signs to be ascertained by the best adepts in auscultation in anything like a satisfactory manner. The patient is too feeble to sit up or to change his position. Besides, be the physical signs what they may, there are stronger signs demanding the immediate exhibition of quinine, capsicum, camphor, laudanum, diffusible stimulants, the hot mustard foot bath, and the external application of sponges or wads of cotton or cloth wrung out of hot mustard water to the jaws, limbs and head. What is called typhoid pneumonia or cold plague is always preceded by a bad cold ; that is to say, the patient has a catarrh and hoarseness for some days prior to the terrible chill and prostration of the vital powers. He has, anterior to that chill, a dense, whitish, semi-fluid expectoration in small quantities, and a sense of stricture in the chest. His tongue is clean or covered with a whitish

fur. At length he complains of headache, and perhaps vomiting, is dejected in spirits, has cold extremities, and if he has any pain in the side, it is always in the *right*, and he has difficulty in finding the place where the pain is, when suddenly he loses all strength, becomes cold, and labors hard to get his breath, which is highly offensive to the smell. In a word, he is poisoned by some deleterious corpuseles or zymotic germs inherent in low, marshy, sickly districts, which by some means or other have found a nidus in his throat favorable to a development of their vitality and power of propagation. The diphtherous web or aphtha found in the throat in spotted fever, cynanche maligna, typhoid pneumonia, cold plague, or black thrush or black tongue, is *their work*, and not the work of any inflammation from constitutional or any other causes, as is generally supposed. Diphtheria is therefore a misnomer, as it leads to error: the old name, *malum Egyptiicum*, is the best, as it has no theory attached to it to mislead. All such terms as gangrenosa, maligna, scarlatina, putrida, mislead the mind in search of truth. Such names associated with the foul odor suggest the idea of gangrene. I have never seen a gangrenous throat, properly so called, in this disease, in any of its forms. The throat may be apparently but little affected, or the whole fauces may be covered with what appears to be a gangrenous, putrid, sloughing sore. But there is no sore at all under the *molluscum*, as Professor Meigs would call it, which covers the mucous membrane of the throat. That membrane is always found to be whole and entire as soon as the molluscum, which covers it, is killed and removed. The aphthæ or molluscum is as much the work of insects as an ant hill. The cure consists in killing and removing the molluscum, and at the same time counteracting the poison it has thrown into the circulation, precisely as we would counteract the poison of a snake bite after it had entered the circulation. One young woman, mentioned by Dr. North, took a quart of brandy in less than twelve hours, and twenty grains of opium without feeling it, in the spotted fever of New England. Ardent spirits is an antidote, though not the best one, for the bite of the rattlesnake. But before the poison has entered the circulation, nothing more is needed in the *malum Egyptiicum* than direct local applications to destroy it. The aromatic sulphuric acid was used by the New England doctors in spotted fever, more with a view to counteract putridity than to destroy any poisonous substance in the throat. The undiluted compound tincture of capsicum and myrrh, applied directly

to the molluscum or aphthæ, by means of a bunch of raw cotton saturated with it and wrapped around the end of a crooked probang, is a much better local application than the sulphuric acid or the nitrate of silver. Hippocrates used a bunch of wool wrapped around a myrtle rod for a probang, and dipped in a solution somewhat like the French preparation called *alcoolat vulnéraire* that the vin aromatique is made from, in cases like *malum Egyptiicum*. Either the cotton or the wool is much better than the sponge. Hippocrates used sponges in such affections, but they were large sponges saturated with hot water, and applied on each side of the throat. He also made a free use of inhalations. What is very remarkable, vermifuges or medicinal agents known to be destructive to the parasitical organisms found in the human body, entered into the composition of the lotion he applied to the throat, and were also put into the vessel containing the substances for inhalation. The lotion used by Hippocrates for gargling and swabbing the throat by means of a bunch of wool fastened to a probang, was composed of an infusion made of the bruised leaves of the *serratula tinctoria* of Lin., the *apium graveolens*, the *origanum dictamnus*, the *mentha pulegium*, with hydromel, nitre and vinegar mixed with it.

The *alcoolat vulnéraire* of the French enters into the composition of the vin aromatique, so much used by Ricord of the present day, as a local application against syphilitic poison. All the plants used in making Hippocrates's gargle enter into the composition of the French *alcoolat vulnéraire*. The father of medicine used another gargle, in another form of the disease, made of figs and pomegranates, to swab the throat with. When the tonsils were affected, he applied *flos aris* directly to them, and used a gargle made of a plant which I take to be the *vaccinium myrtyllus* of Linnæus, which enters so largely into the composition of that celebrated preparation of the Thompsonians, called "Number Six."

When medicine was reformed in Edinburgh, about a century and a half ago, to adapt its *Materia Medica* to that hyperborean region, most of the hot, fiery articles, such as the peppers, were cast out of it, although two thousand years' experience had proved their utility in Asia, Africa, and the South of Europe; yet they were excluded because they were not found to be useful in treating the diseases observed in a little corner of the earth in the north-west of Europe. It is surely quackery to rely upon hot, pungent articles, as the Thompsonians do, in the treatment of all diseases indiscriminately. The

Edinburgh reformers and the Thompsonian reformers, when weighed in the balance of true medical science, are both found wanting. The prejudices of the latter against the lancet and mineral substances, and the prejudices of the former against the myrtle, lobelia and pepper family of plants, are unworthy of the God-like Science, which looks with equal eye upon all created things, and presses them into its service as occasion may require.

(*To be Continued.*)

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ART. XIII.—*Voluntary Suspension of the Pulse.*

MESSRS. EDITORS—At your request I send you an account of a case I saw in London in 1856. You were pleased to say that such an account would not be devoid of interest for your readers at this time, when the many curious phenomena presented by the case of M. Groux are exciting so much attention. The particular point to which I would now refer in that gentleman's demonstrations, is the power he possesses, by making a violent effort upon a full inspiration, of stopping all pulsation in his left arm. Dr. Bowditch took occasion to remark, in respect to this, before the Suffolk District Medical Society, "that to produce the effect in question, an exertion was required on the part of M. Groux, so violent and fatiguing that he had been strongly advised, for his own sake, to make it as seldom as possible." In the case which I will now give, which I was permitted to see by the kindness of Dr. Sibson, of London, in his wards at St. Mary's Hospital, the same effect, to wit, total extinction of the pulse in the left upper extremity, was produced at will by a very moderate and by no means fatiguing effort.

The patient, a young man between twenty and thirty years of age, had, it appeared, lost, as a consequence of phthisis, a large portion of the right lung. The disease had been, to all appearance, arrested at this point, and the patient regaining to a certain extent his former health and vigor, the left lung had become very much enlarged. When this young man took a full breath, with his arms hanging naturally at the side, the pulse in the left radial artery ceased instantly, and remained extinct so long as he held his breath. When, on the contrary, he held his arm above his head, the fullest respiration, made with a strong effort, did not suffice to extinguish the pulse, although a certain diminution of its fulness could at such times be perceived. The following was the diagnosis of Dr. Sibson, to the best of my recollection: "Extensive destruction of the right lung, with a compensating enlargement of the left. On a full inspiration the enlarged lung so forces up the first rib as to entirely cut off the

current of blood flowing through the left subclavian artery. This effect is produced all the more easily that the left subclavian, rising almost perpendicularly from the aorta, is forced to cross the rib at a very acute angle, as compared with its fellow of the right side. Now on raising the arm above the head, this angle is rendered much less acute, and accordingly the pulse is instantly perceived."

This power over the pulsations in the left arm is, I believe, by no means confined to persons presenting deviations from the normal condition in the parts in and about the chest. Almost any one, by expanding his lungs to the full, can assure himself of this with a very little trouble. The case I have just cited is therefore only curious as presenting the thing in a clear, unmistakable manner, and as showing how the simplest anatomical facts come sometimes to be important in the diagnosis of disease.—(*Boston Med. and Surg. Jour.*)

The M. Groux referred to above has recently visited New Orleans, and given us an opportunity of witnessing his remarkable case of congenital fissure of the sternum. Whether the pulsating tumor which is seen within this fissure be the pulmonary artery or the aorta, whether the right ventricle or auricle, has given rise to discussion among the *savans* of medicine throughout the old and the new world. The vast majority, however, think, and no doubt correctly, that it is the right auricle. We are unaware that anything new has been learned from this singular case, but it has afforded thousands the opportunity of seeing and feeling, and thus confirming what was before proved of the motions and sounds of the heart. Nothing we witnessed, caused half so much surprise as his voluntary suspension of the pulsations in the arteries of both wrists, which suspension, it is stated, is continued as high up as the subclavian of each side.

It has long been taught that forcible and prolonged inspiration diminishes both the force and rapidity of the heart's motions, and this has been attributed solely to a mechanical cause, the distended lungs compressing the heart; but the eminent physiologist, Brown-Séguard, asserts that he has abundantly proved by experiments that the pneumogastric nerve also plays an important part in this phenomenon: "I found more than two years ago," says he, "that during the nervous action, which excites the muscles of inspiration to dilate the thorax, and which descends from the cerebro-spinal centre, there issues also from this centre an excitation, which, passing down the par vagus, is conveyed at each inspiratory effort to the heart, diminishing the force and quickness of its movements." However this may be, there can be little question that the cause of this voluntary suspension of the pulse is correctly explained by Dr. Sibson, as above.

It may be well to bear in mind that however wonderful the above cases may be, they are far surpassed by others which lie rusting in old medical records. Celsus records a case of voluntary suspension not only of the pulse, but also of respiration, imitating death so closely that it could not be distinguished from it. And still more recently occurred the case of the Hon. Col. Townshend, who could die when he pleased, and then rise from the dead when he felt so disposed. Three medical men of unquestioned veracity and respectability testify that they saw him remain, for half an hour or more, pulseless, the heart's motions and sounds being imperceptible and inaudible, and his breath gave neither "mist nor stain" to the looking-glass; so dead, indeed, did these three doctors deem him, that they "were just ready to leave him" when a flickering pulse with gentle breathing heralded returning life. A post mortem examination of this case disclosed no pathological changes except in the right kidney, all the viscera being entirely healthy and perfectly natural.

CHAILLÉ.

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## PROGRESS OF MEDICINE.

ART. I.—*Researches into the Influence of Fatty Bodies in the Absorption and Assimilation of Metallic Oxydes*: Translated from the French, by J. P. BARBOT, M. D.

[THE following paper is extracted and condensed from a Prize Essay, by Dr. JEANNEL, Professor of the "École de Médecine," of Bordeaux, read before the Academy of Medicine, at its meeting of Dec. 13th, 1858, and published in the "Journal de Médecine de Bordeaux," for February, 1859, entitled: *Researches into the Influence of Fatty Bodies in the Absorption and Assimilation of Metallic Oxydes.*]

When a watery solution of a metallic salt is placed in contact with an absorbent *living* tissue, two series of phenomena are observable: first, if the solution contain many metallic salts or be concentrated, an eschar will be formed by the union by chemical affinity of the mineral elements and the organic tissue; second, if the solution be dilute, no chemical union will take place between the metals and the tissues,

but the former will be absorbed and carried into the circulation. Between these two extremes, of producing an eschar and simple absorption, there is of course an infinite number of intermediate degrees, characterized by a greater or lesser disturbance of the physiological condition of the absorbent tissue.

Whenever it is desired to introduce into the circulation a metallic remedy, in order to procure its constitutional or dynamic effects, our first care must be to obviate, as much as possible, its local irritative effect; hence the great importance of dilution; our next object is to ascertain as nearly as possible, what changes this metal will undergo in the economy by combining with organic substances. What are these changes? An inquiry into these phenomena forms the subject of the present memoir.

M. Mialhe\* says: "metallic oxydes act in two different ways; some, such as the salts of zinc, bismuth, copper, antimony, etc., being decomposed by the alkaline carbonates existing in the blood, are converted into oxydes, carbonates, or albuminates: others, such as the salts of lead, mercury, silver, gold and platina are also decomposed by these alkalies, but their oxydes, after having been liberated, are changed by the chlorides of potassium, sodium and ammonia existing in the animal fluids, into double chlorides."

It cannot be denied—indeed all chemists admit it—that the decomposition of metallic oxydes by the alkaline carbonates of the blood is imperative; but can we admit M. Mialhe's theory, that the metallic oxydes, when freed from their combination with acids by the alkaline carbonates of the blood, act by being converted into oxydes of carbonates or albuminates, or else as double chlorides?

This ingenious observer, while attempting to explain the action of the alkaline bi-carbonates and the absorption of metallic oxydes, entirely overlooks, as have done all the chemists who have preceded him, the fatty matters, which are nevertheless, perfect solvents of metallic oxydes, and possessed in a remarkable degree, of the property of masking their chemical affinity.

I know, says Dr. J., that we must be very careful not to draw hasty conclusions from experimental reactions, produced at will, outside of the animal economy, and consider that they will occur under vital influence. Still, certain chemical phenomena will occur so fre-

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\* Mialhe, *Chimie Appliquée*, p. 567.

quently and so pointedly, under peculiar conditions, evidently assimilating those that take place under the influence of organic life, that they, as it were, *force* upon us a theory of the organic mechanism.

Assuming as a basis, the known and adopted chemical composition of the blood and intestinal fluids, and trusting to his numerous experiments upon the subject, Dr. J. draws his conclusions, which he hopes will help to elucidate, if they do not explain entirely, the changes undergone by metallic oxydes introduced in solution into the economy.

Perhaps, says Dr. J., these experiments will explain the action—hitherto but little understood—of the acid or neutral fatty elements of the blood, in the absorption or assimilation of metallic oxydes. They may also, to a certain extent, justify the opinion advanced by M. Arthaud,\* adopted by Dupasquier,† and now become “textual,” (*classique*,) on the hygienic influence of the bi-carbonate of lime, which is, in truth, a mineral aliment. They will at any rate prove this singular fact, that in our laboratory experiments the bi-carbonate of lime contained in our potable waters may become the intervening agent for the solution of metallic oxydes in fat oils. In a word, I hope to prove that several important salts, which I took as types, such as the sulphate of iron, the sulphate of potass and iron, the sulphate of copper, the bi-chloride, the biniodide, and the proto-chloride of mercury, after having been decomposed by the bi-carbonate of lime of potable waters, by the alkaline carbonates and bi-carbonates in weak solution, or by the carbonates of the alkaline animal fluids, in contact with fatty oils, the latter will dissolve an amount, sometimes considerable, of the metallic bases, particularly when they are in contact at the temperature of the body.

I will also observe, that when metallic salts, taken into the stomach, are not dissolved in the acid fluids of stomachal digestion, or are not absorbed with the liquids taken as drinks, they must necessarily pass down into the intestines, and be therein decomposed by the alkaline fluids in contact with fatty matter, with which latter their oxydes form new combinations, readily tolerated by the tissues and absorbable under the emulsive form.

I find in the blood, the elements, both alkaline and fatty, which I have artificially combined, and which in my experiments have con-

\* De la valeur hygiénique que l'on doit attribuer à la présence de certaines substances salines dans les eaux potables.—BORDEAUX, 1838.

† Des eaux de source et de rivière.—LYON, 1840.



verted the mineral salts in watery solution into oleo-stearates and oleo-margarates ; and I am led to the conclusion that the metals are ultimately converted into fatty salts (*sels gras*), whether directly carried in solution into the circulation or decomposed by the intestinal animal fluids, and that, consequently, the most rational form of administration of metals, in order to obviate irritation of the intestinal surface, and obtain their constitutional effect, would be that of fatty salts.

Dr. J.'s experiments show, also, that a weak solution of bi-carbonate of soda will decompose calomel and dissolve its mercury with a heat at  $+40^{\circ}$  cent., much more actively than a much more concentrated solution of chloride of sodium, and that a fat oil is an exceedingly delicate test to show the most minute proportions of copper in a watery solution.

Dr. J. read to the Academy thirty-nine of his experiments, in proof of his propositions and assertions. These propositions were divided into eight heads, and the enunciation of the proposition was followed by the reading of corroborative experimental proof. We cannot, of course—much as we are tempted to do so, for they are in the highest degree interesting—give his experiments, but will republish his propositions in their numerical order.

Prop. 1st. Iron is rendered soluble in oil by the agency of the bi-carbonate of lime of potable waters, and by bi-carbonate of soda. 2d. Oxyde of copper can be dissolved in oil by the intervention of the bi-carbonate of lime of potable waters. 3d. Oxyde of copper is dissolved in oil by the agency of carbonate and bi-carbonate of soda. 4th. Oxyde of copper is rendered soluble in oil by the agency of alkaline animal fluids. 5th. Oxyde of mercury, produced by the decomposition of its bi-chloride, is rendered soluble in oil by the agency of the bi-carbonate of lime of potable waters, by carbonate and bi-carbonate of soda. 6th. Oxyde of mercury, produced by the decomposition of its bi-chloride, is rendered soluble in oil by the alkaline animal fluids. 7th. Mercury produced by the decomposition of its biniodide, is rendered soluble in oil by bi-carbonate of soda. 8th. Oxyde of mercury, from decomposed calomel, is rendered soluble in oil by the bi-carbonates of soda and lime.

#### CONCLUSIONS.

1st. When a metallic salt in solution is decomposed by an alkaline

carbonate or bi-carbonate, in contact with a fat oil in excess, at the ordinary temperature, a portion of the metallic oxyde is dissolved by the fatty body. This reaction will be favored by a temperature of  $+40^{\circ}$  cent.

2d. After a very dilute metallic solution has been decomposed by the bi-carbonate of lime of some potable water added to it, if oil be mixed and shaken with the solution, the oil will re-dissolve the metallic oxyde.

3d. When animal alkaline fluids (serum of blood, milk, albumen) are placed in contact with a metallic salt in solution and with oil, the alkaline carbonate of the organic fluids is generally sufficient to decompose the salt, whose oxyde is re-dissolved in a notable proportion in the fatty body.

4th. Should a watery solution of a metallic salt escape stomachal digestion and pass into the bowels, it will there be decomposed by the alkaline fluids in the presence of fatty matters, and its oxyde be re-dissolved by the latter.

5th. The same facts and the same reasoning will lead us to admit that a watery solution of a metallic salt, on reaching the blood in the circulation, will at once be exposed to a double decomposition with the ultimate formation of a fatty salt.

6th. Calomel is decomposed by a weak solution of bi-carbonate of soda; there is formed chloride of sodium and, in all probability, bi-chloride of mercury, both of which are re-dissolved together. The presence of free chloride of sodium interferes with this decomposition and solution.

8th. If, in the administration of metallic remedies, with a view to their constitutional or dynamic effect, therapeutists wish to approximate the compounds formed naturally in the system, they should administer metals in the shape of fatty salts.

#### SUBSIDIARY CONCLUSIONS.

1st. Fat oil is a réagent of exquisite delicacy, by which we can easily detect and liberate  $\frac{1}{400,000}$  part of oxyde of copper in watery solution, provided the solution contain an equivalent quantity of alkaline carbonates or of carbonate of lime.

2d. A weak solution of bi-carbonate of soda will decompose calomel and re-dissolve its mercury much more actively than a weak solution of chloride of sodium.

ART. II.—*Action of the Sympathetic Nerve on the Circulation of the Blood*

M. CL. BERNARD has discovered, as is known, some most interesting facts connected with the action of nerves on the circulation. He has found by exciting successively the two nerves going to the sub-maxillary gland, namely, those nervous filaments coming from the chorda tympani, through the medium of the lingual and the sympathetic filaments coming from the superior cervical ganglion, that the effects are diametrically opposed. In the last case the circulation is observed to be remarkably diminished, at the same time venous blood becomes very black, and the salivary secretion is suspended. In the former case—that is, when the tympano-lingual nerve or its peripheric end after the section is galvanized, the circulation is hastened, the blood flows in jerking jets from the veins of the gland, which latter become the source of an active secretion.

These facts seemed in accordance with the old hypothesis of an active dilatation of the vessels; but that hypothesis is so much in opposition to the principles most generally admitted, that M. Cl. Bernard has been obliged to seek another physiological interpretation. In the last meeting of the Society of Biology he indicated the facts which furnished him the basis of an explanation, which he will endeavor to confirm by new experiments.

1st. If the cervical branch of the great sympathetic be cut below the superior cervical ganglion, no effect is produced on the circulation of the gland. If the section be made on the branches proceeding from that ganglion to the gland, an increased circulation is observed as the section is nearer to the gland. That acceleration is the consequence of the paralytic dilatation of the vessels, and is entirely analogous to that produced in the face and ear of any mammalian after the section of the cervical branch of the great sympathetic. We also observe a change of color of the venous blood of the gland, and a salivary flux through the excretory canal. If the section be made at the root of the gland (and at that point there are, in the dog at least, small microscopic nervous ganglions which are thus removed), all the phenomena that we have mentioned are still more evident. But it is true that they are not as intense as when the tympano-lingual branch is galvanized.

2d. When a dog is poisoned with the curare, we observe at the moment the nerves are paralyzed by the influence of the poison, an

increased activity of all the glands, and consequently of the sub-maxillary gland, and the same modifications in the circulation of the gland are observed.

3d. Finally, a sub-maxillary gland may be locally poisoned by injecting in its arterial system a few drops of a filtered solution of curare, by means of a syringe of Anel. In that case, again, the results are the same.

All those facts plainly show that by the paralysis of the sympathetic branches we may produce all the effects brought about by exciting the cord of the tympanum, and that those effects are the more marked as the abolition of the motory powers of the great sympathetic is more complete. We are thus led, according to M. Cl. Bernard, to consider the great sympathetic, with regard to its action on the vessels, as a nervous apparatus intended to maintain them constantly in a moderate state of contraction, and therefore performing the part of the moderator of the circulation. When it ceases to act, the vessels not being retained any more by that sort of check, dilate, and the circulation is hastened. Hence, M. Cl. Bernard, in order to explain the action of the nervous fibres issuing from the chorda tympani on the circulation of the sub-maxillary gland, brings forth an hypothesis which, confirmed by new experiments, would easily explain the action of these fibres. According to that hypothesis, the tymano-lingual nerve would not act on the muscular fibres of the arteries, but on the extremities of the sympathetic branches. By exciting it we would destroy the moderating action of the sympathetic. It would be the action of one nerve on another nerve—the paralysis of one nerve by exciting another. As observed, if that hypothesis prove true, we will have an easy explanation of a fact unaccounted for until now, and the physiology of the nervous system would be enriched with a most important idea, the application of which would at once extend to several other phenomena.—(*Translated from the "Gazette Hebdomadaire de Médecine,"* by DR. E. CAIRE.)

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ART. III.—*Tannin. Preservative Liquid. Leniceps. Ergot.*

i. *Tannin in large doses in Albuminous Anasarca.*

In an interesting memoir published in the Archives of Medicine,

Dr. Garnier calls the attention of practitioners to the use of tannin in large doses, in the treatment of albuminous anasarca. In three cases which he reports, and which he has compared with analogous observations taken from several authentic records, Dr. Garnier employed with success the following formula :

R.....Acid Tannic, gr. xxxij.  
 Aquæ distil.  
 Syr. Cinchon. àà ʒj  
 M.—S. a teaspoonful three times a day.

Dr. Garnier arrives at the following conclusions : Tannin in doses of from thirty to sixty grains a day will cure anasarca or œdema passively developed and coincident with albuminous urine. Its curative action manifests itself by an abundance of urine, which gradually assumes its physiological character ; by the cutaneous transpiration ; by the easy alvine evacuations ; by the appetite, etc. These favorable signs appear on the second day after the administration of tannin.—(*L'Union Médicale*. Translated by J. A. LA NEUVILLE.)

ii. *Preservative Liquid for Microscopical Preparations.*

The importance which microscopical studies have assumed induces us to record the following formula, advised by M. Pacini for the preservation of microscopical preparations :

R.....Perchloride of mercury,..... 1 part.  
 Chloride of iodine..... 2 “  
 Glycerine (25° Baumé)..... 13 “  
 Distilled water.....113 “ M.

Leave this mixture at rest for two months, then dilute it with three parts of distilled water, and filter.

This liquid is excellent, says the author, to preserve blood globules, nerves, ganglions, the retina, and all the soft tissues, which harden, preserving their form and appearance.—(*L'Union Médicale*. Translated by J. A. LA NEUVILLE.

iii. *The Leniceps.* (Academy of Medicine, Paris.)

Dr. Mattei, in presenting to the Academy a forceps of new form, gives a detailed description of this instrument, which he terms the *Leniceps* (*leniter capiens*). He enumerates its advantages, which con-

sist principally in the facts, that it may be applied without deranging the patient, without exposing her, and even without her knowledge ; that it is more easily adapted to the child's head, seizing it without compressing it, and acts, in fine, without injuring the soft parts of the mother.

The Leniceps is portable, and such is its dimensions that it may be readily carried in the pocket.—(Translated from the *Gazette Hebdomadaire de Médecine*.)

iv. *Ergot*. (Imperial Academy of Medicine, Paris.)

M. DEVILLE read a memoir entitled "Statistical Researches upon the action of Ergot in Parturition." The author first proves that it is nearly always possible to determine the cause or causes which induce the child's death in the mother's womb, such as premature labor, vicious presentations, etc. When none of these causes are present, and when the foetus has arrived at full term in good condition, and when, notwithstanding, this infant is born dead, with all the signs of asphyxia, it may be asserted that ergot has been administered. In analyzing, from this point of view, the documents collected during the years 1845 to 1848, in the different departments of Paris, M. Deville arrives at the conclusion, that of 515 infants born dead, seventy-two, that is a little more than one-seventh of them, have fallen victims to the administration of ergot. He concludes from his researches, that ergot is always dangerous to the child's life ; that it is generally given by unskillful hands, who most frequently do not properly consider the conditions which should be regarded in order to administer it with some chance of success ; and, in fine, that even skillful physicians, when following the rules prescribed by science and experience, can never be sure the child will be born alive where ergot has been given during the labor.

It must be well understood that these conclusions invalidate, in no respect, the precious merits of ergot in uterine hæmorrhages. (Commission : Dubois, Depaul, Dauyau.)—Translated from *L'Union Médicale* by CHAILLÉ.

ART. IV.—*Electricity. Iodo-Therapy. Intermittents. Bismuth. Syphilis. Morbidity of the Spleen. Holloway's Pills. Chilblains.* Translated and condensed from the Spanish : By J. P. BARBOT, M. D.\*

i.—*Electricity a cure for Lead Colic.*

Dr. D. Juan Drumen successfully treated, in the male wards of the Madrid General Hospital, two cases of lead colic. Both patients had suffered from this painful disease several times previously, and had been placed under Dr. D.'s charge only after the usual treatment had failed to procure relief.

Dr. D.'s method is very simple. Rigid diet at first, demulcent drinks, an anodyne at night, and the passing of an electrical current once or twice a day over the abdominal walls, by means of Breton's apparatus. At first the patient complains of sharp pains, as if pricked with needles, as the electrical current is swept through him, but these immediately subside. Wherever the electrical knob has touched there is subsequently a slight injection of the cuticle. Pains existing in any of the articulations are also relieved by electricity. Relief is experienced in a short time, and a cure is effected in from ten to thirty days. (*El Siglo Medico*, January 2, 1859.)

ii.—*On the use of iodated food as a preventive and cure of all the diseases for the cure of which iodine is used internally.*

Dr. Boinet, whose essay *On Iodo-Therapy* recently received a prize (two thousand francs) from the Academy of Medicine of Paris, says in that treatise, that ever since 1849 he has been making experiments on this subject. Dr. Boinet says, as it is well known, that iodine exists normally in the air, water, and a great many of our alimentary substances; it is a normal element of our economy, and he advises its administration in the form of food as a preventive of those diseases which are curable by iodine. Numerous experiments, extending back for more than ten years, fully confirm M. B.'s previsions, and show that individuals using this iodated food are always greatly benefitted thereby.

NEW ORLEANS, March 22, 1859.

\* *Dr. Bennet Dowler* : Dear Sir—In the numbers of the "*Siglo Medico*" of Madrid, which you sent me, I found the following articles that I thought worthy of condensation and translation. I would certainly have preferred translating the articles taken from the French medical press from the original journals, if I had them, rather than give you a translation of a Spanish translation.

I remain yours very truly,

J. P. BARBOT.

In order to obviate the irritative effects of most preparations of iodine, Dr. B. prefers using those plants, such as the marine vegetables, fuci, cruciferæ, etc., or the mineral waters in which iodine exists naturally. These can be used or mixed with the ordinary food or drink, only being careful to administer them in small doses and in a dilute form. It is the only way in which iodine can be taken without danger and with benefit, because it is thus rendered readily assimilable. In a large proportion of the cases in which he tried it, since 1849, for which purpose he selected such as were suffering severely from scrofulous degeneration, and exhibited every variety of scrofula, as ophthalmia, ulcers, diseases of the skin and glandular system, caries of the bones, white swellings, etc., a cure was effected by the steady use, for several months, of iodated alimentation. Another practitioner, Dr. Braive, obtained similar beneficial results from the same treatment.

Dr. B. terminates his memoir by citing cases to show that certain cases of atrophy or injury to certain organs, attributed to the continued habitual use of iodine, only occurred when unskillfully prepared iodated preparations were used.—(*Ibid.*)

### iii. *Iodine as a cure for Intermittents.*

Thirty-seven out of forty cases that were treated by Dr. Barilleau, with the following combination, were cured: Take of infusion of chamomile, 100 grammes (3 ounces) tincture of iodine, 30 drops. Mix. To be taken in three doses. This mixture must be taken steadily for some time.—(*El Siglo Medico*, 9th January, 1859.)

### iv. *Bismuth in Coryza.*

Long since Dr. Monneret had derived very beneficial effects from the use of the sub-nitrate of bismuth in acute coryza, and the editors of the "*Revue Thérapeutique*" obtained equally happy results from its use. But when the coryza becomes chronic, it cannot always be cured by the sub-nitrate of bismuth alone. In those cases Dr. Sobrier says he derived very great benefit from the following combination:

R.....Sub-nitrate of bismuth, 4 grammes (1 drachm).  
 Powdered liquorice root, 8 grammes (2 drachms).  
 Iodide of sulphur, 30 centigrammes (6 grains).

Mix. A little of this powder to be snuffed up the nostrils ten or twelve times a day *p. r. n.*—(*Ibid.*)



v. *Treatment of Syphilis by the Kabyles of Algeria.*

Dr. Leclerc has published in the "*Gazette Médicale de l'Algérie*" the following account of the manner in which the Kabyles cure syphilis :

Zaong (mercury), 15 grammes ( $\frac{1}{2}$  ounce).

Toutya (sulphate of copper), 4 grammes (1 drachm).

Zenaljar (acetate of copper), 4 grammes (1 drachm).

Chnadeur (sal ammoniac), 6 grammes ( $1\frac{1}{2}$  drachm).

Triturate the three latter separately ; mix them in a new or very clean vessel, with walnut bark water, then add the mercury, mix again and reduce to the consistence of a paste : divide this paste into six *pastilles*, which dry.

The treatment consists in two fumigations, night and morning, for three days. A vessel containing live coals is got ready, the patient squats over it, taking care to cover himself well with his "*bournous*," his mouth shut, and having his ears and nostrils plugged with wool. A pastile is broken in two and thrown upon the live coals. The physician or an aid assists in protecting the patient's face from the injurious effects of the fumigation, which must last fifteen minutes. During the three days of treatment the patient must abstain from salt food, vinegar, figs and meat, except mutton and bread without any salt. He must not go out of the house, must avoid exposure, drink cold water and chew sarsaparilla.\*--(*Ibid.*)

vi. *Morbid Alterations of the Spleen.*

Dr. F. Fuhrer, of Hamburg, has been studying the various changes in the texture of the spleen under the influences of different morbid influences. He says that the greater part of these changes consist in a peculiar alteration of the parenchyma proper of this organ, which parenchyma is a species of net work of capillary cells, in which the blood corpuscles are formed previous to passing into the veins.

These modifications are : 1st, Suspension of the development of the organic elements of the spleen (marasmus and sarcoma of the spleen), and even their total disappearance (cirrhosis). 2d, Excessive development, but without any complete formation of cells and fatty degeneration (chlorotic spleen). 3d, Degeneration into compact tissue of the capillary cells and their metamorphosis into permanent capillaries, with dilatation and enlargement of the small splenic vessels (sarco-

\* We wonder whether this treatment originated with the Kabyles, or is only a pale reflection of the treatment used by the more "regular" physicians of Europe?—TRANSLATOR.

matous spleen). 4th, Excessive development of the capillary cells (plethora, hypertrophy, fungus of the spleen). 5th, Infiltration of morbid matter into the cells, which alters or destroys their form, and leads to their disappearance (lardaceous spleen, parenchymatous inflammation with formation of pus, cancer).—(*El Siglo Médico*, 23d January, 1859.)

vii. *The World-renowned Professor. Holloway's Pills. Their Composition.*

Messrs. Gonzales, Minguez y Lozano, apothecaries of Valladolid, have analyzed this celebrated nostrum, the greatest discovery of this age (!), which has yielded such profit to its inventor. Numerous essays have invariably yielded the same result : 100 parts contain—

Aloes .....	64 parts.
Rhubarb .....	24 “
Pepper .....	6 “
Saffron .....	3 “
Sal Soda .....	3 “
	100

Consequently their formula is :

Aloes.....	1 drachm and 20 grains.
Rhubarb.....	34½ “
Pepper .....	8½ “
Saffron .....	4 “
Sal Soda .....	4 “

Mix and divide into 100 pills.—(*El Siglo Médico*, Feb. 6, 1859.)

viii. *A Cure for Chilblains.*

It seems that the government of Wirttemberg has purchased from the parish priest of Kupferzell, his secret for the preparation of a salve, that he had invented for the cure of chilblains. We give its composition, as published by order of the government : Take twelve ounces of mutton suet, twelve ounces of hog's lard, and two ounces of peroxide of iron. Melt them in an iron stewpan, stirring them constantly with an iron rod. As soon as the mixture assumes a deep brown or blackish tint, add to it two ounces of Venice turpentine, one ounce of oil of burgamot, and one of Armenian bole, previously rubbed up with a little oil. This ointment is particularly recommended when the chilblains have already ulcerated.—(*El Siglo Médico*, 16th January, 1859)

ART. V.—*On the Inhalation of Cinchonia and its Salts.* Read before the Biological Department of the Academy of Natural Sciences, Dec. 1858 : By S. W. MITCHELL, M. D.

THERE can be very little doubt that at some future time we shall possess the means of giving to patients many potent remedies in the form of inhalation, rather than in the usual way. This is at least among the hopes of the therapist of the present day. Absorption of medicinal substances by the intestinal mucous surface is but too often uncertain, while the passage to the blood through the lungs seems to be always an open track, when the agent inhaled is in a state of vapor. *How* desirable it would be to possess the means of inhaling quinine in the congestive fevers of our malarious districts, we can very well conceive. Guided by these ideas, I have sought industriously for some means of attaining this result ; and although I have failed, as I shall here show, in evolving any very marked practical benefit from these researches, I have met with certain facts of such interest that I desire to put them on record, as indicating a novel direction for medical thought and action.

At one time the analogy in chemical composition, between certain of the newly formed ethers and quinia itself, seemed to point out these as fit subjects for therapeutic use and trial. The difficulty of procuring them obliged me, however, to relinquish effort in this direction, and I turned from them to examine anew the alkaloids derived from cinchona bark. While thus engaged, one of my friends, now Dr. Bill, of the army, pointed out to me, in Fresenius's Chemistry, his account of cinchonia, which he describes as volatile at high temperatures.

Struck with this, I searched carefully for any account of its inhalation, but as yet have been unable to find in the books on Cinchona any description of inhalation, as a mode of using the alkaloid in question. The last complete work on quinia, by M. Briquet, enumerates many methods of employing the alkaloids and bark, but neither among the means in use or out of use is this one alluded to. Occasionally, in disease of the lungs or throat, inhalation of pulverized cinchona bark has been resorted to, and M. Briquet relates—"Trait  Therapeutique du Quinquina et de ses Pr parations," page 118—that those who work in the storehouses of cinchona bark are sometimes thus cured of malarious fevers. This could only occur through accidental ingestion, and inhalation of the floating particles of bark.

Cinchonia and its salts are the only alkaloids which appear to be volatile by heat. After many experiments I have finally resorted to the following very simple method of inhaling them : About forty grains of pure cinchonia, being mixed up with sand, are placed in a capsule, and heated by a spirit lamp. The sand is useful in diffusing the heat and preventing too rapid a destruction of the alkaloid. A heat of about 300  melts the particles of cinchonia into a brown fluid, and from this, if the evaporation be carefully managed, the volatilized alkaloid escapes in the form of a gray vapor.

When a microscope glass is held over the capsule, and the heat is too elevated, the cinchonia decomposes, and a dark red gummy matter, with the odor of burned benzoin, adheres to the glass. A rather lower

temperature drives off the cinchonia in a gray vapor, which may be made to redeposit the pure alkaloid upon the interior of a funnel held over it, or upon a microscope slide. The alkaloid thus obtained is in branching needles.

On a number of occasions I inhaled the vapors of cinchonia, often breathing them for ten to twenty minutes, without much inconvenience, when care was taken to regulate the supply of heat. The brown or reddish volatile substance which is given off when the heat used is too great, so irritates the throat as to cause nausea and oblige the patient to cease inhaling.

When carefully inhaled, a part of the alkaloid is deposited on the throat and in the mouth, where its sub-bitter taste is soon perceived. To guard against error, which might arise from swallowing these portions of the alkaloid, I refrained from swallowing whilst inhaling, and frequently rinsed the throat with water.

Upon four occasions I noted the symptoms caused by the cinchonia thus employed, taking care to allow the excitement of the system produced by the inhalation to pass away before I counted the pulse. In three instances the pulse *fell*, losing from six to ten beats per minute. In the fourth the pulse remained a few beats above the normal number. The person on whom these experiments were made is liable to still greater depression of cardiac energy when under the influence of quinia. At first it was difficult to separate the ordinary signs of cinchonism from the feelings of cerebral confusion, caused by breathing too rapidly. These sensations, however, were evanescent. At the end of a quarter of an hour, or even less, the head was clear, and within half an hour afterwards the patient felt a quickly increasing headache, with giddiness, and sometimes a feeling as though the brain was swelling into monstrous bulk. These sensations passed away within four or five hours, unless the inhalation was renewed.

Still uncertain as to whether or not the alkaloid entered the blood, I caused a healthy adult, *æt.* twenty-nine years, to inhale the fumes from forty grains of the heated cinchonia four times in one day. Symptoms of cinchonism were felt only after the first inhalation, which was made at 10, A. M. At 12, M., the second inhalation took place, and at the same time four ounces of clear urine, *s. g.* 1023, were passed. The other inhalations occurred in the afternoon and evening, but none other of the urine passed was saved until 7, A. M. next day.

The first specimen was examined by Bouchardat's test, the iodated iodide of potassium. This reagent gave a faint, but decisive, brown precipitate of iodide of cinchonia, when employed in the usual way; when, however, I placed in a test tube a portion of the test solution, and slowly poured upon it the lighter wine, a profuse deposit of the iodide announced the presence of cinchonia in the urine. In the usual mode of making this test—although the precipitate is perceptible enough—it almost immediately re-dissolves in the urine, which appears to possess a remarkable power of dissolving the iodides of cinchonia and quinia, since, when these precipitates are thrown down from an aqueous solution of a salt of either alkaloid, they are found to be very

insoluble. The second specimen of urine contained only traces of cinchonia, and twenty-four hours after the last inhalation no evidence of the presence of the alkaloid in the urine could in any way be obtained.

It will be readily seen, from what I have said, that I do not anticipate any remarkably valuable practical results from the new mode of administering cinchonia in vapor. The want of therapeutic power in this alkaloid, when compared with quinia, dose for dose, the difficulty of regulating the heat so as to volatilize, and yet not decompose it, as well as the unpleasantness of the process of inhalation, combine to deprive these experiments of any great practical utility. In a single case of tertian intermittent fever I employed the inhalation of cinchonia vapor. The patient had no new attack for one month, although no other ulterior measures were employed. The case was a very irregular and uncertain one, and I therefore attach but little faith to this single therapeutic test. I should add that my patient complained a good deal of the effect of the alkaloid upon his glottis and larynx. For a time it altered the tones of his voice very considerably.

In two cases of chronic bronchitis, of long standing, I also used the fumes of cinchonia. One of these dated his first improvement from the use of these inhalations, in which he persisted every other day, for more than two weeks: no other treatment was used until he had been much aided by the means above described. He learned after a time to employ the cinchonia without my aid. The other patient submitted to one inhalation, but declined any further proceedings of a similar character, declaring that the remedy was worse than the disease, only shorter. When we are successful in volatilizing the alkaloid without decomposition, the process of inhalation is not very disagreeable; but when the heat is too high, and the cinchonia becomes altered, it is extremely difficult to continue to breathe it.

The salts of cinchonia are also volatile by heat, but they offer no advantages which do not equally belong to pure cinchonia. The sulphate is quite inadmissible for inhalation use, since sulphuretted gases are given off in small amounts when the heat is too elevated and decomposition takes place.

#### ART. VI.—*Propylamin.*

WITH the progress of discovery since the year 1817, when Sertuener recognized the alkalinity of morphia, the therapist has been more and more convinced that the organic alkalies have been specially endowed with more well defined and concentrated medicinal power than any other grade of organic bodies. These remarkable substances, remaining stable by natural union with acids occurring with them, seem destined in the economy of nature to minister to disease.

as in most instances they do not appear to possess any other uses, Since the discovery by Wurtz and others of the important fact that organic alkalies may be produced artificially in the laboratory, it does not appear that much attention has been directed to the therapeutic power of these derivative bodies ; yet in some few instances it has been accorded, and there can hardly be a doubt that a rich harvest awaits the researches of experiment in this direction by the enlightened physician. The number of these alkalies has been largely increased by Hoffman and Anderson. Several of these, strictly artificial at first, so far as known, have since been discovered in nature, and among them propylamin, the subject of this notice.

Having been several times applied to for propylamin by physicians, and this alkaloid not being procurable in commerce, it has been thought advisable to publish a formula for its preparation, and give a notice of its characters more in detail than is found in authorities generally accessible. The origin of the demand for propylamin appears to have arisen from its asserted power in cases of rheumatism and its variations, by Dr. Awenarius, of St. Petersburg, the following notice of whose researches is translated from Bouchardat's "*Répertoire de Pharmacie*," December, 1858 : "Propylamin, as obtained from the pickle of herrings, cod-liver oil, ergot, human urine, etc., appears, according to the author, to possess the power of a true specific for the various affections of rheumatic origin. The diagnoses of these diseases being often very obscure, one can succeed (says M. Awenarius) by the use of propylamin, in bringing to light in a few days the true nature of the malady. The author has treated, by means of this remedy, two hundred and fifty patients in the hospital of Kaulinkin, at St. Petersburg, between March, 1854, and June, 1856 ; and besides, it has been employed in outside practice in a considerable number of acute and chronic cases of rheumatism. In acute cases the pain and fever always disappear the next day. The remedy was prescribed in the following manner, viz :

R..... Propylamin..... gtt. xxv.  
 Distilled water..... f. ʒvj. Mix.  
 And when necessary add—  
 Oleo saccharum of peppermint..... ʒij

*Dose*—A tablespoonful every two hours.

It is necessary to carefully ascertain if the medicine is fresh and pure."

Propylamin was discovered by Werthiem in 1850, and may be obtained from various sources. Artificially, from narcotina, codeia, bone oil, and by the action of ammonia on iodized propylene ; and naturally, combined with an acid, in herring pickle, the flowers of the white thorn (*crætagus oxycantha*), those of the service berry (*sorbus aucuparia*), *chenopodium vulvaria*, etc. It is most conveniently prepared from herring pickle or ergot, by distillation with potash. Propylamin is a colorless, transparent liquid, with a strong pungent odor that reminds one of ammonia. As made from some sources it has a fishy odor, whilst from others this character is wanting, and as it is metameric with both trimethylamin and methyl-ethylamin, it is possible that the latter may be mistaken for propylamin. (*Gmelin.*)

Propylamin is soluble in water, has a strong alkaline reaction, forms crystallizable salts, and, like ammonia, when a rod dipped in muriatic acid is presented to it, dense vapors of the hydro-chlorate become visible by their union. Its composition is  $C^6 H^9 N$ , which is that of ammonia with an equivalent of propyl. Most of the salts of propylamin are soluble in water and alcohol, but the sulphate is insoluble in the latter menstruum. (Winckler.) The hydro-chlorate is soluble in alcohol, and crystalizes in large tables as obtained from chenopodium vulvaria. All the salts are decomposed with a herring pickle odor by the mixture of potash, and when heated in solution the same fishy odor is perceptible.

Propylamin is prepared by taking any convenient quantity of herring pickle, obtained from the dealers in salt fish. This is put into a retort or tight still, with sufficient potash to render the liquid strongly alkaline, and the liquid heated. A well refrigerated receiver, containing some distilled water, being attached, heat is applied as long as the distillate has the odor of herrings. This is then saturated with hydro-chloric acid, evaporated carefully to dryness, and the dry crystalline mass exhausted with absolute alcohol, which dissolves the propylamin salt and leaves the muriate of ammonia. From the former the pure propylamin may be obtained in solution by means of hydrate of lime, using strong precautions to refrigerate and condense the vapors which are actively disengaged almost without heating. When made from ergot, Winckler recommends that a solution of the extract of ergot, known as *ergoline*, be distilled with solution of potassa, using the precaution to have some water acidulated with muriatic acid in the receiver, which should be well refrigerated. For medical purposes it will be better to employ herring pickle, as the source of propylamin.—[W. PROCTER, JR., *Amer. Jour. Phar.*]

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ART. VII.—*Sanguinaria Canadensis: Its Use in Dysmenorrhœa:* By JOHN D. O'CONNOR, M. D.

AMONG the various afflictions to which the female, from her peculiarity of formation, and the supplementary functions performed in her physical organization, is subject, there is probably none that calls for more care and attention from the judicious practitioner of the healing art than dysmenorrhœa.

The interruption or suppression of any one of the vital functions is always attended with danger, immediate or remote. The ultimate danger, as a general rule, being in proportion to the importance of the functions thus suspended or interrupted in the economy. Or if not important *per se*, it then becomes so in proportion to the complications that may arise—or, we might say, *must* arise—from the necessary derangement of other organs. That these complications, in the diseases under consideration, are often times of such a character that

they supereede, or at least overshadow, the primary affections, I need not take time to argue, as they must be sufficiently evident to the most superficial observer.

Among the vast host of our young females who are annually hurried to the tomb by that fell monster, consumption, how many, how very many of them, if traced to their primary starting point, would be found to have proceeded from dysmenorrhœa! I would almost be willing, from my own observation, to venture the prediction, that were we to learn the history of all cases of accidental or sporadic cases of consumption in the female, we would find that the pulmonary affection had been preceded by dysmenorrhœa, for a longer or shorter period. And in those cases where there was a hereditary predisposition to phthisis, the precedent primary difficulty of obstructed catamenia would no doubt be found not unfrequently to have obtained.

If these preliminary propositions and suggestions contain any considerable amount of truth, it behooves us, as the guardians of suffering humanity, to cast well around us for a remedy, or remedies, that will enable us, by their judicious administration, to relieve the weaker, if not the better part of it. Would it not also be well for us, in our management of anæmic and chlorotic females, to institute a rigid inquiry into the condition of catamenial flux—not only its present condition, but what was its condition at the first onset of ill health? And then, while we were making use of the necessary remedies to restore the general health, to the anæmic or chlorotic habit, would it not also be well for us to conjoin therewith such remedies as would tend in their actions to restore the uterine organs to a free, healthy discharge of their functions? Assent being given to these queries, we will, after this apparent digression, return to the subject announced.

It is not our design to furnish an egotistical array of cases, but to make a simple statement of facts, in regard to the effects of this remedy in the disease under consideration. In the early part of my professional career I used the remedy of one of America's great physicians—viz: tincture of guaiacum, with but indifferent success; in fact, so indifferent that I found my prescriptions were from time to time thrown aside in one neighborhood, on account of their inefficiency, for a domestic preparation; which preparation, on examination and inquiry, I found to be tinct. sang. canad. Not being disposed to denounce a remedy on account of the humility of its origin, I commenced using it in cases of dysmenorrhœa, and found that I was much more successful with it than with any former course pursued by me. I also instituted a comparative test with it and Dr. Eberle's great remedy—tinct. polygonum hydropi; and although I treated several cases with this remedy successfully, yet some, which would not after a fair trial yield to it, readily gave way under the use of tinct. sanguinaria canadensis. For the last twelve years I have used the tincture of sanguinaria exclusively in the treatment of dysmenorrhœa, and have recommended it to others, who speak favorably of its effects. In that time I have probably treated as many cases of this disease as usually falls to the lot of the village practitioner, and as yet have no cause to find fault with the efficiency of the remedy.

My mode of administering is to commence a fortnight before the



expected return of the menses, and give teaspoonful-doses of the tincture three times a day, and a tablespoonful on going to bed (preceded by a warm pedeluvium, or, in bad cases, a scmicupium). If the secretion is not restored at the time, I remit the use of the remedy for a fortnight, and proceed as before. In the meantime I make use of such hygienic treatment and regulations as the general indications may demand. These, of course, are not uniform in all cases, but are governed by all the various circumstances and conditions that regulate us in the treatment of any other disease. When the full effect of the remedy is produced, it is characterized by slight nausea, pain in the loins, extending through the hypogastric and iliac regions, as well as down the thighs. The symptoms sometimes manifest themselves once or twice, before the discharge is completely established.

Of the *modus operandi* of the sanguinaria in relieving suppressions of menses, I know nothing; and yet, perchance, this may be as much as any of us know in relation to the *modus operandi* of many other articles in the relief of diseases for which they are so confidently administered. I believe it is a conceded fact, that each and every organ and tissue in the animal economy is possessed of a *vires vita*, which *vires vita* is peculiar to and inherent in such organ or tissue. That this *vires vita* is susceptible of being acted upon, stimulated or depressed by appropriate agents, follows as a necessary sequence. Add to this the well established principle in therapeutics, that each remedy in the great arcana has some inherent property or quality that directs its action to one organ or tissue in preference to all others—in other words, it is possessed of an elective affinity or franchise, which directs, controls, or modifies its action; and we derive our knowledge of this affinity from accident, from experiment, or after a chemical analysis. We venture to use it when there appears to be a rational adaptation to the pathological condition of the organ or tissue. From these aphorisms, physiological and therapeutical, we may be able to deduce the *modus operandi* of the tincture of sanguinaria in dysmenorrhœa.

The uterus is possessed of a *vires vita* which is peculiar to that organ, and it can only be acted upon when in an abnormal condition by the appropriate stimuli. The sanguinaria canadensis is an appropriate stimulus to the uterus when in an abnormal condition. At least such has been my experience for a series of years. To it as an efficient remedy in dysmenorrhœa we would respectfully call the attention of the profession, feeling assured that, should it answer their expectations, as it has met ours, it will prove a much safer remedy to use in general practice than the tincture of antacrida, or many other prescriptions (polypharmaceutical) that are so much in vogue, and are so soon superceded by others of like doubtful efficiency, and of equal liability to deteriorate.

Give it a fair trial, and us the result of your experience, that we may be confirmed in, or have our minds disabused of, the confidence we repose in this article.—*Cin. Lancet and Observer.*

ART. VIII.—*Hydrochlorate of Ammonia*: By J. M. RAE, M. D.

ALTHOUGH the value of new remedial agents, which are from time to time added to the pharmacopœias, may often admit of question, there can be no doubt of the great practical importance of ascertaining the therapeutic action of those medicines which have long held a place in the *materia medica*; and of determining the diseases which they have the power either of mitigating or curing, and also of determining their comparative value over other and similar remedies in the treatment of such affections. There are medicines possessing considerable and even great curative virtues which are seldom employed by practitioners; and this may be attributed partly to prejudice, to the rage for new remedies, to want of knowledge respecting them, and to other causes. The muriate of ammonia appears to me to be one of these; for although it has long formed part of the *materia medica*, it has been little used by practitioners in this country, except as an external application. Amongst continental physicians, however, it has been long esteemed as a valuable internal remedy in the treatment of many chronic and febrile disorders. Entertaining a very high opinion of its curative powers, I have prescribed it pretty extensively, in various diseases, for the last eight years in private and for the last four years in dispensary practice, and with satisfactory results. The hydrochlorate of ammonia, besides being liquifacient and resolvent, as mentioned by Sundelin, Wibner and others, appears also to possess considerable neurotic action, as is shown by its curative power in neuralgia and other nervous disorders. Its remedial influence is often so rapidly manifested in these affections as to preclude the idea of the effect being owing to any alterative or resolvent action; it seems more rational to refer it to a direct or peculiar influence of the salt on the nerves or their centres.

I have used the salt with marked success in goitre, and am not aware of its ever having been tried before in the treatment of that deformity. In several cases, where the local application of the muriate was conjoined with its internal administration, the tumors—some of which were very large—rapidly diminished in size, and were soon reduced to the normal condition. It cured the whole of the cases (ten in number) in which it was tried, the period of cure extending from a fortnight to two months. The subjects of treatment were mostly factory girls, of ages varying from fourteen to twenty. To test the powers of the muriate fairly, it was given alone in mucilage, or infusion of quassia, and combined with soap liniment for external use.

As goitre, from some unknown cause, prevailed here last year to a considerable extent, opportunities were thus afforded of contrasting the curative power of the muriate with iodine in this affection. Cases were selected where the tumors were nearly of equal size and duration, and where the age, temperament, general health, and sanitary condition of the individuals corresponded as nearly as possible; and in the cases treated with the muriate, which was used both internally and locally, the tumors generally yielded as readily, and sometimes more quickly, than in those subjected to the trial with iodine similarly employed, and apparently quite as permanently. The muriate appears

to be a safe and efficient substitute for iodine in the cure of bronchocele, and worthy of further trial. The hydrochlorate of ammonia is also a valuable remedy in whooping-cough. I was first led to make trial of it in the treatment of pertussis, from a belief that if the disorder was dependent—as it is considered to be by some pathologists—on an enlarged or morbid condition of the lymphatic glands, or that the exciting cause of the paroxysm was owing, as is very probable, to the presence of irritating glairy mucus in the bronchial passages, the muriate, on account of its alterative power in glandular enlargements and diseased mucous structures, and its effect in promoting the healthy secretion of the mucous membrane in cases of bronchitis, accompanied with the discharge of tenacious, glairy mucus, ought to prove an excellent remedy in the treatment of that often troublesome affection. The result was most satisfactory. It was tried in thirty-seven cases, ten of which were private patients, and the rest home patients at the dispensary, which were, for the most part, under the charge of Mr. Langsford, house-surgeon to the institution, to whom I am indebted for the efficient carrying out of the treatment, and for a report of the cases. Of the number, two died—one, a weekly nurse-child, aged three months and a half, on the third day of treatment, and fifteenth of the attack; the other, which had been under the *druggist's* for a month previous to being brought to the dispensary, and was then almost moribund, died shortly after the commencement of the treatment. Both these were hopeless cases, and unfavorable for a fair trial of the medicine. There were two doubtful cases, the patients having been removed from town before the cure was completed. In the thirty-three remaining cases, the majority of which were of more than ordinary severity, the average period of cure was about twenty days. But, in most instances, when the patient was at all favorably placed, and came early under treatment, the disorder yielded in from nine to fifteen days.

The remedial influence of the muriate in the disorder is immediate and decided. Under its use the expectoration soon loses its irritating, glairy character, becoming bland and less tenacious, and the paroxysms are rendered milder, less frequent, and of shorter duration; in fact, by its influence the little patient seems to be carried more easily, quickly, if not at the same time more safely through the attack than by the agency of any other remedy with which I am acquainted. In most cases, the muriate was given in mucilage, or with liquorice water, combined with an aromatic, and in doses of one to five grains, according to the age of the child, and repeated every four or six hours.

When pneumonic or bronchial complications existed, or were threatened, antimonial or ipecacuanha, with morphia or hyoscyamus, were added to the ordinary mixture. The only inconvenience observed to result from the use of the muriate was the occasional supervention of a slight mucous diarrhoea, which was easily checked, and did not interfere with the treatment.

I can confirm the favorable opinion of other observers as to the efficacy of the muriate in enlarged lymphatic glands, and in indolent bubo, and can confidently recommend it in scrofulous ulceration of

the lymphatic glands. There are few more intractable cases to be met with in dispensary practice than those of extensive ulceration of the cervical lymphatic glands, which frequently occur in weak, under-fed, and badly-lodged children. In several aggravated cases of this sort which have come under my own observation, some of which presented a chain of foul, ragged ulceration extending from ear to ear, the muriate acted with great rapidity; and in some instances, where iodine, syrup of iodide of iron, and other medicines, had no effect, the ulcerations quickly healed under its employment.

It is also a very excellent remedy in many forms of cutaneous affections, more especially in the scaly variety. I have seen cases of psoriasis inveterata which had resisted the long-continued use of arsenic, iodine, and other remedies, quickly yielded to its influence. It seemed to me to have the most decided effect in those cases of psoriasis occurring in patients of dissipated habits, or when complicated with enlarged liver. It is also very useful in eczema and syphilitic squamæ. Drs. Watson, Ebdon, and others, recommend the muriate in tic and facial neuralgia, and it certainly possesses very considerable curative power over these painful affections, and particularly over that form of neuralgia mentioned by Dr. Watson, which is confined chiefly to the lower part of the face, and in a very troublesome variety affecting one or other side of the neck, and probably connected with a morbid condition of the cervical lymphatic glands.

The muriate, like other remedies in neuralgia, does not succeed in every case; but in those cases in which it proves successful, the beneficial effect generally follows soon after its administration. In my hands the best results were obtained with it in neuralgia when it was given in the ordinary dose, and repeated every half hour or hour.

My experience of the muriate in catarrhus vesicæ, enlarged prostate, muscular rheumatism, sciatica, and other analogous affections, has as yet been too limited to enable me to report with confidence on its value in their treatment; but judging from the result of the trials which I have made already with it in these disorders, I think it deserves the high opinion entertained of it by René, Vaneye, Dr. Fuller, and others. Never having occasion to prescribe the salt in the large doses recommended by some authorities, I have not observed any irritant or injurious effects on the stomach, intestines, or other organs, to follow its employment. When given to adults, in from five grain to scruple doses, in mucilage or bitter infusion, with aromatics and anodynes, it may be continued for a considerable time without producing any unpleasant results. The ordinary dose to adults was from five to ten grains three or four times daily. It was seldom necessary to increase the dose beyond the latter quantity.

The muriate of ammonia is unquestionably a valuable medicine, possessing active curative powers; and having a wide range of action, and being cheap, and therefore the more likely to be pure, it is well fitted for hospital and dispensary practice and deserves more of the attention of the profession generally in this country than has hitherto been given to it—*London Lancet.*

ART. IX.—*Chloroform in Operative Surgery.*i. *Death from Chloroform*: Reported by LAFAYETTE AVERY, M. D.

I observe with pleasure, that you turned your attention to the subject of anæsthetics, and, in a measure, criticize the use of chloroform. I imagine that a small part of the misfortunes resulting from the use of that article have yet seen the light, and that when the lapse of time shall have removed the fear of personal discredit in communicating the whole truth, by those who have had experience with it, a fearful picture will be presented. Having myself, put a fellow-being out of existence with the article, I am anxious to bear my testimony against its use, by relating a case.

In the fall of 1850, I was requested to remove the leg of Waldo Ferris, who had been under the treatment of a quack, for an injury of the kncc, which had resulted badly, rendering an amputation at the thigh indispensable. Ferris had been a strong man, and had indulged in intemperate habits, but now, at the age of 40, was free from disease, except the injured limb. He was considerably emaciated, being much worn down by his disease, yet his appetite was good, and he smoked his pipe and conversed freely. He was prepared for the operation when I arrived, and had an experienced dentist on hand to administer him chloroform. I advised against its use, on account of his debility, but finally yielded to the solicitations of the patient, and allowed it to be given.

He was placed upon the table, and, assisted by Drs. Jaimeson and Ford, I proceeded to the business before us. Some chloroform was poured upon a silk handkerchief and applied to the patient's mouth and nose without intermission, for, as near as I could judge, a minute, when I noticed a labored breathing on the part of the patient, and at the same time Dr. Jaimeson mentioned a partial failure of the pulse. I immediately ordered the dentist to desist with the chloroform, and gave a swallow of brandy, which improved the pulse, but still the patient breathed in a stertorous manner, and his face was congested and purple. No more chloroform was given. I hesitated a moment, and then proceeded to remove the leg and dress the stump, the patient continuing insensible and snoring; face still of a purplish hue, and pulse feeble and frequent. We then put him in bed, and used all the means we were master of to rally him, but without avail. He died in about twenty-five minutes from the time he first inhaled the chloroform, his face purple to the last.

The dentist was a reliable man, and he assured me that he had previously used of the same chloroform, and found it to be good. On examining the vial containing the article, not more than two drachms were wanting, and the dentist said the vial was not full when first uncorked.

Now, this patient evidently died of the chloroform, as the reflections of any physician will satisfy him; and I desire to know what right a surgeon has to use an article that sometimes produces such effects as this. I, for one, have parted company with the poisonous thing, and am very confident that I shall not renew my acquaintance. (*Buffalo Med. Jour.* March, 1859.)

ii. *Further Observations on the Effects of Chloroform on the Results of Surgical Operations: by James Arnott, M. D.\**

Two years ago I laid before the profession the result of a statistical investigation showing, as clearly as can be shown by this kind of evidence, that a great increase of mortality after amputation and lithotomy, had taken place since the introduction of chloroform, and in consequence of its ulterior effects upon the system. But although statistics furnish not only the best but the only reliable evidence which certain truths admit of, they are not satisfactory to those who have not made themselves acquainted with the principles of this mode of investigation; and a prejudice exists against them in medical inquiries particularly, because in these they have been too often grossly abused. To such a degree does this objection amount, that I am persuaded some surgeons would not desist from employing chloroform in certain operations, although it could be thus conclusively proved that half the number so operated upon died in consequence of its use, unless there were symptoms indicating the immediate connection between the cause and effect. Such objectors to statistics admit that foul air, hæmorrhage, intoxication, and other similar causes of debility, prove pernicious by predisposing to fatal disease, only because this has long been an universally admitted truth; and they have probably never reflected that though one of the most important in medicine, this truth was established by statistical investigation alone. To such a person, the evidenc of the cause of death may be satisfactory when a man dies suddenly from descending into a well or cess-pool containing foul or unrespirable gases; but, were the inquiry new to him, he would hesitate to admit that the foul air of a crowded and badly-ventilated hospital was proved to be injurious, merely by the fact that a much greater mortality had occurred in such an hospital than while it was in a different condition, although no other causes of the fatality could be alleged. He would ask, what symptoms or what peculiar diseases were produced by the poison in the air or the insufficiency of its oxygen? and finding there were none of these, he would probably deny the cause assigned and imagine some other one. The same increase of mortality occurring under the same circumstances, in a great many hospitals, and during a period of several years, would probably make no alteration in his opinion.

A singular circumstance connected with this inquiry, though under the circumstances perhaps a natural one, is the contrast between the unsuspecting eagerness with which the statistics of Dr. Simpson and others favorable to chloroform were received, and the opposition since made to those of a contrary character. The decrease of mortality supposed to be shown by the former was attributed without one dissentient voice to the introduction of etherisation. No improvement in operative processes, nor in the ventilation or general management of hospitals, was allowed to share the credit. But when more correct and more extended statistics bring the most undeniable evidence, that instead of a decrease there has been a great increase

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\* The length of Dr. A.'s papers prevents their insertion in the N. O. Med. and Surg. Jour. entire; but the most important portions will be reproduced.

of mortality since the introduction of chloroform, the ingenuity of those who looked fondly upon this agent as a faultless boon, is set at work, and various other more or less plausible explanations of the fatality are forthwith afforded.

I have called the evidence of the statistics showing a great increase of mortality undeniable, because the facts or data constituting them are admitted to be authentic and accurate, and because no objection has been made to the manner in which they have been used, that has not been refuted. The tables in which these were collected, and which appeared in the *Medical Times and Gazette* of October 25, 1856, and April 25, 1857, contained the data that had been previously published by Dr. Simpson, in addition to those of equally authentic character that had subsequently been included in the periodical hospital reports contained in that journal.

As it has been incontestably shown by above a thousand cases of unquestionable authenticity, that an increase of mortality after amputation and lithotomy has occurred since the introduction of chloroform, to the extent of about twelve per cent. as regards the first of these operations, and twenty-four as regards the other, it must follow, that as the surgeons who continue to recommend it on these occasions cannot possibly think the freedom from pain in the survivors a sufficient compensation for so great a sacrifice of life, some other cause or causes than chloroform must be supposed by them to have produced this mortality. My principal object in returning to the subject is to inquire into the validity of the causes that have been so assigned since the publication of my paper.

In searching for a cause of this general increase of mortality after the severer operations, we have first to inquire whether any great change of practice, or of other circumstances bearing on the result of operations, has occurred within the period indicated; and when we have found that the introduction of chloroform is the only great change, we have next to inquire, whether, from what we know of its powers or properties, it is likely to have been the cause of the increased mortality? The freedom from pain obtained by the use of this drug is an immense advantage, but, unfortunately, this is counterbalanced in many cases by its injurious properties. Chloroform has been proved to be a virulent poison by experiments on animals, by the numerous sudden deaths that have occurred during its administration, and by the suffocations, faintings, etc., bordering on death, that have still more frequently been produced by it. That its agency is not of that transient character which has been represented, is evident from the extreme and long-continued prostration that so often follows it, as well as from the persisting vomiting, headache, shivering, and other symptoms denoting severe constitutional disturbance. It has been detected in the blood by chemical analysis several days after its administration. But the fact, that in such cases as those mentioned by Mr. Mouat as happening in the Crimea, death has taken place hours or days after it has been exhibited, and without any intermission of the unfavorable symptoms produced by it, sets this question at rest. That in other cases its pernicious agency should be unaccompanied by any peculiar symptoms, is not more extraordi-

nary than that the pernicious agency, or predisposition to disease, caused by foul air, hæmorrhage, insufficiency of food, or intoxication, should not be marked by any peculiar signs or symptoms.

Enough has been said to show that there is nothing in respect to the nature or properties of chloroform which renders it unreasonable to attribute the great increase of mortality that has unquestionably taken place after certain dangerous operations to its universal employment. There can surely be no doubt that we have abundant evidence of its noxious properties, both immediate and more remote, to consider it a real or sufficient cause. But, doubtless, other circumstances or changes in the practice of surgery, insignificant though they may be in comparison with the introduction of chloroform, have taken place during the last ten years, and as they may have influenced the result of operations, they must not be passed over in the present inquiry. Some of these circumstances have had the tendency to increase the former mortality after amputation and lithotomy; others to diminish it.

It has been said that lithotomy is more fatal than formerly, because lithotripsy has superseded it in all the more favorable cases. The cases, however, of this description are too few to have caused a perceptible difference. In only one instance was lithotripsy performed in the London hospitals during the last year of the period of my statistics, and it is expressly mentioned in the report of the case, that lithotripsy was preferred to lithotomy, because the circumstances were unfavorable for the latter operation. In the same way, the substitution of excision of joints for amputation has been supposed to account, partly at least, for the increased mortality.

But here again the cases were, during the period referred to, not only too few to make an appreciable difference, but in the greater part of those that did occur, amputation would not have been performed in so early a stage of the disease. There can hardly be a doubt, however, that frequently when excision has been performed, rest, and the other remedies of inflammation, would have effected a cure; and in the cases where for the removal of mere deformity excision has been adopted, it is unlikely that amputation would have been employed. When an operation is novel, or is confined to a few operators taking a peculiar interest in it, the occasions for the supposed necessity of its performance multiply wonderfully. We have another curious illustration of this in the new operation of perineal section for stricture of the urethra, and in the remarkable difference of success that has attended different operators. When it is only made use of as a last resource in comparatively unfavorable cases, we cannot expect the same number of recoveries as when, instead of the exception it has become the rule, and is systematically employed in cases that might be equally well relieved by measures unaccompanied with danger.

Mr. Erichsen, while admitting in his "System of Surgery," that part of the increased mortality after operations may be accounted for by the deleterious agency of chloroform, thinks that another part is due to the absence of pain inciting the surgeon to operate in cases in which, from the little chance of success, he would have formerly de-



clined operating. This may have occurred in certain instances; but has not any such effect been more than counterbalanced by the fact that since chloroform has been in use, many cases in which operations have been performed, have been more favorable than they otherwise would have been? The constitution of the patient has not suffered from a long-continued dread of pain; nor have operations been delayed, and the chances of recovery lessened by dread. To this advantage of chloroform must be added its effect in putting nervous and inexperienced operators at their ease, by preventing the cries and struggles of patients, and thus enabling them to operate more correctly and safely.

Those who have endeavored to account for the increase of mortality by alleging that for the above or other reasons, such as improved means of cure, there is a less number of amputation cases than before the introduction of chloroform, and that these cases are of a more unfavorable kind, do not reflect that the period referred to is much too short for such a change to have occurred in it. It is very true that forty or fifty years ago, amputation was more common than at present; but it is long since the discovery was made that the common disease of the joints for which it was then almost invariably resorted to, namely, white swelling generally admits of better remedies. Within the last twenty years there has been no such change of practice.

Etherisation is too valuable an addition to therapeutics to stand in need of such an apologist, etc. The time may come, when by the aid of advancing science, this powerful agent may be employed without danger; but, in the meanwhile, we cannot disregard such a startling fact as that related in the last number of the *Medical Times and Gazette*, namely, that the mortality from lithotomy on the adult is now more than fifty per cent. of those operated upon. Before the introduction of chloroform, it was only twenty-two and a half per cent. Such facts waken us from the dream that chloroform prevents death as well as pain. Ignorance of them may be bliss to the patient who is persuaded to use chloroform, but it is the duty of the surgeon to make himself acquainted with the real state of the case; for surely, as respects him, it is not "folly to be wise."—(*Med. Times and Gaz.* Jan. 1859)

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ART. X.—*Longevity.*

"CAN the limits of human life be extended? This is the question propounded by a celebrated professor of chemistry in a memoir presented to the Academy of Sciences: 'upon the causes of old age, and senile death.' This chemist is M. Ed. Robin. He believes human life may be prolonged, and courageous in this opinion, seeks for rational

means to arrive at his conclusion. He will seek a long time still, says the reader. Such is our opinion, and, no doubt, M. Robin's, who, waiting further developments, furnishes us matters of interest on this topic.

“Living beings may be compared to furnaces, always kindled; life exists only in a state of combustion; but the combustion which occurs in our bodies, like that which takes place in our chimnies, leaves a residue, a detritus, ashes. This detritus, which is always accumulating, is, according to M. Robin, the principal cause of old age and senile death.

“Food, whatever may be its nature, whether vegetable or animal, liquid or solid, is charged with mineral matters which are left in different parts of the organism by the process of combustion. At first, these serve for nutrition; but when the bones are all consolidated they continue to flow into the system, and then incrust and mineralize different parts of the mechanism.

“The manner in which mineralization causes old age in man is clearly pointed out: on the one hand, ossification of the cartilages of the sternum, greater rigidity of the posterior ligaments of the ribs cause the respiration to become slower and slower, limits its extent, and finally render it altogether diaphragmatic: on the other, ossification of the vessels and their valves, diminution of the calibre of the arteries, obliteration of the capillaries, reduction of their number, and enlargement of the pulmonary cells, render the circulation more and more difficult, and diminish the respiratory surface.

“The air coming in less close contact with the blood does not so well aërate it, this liquid therefore becomes of a deeper color, engorges the venous system, as in asphyxia; and experiments upon the quantity of carbonic acid exhaled, upon animal temperature, upon the passage of certain elements of the blood in the urine, do not permit us to doubt, that beginning with a certain age, a combustion gradually less abundant takes place.

“As the combustion and heat diminish, so do the electricity and nervous fluid, therefore, sensibility and contractility are lessened, motion and the general activity decrease; enfeebled by all these causes, the nervous action contributes in its turn to diminish the combustion, and . . . . all the world knows the result of this.

“By force of such arguments, which have some good in them, M. Robin hopes to prove, that it is easy to retard old age, and death in

a large number of animals, by abating the phenomena of slow combustion.

“In consequence he proposes to institute three series of experiments upon animals whose lives are of short duration. One class will be fed upon those aliments which contain the smallest proportion of mineral matters of an incrusting nature ; another upon food entirely deprived of these matters by appropriate dissolvents, and the last upon ordinary food ; but with it is to be administered, beginning with a certain age, lactic acid, which has incontestably the power of dissolving mineral matters, and would appear to be sufficient to dissolve, during life, those which have been already deposited in the organism.

“If it be demanded, what we think of the subject which M. Robin has chosen for his researches, we would reply that we highly approve them, and encourage the author to persevere. We well know what will be said : Paracelsus ! Van Helmont ! etc. But names, whether good or bad, have but little weight with us ; and, for approving the researches of M. Robin, which are connected with one of those beliefs the most generally and deeply felt by humanity, which, in our opinion, would alone justify them, we have another reason still, or a principle which we may reduce to the following formula : *Every quality which appears to be an exception in a species, indicates a new rule, to which this species may be subjected.*

“Applying this principle to the present subject, we say, there are macrobites or centenarians in the human species ; then macrobie is compatible with the human organization, and since it exists, its cause may be determined. Now, to possess a knowledge of the cause, is to be master of the effect ; and that which has heretofore been the exception, may become the rule.

“The long life of the patriarchs provokes a smile. But in times less distant from our own, facts may be found which are but little more credible, and which cannot be disputed. It may gratify our readers to cite some of these facts :

“Ponce Lepage died in 1760, in the Duchy of Luxemburg, at the age of 121 years ; a short time before his death he cultivated his field, and made excursions on foot of six or seven leagues. Eleanor Spicer, died in Virginia, in 1763, aged 121 years. All her senses were perfect at the time of her death. Madame Barnet, died in Charleston in 1820, aged 123 years. She recalled perfectly events which had oc-

curred a century before. Grandez died in Languedoc, aged 126 years. He was a journeyman goldsmith, and was in the habit of working still, ten or twelve days before his death. The Englishman John Newell died in 1761, at the age of 127 years, in the full enjoyment of reason. Another Englishman, John Bayles, a sheep-seller, died in 1706, aged 130 years. During the last years of his life he drove his flocks of sheep to the neighboring markets. Margaret Lawler, English, died in 1739, aged 135 years. A few days before her death she walked some three or four miles and returned home the same day. Joseph Barn, a negro, died in Jamaica in 1808, aged 140 years. A few days before he was in the habit of walking four miles. Polotiman, a surgeon in Lorraine, died in 1825, at the age of 140 years. The day before his death he operated upon a cancer with much dexterity. Thomas Parr died in London in 1635, at the age of 152 years. Until he was 130 years old he engaged freely in all the labors of a farmer, and even threshed his wheat.

“Obst, a village woman of Silesia, died in 1825, aged 155 years. She had labored in the field the evening before her death. Joseph Surrington, a Norwegian, died in 1797, aged 160 years. He preserved to the last his reason and his senses. John Bowin, born in the Bannat of Tameswar, died in 1740, aged 172 years. Peter Zortan, a countryman of the preceding, died in 1724, aged 185 years.

“If the life of these extraordinary beings be examined, it will be found very difficult to determine the causes of their longevity; the privilege which they enjoyed appears to have been compatible with every mode of living. For instance, Annibal Camoux, who died when 121 years, and who figures in one of Horace Vernet's paintings, drank much wine, and lived upon the coarsest food; so also the surgeon Polotiman, never passed a day without being intoxicated; the peasant woman, Obst, who died at the age of 155, drank ordinarily two tumblerfuls of brandy daily. [At a recent meeting of the Detroit Historical Society, it was stated that a French resident of that city died a few years since at the age of 116 years, during 104 of which he never drew a sober breath.—Ed.] From such examples it might be concluded that drunkenness gave one a lease for a long life.

“But on the other hand, Eleanor Spicer, who lived to her 121st year, never drank spirituous liquor; Grandez, who died at 126 years, never drank wine; John Effingham, who died at the age of 144 years, knew liquor only by sight. In addition to this contrast, here are

some facts through which it is difficult to see clearly: Denis Guignard, who died at 123 years, dwelt in a cavern dug in sandy stone; Drahekemberg, who died in his 146th year, had been captured by Corsairs, and endured for fifteen years all the hardships of a cruel captivity; Jean Lafitte, who died at the age of 136 years, had contracted from early youth the habit of bathing two or three times a week, and preserved this habit to the end of his life; Jean Causenr, who died aged 146 years, lived chiefly on milk food (*laitage*); Jean d'Outrego, who died at 146 lived upon Turkey wheat and cabbage; Thomas Parr, who died aged 152 years and 9 months, fared all his life upon bread, old cheese, milk, whey and table-beer; and, in conclusion, Peter Zortan, who died at 185 years of age, lived solely upon vegetables.

“All of this is sufficiently contradictory, nor is it easy to deduce from these facts, rules for a regimen proper to produce macrobie; so that it is not here that it is necessary to seek for them. The knowledge afforded by these facts is sufficiently precious without demanding more from them; like all exceptions to natural laws, they furnish us with a revelation. By proving to us that human life may be prolonged far beyond its ordinary limits, they invite us to researches, which, in their absence, would not have been thought of, except to reject at once the idea of them. It is now left to us to discover the causes, and to conquer the means to obtain the result. This research does not appear to us a matter of indifference; and if, in order to enjoy longevity, it is sufficient to subject oneself to a regimen of lactic acid, we would willingly submit to it.

“If it were true, that the art of greatly increasing the duration of human life, was accessible to us, we could only applaud the contrast of future longevity, with the brevity of existence in the past. However short the life of our fathers, it sufficed for them to sow an ample harvest of sorrows; and however long may be the lives of our sons, or ourselves, it will not suffice to exhaust the noble pleasures which the remunerating future holds in reserve for good and honest men.”

Dr. Legrand remarks upon the above: “So let it be, say we; and we have only to add that Harvey has left us some curious details in regard to Thomas Parr, whose autopsy he made. He was married at 120 years, and at 130 he was summoned before the House of Commons for a misdemeanor. Harvey is said to have been an eye witness to a coition successfully accomplished by Thomas Parr, at 140 years

of age. One is not a great anatomist without being possessed by the demon of curiosity!"—[Translated from *L'Union Médicale*.]

In the "Curiosities of Medical Experience" it is stated that "Henry Jenkins lived to 169, and we have on record the case of a negress, aged 175. The Hungarian family of John Rovin were remarkable for their longevity: the father lived to 172, the wife to 164; they had been married 142 years, and their youngest child was 115; and such was the influence of habit and filial affection, that this *child* was treated with all the severity of paternal rigidity, and did not dare to act without his *papa's* and *mama's* permission."

To these might be added a number of reliable cases who lived more than 150 years, and some to 180. We will mention only three of the most recent: Marie Prion died in 1838, aged 158 years. A Polish peasant died in 1834, aged 188 years; and about ten years since, Madelon, a negress and a native of Louisiana, died at Pass Christian, Mississippi, who had seen her century and a half. Her daughter and grandson, living at that time, had reached a very old age; and Madelon is reported not only to have recovered the sight, which she had lost, but to have been blessed with a third set of masticators.

These instances of modern longevity suffice to prove that, in this respect, at least, mankind is not retrograding. The oldest of the biblical patriarchs did not surpass them. Abraham attained but 175 years, and his son Isaac only 180.

As to ancient Methuselah and the balance of the antediluvians, it is supposed, for good reasons, that their years were estimated by our seasons, and should consequently be divided by four. However, in non-medical works are recorded many cases of persons who lived two, three, and even more than four centuries; but, once trusting ourselves to these, we find such records vastly eclipsed by the equally reliable case of the Wandering Jew, who was *actually seen* by various parties some 600 years ago, and is thought by some credulous individuals to be still engaged in those peregrinations which he has so industriously prosecuted for the last 1800 years and more!

Dr. B. Dowler, in his researches upon the vital statistics of New Orleans, found in the African Cemetery that there was one centenarian in every fifty interments selected at random, which gives a proportion thousands of times higher than is recorded of any other race, or in any other part of the world.

[Although these enumerations (here alluded to by my friend and

editorial associate, Dr. Chaillé,) were made at random, they were all taken from *monumental inscriptions*, and may virtually (without on my part intending it) have the *numerical effect of selection, if, as is probable, the ratio of monumental inscriptions be less for infants and young persons than for the adult and the aged class.*—B. DOWLER.]

There are various sources of error which might render these selections at random unreliable as a true indication of the ratio of centenarians to the total mortality ; still there is no doubt that among the natives of Louisiana and our Gulf shore, there is to be found a larger proportionate number of persons who have passed their hundredth year, than in other parts of the United States or in Europe. The official statistics of the deaths in New Orleans for the past three and a half years, give us sixteen centenarians, which is one in less than nineteen hundred of all the deaths. If from this calculation the yellow fever deaths be excluded, as they should be (since these are among our unacclimated strangers), the proportion would amount to one in less than fourteen hundred. In either case the ratio is far more favorable than is afforded by any European statistics.

The United States census for the decade ending with the year 1850 equally proves how favorable our Southern climate is to longevity, the number of centenarians being vastly greater, in proportion to the population, in the Southern than in the Northern States, and this holds true whether in reference to the black or white race, the slaves or freemen. In no States is the proportion of centenarians so large as in Texas and Louisiana.

The learned physiologist, M. Flourens, has written an interesting work on "Human Longevity." He concludes that the normal duration of man's life is one hundred years, and fortifies this conclusion by researches in comparative anatomy and physiology. He states that the duration of life, in mammalia at least, is proportionate to the duration of growth ; the duration of growth to that of gestation, and the duration of gestation to the height. The larger the animal, the longer the gestation, etc. Buffon and other naturalists, as far back as Aristotle, have asserted that there was a fixed ratio between the duration of life and that of growth ; but M. Flourens claims to have discovered the certain sign that marks the term of growth, which is the union of the bones with their epiphyses, after which the animal necessarily ceases to grow.

He finds in all animals which he has subjected to his observation

and investigations, that the duration of their lives is five times that of their growth. For instance, the union of the bones with their epiphyses (*i. e.* the term of growth) occurs, in the camel at 8 years, and this animal lives about 40 years; in the horse at 5 years, and this animal lives about 25 years; in the ox at 4 years, and this animal lives about 20 years; in the lion at 4 years, and this animal lives about 20 years; in the dog at 2 years, and this animal lives about 10 years; in the cat at  $1\frac{1}{2}$  years, and this animal lives about 8 years. From these and many similar examples he concludes, that, since in man the union of the bones with their epiphyses is effected at 20 years of age, the normal duration of his life must be 100 years.

We may conclude this paper, which, perhaps, is more curious than instructive, with the following quotation from Flourens: "Just as the duration of growth, multiplied a certain number of times, say five times, gives the ordinary duration of life, so does this ordinary duration, multiplied a certain number of times, say twice, give the extreme duration. A first century of ordinary life, and almost a second century, half a century (at least) of extraordinary life, is then the prospect science holds out to man." We leave it for those who love it, more than the undersigned to say, Amen!

CHAILLÉ.

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ART. XI.—*Observations on Longevity, with cases; supplementary to the preceding article:* By B. DOWLER, M. D.

THE maximum estimate of the world's population among the best statisticians is 1,200,000,000 souls; of this number sixty-one die every minute. And although a considerable number die of accidents, an overwhelming majority perish by diseases, which the future progress of medicine may possibly, for the most part, prevent or cure, until the natural decacy of old age

" Shall end this strange, eventful history."

It is but sixty-three years since Jenner vaccinated the first child. Before his discovery, according to some of the best statistical writers,



one-thirteenth of the human race died of small pox. Here is a remedial measure of easy application, which, if rigidly enforced, eliminates the most fatal of all exanthematous fevers from the bills of mortality, and consequently enhances the mean duration of life, while it affords presumptive proof that similar means of prevention or cure are within the category of possibility in relation to other maladies. If a preventive or cure should be discovered for consumption, one-fifth of the human race would be transferred to the account of longevity, or, at least, to that of an increased expectation of life.

Hygiene and morality—both eminently practicable, but generally not practised by erring humanity—are to a great extent adequate to the task of eliminating many of the causes of premature death. Thus, intemperance, war, murder, syphilis, and many similar causes of the short duration of life, would be extinguished by observing the merely negative rule of virtue found in the first reading lesson of the old school primer, namely, "*My son, do no ill.*" Inaugurate morality, hygiene, the physical comforts; let physic and surgery advance as they have already advanced during the present half century, and each revolving century will add, perhaps, an improvement in the human constitution, with an increased mean duration of life, and probably an increasing number of extreme cases of longevity. Every day's occurrences confirm the Sacred Record that many "do not live out half their days," simply because they are not temperate, just, virtuous and provident. A degraded moral status conjoined to the physical evils of extreme poverty and bad hygienic conditions, with a reckless neglect of the incipient stages of disease, are the great abbreviators of human hopes and life, and not solely the decrees of the Eternal.

The learned Dr. Sherlock, who lived two centuries ago, in his work on Death, in reference to the question of the assumed absolute and unconditioned determination of each person's life, says: "This is that famous question which Beverovicus, a learned physician, was so much concerned to have solved, and consulted so many learned men about; as supposing it would be a great injury to his profession did men believe that the time of their death was so absolutely determined, that they could neither die sooner nor live longer than that fatal period, whether they took the advice and prescriptions of physicians or not. But this was a vain fear; for there are some speculations which men never live by, how vehemently soever they contend for them: a sceptic who pretends there is nothing certain, and will

dispute with you as long as you please about it, yet will not venture his own arguments so far as to leap into fire or water, nor stand before the mouth of a loaded cannon when you give fire to it. Yet these men will eat and drink, to preserve themselves in health, and take physic when they are sick." (140-1.)

During the historic period, outside of the longevity recorded in the Sacred Writings, it is probable that the average or mean duration of life has increased, owing to the progress of medical knowledge and the increase of the physical comforts throughout the civilized world. Authentic examples of great longevity seem to have been also greatly multiplied.

Pliny, the Elder, nearly contemporaneous with the beginning of our era, in his vast work of fact and fiction (*Historia Naturalis*) gives the most remarkable examples of longevity among the ancients. He mentions some instances of longevity taken exclusively from the region between the Apennines and the Po, as found on the record of the census instituted by Vespasian, and within these narrow limits he enumerates fifty-four persons who had reached the age of 100 years, fourteen of 111, twenty of 125, forty of 130, forty of 135, thirty of 140. In the single town of Valciatium, near Placentia, he mentions six of 110, four of 120, one of 150 years."—(*New Am. Cyc.*; New York, 1858.)

Before proceeding to make a few remarks on longevity, it is intended to give an enumeration of cases from 120 years upwards, having excluded such as Dr. Chaillé has already given, nearly all of which, however, had been previously noted in my list. Imperfect as the latter is, I am not aware that any one so extended has hitherto been published. I have excluded cases recorded by authorities, when contradicted by other authorities. The authorities which are relied on in this catalogue are not generally named, because such citations would occupy much more space than the facts themselves. Owing to my limited leisure, I may have omitted to copy not a few cases which I have on record, but not easily found on this occasion.

DIED, AGED 120: *Death of a Venerable Servant.*—Sarah Mallory, a colored woman belonging to the estate of the late Capt. Gilbert, died at Norfolk on the 22d of March, 1859. She is said to have been born in 1740, and was therefore in the 120th year of her age. The *Day Book* says: "Her youngest child attended her funeral, as the last of the family. His back is bent, and his locks frosted o'er with

the snows of eighty-seven winters. She remembered many of the exciting scenes which were enacted during the Revolution, and the surrender of Cornwallis at Yorktown was but as yesterday to her. At that time she was forty-one years of age, and her youngest child nine years old." *Heléne Massy*, free woman of color, at Mr. James Papplye's plantation, July 30, 1849. (*N. O. Delta.*) *Mary Good-sall*, negress, at Spanish Town, Jamaica (December, 1830); *Mrs. Mary Inness*, Mount Grace, Jamaica (December 20, 1820); *William Marshall* (1792); *Flora Gale* —; *Dominick Joyes* (1765); *Moneula*, negress (1780); *Mrs. Moore* (1765); *John Mackay* (1766); *Sir Fleetwood Sheppard* (1768); *John Ryder* —; *Mrs. Adams* —; *John Chump* (1769); *Mrs. Sands* (1770); *Patrick Blewet* —; *Richard Gilshenan* (1771); *Barbara Wilson* (1772); *Sieur de la Haye* (1774); *H. d'Arcary de Beaucovoy* (1778).

LIVING AT 120: "*Old People.*—The natives of Louisiana are remarkable for the extraordinary instances of longevity which they afford. During the past six months we have noticed the death of several old persons who had exceeded the age of 100 years. We, the other day, saw a negro woman in Conti street, 109 years of age, who lives with her daughter, 82 years old. Her eye-sight, hearing and health are tolerably good. A more extraordinary case of longevity is that of an old French lady living at No. 140 Marigny street. Incredible as it may seem, she is said to have attained the age of 120 years, and the story seems to be true, for there are old people alive now who remember her a very old woman in their youth. Of course she is extremely infirm, and the cold weather of the present winter has affected her health so much that her demise is daily expected. This climate is undoubtedly one of the most favorable to longevity in the world."—*N. O. Bee*, Feb. 6th, 1856.)

DIED, AGED 121: *C. Lane*, of Campbell county, Virginia (1821); *Elizabeth Hilton* (1760); *Francis Bons* (1769); *Mrs. Gray* (1770); *William Farr* —; *Owen Tudor* (1771); *Margaret McKay* —; *John Whalley* (1772); *Henrietta Long* (1788).

AGED 122: "*Death of the Oldest Man in Pennsylvania.*—*Timothy Sweeny*, aged 122 years, died in Fairview township, Butler county, on the 27th ult. Mr. Sweeny was born in the year 1737, in Curahan, parish of Ardfert, county of Kerry, Ireland, and emigrated to this country in 1837, being then 100 years old. He was never known to have had an hour's sickness, having the full use of his faculties to the

last, with a delicacy of hearing and quickness of perception that was really miraculous for one who had journeyed so far beyond the allotted bounds of earthly existence. His last day on earth was spent, as usual, at the genial fireside of his daughter, Mrs. Nolan, surrounded by affectionate grand-children; and nothing occurred to warn them that in the sleep of the night the patriarch was to sink silently and at once into the deep slumber of the grave.—(*Pittsburg Post*, March, 1859.) Margaret Annesley (1752); Catharine Giles (1758); Mrs. Carman (1771); Andrew Brizin Debra (1774); Mrs. Neale (1785); Archibald Cameron (1791).

AGED 123: Martha Preston (1769); Jean Arragus (1779); Matthew Taite (1792).

AGED 124: Thomas Wishart (1760); Catharine Brebner (1762); Andrew Vidal (1774); Abraham Vanverts (1774); Thomas Bright (1708); Elizabeth Stewart (1725); Andrew Bueno (1753); Robert Parr (1757).

AGED 125: John Dance, Virginia; — Rice —; John Tice (1774); Mr. Gernon (1780); Mr. Frome (1875).

AGED 126: Robert Montgomery (1670); John Bales (1706).

The journals of 1844 contained the following statement: Mr. John Hightower, of Marengo county, Alabama, died in August, 1844, aged 126 years; was a soldier of the Revolution; was in Braddock's defeat, and was wounded in that battle. His age can be established by an authentic family record.

A negress living in Virginia, aged 126: A correspondent of the *Richmond Enquirer*, writing from Powhatan, December 30th, 1854, says:

"There is a negro woman in Powhatan, now living in my immediate neighborhood, whom I have talked with, who was born the year after George II. ascended the throne of England, and four years before the birth of George Washington! She is now 126 years old, and was, of course, very near half a century old at the time our Declaration of Independence was proclaimed, the 4th of July, 1776.

"I state this upon information that I believe to be true, and the appearance of her person serves to confirm it. Her memory seems to furnish her an indelible record of all the events, great and small, of her long life, when aroused from the state of drowsy forgetfulness that frequently betides her. When I saw her she was wide awake and full of chat. She had remarkably fine eyes, and, I was told, could thread a needle and sew nearly as well as she ever could.

“ She said she had been the mother of sixteen children, all of whom died of old age, and that there was precisely one year and one day between their births, respectively ; that she had never been sick, never had a physician to see her, and never took a single dose of physic in her life. She talked cheerfully and fluently, and quoted many passages of scripture readily and appropriately.

“ Whenever she touched upon religion, her mind seemed to become absorbed at once ; and the (to me) unexpected fluency and beauty of her language indicated ‘ the gift ’ that we sometimes hear of. Though no doctor, I was prompted by curiosity to feel her pulse, to see if it beat like other people’s. I found it quite regular and strong. I inquired if she had never lost her eyesight. She said, ‘ No, never, nor appetite either.’ ”

AGED 127: Davie Grant (1658); Mrs. Bampton (1758); William; Hughes (1769); Madame Giròdolle (1772); Daniel Mullecry (1775) Martha Jackson (1776); John Newell (1761).

AGED 128: John Jacob, of Mount Jura —; Mary Yates (1776); Edglebert Hoff (1755); Mary John —; Mr. Fleming (1771); Abraham Strodtham (1772).

AGED 129: Thomas King (1768); Joseph Gale (1769); John Gough (1771).

AGED 130: Donald Cameron (1759); John De la Somet (1766); George King; John Taylor (1767); William Beaty (1774); John Watson (1778); Robert Macbride (1780); William Ellis; Satira, negress, Antiqua (Feb. 17, 1823); Tom, belonging to Mrs. Bacon, South Carolina (1829); Lucretia Stewart, Jamaica (1817); Margaret Darby, negress, Fellowship Hall, Jamaica (April 6, 1821), retained her faculties to the last.

AGED 131: Elizabeth Taylor (1764); Peter Gardner (1775).

AGED 132: Jeremy Gilbert, Northamptonshire.

AGED 133: Elizabeth Merchant (1761); Mrs. Keith (1772).

AGED 134: Francis Ange (1767); John Brookey (1777); Jane Catherine Lopez, negress, Kingston, Jamaica (1808).

AGED 135: Jane Harrison (1714).

AGED 136: James Sheile (1759); Catherine Noon (1768); Margaret Forster (1761); John Mouat (1776).

AGED 137: John Richardson (1772); — Robertson (1793); H. Francisco, Whitehall, near New York (1820).

AGED 138: William Sharpley (1757); Joan McDonogh (1768);

Mrs. Clum (1772). LIVING CASE: — Fairbrother of Lancashire, was still living, (1771).

AGED 139: Thomas Dobson (1766); Mary Cameron (1785).

AGED 140: A negro woman died on the 4th of July, 1858, at Kenner, aged 140 years. She was born in Guinea, Africa, and retained her faculties until within a short period of her death. She belonged to Minor Kenner. Willam Leland (1732); James Sands (1770); Rebecca, negress, Falmouth, Jamaica, her age having been correctly traced from the deeds of her owners; Abraham Paiba, Charleston, South Carolina.

AGED 142: Swarling, a Monk (973). About fifteen years ago, (1844), the following case was reported and credited as true: Late papers from the Island of Jamaica announce the death, at Spanish-Town, of a black man named John Crawford Ricketts, at the extraordinary age of one hundred and forty-two years; and, what may be considered as very unusual, he was in good health till within about two weeks of his death.

AGED 143: Charles McFindley (1773). Baron Humboldt mentions, that while he was in Lima, an Indian, Halario Pari, died at the age of 143.

AGED 145: Evan Williams (1782); Countess of Desmond.

AGED 146: Thomas Winslow (1766); Joseph, negro, of the estate of Morice Hall, Jamaica (1821); Anne Wignell (1812).

AGED 147: Solomon Nibel, in Laurens District, South Carolina.

AGED 148: William Mead (1652).

AGED 150: Francis Confit (1768); Marcus Aponius, Ramino, Italy; Marc Albuno, Ethiopia; Catherine Heath, Jamaica (Sept. 3, 1831), never had a child, retained her faculties to the last.

AGED 151: Mrs. Judith Crawford, of Spanish Town, Jamaica (Nov. 21, 1829).

AGED 152: John Bowels (1656); Henry West, of Upton, Gloucestershire.

AGED 154: Thomas Damme (1648).

AGED 160: Robert Lynch, a negro, the property of Sir H. East, at St. Andrews, Jamaica (Dec. 5, 1830).

AGED 164: Mrs. Margaret Moser, Montgomery County, Pennsylvania (in 1854), aged 164 years, four months and seventeen days. (*Baltimore Sun*.)

AGED 175: Louisa Truxo, negress of Tucomea, South America (1780).

AGED 180: At Fredericktown, United States, a mulatto (1797).

*Longevity of certain Animals.*—Mr. Wilson relates from Hufeland's *Art of Prolonging Life*, that a gentleman from London, a few years ago, received from the Cape of Good Hope, a falcon that had been caught with a golden collar, on which was inscribed in English, AN. 1610. It had been at liberty 182 years.

M. Buffon says that Count Maurepas had in a moat of his, a carp known to be 150 years old. Gesner says that a pike was caught in Suabia, in 1497, having an inscription dated in 1230, showing that its age, at the least, was 267 years. The Rev. Mr. Kirby, in his *Bridgewater Treatise* (372), says: "A pike was taken in 1754, at Kaiserslautern, which had a ring fastened to the gill-covers, from which it appears to have been put into a pond of that castle by the order of Frederic II, in 1487, a period of 267 years."

In the fourth volume of Buffon's Natural History, the late Mr. Audubon, of Louisiana, is quoted as saying, that in company with Mr. Augustin Bourgeat, he killed an alligator seventeen feet long: "It was apparently *centuries old*; many of its teeth measured three inches; we took some of them for powder charges."

On Thursday, the 23rd of July, 1858, while Mr. Joseph Hartman, of Bedminster, was in his meadow, with the mowers, he found a large land tortoise, upon the bottom of whose shell there was the following inscription in legible characters, viz: A. E. F., 1736, which would make it at this time 122 years old.—(*Doyleston (Pa.) Democrat.*)

The *Leicester* (England) *Mercury* (1854) has the following statement:

"A GOOSE NINETY-THREE YEARS OLD.—Mr. Everett, farmer, of Kirby Lodge, near Rockingham, has a goose which he vouches to be at least ninety-three years old. It has been on his farm full fifty years, and passed the former part of its life on the farm adjoining. It is a large fine fowl, with a head and neck as white as snow, and has lately hatched a brood of goslings from its own eggs. Mr. E. has a book stating its age and history, which he can authenticate."

But it is time to pass from this digression to the starting point, human longevity.

A few examples of long life, yet not reaching 120, relating chiefly to the South, will be subjoined:

"*Longevity.*—Instances of extreme old age are perhaps more fre-

quent in this city than in any other city on the continent. Every week or so we hear of the demise of some ancient individual whose years run up to the neighborhood of a century, and occasionally of one who has made a commencement on his second century. Yesterday a free woman of color, named Suzanne Blandine, died at her residence, No. 8 Greatmen street, at the age of 105. She was a native of St. Domingo. As she died suddenly and unexpectedly, the coroner was sent for, but the decision of the jury of inquest was that she had died of old age."—(*N. O. Picayune*, Feb. 21, 1856.)

"*Longevity*.—We learn, on unexceptionable authority, that Victorie Simon Mercier, a colored woman, who died yesterday in a house on Royal street, between Conti and St. Louis streets, had reached the patriarchal age of 111 years. She was the eldest sister of the wet nurse of the father of Dr. Mercier—fourteen years older. She remained active and sensible to the last. On Friday last she walked a distance of three squares."—(*N. O. Picayune*, Oct. 3, 1855.)

In the parish of St. James, December, 1846, a negro died at the age of 110.

In his History of South Carolina (1808), Dr. Ramsay mentions that during the eleven preceding years, one person had died in that State, aged 100; two at 103; one at 104; one at 105; one at 108; one at 109; one at 112; one at 114. He further mentions that two were then living in the same State, at the age of 111; three at 102; one at 104; one at 107; two at 110. (ii, 418, *et seq.*) He says: "Mr. Neighbors died aged 114—his wife 109. The latter at the age of 105, broke her thigh in three places, which healed very kindly."

Dr. H. Howard, in his history of North Carolina (ii, 288-9, A. D., 1812), says: Mrs. M'Allister, died in 1798, in the town of Fayetteville, N. C., aged 112. Living in the same village: Mrs. Tommie, at the age of 106; Mrs. Cruise, 105; James Mears, 106; Ab. Grimes, 104; Duncan Campbell, 108; while others were living at the age of 106, 103, 100, etc. In 1794, William Taylor, aged 114, and William Howard, aged 108, were living in North Carolina.

Benjamin Bushe, born in Massachusetts, died at Greensborough, Vermont, March 21, 1845, aged 115.—(*Nat. Intel.*)

M. Ferry, who at the age of 82, wrote the article *Vieillesse*, in the *Dict. de la Conver.*, thinks that couriers, particularly in Chili, oftenest attain great longevity. The speed demanded of these runners or couriers requires great muscular exertion with consequent exposure,



which more recent statistical observers consider, though contrary to former opinion, unfavorable to health and longevity.

The effect of occupation and exposure upon health and longevity has been recently stated in the following summary: "It has oftentimes been asserted that those exposed to severe labor in the open atmosphere were the least subject to sickness. This has been proven a fallacy by Mr. Finlaison, Actuary of the National Debt Office, in London. Of persons engaged at heavy labor in out-door exposure, the percentage of sickness in the year is 28.05. Of those engaged at heavy labor in-doors, such as blacksmiths, etc., the percentage of sickness is 26.64—not much difference, to be sure; but of those engaged at light occupations in-doors and out, the percentage of sickness is only 20.80, 21.58. For every three cases of sickness in those engaged at light labor, there are four cases among those whose lot is heavy labor. The mortality, however, is greatest among those engaged in light toil, and in-door labor is less favorable to longevity than laboring in the open atmosphere.

"It is established clearly, however, Mr. Finlaison says, that the quantum of sickness annually falling to the lot of man, is in direct proportion to the demands of his muscular power. How true this makes the assertion: every inventor who abridges labor and relieves man from the drudgery of severe toil, is a benefactor of his race. There were many who looked upon labor-saving machines as great evils, because they have supplanted the manual toil of many operatives. A more enlightened spirit is now abroad; for all experience proves that labor-saving machines do not destroy the occupations of men, but merely change them. Man is relieved from drudgery by the iron sinews of the machine, and his own are left to move more lightly and free in pursuing avocations demanding less physical, but more mental and noble exertion."

Upon this subject Sir Henry Holland asks and ingeniously answers the following questions: "To what extent the bodily powers may be preserved, by maintaining their assiduous exercise—whether, to take a single instance, the muscular organs may be kept longer in vigor by exertion to the full extent of ability at each successive period of life? or whether the powers, whatever they be, which minister to their actions, are longer and better sustained by comparative repose? Taking life, not as a definite, exhaustible quantity of an unknown influence, but as expressing a common result of the actions of many

parts differently constructed and endowed, we are furnished with what comes nearest a satisfactory answer: Whatever habits sustain the greatest number of organs or functions in a healthy state (having regard also to the relative importance of these functions) may be considered as most conducive to length of life. The positive fatigue of any organ from its exercise must be deemed an excess—of little import, it may be, in single instances; certainly injurious by frequent or habitual repetition. All exercise of a natural function within this limit may be viewed, without material risk of error, as salutary in itself, and maintaining the integrity of the organ concerned longer than the opposite of inertness and disease. The same rule may be applied without error to the mental functions. Disuse is not preservation. We have reason to believe that the integrity of these faculties is best and longest sustained by habitual exercise, within those bounds which are reached without fatigue at each successive period of life." (*Notes*, 264–5.)

Experience, however, often makes sad havoc of the finest theoretical definitions—an example of which I may here relate, though it does not coincide with Dr. Holland's explanation—an example, the truth of which will sufficiently excuse its egotism. I confess—all are willing to confess that which is far from disgracing themselves in the estimation of others—I confess to great intemperance in the use of the eyes. In early life parental authority and the withholding of lamps and candles were insufficient to prevent my prolonged night-reading. I substituted the light of ignited wood, when denied other means of illumination. I have read scores of volumes in the open sun-light, on horseback, when traveling to visit the sick, etc., and have practised reading, writing and proof-reading at night to a vast, if not a profitable extent, not only seeing well all the while, but with an increasing vision which of late amounts to microscopic power of a low order, without any disadvantage except a very slight degree of shortsightedness.

The effect of climate upon the mean and the extreme duration of life has not been ascertained by reliable statistical data. Some writers claim, in this behalf, superiority for the rigid climate of Scandinavia; others for the eternal summer of the South, which, according to them, is the paradise especially of the aged.

The winter season of cold climates appears to be peculiarly dangerous to the aged, often causing senile catarrh, apoplexy, palsy,

typhoid pneumonia, etc. Hence, on theoretical grounds it might be assumed, in the existing deficiency of exact statistical data necessary for either a reliable comparison or a contrariwise probability, that, inasmuch as the calorific power of the old is impaired, a warm climate, other things being equal, must be more favorable to senile life than a variable, cold one.

“Men who have passed the meridian of life in England suffer less from heat than younger persons, and indeed life might often be prolonged by removing from a cold to a warmer climate at the period when the power of producing animal heat becomes more feeble.”—(*Med. Chir. Rev.*, July, 1844.)

“In climates very warm, and at the same time very dry, the human species enjoy a longevity perhaps greater than what we observe in the temperate zones. Europeans who transport themselves at an age somewhat advanced into the equatorial part of the Spanish colonies, attain there, for the most part, a great and happy old age.”—(*Humboldt: New Spain; Anno 1803.*)

“In those wild regions [Upper Orinoco] we are involuntarily reminded of the assertion of Linnæus, that the country of palm trees is [was] the first abode of our species, and that man is essentially *palmivorous*: ‘*Homo habitat intra tropicos, vescitur palmis, lotophagus: hospitatur extra tropicos sub novercante Cerere carnivorus.*’”—(*Syst. Nat.* i. 24. *Humboldt: Narrative*, v. 208.)

Mr. Lewes, in his recent *Life of Goëthe*, says: “Old age is a relative term. Goëthe at seventy was younger than many men at fifty; and at eighty-two he wrote a scientific review of the great discussion between Cuvier and Geoffroy St. Hilaire, on *Philosophie Zoölogy*, a review which few men in their prime could write. But there are physiologists who deny that seventy is old age. M. Flourens, for example, maintains that from fifty-five to seventy man is at his most virile period; and M. Reveillé Parise, in his work, *La Vieillesse*, declares that between fifty-five and seventy-five, and sometimes beyond, the mind acquires an extension, a consistence and a solidity truly remarkable—‘*C'est véritablement l'homme ayant atteint toute la hauteur de ses facultés.*’

\* \* \* Sophocles is said to have written his masterpiece at eighty. The reflective powers often retain their capacity, and by increase of material seem to *increase* it,” etc. To multiply authorities on this subject would be too severe a trial of the patience of Young America. Youth of that class, unfortunately no myth, would, like a prodigal

heir, make no manner of objection if the old would make a little more haste, so as to leave their places, honors and estates to the rising generation. There is, blessed be Bichât ! a very different class of young men. Young in years, but old in labors, researches and discoveries. Professor J. Aikin Meigs, who adorns what he touches, has in his late eloquent and truthful address to the juvenile graduates of the Pennsylvania College, enumerated many medical writers and discoverers, who, in early life, sounded the utmost depths yet reached in science. It is true, as Professor Meigs declares, "that young men are the apostles of new truths the world over." Rising above the dense, dark stratum of ancient dogmatism, like the mountain peaks, they are the first to receive and reflect the light of advancing knowledge.

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## CORRESPONDENCE.

I.—*Letter from Paris, to WARREN STONE, M. D., Professor of Surgery, in the Medical Department of the University of Louisiana.*

PARIS, Dec. 12th, 1858.

MY DEAR DOCTOR.—I have not had time to write you a word since you left Paris. Of course as my professional occupations alone occupy my time, you must think that I have some queer things in medicine to relate, for a full practice would hardly let one off from something strange for three months.

I think I told you about a case I had last spring, where the parotids became inflamed and supurated, the pus being diffused into the substance of the glands about two weeks after the commencement of a typhoid fever, and inflammation of the upper lobe of the right lung, all commencing with symptoms of jaundice—the patient dying of purulent resorption eight days after the glands began to swell. The second case was just reported from Chassaignac's wards, occurring after puncture for pleuritic effusion (drainage system and very bad for paracentesis in my opinion), death from purulent resorption in about a week. I have had in the last two months, a case more re-

markable than either one, which, in my opinion goes farther than either to show the mysterious and powerful fatal action on the economy of acute parotiditis, occurring in the train, or as the suite of an acute disease. In the first place it is rare, and I consider myself as very fortunate in having been able to record three cases within a year, each occurring during the march of different acute diseases, and all fatal, when Professor Trousseau tells me that in all his consulting practice, he has encountered but three cases in his life.

The patient, a lady sixty-two years old, was taken suddenly, while walking in the street, with acute pain in the abdomen—she managed to get into a shop, whence she was taken home in a carriage. I saw her the next day, and found a tumour in the left iliac region, the size of a nine months foetus' head. She was ignorant of the existence of this tumour. She had acute peritonitis, and in my opinion caused by the rupture of one of the ovarian vesicles with effusion of the cystic fluid into the peritoneal cavity. I employed calomel in minute doses, leeches, blister and frictions with mercurial ointment, and extract belladonna—numerous other bedside means, which occurred to me at the moment, but the detail of which I do not need to give to a veteran in our army like yourself. All went on favorably to a cure of the peritonitis, and in three weeks the tumour was not larger than a lady's fist. As not unfrequently occurs, the ovarian tumour was cured by or with the peritonitis—then appeared a soreness or swelling of both parotid glands—the left soon subsided, but the right continued on to suppuration. The fluctuation at last appeared, showing that the pus instead of remaining infiltrated, was collecting in form of an absces, and I had hope. I did not dare to open it, the fluctuation was so deep, but it soon opened itself into the ear and discharged freely—arrived nearer the surface, I made four incision—found the tissues of the gland gangrenous, and in course of time the entire substance of the organ came away, leaving an enormous cavity. All this time she was taking bark, brandy, quinine, food, etc., to the last limit.

Up to the time of her death no granulation appeared in the wound, and a few days before that event, which occurred five weeks after the commencement of the parotiditis, the angle of the lower jaw became necrosed, and continued to progress. In the mean time two large sloughs formed, one on the sacrum and one on the trochanter, immediately took on a gangrenous appearance—tremendous oedema of up-

per and lower extremities, facial paralysis to the opposite side from the parotiditis. Muguet from the lips to the arms. Everything indicative of a most profound alteration in the fluids of the body and the *vis vitæ*, so to speak, in spite of the most potent nourishing, stimulating, tonicising and care, she lived with all this sixty-five days, and for a month, I assume that she was only kept alive by the means above stated. My letter is so long that I must leave the analysis of the case to your own mind. She was cured of the peritonitis and died from the effects of the parotiditis. There is much to be gathered from these three cases, and you can gather more and better than I. I cannot but prognosticate a fatal result under the most favorable circumstances, from a complication of acute parotiditis occurring during or at the close of any acute affection, like pleurisy, pneumonia, typhoid fever, peritonitis, etc., for never was a fairer standing up fight seen between death and the doctor than in the case last mentioned; digestion and bowels good all throughout the disease; every thing went to the spot and acted as was desired, and still there was this constant wearing away and sinking nature seeming completely paralyzed from the start of this last disease; the sloughs cleared away, the wounds all discharged a sufficiently laudable pus, but not the slightest attempt was made by nature to repair them.

I have had a case of paracentesis this last week, which ought certainly to offer encouragement to the medical world in this line. Mr. F., of Boston, was taken sick in February last, with severe pain in his left side, fever, etc.; sent a lock of his hair to a somnambulist, and in three weeks was just able to drag himself out a little. Shortly after he found his breathing very difficult, and didn't seem to get on, so he decided, (whether on the advice of the somnambulist or not I don't know, but hope so for the sake of the profession) to make a packet ship voyage to Smyrna. There they blistered and iodinated, and purged; the old ones shook their heads and advised him to go home, and the young ones advised him to go home without shaking their heads. Same history in Athens with the exception of one bright young man, who advised him to stop in Paris. We found the point of his heart thumping against the 7th and 8th intercostal spaces, on the right side, about *two inches to the right of the right nipple*, great curvature of the spine to the right, right side, all pushed out of shape and distended like a balloon—no respiration of course on the right side, dullness, etc., etc. Couldn't lie down or sit up, ride, walk, or

eat. Advised him not to go home, as he might die at any moment, and at any rate a sea voyage wouldn't cure him, but to be operated on at once. He accepted, and the next day, December 1st, we took thirteen fluid pints of clear serum from his chest, and left probably three or four in. The operation lasted fifteen minutes; he coughed a little as the lung expanded, and perhaps for half the day; his heart assumed its usual place during the operation, and he has not had since the slightest accident of any kind; his respiration more feeble still on the right side, but is heard to the bottom of the chest, before and behind. He got up and dressed himself the fourth day, and to-day took a drive of a couple of hours (the 12th day); good appetite; lies on his back and right side as well as ever, etc.; in fact is a cured, perfectly cured man, a *saved* man, doctor, for he would have died without the operation.

Another little thing, and I have done with my chatter. I operated a couple of hæmorrhoidal tumours by *écrasement*, on the 29th November; patient out the 5th day; no febrile movement even after the operation, and not one drop of blood lost—cured.

DR. S. J. BIGELOW.

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II.—*Letter from* JESSE PEEBLES, M. D.

DR. BENNET DOWLER: *Dear Sir*:—Dr. W. A. McPheeters, in the January No. of N. O. Med. and Surg. Journal, speaking of the Dublin Lying-in-Hospital, remarks: "It is the common practice here to push out the placenta by grasping the fundus uteri and making considerable pressure, which is generally effective, if the placenta is detached."

A much less painful and equally effective practice is, for the patient to make a full inspiration; then with the open hand laid tightly over the mouth, make a forcible effort at expiration, or as the "old women" sometimes require, blow forcibly into an empty bottle. This practice is also useful after version, and in cases of breech presentation after the body has been delivered, and the uterus has comparatively little power to expel the head.

Yours, very respectfully,

JESSE PEEBLES.

Lowndes Co., Miss., March 17, 1859.

III.—*Letter from DR. BOYS DE LOURY*, (Translation) of Paris. Société de Médecine de Paris. March 2, 1859. (Séance à l'Hôtel de Ville.)

[It will be seen that the distinguished writer of this letter was not aware of Dr. B. Dowler's editorial connection with this Journal.]

*Monsieur le Rédacteur-en-Chef du Journal, etc.*: I would have addressed to you, long before this, my sincere thanks, and begged you to convey them to Dr. Bennet Dowler, also, for the very honorable mention made of me by him, in his remarkable essay on the "*Pathological Anatomy of the Large Intestine*," etc.,\* had I been able to ascertain sooner in what medical journal it had been published.

\* \* \* I have instructed one of our young and learned *confrères*, Dr. Auguste Voisin, to draw up an analysis of this memoir, which will be read before the Society of Medicine of Paris, and published in its journal.

It is very flattering to me that Dr. Bennet Dowler should have noticed in so kind a manner a memoir of mine, whose only merit consists in describing some practical facts, no doubt known to a majority of our *confrères*, but which, by being overlooked, may lead to fatal errors.

But, *Monsieur et très-honoré confrère*, what gave me the greatest pleasure in reading my honored *confrère* Dr. Bennet Dowler's citation, was to find that outside of our Europe, practical facts, even the most simple, do not escape the observation of American physicians, and to notice the happy concordance existing between men, seekers after science, even when separated by broad oceans.

With sentiments of the most cordial fraternity, Mr. Editor, I tender to you, as well as to Dr. Bennet Dowler, my sincere thanks and the assurance of my profound esteem.

DR. BOYS DE LOURY,

Surgeon-in-Chief of St. Lazare, General Secretary of the Society of Medicine of Paris,  
Chevalier de la Légion d'Honneur, etc.

*Paris, March 2, 1859.*

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\* The sheets of the *N. O. Med. and Surg. Jour.* (for Jan. 1859) containing this contribution, were sent to the *Medical Society of Paris*, not the Journal itself.—Eds. N. O. M. AND S. J.



## REVIEW.

*A Review of Medical Literature, including critical remarks on PROFESSOR PALMER'S Report on that subject, as published in the Transactions of the American Medical Association, Vol. XI. By B. DOWLER, M. D.*

PROFESSOR PALMER'S Report on Medical Literature is highly interesting, being comprehensive and practical in its scope, and genial in its spirit. As a literary production it is perspicuous and lively, without being always severely classical in the collocation of words or the arrangement of sentences. An exuberance of metaphor and a pomp of diction, together with an earnestness of manner, abound in this essay, which are the antitheses of what Professor C. D. Meigs terms "medical dullness and time-honored clergyableness." A chaste yet ornamental style is no less pleasing to the mind than a symmetrical edifice, adorned with columns and turrets towering above ordinary houses, is to the eye.

Professor Palmer's Report breathes the ardent spirit of patriotism, but it also reveals an excessive—nay, an unnecessary timidity as it regards any claims to profundity, learning and genius on behalf of his compatriots, whose sciolism is the occasion of many apologies. Nevertheless, as the sequel will show, he urges with great pertinacity that, with all their shortcomings in scientific principles, they excel all the world in *practice*, as if by any possibility there can be a fundamental antithesis to, or even an absence of, the former, without detriment to the latter. Superior excellence in practice is the most reliable criterion of superiority in science. The complete utilization of the experimental is the highest aim and end—nay, the very essence of the inductive or practical philosophy. If American practitioners are the best, they are necessarily the wisest.

Dr. Palmer displays great ability in his *ex parte* examination of the question, long agitated, in regard to the expediency of an international copyright, as will be seen hereafter.

Admitting the validity of his premises concerning the juvenile character of this Republic, the immaturity of its literature, its sciolism, its destitution of genius and originality, and its poverty in the essential elements of intellectual and scientific capital, there can be no question as to the conclusiveness of his logic adverse to the expe-

diency of this measure, inasmuch as all, both old and young, particularly the latter, having but little knowledge of their own, and yet needing a great deal, should get it from abroad, and get it at the cheapest rate—that is, appropriate it without leave or license from foreign authors. This is a communism of immaterial wealth, rivals that of the Socialists in regard to material riches. Free appropriation, literary phalansterianism, with letters of marque and reprisal against all foreign realms rich in immaterial treasure, are virtually—nay, directly advocated in this Report. The interests of authors who produce the great staples of thought, knowledge and discovery are merged and lost in those of the general mass. This is neither free trade nor reciprocity, but the forcible seizure of all intellectual treasure upon the principle that might is right. A learned judge, Sir John Fortescue, saith, *a fortiori*, that “if an Englishman be poor, and see another having riches which may be taken from him by might, he will not spare to do so.”—(*Hallam: Middle Ages*, ii. 297.)

Before proceeding further in the investigation of Dr. Palmer's doctrines of appropriation and anti-international copyrightism, it may be well to say that at the meeting of the American Medical Association held at Boston in 1849, a committee was appointed to memorialize Congress in favor of an international copyright. At a subsequent annual meeting Professor G. B. Wood, of Philadelphia, chairman of this committee, reported: “That our institutions are peculiar, and, in order to be legitimately carried out, should not only be well understood by all concerned, but should be supported by habits of thinking and feeling in the community in full accordance with them. But how can this object be accomplished when the great mass of our literature is of foreign origin; when, of the books which tend to form the character and mould the opinions and mental habits of the reading classes, perhaps nine-tenths are the production of authors born and educated under a different political system, with predilections for manners adapted to other forms of society, and altogether unsuitable guides for the young minds of this country? By allowing equal opportunities for the literary tendencies and genius of our population, considering the much larger proportion among us than in almost any other country, of those who are so far educated as to come within the capacity of literary effort, there can scarcely be a doubt that we shall soon exhibit the same relative progress in the various branches of authorship as in all other departments of human action and industry.

Our literature will start forward at a rate that will probably astonish the world as much as our progress in the arts, in wealth, and in all the physical comforts of life—adorning, ennobling and strengthening the national character.”

This argument, if applicable to general literature, applies *a fortiori* to the medical literature and science of a country wherein great differences of climate, meteorology, medical topography—differences of races, as white and black—differences in diseases, although mathematics, physics, astronomy, etc., are unmodified by climate, etc.

Dr. Palmer, however, on the other hand, deplores most emphatically the excessive youthfulness of this Republic: “Our country is young, and our medical literature younger still. In the great family of enlightened nations we were the last born. Our scientific and literary wants are beyond our own capacity to supply. In our youth we must be sustained. The mother that bore us and our elder brothers must help us. We may be charged with appropriating materials, the results of the labors of others. We do, and we must thus appropriate. Our necessities and our destiny demand it.”

This picture of helpless infancy, exhibited from page to page, must be touching to maternal hearts, including all of our Insular and Continental grandmothers in Europe. The British people are told not to complain, “if what they consider their superior knowledge is appropriated by their own offspring in the West,” etc.

Dr. Palmer does not despair, but prophesies the final success of Young America. He says, in his usual ornate style: “Your committee would say, that if, as sentinels placed upon the walls of our Medical Zion, they are asked in relation to its literature, ‘What of the night?’ the response must be, ‘The morning cometh.’ The darkness which has hung over that literature is breaking away. There is at least dawning in the east, and though the chariot of day may roll on but slowly, the full effulgence will come at last.”

Dr. Palmer says our literature is in its infancy, whereupon he concludes as follows: “That which is in its infancy cannot be mature. The springing plant cannot be crowned with flowers. But if our literature has not yet great expansion of branch and leaf, it has depth of root and vigor of stem.” The flowers might be ornamental—the fruit useful; but with branches and leaves the student must be contented for an indefinite period, as “the chariot may roll on but slowly.”

## INTERNATIONAL COPYRIGHT.

“With us,” says the Reporter, “all shadow of [copy] right is limited to fourteen years.” This is a mistake. Copyright with us extends to forty-two years; that is, twenty-eight years positively, and provisionally fourteen years longer, if reëntered, which is optional with the person holding the right for the first named period. It is a foregone rather than a legitimate conclusion, that the law of copyright has not contributed largely towards the advancement of literature, science, and the public good. Nations the most distinguished for literature have enacted the most stringent laws in favor of securing to authors, for long periods, the exclusive right to publish and to enjoy the benefits arising from the publishing of their literary works. For example: in England (whence most of our foreign books come) “it is enacted, [Acts 5 and 6 Vict. c. 45] that in every book published in the life time of the author (after July 1st, 1842), he and his assigns shall have copyright for the term of the author’s life, and for seven years after his death; or, if these seven years expire before the end of forty-two years from the time of publication, then for such period of forty-two years. The copyright of a book published after the author’s death extends to forty-two years. With regard to encyclopædias, reviews, magazines, periodical works, or any book in which the publisher shall have employed persons to write, on the terms that the copyright shall belong to himself, the copyright shall be in the publisher or proprietor, after he has paid for it, in the same manner and for the same term as is given to authors of books, except only in the case of essays, articles, or portions forming part of, and first published in reviews, magazines, or other periodical works of a like nature, the right of publishing which separately shall revert to the authors at the end of twenty-eight years after publication, for the remainder of the term given by this act; and during these twenty-eight years the publisher or proprietor shall not have the right to publish any such essay, article, or portion separately, without the consent of the author or his assigns. The remedies for the infringement of copyright are, an action for damages, and a power given to the officers of customs and excise to seize and destroy all foreign reprints of books in which copyright exists, with a penalty on the importer (if he be not the proprietor of the copyright) of £10, and double the value of every copy of any book imported, on conviction before two justices of the peace; £5 of the penalty to go to the officer

of customs or excise, and the remainder to the proprietor of the copyright. These powers have been exercised vigorously by the custom-house authorities, and found very effectual to prevent the importation into this country of the French, Belgian, German and American reprints of popular English works; but English authors still suffer by the circulation of these reprints abroad; a practice so destructive of the fair profits of mental labor can only be effectually redressed by prevailing on foreign countries to extend the benefits of their own laws against literary piracy to aliens as well as native authors.

"The 7 Vict. c. 12, entitled 'an act to amend the law in relation to International Copyright,' empowers Her Majesty, by order in council, to enable authors of works first published in foreign countries to have copyright in the British dominions for periods not exceeding those allowed by the various copyright acts for the respective classes of works when first published in this country, on conditions of registration and delivery of one copy in Stationers' Hall."—(*Nat. Cyc.* iv. 927, *et seq.*)

Now, inasmuch as those nations whose works Dr. Palmer prefers as better than our own, have afforded the most complete protection to literary property, and have enacted the severest penalties against free trade, communism, and piracy in literature, the presumption is that we should adopt the same line of conduct, as well as read their books.

Without taking into consideration the justice due to alien authors, or the charge of piracy levelled by the grand jury of the European world against American publishers and annotating authors, who seize upon the high seas of literature, as lawful prizes, the intellectual merchandise of all foreigners, without compensation, there is another consideration of paramount import, particularly in reference to medicine, namely, the benefit which experience has everywhere shown as the result of encouragement and protection to domestic or home literature and science. It is contrary to the practical laws of trade to buy that which can be obtained without any cost; to pay thousands of dollars for a native work in manuscript, when an equally good one, already in print in a foreign land, can be had by paying the price of a single copy. The traders in books are like the traders in cotton bales: they buy as cheap and sell as high as they can.

Supposing that the American medical mind is equal to the European, as it regards capacity knowledge and literature, still, as it

regards certain practical departments of science, inequality must exist. Thus the most scientific farmer in England will not be equal to the Southern planter in growing cotton, sugar, rice, maize, tobacco, etc. The British physician may know more of the typhus fever among the paupers of his native land, than the Southern physician ; but the former might not be equal to the latter in the medical treatment of negroes and whites in the Southern States. Almost all sciences may be imported but that of medicine, as climate, topography, ætiology, practice, etc.

Dr. Palmer sums up his argument in the following words, making "the general intelligence of our people" the basis of our "glory," notwithstanding all he had advanced to the contrary: "Should it be admitted that the pecuniary interests of a few authors are injured by the free circulation of foreign works, those interests sink into utter insignificance when placed in opposition to the intellectual and scientific wants of a great profession and a great people ; and should it even be admitted that by the same means the variety and richness of our native literature is diminished, it should be remembered that the glory of our nation chiefly consists in the general intelligence and welfare of the people."

Science, learning and intellectual ability are not hereditary. They are not national, but individual. They are peculiar to no country of the civilized world. The antiquity or age of a nation affords no positive evidence of superiority in science. The absence of schools, colleges, books, and the physical comforts, may often prove unfavorable to both national and individual mental development. If these privations be retarding causes, they are probably less operative in this Republic than in any other nation, however old. During four score years of independence, the science and literature of the country have kept pace with its commerce, wealth and vital progression. Were its native writers protected by an international copyright (which is but sheer justice to foreign authors), American contributions to the treasury of knowledge, now not small, would be greatly augmented, while an honorable rivalry would prevail, advantageous to both as well as to the public. The age of a nation or of an individual is no criterion of wisdom. India, Egypt, Greece and Rome declined with the lapse of centuries. The unparalleled progress of the United States as a nation, in government, wealth, population, education, periodical publications, commerce, mechanical arts, etc., affords presumptive

proof of equality with other nations in regard to literature and science. This equality is fully proved by numerous original works on fiction, history, political economy, lexicography, travels, navigation, geographical explorations, antiquities, jurisprudence, codification, physiology, forensic and practical medicine, *Materia Medica*, botany, medical topography, surgery, obstetrics, dentistry, meteorology, mineralogy, natural history, ornithology, ethnology, craniology, not to mention an extraordinary number of important discoveries, inventions and improvements.

#### MEDICAL JOURNALISM.

The Reporter's account of the large number, and the dismal prospects of the medical journals as viewed from a pecuniary standpoint, are but too true. There must be nearly forty journals—a number as great, if not greater than all the residue of the world can boast (?) of at the present time. Is it not possible that the supply may exceed the demand in medical journalism as in every thing else? If there be, as Dr. Palmer asserts, a great dearth of genius, research, literature, or learning in this land, would not four or five, in which all the mental activity of the country might concentrate, find deliverance, and extension, instead of forty, suffice to disseminate all that is new, true, and practical, and the more so because mail facilities are favorable to the easy, cheap and rapid transmission of thought in manuscript or print from the circumference to the principal medical climates and geographical centres of this vast Republic?

Waiving the illusory expectations of realizing from medical journals a pecuniary return for the actual expenses of their publication, without estimating the time, care, mental and bodily exertion of editors and writers, sufficient motives and laudable ends remain to impel the good and the wise to embark in this most useful department of literature, namely, charity and the advancement of the science and art of healing. Money cannot be more usefully bestowed than in propagating useful knowledge; but in proportion to the native poverty of a country in science, so should its home journals be restricted and foreign ones multiplied. There can be no necessity of building mills or mints where there is neither grain nor ore.

Dr. Palmer says: "That there are more journals in the country than receive a remunerative support is true, and that perhaps none can offer pecuniary inducements for an editor of proper qualifications

to devote the entire, or even more than a small proportion of his time to the performance of editorial labors, is also true.

“As no nation has so great a proportional number and so large a circulation of newspapers and literary magazines as our own, so none has so large a number and so wide a circulation of medical periodicals. It has been suggested that there is a profusion—a positive excess of these periodicals; that because of their great number and the consequent deficiency of patronage to each, their general quality is of a lower grade than though fewer existed. That there are more journals in the country than receive a remunerative support is true; that very few receive an income enabling their proprietors to pay for contributions, and that perhaps none can offer pecuniary inducements for an editor of proper qualifications to devote the entire, or even more than a small proportion of his time to the performance of editorial labors, is also true; and it cannot be denied that these circumstances tend to deprecate the quality of the journals. But there are advantages in the large number and diffused localization of these journals which more than compensate for such defects in their average quality as the greater number may induce. If no articles are received [accepted] which have not attained to a high degree of excellence, our periodical literature will become meagre indeed. If every physician in regular standing throughout the country took a single journal and promptly paid for it (and no one should take less than two or three) the present number of these periodicals, it is believed, would be well sustained, and their quality would be much improved. While, then, a material diminution in the number of journals would be a source of regret, as diminishing the number of contributions and repressing in different sections the development of a local literature, a more liberal support of those that exist is demanded of the profession.”

Here, as in every part of this report, the law of kindness sways the mind of the Reporter. Himself an able, young, hopeful editor, he sympathizes with his editorial brethren of the medical press. He is not the man, rudely to say that nine-tenths of the journals, or even one should retire, because such labor does not receive pecuniary remuneration.

Admitting (what Dr. Palmer will not admit) that this juvenile country stands at a level not a whit lower than that of the mother country, the number of its medical periodicals is excessive, if compared with any other in Christendom. Dr. Palmer's argument for a



multiplicity of journals, because no nation has so many newspapers, labors under a disadvantage in one point of view, at least. According to a recent estimate in one of these newspapers, it appears that the average life of ninetenths of the newspapers in this great newspaper Republic, is but two years. They not only die young, but in the most cruel manner, by starvation. Political economists are not fastidious in these matters. They admit the possibility of over trading, that the supply should be regulated by the demand.

“As no one need expect to prevent earthquakes by building cities in the neighborhood of volcanic mountains,” neither can any expect by building an excessive number of literary magazines to prevent a mental famine. “Thirty medical journals,” says the Reporter, “are at present published.” This report had hardly reached the reader before ten new ones, more or less, appeared—an average birth exceeding one per month. It may not be an unjustifiable rudeness to say, that probably the new as well as the old journals, will find that such enterprizes will be neither self-sustaining nor remunerative. Nevertheless, scientific and literary almsgivings are among the noblest of all charities.

The truth is, scientific journalism should, and generally does, occupy a higher platform than that of money. A disinterested love of knowledge for its own sake, a desire to promote the sovereign good by the accumulation and extension of science and the consequent augmentation of intellectual wealth, moral improvement, and health, afford a broad and elevated basis for a sufficient number of medical journals in every great centre of population in the different climates and topographical districts of the country.

If American medical journals were not only self-sustaining in a pecuniary point of view, but so many mines of wealth, should not their multiplication be restricted, on merely scientific grounds, as in no age of the world, not excepting the Hippocratic, has science progressed so rapidly as to demand so many organs for its diffusion? Read, if that be possible, forty journals, whether foreign or domestic, received by the last mails, issued tri-weekly, weekly, monthly, bi-monthly, quarterly or annually, and you will find a great many repetitions, or, as the profane Falstaff would express it, “damnable iterations!” In this particular the American journals possess some advantages not so generally met with in certain foreign journals. In the able journals of Paris, for example, one is to a great extent a

stereotype or *fac simile* impression of all, so far as the transactions, debates, *polemics*, and reports of the academies and medical societies form a common staple for the whole medical press of that city. In France quackery is crushed by the *vis inertia* of the government, while science is salaried, honored and protected. Leaving out of view that presumptuous question, *quality*, an American may venture to look into the category of *quantity*—that is, he may enumerate what appears in the French journals under the head of TRAVAUX ORIGINAUX, and what appears in the American journals, under the title of ORIGINAL COMMUNICATIONS, without blushing, while he may truly say, of American journalists, that their selections from every point of the medical horizon are more copious than those of perhaps any other land: Whether this be creditable or discreditable, must be left to the arbitrament of the impartial critic, who is not swayed by the biases and blandishments of the *amor patriæ*. The æsthetic of the true, the beautiful and the good cannot stand on the soldier's platform and shout—"My country right or wrong!" What Goëthe says of the poet is still more applicable to the man of science and letters. "The poet may, as a man and a citizen, love his native land; but the native land of his poetic energies and action is the good, noble and beautiful; which he must seize upon and body forth wherever he finds them."

What Coleridge says of literature as a trade, applies to medical journalism as a trade: "I would," says he, "address an affectionate exhortation to the youthful *Literati*, grounded on my own experience. It will be but short, for the beginning, middle, and end converge to one charge: *never pursue literature as a trade*. With the exception of one extraordinary man, I have never known an individual, least of all an individual of genius, healthy or happy without a *profession*, that is, some regular employment, which does not depend on the will of the moment. Money and immediate reputation form only an arbitrary and accidental end of literary labor. The hope of increasing them by any given exertion will often prove a stimulant to industry; but the necessity of acquiring them in all works of genius will convert the stimulant into a narcotic. It is one contradiction of genius from talent, that its predominant end is always comprised in the means, and this is one of the many points, which establish an analogy between genius and virtue. Now, though talents may exist without genius, yet as genius cannot exist, certainly not manifest itself, without talents, I would advise every scholar, who feels the genial power working within him, so far to make a division

between the two, as that he should devote his talents to the acquirement of competence in some known trade or profession, and his genius to objects of his tranquil and unbiassed choice; while the consciousness of being actuated in both alike by the sincere desire to perform his duty, will alike enoble both."—(*Biographia Literaria*. i, 231-2.)

After having reproduced this noble passage, worthy of being inscribed in letters of gold, the reader will thank me for suppressing six additional pages, written upon Medical Literature. Whether what has been already written shall do more good than harm, judge ye.

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## MISCELLANEA.

1. PROF. BENNETT, of Edinburgh the opponent of bloodletting, *has submitted to venesection*. Prof. Miller (of Edinburgh), in a lecture on Inflammation (Dec. 15, 1858), says, in reference to bloodletting in Prof. Bennett's case: "We grieve to think that this fact has recently met a confirmation in the personal experience of the very Coryphæus of the abolitionists. We grieve that for such a malady he should have needed such a remedy; and yet we are glad to be assured thus certainly of what, from his antecedents, we had confidently predicated, that his system remains wholly of the sthenic type—not only permitting, but demanding the heroic remedy, and bearing the application with perfect impunity. Long be it so!"

2. *Anatomy*.—Guided by Anatomy as his polar star, he (the surgeon) has a sure support under every trial, and is guarded against every danger. Accident finds him ever prepared, and even in most desperate circumstances, his science enables him to hold death in abeyance. It is from anatomy that he obtains the intrepid steadiness which conducts his knife, although his face may be pale with intense anxiety for the safety of his patient. The confidence derived from his anatomical studies imparts the calmness and coolness with which he attends to those who demand his assistance while his country's eagle is screaming over the field of battle and blood. There, enough anxious and sick at heart, stunned to deafness by the martial thunder, his soul wrung by the sight of unfortunates too dreadfully mangled to be aided, he is still able to operate with safety, because he bears within his mind that light which alone flows from positive knowledge of the structure and relations of the human fabric. Surgical anatomy cannot exist where there is doubt. The most absolute and unequivocal decision is demanded—the most perfect certainty is required as to the regular connections and positions of parts—no evasion will serve where the lives of patients are to be saved or destroyed by recollecting whether a vessel or nerve is to the right or left—interior or exterior, superficial or deep-seated, in reference to parts which are to be the subjects of operation.—DR. GODMAN.

*Commencement of the Medical Department of the University of Louisiana.*

The Annual Commencement of this institution took place at Lyceum Hall on Saturday, March 19th, 1859. The Degree of Doctor of Medicine was conferred on ninety-seven of the candidates for professional honors. There was also one graduate in the department of Pharmacy.

Dr. Hunt, Dean of the Faculty, delivered to the class an address. Dr. J. W. Saunders next followed in a valedictory oration.

It will no doubt be gratifying to the friends of the University to hear of its continued prosperity. The number of Matriculates for the Session of 1858 and 1859, was three hundred and thirty-three, being an increase of fifty-seven above that of the previous Session.

We subjoin a list of the graduates.

Cornelius F. O'Neil, John G. Mestayer, John F. Moodie, Robert A. Stewart, Benjamin F. Barnett, Edmund H. Fournier, Ralph C. Callaway, Thomas H. Lanier, James W. Talbert, Charles L. Tarlton, Harry Percy, George O. Brosnaham, John S. Gardner, John Goodman, John J. Roberts, David J. Smith, James P. Oliver, Alex. P. Bréda, Patrick H. Griffin, Francis M. Traylor, Joab B. Spears, Lewis H. Hill, Pinkney D. Chrisman, James M. Owens, Richard L. Dunn, John J. Lyons, Sheldon Stringer, William Offutt, John H. Davis, Calvin U. Silliman, James B. Knowles, James W. Wingate, James H. McDonald, George W. Monroe, Wiley P. Smith, John S. Stephens, Sumuel A. Hill, Marshall Pope, Alex. P. Hall, John B. P. Johnston, Zachariah J. Scott, John M. Morrison, Augustus H. Moore, James C. Fears, William A. Walton, Charles D. Lewis, Charles G. Cowan, Rufus H. Kilpatrick, James E. Reeves, John D. Smith, Peter L. Hammond, John W. Cothran, Isaiah G. W. Steedman, Elisha M. Harris, Alfred G. Tebault, Alexander T. Howe, Daniel F. McEacham, Samuel R. Davis, James M. Quinney, Peter Taylor, James W. Bennett, Virgil V. Madden, Lucius L. Holcombe, William J. Barron, William P. Finley, Thomas Gaddis, William L. Jones, David B. Turner, William P. Powell, James M. Haynes, Daniel S. Chessher, William D. Jenniugs, John P. De Jarrette, Henry F. Wade, Thomas H. Castleton, James Moore, William T. Haddox, Charles M. Erwin, James A. Jones, Samuel Meeker, James W. Oswald, Henry Snow, Edward Caire, Charles E. Buck, Joseph Stephens, James Barns, Rufus H. Wooten, Numa Lambert, William M. Mercer, James W. Saunders, Martin D. Morgan, John J. Oatis, Andrew W. Smith, Samuel C. Murphy, William W. Leggett, Ebenezer M. Smalley.

Graduate in Pharmacy—Auguste Leicher.

## MORTALITY STATISTICS.

*Statistics of the Charity Hospital for 1858.*

ADMISSIONS.		DEATHS.	
Total number.....	11,337	Total number.....	2,290
Males .....	9,135	Males.....	1,927
Females .....	2,202	Females.....	363
Total from United States,	1,751	Greatest No. deaths in Sept.	521
(Of these from Louisiana,	360)	“ admitted in Aug.	1,501
Of foreign birth.....	9,586	BIRTHS.	
{ Of these from Ireland..	4,568 }	Total 123—males, 52, females	53
{ “ Germany, 1,883 }		Stillborn.....	18
Number of patients remaining in the Hospital, Jan. 1st, 1859.... 644			

SOME OF THE DISEASES.	NUMBER OF ADMISSIONS.	NUMBER OF DEATHS.
Yellow Fever.....	2727 (5 of these colored).....	1382
Intermittent Fever.....	1727 .....	5
Remittent Fever.....	122 .....	1
Typhoid Fever .....	129 .....	53
Bronchitis .....	129 .....	4
Diarrhœa .....	698 .....	134
Dysentery .....	195 .....	69
Delirium Tremens.....	159 .....	50
Pneumonia .....	104 .....	39
Pleurisy .....	47 .....	5
Rheumatism .....	429 .....	3
Venereal.....	382 .....	4

*Mortality of the year 1858, in New Orleans. (From the Report of the Board of Health.)*

Total Deaths.....	11,710	Deaths under 5 years .....	3,548
		“ from 5 to 20 .....	1,237
Whites.....	10,592	“ from 20 to 50.....	6,066
Negroes.....	1,118	“ from 50 to 100.....	853
		“ over 100.....	6
United States.....	5,624		
Foreign .....	6,086		

N. B.—Deducting the 4,855 deaths caused by yellow fever, the mortality for the year would be 6,855. The total mortality for 1856 was 5,689; for 1857, 6067; these years were free from epidemics.

*Remarks.*

“ACCIDENTS—52 this year, against 32 last year; this includes falls, railroad accidents, etc., and comes under the head of casualties.

APoplexy—114 this year, against 99 last year. The smallest number of deaths occurred in April, viz : 5 ; 13 each in July and August.

BRONCHITIS—74 this year, against 55 last year ; the greatest number of deaths being in January and February.

CHOLERA—26 deaths from this disease this year ; 24 last year.

CHOLERA INFANTUM—108 this year, against 91 last year ; 29 deaths in June, the most in any one month.

CONGESTION OF BRAIN—156 in 1858, against 92 in 1857 ; greatest number, 28, in September.

CONSUMPTION—729, against 624 in 1857. Great uniformity in the deaths each month ; in 1856 there were 652 deaths of this disease.

INFANTILE CONVULSIONS—521, against 375 last year.

DELIRIUM TREMENS—The deaths reported this year are 85 ; last year, 58.

DIARRHŒA—297 deaths in 1858, against 224 in 1857 ; 35 deaths in September, the greatest number.

FOUND DROWNED—86 in 1858, 67 in 1857 ; 13 in April, the greatest number.

DYSENTERY—229, against 200 last year.

ENTERITIS—117, against 106 last year.

EPILEPSY—20, against 21 last year.

CONGESTIVE FEVER—328 reported this year ; 72 last year. Under this head are included all deaths reported as pernicious fever.

TYPHOID FEVER—189 this year ; 118 last year.

YELLOW FEVER—4,845 the number of deaths reported ; 10 interments are to be added, one of the Hebrew Cemeteries having failed to report at the proper time, making 4,855 ; the greatest number of deaths, in September, being 2,204.

GASTRO-ENTERITIS—87 deaths this year, 71 last year.

HEART DISEASE—87 in 1858, 71 in 1857.

INFLAMMATION OF LIVER—65 this year, against 32 last year.

INFLAMMATION OF LUNGS—231, against 175 ; greatest number of deaths in December, viz : 38.

INFLAMMATION OF THROAT—95 this year, 5 last year.

INTEMPERANCE—44, against 26 in 1857 ; 11 deaths in July, greatest number.

STILLBORN—338 this year ; in 1857, 358—20 less this year.

TEETHING—189 ; in 1857, 151 ; greatest number of deaths, in June, being 31.

TRIS. NASCENTIUM—222, against 188 in 1857.

749 deaths from the three last named causes, during the year, being about thirty per cent. of all the deaths under one year.

H. D. BALDWIN, M. D.,

*Secretary of the Board of Health."*

[SCARLET FEVER—79 ; in 1857 there were 86.

SMALL POX—99 ; in 1857 there were 98.—ED.]

*Mortality Statistics of New Orleans, from Jan. 2d, to April 10th, 1859, compiled from the Weekly Reports politely furnished by Dr. Baldwin, Secretary of the Board of Health. Population of New Orleans by the last census, 138,000.*

<i>Time.</i>	<i>Total Deaths.</i>	<i>Children under 2 yrs.</i>	<i>Under 20.</i>	<i>U. States.</i>
January (4 weeks).....	406	138	210	275
February (4 weeks).....	472	131	233	319
March (4 weeks).....	438	125	206	311
April (2 weeks).....	207	55	91	142

<i>Principal Diseases.</i>	<i>Jan (4 weeks.)</i>	<i>Feb. (4 weeks.)</i>	<i>March (4 weeks.)</i>	<i>April (2 weeks.)</i>
Still born.....	31	21	25	8
Tris. Nascent.....	20	12	8	8
Teething.....	3	9	8	1
Cholera Infantum.....	2	0	3	5
Infant. Convulsions.....	12	17	21	8
Infant. Marasmus.....	6	15	6	6
Croup.....	17	10	11	3
Inflammation of Throat.	9	31	26	8
Scarlatina.....	5	14	6	6
Rubeola.....	0	0	0	0
Variola.....	9	7	10	3
Tetanus.....	2	1	7	4
Diarrhoea and Dysentery	25	21	24	12
Gas. Enteritis.....	6	3	3	2
Inflammation of Liver..	3	3	4	2
Inflammation of Lungs.	29	45	23	17
Phthisis.....	64	69	67	34
Apoplexy.....	6	7	7	6
Congestion of Brain.....	4	3	8	1
Fever Typhoid.....	10	12	12	3
• " Miasmatic.....	11	8	6	2
" Yellow.....	0	0	0	0

*Statistics of the Insane Asylum, at Jackson, La., for the year 1858.*

Number of patients remaining Dec. 31, 1857.....	125
“ “ “ “ 1858.....	137
“ “ admitted in 1858.....	84
“ “ who died in 1858.....	27

Of these deaths, 15 were caused by chronic diarrhœa. Of the discharges, 32 had “recovered,” most of whom were recent cases. Two-thirds of the patients were of “foreign birth;” about one-fourth only were born in Louisiana. More than half the admissions come from the parish of Orleans, and nearly one-half have been “laborers” or “servants.” The Asylum can accommodate 225 patients; the average number accommodated during 1858 has been 150.

CHAILLÉ.

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.—*From the Medical Purveying Office, U. S. Army, N. O. New Orleans, La., Lat. 29 deg. 57 min. 30 sec. N.; Long. 90 deg. W. Altitude of Barometer above the level of the sea, 35 feet.*

1859.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
	Max.	Min'm	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
January...	7 A. M. 23 <sup>o</sup> 30.768	2 P. M. 6 <sup>th</sup> 29.964	30.347	2 P. M. 28 <sup>th</sup> 73	Several, 48	60.18	2 P. M. 29 <sup>th</sup> 68	Several, 30	53.91
February.	7 A. M. 13 <sup>th</sup> 30.516	9 P. M. 8 <sup>th</sup> 29.826	30.226	Several, 78	7 A. M. 10 <sup>th</sup> 55	67.54	Several, 79	7 A. M. 10 <sup>th</sup> 44	63.40
March.....	7 A. M. 19 <sup>th</sup> 30.382	2 P. M. 28 <sup>th</sup> 29.650	30.111	2 P. M. 24 <sup>th</sup> 79	7 A. M. 8 <sup>th</sup> 57	68.95	2 P. M. 29 <sup>th</sup> 81	Several, 52	66.48

1859.	HYGROMETER.			PREV'G WINDS.	WEATHER.		RAIN.	
	Max.	Min'm	Mean		Cloudy.	Rain.	Days.	Quantity.
January..	2 P. M. 29 <sup>th</sup> 67	7 A. M. 23 <sup>o</sup> 34	52.24	N., N. E & N. W.	13.66	17.33	11	6.40 inch.
February.	2 P. M. 18 <sup>th</sup> 78	2 P. M. 10 <sup>th</sup> 42	61.44	S. E., and N. E.	9.33	18.66	7	3.77 inch.
March.....	2 P. M. 29 <sup>th</sup> 76	7 A. M. 19 <sup>th</sup> 51	64.18	E., S. E., & N. W.	13.66	17.33	12	7.84 inch.

## REMARKS.

January, 1, 2, 3, 4, 9, 10, 16 and 19, white frost; hard frost, with ice, on 8, 15, 22, 23 and 24th.

February, 11th, peach in blossom; frost 3d and 4th.

March 19th, white frost.

T. HARRISON, Clerk.



THE  
NEW ORLEANS  
MEDICAL AND SURGICAL JOURNAL.

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JULY, 1859.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Observations and Reflections on Yellow Fever* : By M. MORTON  
DOWLER, M. D. (Continued from page 324.)

IN the May number of the Journal, I offered a few remarks on the doctrine of yellow fever importation, in which, I believe it satisfactorily appeared, that it cannot be possible that the yellow fever epidemics of this city can have their origin in importation. We saw that, were such truly their origin, these visitations would not, as they have always done, make their appearance in an arbitrary and irrelative manner in the presence of constantly augmenting sources of exposure. If the importation elements were such between the years 1797 and 1802, as to permit a non-epidemic interval of four years, it is not possible that the non-epidemic intervals could have remained as they did *exactly the same*, for the twenty-three years next succeeding, amidst constantly increasing exposure ; nor, *a fortiori*, could it have happened that in the midst of our almost boundless exposure, the non-epidemic interval could have been *lengthened out to five years*, between the years 1847 and 1853. If the chances, on the importation basis, between the years 1797 and 1802, were such that in the space of five years there were one chance *for*, and four *against* the occurrence of annual epidemics, it could not have happened, as it did happen, that between the years 1847 and 1853, the chances of annual epidemics could have been *so reduced* that for the space of *six* years the chances could have

stood as 1 to 5 against such result. So far from there occurring an extension of the non-epidemic interval in the latter period, our augmented exposure would not only have cut off all hope of escape from annual yellow fever epidemics, but would have placed us in such position in the category of probabilities, that should it so have fallen out that one epidemic chance should have happened to fail, there would have been left ninety-nine chances to one, to have rendered annual epidemics inevitable. If our epidemics really owed their governing antecedents to importation, they would exhibit such obedience to the laws of importation, that we could have little more doubt of the fact, than of the fact that our coffee and cocoa are imported. Were such the origin of our yellow fever epidemics, the proofs would long, ere this, have become so cumulative and palpable, that the importationists would have been relieved from the necessity of special pleading—from the necessity of watching for their "first cases" amongst the shipping, and of avoiding cognizance of all "first cases" that happen to occur elsewhere. That yellow fever can never be rendered epidemic in New Orleans through the agency of anything that can be shipped into her port; and that no epidemic of that disease can ever occur in this city—unless there exist all the internal, local, and independent conditions necessary to the production of the pestilence in an absolutely isolated city, are propositions which are, to my mind, neither more nor less than the plain teachings of experience.

After making the above recapitulation it had not been my intention to have said anything more specially upon the doctrine which ascribes the governing antecedents of our New Orleans yellow fever epidemics to importation, but to have dismissed that doctrine as necessarily untrue. While writing the above, however, the Report of the Board of Health, addressed to the General Assembly of the State of Louisiana has fallen into my hands. On referring to that document I perceive that the distinguished President of the Board of Health has entered into an elaborate argument to prove to the Legislature of the State that the deadly and protracted epidemic of 1858 was imported into the City of New Orleans on board of the ship Elizabeth Ellen, a vessel which he emphatically pronounces an "ill-starred ship" of "unhappy notoriety." The President of the Board, in this public document, does not hesitate to inform the Legislature of the State that "all lingering doubts" are removed touching the position he assumes. It is

therefore a matter of the gravest importance, not only to the people of this community, but to the world at large, that the grounds occupied by Dr. Axson, in support of his position, should be thoroughly surveyed and scrutinized. If he has in reality dissipated all "lingering doubts" of the importation of a single yellow fever epidemic, he has achieved more for this department of medical science than any man has ever achieved before. It may not, therefore, be inappropriate in the present communication, to so far digress from my original design as to offer a few remarks on the evidence which he finds so satisfactory to himself, and which he thinks ought to convince the Legislature of the necessity of making the following modifications of the quarantine law, which, as President of the Board, he would "earnestly recommend."

1stly. To prohibit, from and after the first day of May, and until the first day of November, all vessels arriving from ports where yellow fever habitually prevails, to proceed higher up the river than the quarantine station, except with the sanction of the Board.

2ndly. To store all cargoes, coming in such vessels in the United States Warehouses, with the privilege to consignees or owners of them to reship, at their option, to go beyond the limits of the State. This, of course, to apply only to what are called susceptible goods, as hides, wool, etc.

3rdly. To compel all vessels thus detained at the station to police their own crews, etc., by mulcting any vessel, in damages, which shall permit the escape from on board of any of the crew.

4thly. To forbid, by suitable penalties, any towboat, towing up any such vessels, to come to the city for the same period of time, unless they obligate themselves to carry only acclimated crews.

Very plausible arguments have at different periods been made in behalf of the importation of certain yellow fever epidemics, but the reasonings of Dr. Axson in this behalf, as set forth in this report, are not of this class, and have not even the interest which attaches itself to *plausibility*. A legislature that would at this period of our history invest a corps of interested officials with the extraordinary authority above designated, could scarcely expect less than a visitation with a writ of lunacy. The whole gist of the evidence as presented by Dr. Axson when eliminated of its equivalents and stripped of its coefficients, may be set forth in a few words.

On the 4th of June, 1858, the ship Elizabeth Ellen, Capt. Staigg,

seventeen days from St. Thomas, was moored at post 33, Third District, New Orleans, after having undergone "fumigation" and the other rites and mysteries to the satisfaction of the quarantine officials. By the way, with regard to the process of "fumigation," it is an open and even question for a debating society, as to whether that process is calculated to increase or diminish the activity of the material cause of yellow fever, should it unhappily be proven to be on board a vessel. This vessel was moored abreast of the ship Independence, Capt. Eustis, whose family, comprising self, wife, daughter, and son, were residing on board of their ship. On the 10th, six days after the arrival of the Elizabeth Ellen, the daughter of Capt. Eustis, and on the 14th his son, were attacked on board of the Independence, with what was pronounced to be yellow fever, the former patient dying on the 22d, and the latter on the 20th. On the 27th, Mervins, a seaman, attached to the latter ship, fell sick and died on the 7th of June. The cases of the son and daughter of Eustis, are declared by Dr. Axson to be the "first cases of the season."

Now when we banish all the eccentricities of the importation logic, and resort to the plain, natural, and direct deductions which are borne out and warranted by experience in this city, the rationale of the sickness and death in the family of Capt. Eustis, is simply reduced to this: The family were "unacclimated," that is to say, they had never had the yellow fever, and it was their misfortune to find themselves at an unfavorable season of the year, in a city which for forty years has certainly never passed twelve months without loss of life from yellow fever, as appearing either epidemically or sporadically; and it so happened that the victims are residing at one of our wharves when the unknown local and spontaneous conditions which periodically arise and produce yellow fever in our midst began to manifest themselves. The distinguished President of the Board of Health however has no relish for so simple an explanation, and declines to rest satisfied with anything short of a vigorous effort to place the epidemic of 1858, securely on the importation basis. To this end the Elizabeth Ellen, that happens to be the most convenient vessel which can be selected from the *numerous vessels*, newly arrived from the hot beds of yellow fever, is arraigned on the charge of having occasioned the death of about five thousand persons, commencing with the victims of the ship Independence in June, and continuing the work of death till late in December. What were in reality the *effects* of an in-

cient epidemic, as seen along-side of the Elizabeth Ellen, are exhibited to the Legislature as its *cause*—the cause of the most protracted and perhaps the most malignant yellow fever epidemic that ever occurred in our country. A conclusion so little in accordance with both reason and experience can only be countenanced or entertained on the strongest possible proofs, and after the most thorough investigation at the hands of competent parties having no interest either directly or indirectly in maintaining the position here so boldly assumed by the President of the Board of Health. The conclusion, however, presents itself in the report wholly devoid of all these necessary passports to credibility. Where there should be a regularly connected chain of testimony, wrought out by the process of examination and cross-examination, we have merely elegantly turned periods and fervid declamation.

The question as to whether the Elizabeth Ellen had, or had not, cases of yellow fever on board previously to her arrival in New Orleans on her voyage from St. Thomas, does not appear to have been raised by the quarantine officials at the time she was “admitted to pratique.” It appears that it was only after the appearance of the yellow fever cases on board of the Independence, lying in close proximity, that circumstances rendered it necessary to agitate the subject. It does not appear that the ship in question found the least difficulty in passing review at quarantine. There was nothing on board that looked suspicious. There was no yellow fever on board at the time, nor was there anything at the time to show that there had been any; nor is it in evidence that she ever had any cases of the disease on board during her sojourn in this city. To my mind, it is of course a matter of no importance in relation to the supervening epidemic, whether the Elizabeth Ellen had or had not cases of yellow fever on board during her voyage from St. Thomas; but to the mind of the President of the Board of Health, it is of course a matter which is all important, and I am constrained to say, that after duly weighing the most unsatisfactory evidence which he has set forth in his report to sustain the affirmative of this question, I look upon it as next to certain, that in making this voyage there was not a case of yellow fever on board of that vessel. Amongst the numerous reasons I have for this conclusion are the following: 1st, that the testimony of Capt. Staigg, of the Elizabeth Ellen, as also the testimony of his mate and stewardess, is positive that there occurred no cases during

the voyage, and that the single death that occurred resulted from consumption, accompanied with hæmorrhage ; and, 2d, that if “ *about* six or seven others of the crew were sick, but recovered,” as asserted by the witness Wolffe, it is scarcely possible these cases could have been yellow fever ; for every one at all familiar with yellow fever will bear witness, that no six or seven patients, during a voyage of only seventeen days, could have passed through so uniform a convalescence, and have so completely recovered from the effects of yellow fever, as to escape the commonest observation at quarantine. In order to establish the position that the yellow fever did prevail on board of the Elizabeth Ellen, it is attempted through the testimony of Wolffe, to prove the untruth of what is asserted by Capt. Staigg, his mate, and stewardess. Capt. Staigg’s testimony is further sought to be impeached by hearsay evidence, going to show that he had subsequently told Capt. Eustis, and it is believed Capt. Healey also, that his (Staigg’s) original statement, that on the voyage he had had “ some fever, which was followed by slight jaundice, and abscess, but that his case was not yellow fever—a disease which he had had before”—was untrue, and that his disease was really yellow fever. The latter impeachment is certainly a case for a complete suspension of judgment till Capt. Staigg is called on to explain as to whether he really did or did not so far compromise the faith and honor, which ought always to belong to a shipmaster, as to resort to the extraordinary tergiversation which is here charged on him. The cause of truth required that the President of the Board of Health should have instituted a most diligent inquiry into this hearsay issue between Captains Eustis and Staigg, and that he should have fully elucidated this question of adjourned veracity which arose between these two commanders. As the case stands there is no valid or direct proof that Capt. Staigg ever made the counter statement which is attributed to him.

The yellow fever cases on board of the Elizabeth Ellen, as attested by Mr. Wolffe, arise before us like Falstaff’s “ men in buckram.” After telling us that “ one of the crew died,” whom he tells us “ *he did not see, but understood* had vomited blood and black matter,” he further says that “ *about* six or seven others of the crew,” [on the nature of whose illness he throws no light] “ were sick, but recovered.” It appears, however, that though he subsequently had an excellent opportunity of dispelling the mists that enshrouded his written testi-

mony, as per Report appendix B,\* in an interview which he had with no less a gentleman than the President of the Board of Health himself, he makes no disclosures in that interview as to there having been "*about* six or seven others of the crew having been sick." The President of the Board appears to have derived no other information from Mr. Wolffe, than the assurance by the latter that he had "no doubts, whatever, that Capt. Staigg and his son had yellow fever on the voyage. and that both were taken down sick *subsequently to the death of one of the crew.*" Here was an admirable opportunity for the President of the Board to have drawn from Mr. Wolffe, for "whose long familiarity with the appearance and symptoms of yellow fever subjects" the President so fully vouches, as rendering Mr. W. peculiarly qualified as an observer, and imparting to his observations the highest value"—here we say was an admirable opportunity for the President of the Board to have propounded amongst others, the following questions to the gentleman from St. Thomas, the truthful answers to which would have proven *something*, though we apprehend they would have failed to prove that the yellow fever existed on board of the Elizabeth Ellen on her voyage to this city.

1. You say that you were seventeen days at sea, having sailed on the 18th of May ; what was the health of all on board at the time of sailing, according to your knowledge ?

2. What is the extent of your knowledge respecting the existence of yellow fever at St. Thomas, at the time of your departure ?

3. Did you notice any one of the crew whom you might have supposed to be far gone in consumption ?

4. Of how many persons did the ship's crew consist, according to your knowledge ?

5. We have said, in our report, that "the ship Elizabeth Ellen was freighted with coal, and lay sixty days in the port of St. Thomas, a port seldom free from yellow fever ;" during this long time what is there, within your knowledge, respecting the health of the crew, and were they acclimated or unacclimated ?

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\* Appendix B, is an extract from an excellent article by a talented editor and proprietor of this Journal, Dr. Chaillé, published in the November number (1858), giving an account of the epidemic. It is remarkable that while the whole tenor of Dr. Chaillé's article shows that he considered that nothing was conclusively proven in behalf of the Elizabeth Ellen importation, the President of the Board rests his whole elaborate defence of that importation on the testimony afforded by this extract from Dr. Chaillé's article. Dr. Chaillé has no where, that I can perceive, committed himself in the least, even in opinion, as to that importation, whatever his real opinions on the subject may be. It is evident that Dr. C. did not think that anything definite could be made out of the problem of contradictions which was revealed to him in seeking out the origin of the epidemic.

6. You say there were "*about* six or seven of the ship's crew sick, but that they recovered;" what was the nature of their sickness, in your opinion, and what are the grounds of your opinion?

7. Will you be particular and name the date of their respective attacks, as far as you know, and tell all you know about their respective convalescences?

8. Speaking of the man that died, you said there were "*about* six or seven others sick," without having told us of what disease they were sick; does this six or seven include, or exclude, the captain and son?

9. What impression was produced on the minds of the passengers, from the fact of there being six or seven cases of sickness on board, and what was said about the disease being yellow fever?

10. What was the result on the management of the vessel, from the fact of there being so much sickness amongst the mariners?

11. Did any of the passengers mingle much with the sick, and if aye, what was said by them respecting the nature of the cases?

12. Was the mortal case of sickness the first case of sickness that occurred on board?

13. You were informed by the captain that one of the crew, *whom you did not see*, had died, and the captain declared he died of consumption, and so reported at quarantine; but you *understood* that he not only vomited blood, but black matter; how came it to be so understood by you, contradictorily to the captain's statement?

14. You say the captain and his son were attacked with what you have no doubt was yellow fever, and that they were both taken sick *subsequently to the death of the seaman*, which you tell us took place on the 24th of May, that death being just 11 days before the mooring of the Elizabeth Ellen in New Orleans; now be very particular and state how long after that event did the sickness of the captain and his son occur, which of them was taken first, or were they simultaneously attacked?

15. "Your long familiarity with the appearance and symptoms of yellow fever subjects," we shall report to the Legislature "as peculiarly qualifying you as an observer, and as imparting to your observations the highest value, as being free from professional or speculative bias" (Report, page 11); now state if you think it is possible that the captain, son, and "*about* six or seven others of the crew," could have been attacked with yellow fever during this seventeen



days' voyage, and that they could all have so completely recovered from the genuine disease, as to excite no suspicions at quarantine that there had been an epidemic on board of the ship?

16. A New Orleans physician, who should have this number of patients in one house, attacked with the disease in the ordinary succession, would be likely to lose at least one-third; and it would be morally certain that visitation, treatment and convalescence would exceed one month; do you not therefore think, if you considered the cases on board, cases of yellow fever, that they presented an extraordinary favorable result, both as to mortality and duration, when we reflect that every one attacked on board of the Independence—which we accuse the Elizabeth Ellen of infecting—died, and that of the first three that died, one was sick well nigh as long as your whole voyage?

17. You say that of the passengers, eleven in number, none were sick, and that they were *principally* from St. Thomas and Rio de Janeiro; are you acquainted with any fact going to show that they were *all* acclimated, as embodied in our report?

18. Do you think it is likely that they were *all* acclimated?

19. If they were all acclimated, they must *all* have had more or less knowledge of yellow fever, and like yourself "peculiarly qualified;" what remarks did you hear from any of them in regard to the character of the disease on board?

Now, these questions, and sundry others, that might have been put to the "peculiarly qualified" Mr. Wolfe, and others that might have been propounded to the Captain, crew, and passengers of the Elizabeth Ellen, accompanied by a thorough and searching examination of the quarantine officials, the whole conducted under oath, and before a Legislative Committee, would in our opinion have laid bare a state of facts utterly incompatible with the hypothesis that the yellow fever existed *at all* on board of that vessel during her voyage. The truth is, after all, the distinguished President of the Board of Health, himself, merely regards the testimony of "the peculiarly qualified" Mr. Wolfe, as available only "in confirmation of the reasonableness of his (the President's) presumptions." The latter appears to be quite conscious that a most inconclusive case has been made out, and he takes a most extraordinary method in an attempt which he makes to "dissipate any lingering doubts that may exist on this subject." With this view he tells us that a fatal case of yellow fever made its

appearance in six days after the "fumigation," and arrival of the Elizabeth Ellen, another in ten days, and another in twenty-three days ; not, it is true, on that vessel, but on the ship Independence, as though the occurrence of these cases whether on one vessel or the other, so far from removing "these lingering doubts" of the fact of preëxisting yellow fever on board of the former, did not rather render the position inevitable that these cases owe their origin solely to the locality of New Orleans. How it is possible that essentially inconsistent and inconclusive evidence of the preëxistence of yellow fever cases on board of the Elizabeth Ellen, can be rendered, consistent and conclusive by the mere occurrence of yellow fever cases in any number of days after the arrival of that vessel, and these cases, too, on board of another vessel, is what cannot be readily conceived. If the preëxistence was untrue, it could not be rendered true by any number of cases that might have occurred, even though they had occurred on board of the Elizabeth Ellen herself. Whatever "lingering doubts" existed as to the importation, must, by all logical rule attach themselves to these cases in their bearing on that importation. The value of these cases to the cause of the official doctrine, must necessarily be determined on the strength of the previous proofs of their connection with importation. If there were "lingering doubts" of the yellow fever having been on board of the Elizabeth Ellen, there is not a lingering doubt removed by the occurrence of yellow fever on board of the ship along-side. The official method of removing "lingering doubts" by means of doubts, is an exemplification of dubiety in its most complicated form.

But no sooner does the President of the Board exorcise "one lingering doubt," than from his mind arises another "in its seat," and "will not down at his bidding." There are some facts, he admits, which "*seemingly* contravene" the hypothesis of the Elizabeth Ellen importation, which relate to no less an event than the wholly independent occurrence of the fatal case of a certain Catharine Maxwell, who was attacked on the 16th of June, and the cases of the Italian sailors on board of the brig Rosalie, all of whom died between the 20th of June and the 5th of July, at quite a *secure distance* from the Elizabeth Ellen. The President of the Board kindly informs the Legislature that the reconciliation of these latter facts, which "*seemingly* contravene" his redoubtable "array of proof" of the Elizabeth Ellen importation, "*involves one of the nicest problems in medical philosophy,*"

and he devotes seven pages of the report to the solution of this problem, with great eloquence and zeal, but without losing or gaining an inch of ground in his effort to trace these cases to the Elizabeth Ellen. The report is characterized by two signal failures: 1st, there is no satisfactory proof that there was any case of yellow fever on board of that vessel, but both positive and inherent evidence tending to show that there was not; and, 2d, the vessel in question has in no manner, under the evidence, been identified with the origination and spread of the epidemic of 1858. But there are other doubts which pertain to the "first cases," which require official explanation. The report of the President of the Board fixes the first cases (the Eustis cases, two in number), in the week ending on the 27th of June; whereas the official reports of the Board, as from time to time published in the daily papers, during the epidemic, *show that during the week ending on the 18th of June, there were two deaths from yellow fever.* On what authority has the President of the Board in his zeal for the Elizabeth Ellen importation, ignored the previous official statements of the body over which he presides, as published in the newspapers? The two deaths in question may have occurred five or six days previous to the 18th, in which case the actual deaths may have occurred on the 11th or 12th, or earlier, and the inception of the disease, in these cases, should they have been as lingering as the case of either the daughter of Capt. Eustis, or of the seaman Marvins, would date back several days before the arrival of the Elizabeth Ellen. Besides, how can the President of the Board take it on himself to say that these Eustis cases were the first cases that occurred, even though it were not proven by the official statements of the Board, that they were not even the first *fatal* cases? Would the distinguished gentleman undertake to make us believe that there can be no "first cases" of yellow fever, without death? Very generally speaking nothing is known of the actual "first cases" in New Orleans, as in the earlier days of our epidemics, there are numerous mild cases, and where there is no death there is no publicity. In such a place as Norfolk, for instance, it is practicable to know something about "first cases," however little distinctive importance ought to be attached to them, but in New Orleans I think I may safely say that the first cases are rarely if ever of public record.

The distinguished President of the Board of Health has boldly proclaimed that the epidemic of 1858 was imported on board of the

Elizabeth Ellen, and what he has asserted boldly it was incumbent on him to prove clearly. Instead of presenting to the Legislature a rhetorical and romantic manifesto to prove what nobody here believes; instead of resorting to the testimony of defunct newspapers to prove that well nigh all our epidemics have been imported; instead of dealing in mere assertion to discredit his own conclusive testimony, as set forth in the Report of the Sanitary Commission that the epidemic of 1853 was of local and spontaneous origin; instead of partisan testimony, picked from the wreck of worn-out controversies, brought forward to prove that certain epidemics in distant cities were imported; instead of vehement denunciation of our city authorities on the charge of "tolerance, indulgence, and actual connivance" with the nurture of "filth" and "accumulated nuisances," which, according to him, "are a portion of the inherited traditions and usages of the city government," which he finds "it an act of charity" to denounce to the Legislature—instead of all this, it was simply incumbent on him to have presented to that body a mass of testimony to have proven what he has so emphatically declared—and which is the only point of interest he has raised—that the epidemic of 1858 was "imported from St. Thomas in the ship Elizabeth Ellen."

But if the distinguished President of the Board is quite pleased with his process of reasoning, he may congratulate himself in the reflection that *such* proofs as he has adduced will never be absent when required. Between the first day of May, 1858, and the first day of November, there were at least one hundred arrivals of vessels in the port of New Orleans from the perennial seats of yellow fever, including vessels from Rio de Janeiro, a city in which since the initiatory epidemic of 1849-50 the yellow fever has become a *fixture*. About fifty of these arrivals occurred before it could be said that our yellow fever of last season had become epidemic, and thirty of them occurred before the appearance of the first yellow fever case alongside of the Elizabeth Ellen. Now, should it have so happened that no case had occurred in the vicinity of that vessel, or that she had gone unfumigated to the bottom on her voyage, the President of the Board need have been at no manner of loss; for having a whole fleet of vessels to fall back upon, he could have very readily discovered a dozen of equally plausible coincidences to have sustained his doctrine.

Indeed, the Elizabeth Ellen presented one of the least plausible of the media of importation, as she had been seventeen days at sea, had

carried besides ballast, nothing but passengers, and had been regularly fumigated, and had passed quarantine, and though "unacclimated" persons on coming into a yellow fever locality are often attacked in twenty-four hours with the disease, there was no case in this instance in the vicinity for the space of six days. She had long since been relieved of her "susceptible" cargo of coal, a very harmless article as it is found in New Orleans, but a most deleterious one in the opinion of "Dr. Paulo Candido, physician to the Emperor of Brazil, and one of the most enlightened and distinguished sanitarians of the age," who testified before the Sanitary Commission. And the whole of this time the port was full of vessels that had arrived after only a few hours run from the seats of yellow fever.

But not the least remarkable assertion made by the President of the Board, is made in relation to the epidemic of 1853, and exhibits a present importation proclivity which is little short of marvelous. After summarily disposing of all of the previous malignant epidemics of our city, as undoubtedly of imported origin, he thus briefly dispatches the memorable epidemic in question: "1853; epidemic; origin doubtful; there were many vessels arriving in the early summer months from Rio de Janeiro, Kingston, etc., having lost persons on board with yellow fever." He however told us in the Report of the Sanitary Commission, after having elaborately investigated the rise and spread of that epidemic, that its origin was *not* doubtful, but that it was brought about by certain "meteorological conditions" which "existed in an exaggerated degree," in combination with "local causes," and that it was not possible that the epidemic could have been imported, and especially from either "*Kingston or Rio de Janeiro!*"

The most cherished authorities of the distinguished reporter are derived from localities far distant, and in which the observers have necessarily drawn their conclusions from a very partial and limited field of observation. He prefers to resort for proofs of his doctrines to what is said to have happened in the harbor of New York, and he does so for the simple reason that "in that city the truth is less obscured by speculations as to the local or domestic origin of the fever," thus giving the preference, on a question of vital importance, to a limited over an extended body of facts, to New York over New Orleans, on a question relating to yellow fever, as on another occasion he rests his high appreciation of the testimony of Mr. Wolffe, from

the fact that Mr. W. is not a member of the profession. But what is more especially observable on the part of the distinguished reporter, is that throughout nearly his whole official document, he is constantly investing the material agent of yellow fever with properties which are not only wholly inconsistent and incompatible with each other, but impossible in their individual capacity.

In one part of the report he quotes authority from the port of New York, of the date of 1822, to show that the material agent of yellow fever is so *fixed, permanent, persistent and irremovable*, on board of a vessel, that "cleansing, washing, white-washing with lime in a ten-fold degree, treating the timbers with slacked lime in large quantities, subsequent to re-purification, *artificial ventilation with wind-sails*"—all, all failed to dislodge the pestiferous agent from on board, and he seeks to fortify this position by relating the following incident on the authority of a member of the Legislature, who was, no doubt, free "from professional bias," and who must have either witnessed the facts himself, or heard of them from some reliable neighbor. The italics are ours :

"During the epidemic visitation of 1853, *in one of our parishes*, a case of fever occurred *in a certain family*, which died. The rest of the family sought safety in flight, and closed their house. The ensuing summer they returned, and on reoccupying it for the first time since the fatal event, the fever broke out again, although there was none *known* in the neighborhood."

The President of the Board next proceeds to clothe the pathogenic agent with the directly antagonistic properties of *diffusibility, volatility* and *levity*, not only tracking the footsteps of human intercourse, but spreading through the propulsion of atmospheric currents, so as to satisfactorily explain the remote cases of Catharine Maxwell and the Italian sailors, as owing their origin to the Elizabeth Ellen. To prove these habitudes of the agent in question, he resorts again to the harbor of New York, as exemplified in the events of 1856 and 1858. "The material cause of yellow fever," says the reporter, "is something capable of transportation by currents of air, to distances varying from half a mile to a mile or more," and his object "in going abroad for his examples to illustrate this feature of this remarkable pestilence, is not because similar facts of equal weight and authority are not to be had under our immediate observation, but because the principle they involve is less mixed up with vexed and unadjudicated

opinions as to the spontaneous growth of fever." The adoption of the settled and adjudicated opinions of New York, which we do not receive as authority, and in which, as is too often the case in New Orleans, the claims of politics predominate over those of science, in order to keep clear of the vexed and unadjudicated ones of New Orleans on the same subject, is merely an adroit waiver of the issue, and cannot warrant the reporter's adjournment either from this city to New York, or into "one of our country parishes." Here, in this city, must the issue be made and met, and here all of the necessary data exist. Moreover, the authority of the learned President of the Board himself, as set forth in the voluminous report of the Sanitary Commission can be adduced to show, that with him at least "the spontaneous growth of yellow fever in New Orleans is, as the lawyers say, *res judicata*."

Now, the above deductions of the distinguished reporter plainly resolve themselves into the two following categories:

*First.* The pathogenic agent of yellow fever is so fixed, permanent, adherent and irremovable, that it fastens itself to the hold of a ship for a whole season, and cannot be expelled either by purificative agents or currents of air; that this agent lies over from year to year in dwellings without being dissipated, and that it fastens and fixes itself with great tenacity on merchandise, and is transported in fomites. "It is governed by no known law controlling the operation of pestiferous gases."

*Second.* The pathogenic agent of yellow fever is so volatile, diffusible, so miscible with the air, so movable by atmospheric currents, that it rapidly issues from the hold of a vessel, and permeates hundreds of millions of cubic feet of air, and that its motion is sometimes so direct, and it may become so *dense!* that the pathogenic current may be arrested "by the intervention of a large tree, with dense foliage, or a dwelling on shore," which was found "to afford complete immunity on Staten Island!" "It" *is* "governed by known laws controlling pestiferous gases."

No physical agent of which we have any conception ever, of course, possessed these two categories of properties in common, and I propose to show in the course of these papers, should time be allowed me to continue them, that actual experience in New Orleans demonstrates *both* of these positions to be, in the main, fundamentally erroneous.

ART. II.—*A Case of Tania Solium*: By Dr. J. R. DOWLER, of Beardstown, Illinois.

THE subject of this notice is a daughter of Mr. Eb. Fish, of Beardstown, Ill., about six years old. The only point of special interest in the case, consists in the efficiency of the remedy—to me wholly new, and accidentally brought to my notice—which was used in its treatment.

I was treating a little brother of this patient, in the latter part of last July, a part of my prescription for whom was, as a drink, the mucilage of elm bark, made by putting pieces of the solid bark into water. The little girl was seen to be frequently eating portions of the bark during the day; the next morning after which, upon my visiting the little boy, the mother, with much anxiety, showed me a vessel containing something that had that morning passed the little girl's bowels, with bits of the elm bark, enveloped in mucilage, which, upon examination, proved to be about three feet of tape-worm. As I supposed the passage of the worm was accidental, and had occurred simply from the looseness caused by the bark, I proceeded to prescribe, what I supposed a much more potent anthelmintic, a large dose of turpentine and castor oil. The turpentine and oil were given several times during the three consecutive days, causing pretty active purging, but with no appearance of any portions of the worm. The little girl being slender, and of irritable temperament, I was forced to desist from further active medications; and partly to allay irritation of the bowels, and partly to test the influence of the bark on the worm, I directed that she should resume the use of the bark as before, by chewing and swallowing in moderate quantities.

On visiting my *new* patient (this little girl), the succeeding morning, I was shown portions of the worm, mostly in separate joints, that had been passed over night. Feeling now some confidence in the *anthelmintic* powers of the elm bark, I directed the continued use of it, in the solid form, as before, while there should be any portions of worm passing. In my daily calls for some days, I had the satisfaction to learn that portions of the worm continued to pass, from day to day, and sometimes several times a day.

I now ceased to visit my little patient, intending only an occasional visit; but my confidence in the efficacy of the *elm bark* being so well established, I advised its use to be continued, even for two or three days after any portions of the worm should be seen in the evac-



uations. The portions of the worm expelled—even the separate joints, were alive, showing more or less motion; a sense of their presence in the rectum, from their action, seemed to urge the patient to go to stool for their removal.

Having given direction for the links or joints to be counted, care was taken to do so, by the mother; and from my notes of the case, taken on the 17th of September last (1858), I find that during about seven weeks of the intervening time, there had been expelled, by estimate (taking the average lengths of the joints), about forty-five feet of worm. At this time there had been no portions of the worm passed for two weeks, during which time the use of the bark had been omitted. The head of the worm with about fifteen inches of the body attached, had been expelled; but thinking that all portions of the worm or *worms* might not have been removed, I advised that the patient should resume the use of the bark. Very soon the next day, after doing so, further portions commenced coming away, among them one about six feet long, tapering to a thread-like termination.

The next time I took notes of the case was March 23, 1859, at which time my estimate of the entire length of the worm that had been expelled, footed up one hundred and thirty-five feet, whether of *one* or more worms, I am unable to say, as in the portions I saw, there were a head and tail, of what I supposed one worm. Since the last estimate, there have been joints occasionally evacuated.

This patient, when first treated, was thin in flesh—had been growing so for some two years—attended with the usual nervous symptoms, startings out of sleep, variable appetite, etc., but with no great departure from good health.

As to the influence of this very bland agent in the dislodgment of the tape-worm, in this case, I think there can be no doubt, whatever may be the *theory* of its action.

The mode by which this parasite maintains its position, must be by its verminous contraction, and adaptation of its flat surface to the inner surface of the bowels, thus preserving itself from being carried along with their fecal contents, and cast off. That it should maintain its position by the force of *suction*, exerted by the mouth, as some authors have supposed, would seem to be absurd, when the great length of the animal is considered, and the forces in operation tending to its removal. The passage of portions of the worm, so promptly,

on the use of the bark, and the ceasing to do so on discontinuance of its use—even while active purgative anthelmintics were used—leave no room to doubt its effectiveness, in at least this case, as a worm-expelling agent.

It seems probable that the bark, with its thick mucilage, so interposes between the animal and the inner surface of the bowels, as to prevent its lateral grasp on their surface, in consequence of which it is compelled to yield to the forces naturally operating, and is carried out with the discharges. But as my object was simply to state the practical *facts* in this case, I will offer no further reflections.

*Beardstown, Ill., April 23, 1859.*

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### ART. III.—*Radical Cure of Hernia.*

MESSRS. EDITORS: I have recently had several cases, which I think are of sufficient practical interest to claim a spare sheet in your journal, and I give them very briefly for what they are worth.

*Radical Cure of Hernia.*—Mr. George Watson, a very respectable planter of Alabama, about a month ago sent me down to Mobile, two cases of scrotal hernia, for treatment, and as they were treated by different methods, and both I believe successfully, they may aid us somewhat in coming to more definite conclusions respecting this common and troublesome affection.

*Case I.*—This was a healthy young negro man, aged about 19, with a scrotal hernia extending well down into the the scrotum; the external ring was small, only admitting the index finger covered by the integuments. It seemed to be in every particular a very favorable case for Wutzer's operation, which I accordingly performed. The whole apparatus was applied in the usual way, with the hernial coverings well invaginated, about an inch and a half into the canal. A good deal of tenderness, and some swelling around the integument, ensued, so much so, that it was thought advisable to remove it on the fifth day. On removing the instrument some ulceration was found both within and without, to the depth of the skin, but on the whole

there was no more compression than seemed to be proper. The patient continued in my private infirmary, walking about freely after the first twelve days, and the invagination and adhesions appeared to be so firm that I have every reason to believe the cure will be permanent. He was sent home to the country, provided with a Marsh's truss, which he was directed to wear for two months.

*Case II.*—This case presents some peculiarities, and was operated on by a new method. Like the other, it was a serotal hernia, but not reducible, and omental instead of intestinal. The diagnosis was difficult, and to me impossible before the operation. On using taxis, I found a flabby, long tumor extending from the external ring along the course of the cord. Failing in reducing it, I placed the subject on his back, gave him a purgative, put him on low diet and kept him in this position for five days, resorting to the taxis every day, without success, though there was no strangulation or pain in it.

Before making any step towards an operation, it was of course necessary to know exactly what the disease was, and I accordingly determined to proceed as in the ordinary operation for strangulated hernia, to cut down upon the sac and open it. On opening the sac I found an omental hernia, with slight adhesions, which were easily broken up by passing freely around the omentum a female catheter, and with the same instrument, the protruding part was pushed back into the cavity of the abdomen.

When I had arrived at this point, I was just where I would have been in an operation of strangulated hernia, and the question was, what step could be taken to close the canal and prevent another hernial protrusion from occurring? The following expedient occurred to me, or rather had been previously determined on: The integuments were dissected loose from the superficial fascia, and the sac with its other coverings was detached from the surrounding parts. I then with my finger invaginated *the sac* into the inguinal canal. The next step was to fix it firmly *in situ*. This was done in the following manner: I took a silver ligature and armed at each end with a long curved needle, with my finger thrust as far as I could press it into the canal, I passed along one border of my finger, on a grooved director, one of the needles, and brought it out through the parietes of the abdomen opposite the extremity of my finger. The other needle was passed in a similar manner along the opposite border of my finger and brought out about half an inch from the end of the ligature al-

ready passed. The reader will now perceive that the ends of the silver wire are brought through the integuments at the same point that the needle passes out in Wützer's operation, and that when they are pulled upon they have the effect of holding firmly the hernial sac, with all its covering, except the skin, in its invaginated position. To complete the operation I placed on the skin, between the wires, a small hard roll of lint, and tied the two ends of the wire firmly over it in a hard knot. The wound made below, when cutting into the sac, was brought together with stitches and dressed in the usual way. Some swelling and tenderness, with slight fever, followed the operation, and a small abscess formed around the wires and discharged at the openings through which they passed; but in about ten days the discharge ceased, all tenderness disappeared, and the adhesions seemed very firm.

Having the two cases under treatment at the same time, I could compare their progress, and the one operated on by the wires suffered less and advanced more rapidly than the other. Like the other he was directed to wear a Marsh's truss for a few weeks.

The owner of these patients, when they arrived at home, was so much pleased with the apparent success, that he has just brought me down two other cases, which I shall operate on in a day or two. One of them has a large external ring, which I propose to operate on by taking two or three stitches with the silver wire instead of one. I should have remarked that the wire in the above case was cut and pulled out on the 6th day.

In Wützer's operation we are told, by those who have most experience with it, that the invaginated serotum after a time is dragged out and not a dimple left to mark the spot, and yet the cure is permanent from the adhesion of the sac and subcutaneous tissues; if so, the plan I have proposed of invaginating the sac without the integuments, with the early use of a truss to bring the parts well together, would seem to present advantages over the operation of Wützer.

I have another idea in my head which I think an improvement on Wützer; it is this: Let the serotum be invaginated and fixed, as in my operation above, by a double wire with a thick skein of thread tied in the loop. The wad of thread thick enough to plug the canal pretty fully is drawn up and fixed there by the wires tied over a plug of lint as above, and a truss is then placed over the ring. When Wützer's instrument is used, the clamp acts only on the tissues *in*

*front of the ring*—with the thread, plug and truss, you have pressure in every direction, and the adhesions should be more extensive.

Certainly a great step has been made towards the radical cure of hernia, and I feel assured that we shall soon arrive at fixed principles which will enable us to overcome all the difficulties in the great majority of cases.

I am about starting for Europe, and shall be able to post you up in all that is new in this department of surgery.

In haste, yours, etc.,

J. C. NOTT, M. D.

MOBILE, ALA., April 15, 1859.

P. S. I had intended to have given you another case or two of a different kind, but have already taken up more space than intended.

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ADDENDUM.—To the above important cases reported by Professor Nott, the following remarks and additional cases are deemed more or less pertinent.

The fundamental principle which underlies the so-called radical cure of hernia, must have been long apparent from both direct occasional observation and analogical ratiocination. But the great timidity in its application, whether well or ill-founded, forms a strong contrast to the boldness—one might almost venture to say, temerity with which modern surgery fearlessly manipulates the very centres of vitality, when the chances of success are sometimes but slight, though still more promising than such as “Nature in disease” offers.

The fear of inflammation as the result of any operation by which the peritoneum should be intentionally or accidentally wounded, has doubtlessly deterred most surgeons from attempting to effect a radical cure of hernia. Nevertheless, in other cases unconnected with hernia, frequent punctures into the peritoneal sac, as in tapplings for dropsy, seldom produce fatal peritonitis, although such patients, unlike those affected with hernia, are greatly diseased, and the incidental manipulations, not to name the possible ingress of air, are all calculated to produce peritoneal inflammation.

A distrust in the permanence of the so-called radical cure of hernia by the occlusion of the hernial tract by means of an artificially excited adhesive inflammation, may be reckoned a retarding cause to the movement to which Wutzer's operation has given a new impetus of late. Without dwelling upon the facts occasionally witnessed in

wearing of trusses from which accidental inflammation has obliterated or closed the hernial outlet, the pathological anatomist may avail himself of abundant evidence from analogy that must be more or less satisfactory. He will find a large per centage of both original and intercurrent old healed lesions of the serous membrane in all the cavities, among all classes of individuals dying of lesions and maladies in no wise connected with morbid alterations of that tissue. Thus in death from yellow fever, typhus, and so on throughout the nosology, the pleural sac is often found totally obliterated from former pleurisy, pleuro-pneumonias, etc., so that the lungs are tied or soldered to the sides of the chest by organized tissue, the product of fibrinous exudation during antecedent active hyperæmias, the adhesions being stronger than the pulmonary tissue, which latter may be torn away piecemeal without breaking up the adhesions. I have met with old adhesions of the cerebral envelopes to the brain, as well as among themselves, more or less obliterating the arachnoidal sac; also strong adhesions among other contiguous organs, as the liver, diaphragm, spleen, etc.; again, I could enumerate cases of complete obliteration of the pericardial sac which adhered everywhere to the heart, etc. Now the lungs and heart sac possess an inherent mobility greater than that of the intestine and being more directly connected with the fountains of life, yet they had not only suffered from violent disease, but had healed, with obliterations of their serous cavities. These obliterations presented every appearance of durability. *A fortiori*, it might be assumed, until positive data appear proving the contrary, that there is no known cause inherent in a floating intestine calculated to destroy the adhesions in the site of a hernia, stronger than such as exist in the lungs, heart purse, etc.

Wounds and fractures in man and animals how rapidly so ever they may appear to heal, require a long time to acquire the maximum strength of material of which they are finally susceptible, and the same may be assumed with regard to the means now in progress for obliterating hernial openings. It may be reasonably assumed that neither a fortnight nor a month, but several months may be required to consolidate newly agglutinated tissues, and to give cicatrizations their utmost strength, together with sure guarantees of perpetuity.

The best truss is generally nothing more than an alternative between two evils in any case, not excepting that in which occlusion has recently taken place, particularly where the lymphous organiza-

tion is not fully consolidated, seeing that its constant mechanical effect might tend to break up recent adhesions, as well as to cause the absorption of new coagulable lymph plasma. Pressure has been often applied for the purpose of limiting, repelling or resolving hypertrophous deposits, tumors, aneurismal and other coneretions.

The judicious application of the truss on the other hand, may be necessary to the prevention of the hernial descent during the period of consolidation, and may occasionally relume the expiring adhesive inflammation, thereby augmenting the exudation, and, consequently plugging up the opening that may not have been otherwise completely fortified against the chances of relapse.

The great oracles of surgery in both hemispheres — oracles more reliable than the Delphic — speak doubtingly of the radical, that is the permanent cure of hernia by the means which have recently been adopted for that greatest desideratum in the whole realm of operative surgery.

The report on the radical cure of hernia by the committee appointed by the American Medical Association (the distinguished surgeon, Geo. Hayward, being chairman), was published in the transactions (1852), and enunciates the following conclusions: "1. That there is no surgical operation at present known which can be relied on with confidence, to produce in all instances, or even in a large proportion of cases, a radical cure of reducible hernia. 2. That they regard the operation of injection by the subcutaneous method as the safest and best. This will probably in some cases produce a permanent cure, and in many others afford great relief. 3. That compression when properly employed, is, in the present state of our knowledge, the most likely means of effecting a radical cure in the greatest number of cases."

Henry H. Smith, M. D., Professor of Surgery in the University of Pennsylvania, says in his recent Treatise on Surgery, that "As hernia is a very common complaint — about one person in every eight suffering from it — and as it is one which exposes the patient to great inconvenience, and may at any moment result in his death, it is an affection to which surgeons have always paid great attention." 639.

According to M. Malgaigne, one in thirteen of the male population in France, suffers from hernia.

The greater prevalence of inguinal herniæ among males than among females, may be owing not only to the severer exertions among the

former, but also to the incomplete consolidation or obliteration of the peritoneum after the descent of the testes.

Every male would be a hernial subject were it not for the physiological process which is set up after the testes descend, carrying down into the scrotum a funicular prolongation of the peritoneum. This non-inflammatory process by which the peritoneum is transformed into a dense fibro-cellular tissue sealing up the opening, art cannot reach in a manner so gentle, painless, and certain. While the rough and painful route which art pursues, contrasts strongly with that of Nature, the result when once fully attained is probably more permanent for art than is generally conceded. Judging from analogies founded on the obliteration of sacs from traumatic and morbid causes, there would seem to be little reason to distrust the permanency of the radical cure of hernia, provided the obliteration were complete *ab initio*. Here, perhaps, lies the cause of failure, the real difficulty. For, if any part of the opening should remain unclosed, a relapse might be expected.

The revival of Gerdy's, Pancoast's, Jameson's, Wützer's, or any one's operation having in view the radical cure of hernia, provided such operation had not proved a failure after a prolonged and reiterated trial, is, it may reasonably be supposed, a prelude of progress, and at the same time, a desirable counterpoise to the *vis inertia* of mere authority, how venerable and heavy so ever that may be.

It were premature to pronounce in favor of the hopeful radicals whose cases of alleged success are few, too few to inaugurate their theory. How numerous soever their cases may be, a good degree of scepticism and reservation are allowable, until time shall have tested the durability of these apparent cures. Time which moulders the pyramids will preserve the name of the man who shall discover a remedy for a loathsome and dangerous affection which is said to prevail in one-eighth of the entire population.

Honor to the Radicals! Let them push on a column a thousand strong into the department of pathological experimentation. Let them offer up hecatombs of animals to illustrate the surgical regions of the hernial outlets, their tissues, adhesive inflammations, dangers, permanent consolidations, etc.

B. DOWLER.

*Radical Cure of Hernia.*—Two cases of Radical Cure of Hernia: by EDWARD LISTER, M.R.C.S. and L.A.C., Surgeon to the West Derby Hos-



pital.—John Welsh, aged 56, dock laborer, was admitted under my care into the West Derby Union Hospital on the 18th September, suffering under diseased liver and jaundice, from which affection he had suffered for a period of eighteen months. He has gradually improved, although at the present time, October 25, there are some traces left of his former malady. He has an oblique inguinal hernia of the right side of eight years' standing, and states that falling into a ship's hold was the cause of the tumor which he noticed about four weeks after the accident. He has always worn a truss; but for the last two years the rupture has increased in size, and it has given him a great deal of inconvenience and pain; also prevented his being engaged in any laborious occupation, the truss being insufficient to retain the hernia in the abdomen when reduced. The external abdominal ring was relaxed, and the inguinal canal very much shortened from approximation of the inguinal openings, so much so that it was at first very much doubted whether the hernia was oblique or direct.

The operation was performed for the radical cure on the 28th October, by Wützer's method, according to the description given by Mr. Speneer Wells, in vol. xxxvii *Med. Chir. Transactions*, and Rothmund's instrument being used of the medium size.

October 29.—There is a slight blush of redness around the exit of the needle; has had neuralgic pain in the testicle and spermatic cord; bowels confined. Ordered *Ol. Ricini et haust. opii. h.s.* There was no urgent symptom demanding the administration of an opiate.

30.—Has had some pain during the night, but limited in extent to half an inch around the exit of the needle. I find the patient sitting upon the edge of the bed, as that position gives him most relief. The pain has somewhat diminished in the testicle and spermatic cord. No discharge perceptible from the wound of the needle. Ordered *haust. opii.* This was given on account of his irritable and restless disposition.

31st.—Feels very comfortable; increased tenderness and redness around the exit of the needle; also tumefaction of the serotum; complaining of the instrument being tight, which I attributed to the needle being too short; bowels confined; ordered *Ol. ricini.*

November 2.—I find the needle has slipped in consequence of being much too short, thereby requiring reapplication of the instrument with a longer needle; the swelling is greater around the puncture than has been observed in former cases by my friend, Mr. R. Jones of the Workhouse Hospital.

3rd. Increased swelling and more diffused inflammation around the puncture with some small amount of discharge; but the patient does not complain of any serious pain; constitutional disturbance very slight.

4th.—Feels himself very comfortable; increased discharge from the exit of the needle; no exudation from the plug, in consequence of which I applied a small quantity of *emp. lyttæ*, with a small spatula, into the duplicature of the serotum.

7th.—Copious discharge from the puncture, as well as from the

parts surrounding the plug ; swelling still continues around the needle ; does not complain of any pains. The bowels have a strong tendency to be confined, which are ordered to be regulated when necessary.

8th.—Remove the instrument, and find by gentle traction the adhesions to be strong ; the slough was about half an inch in diameter. A linseed poultice was ordered, and a T bandage to support the scrotum and testicle.

16.—The slough has separated a day or two, and the granulations are healthy—the discharge at the same time being very copious.

28th.—Granulations almost healed, and discharge very small.

Since the operation was done he feels perfectly comfortable, and there does not appear the slightest disposition to a return of the hernia. He has always suffered more or less from a chronic cough, which caused the hernia to descend even when a strong truss was applied ; supination or flexion of the body produced the same effects.

During the last few days the cough has been very troublesome. He was ordered ipecac. c. morphia ter die. He states that in the place of the hernia descending he feels a kind of jerk at the point where it formerly used to come down ; and the only inconvenience which he experiences is that of being kept in the recumbent position for such a long time.

I was rather reluctant to perform an operation, however trifling in its nature, on an individual so far advanced in years and possessing comparatively inferior physical and reparative powers. However, my apprehensions were dispelled by seeing the success of other cases safely operated upon under more unfavorable circumstances at the Liverpool Workhouse Hospital. The consequences generally attending this operation have been greatly exaggerated, and are at variance with the result of sixteen cases which I have had the opportunity of witnessing both at the Liverpool Workhouse Hospital and that of West Derby.

I have not seen the slightest indication of danger or peritonitis in a single case. Some anomalous symptoms might easily be misinterpreted by a superficial examination : for instance, abdominal tenderness might, and has been, referred to peritoneal inflammation, even in the absence of any positive symptoms of peritonitis. Such a misconception of the concomitant symptoms has been a great drawback to the more general adoption of the Radical Cure.

Wm. Allen, aged 49, laborer, was admitted into the West Derby Hospital on the 19th August, 1858, suffering from chronic bronchitis. He has a bubonocoele of the right side of twenty-four years' standing, which was caused by over-exertion in lifting a bar of pig iron. He has frequently suffered great inconvenience and pain when attempting to use great physical force. The hernia always remained quite reducible, but would descend on assuming the erect posture or lying on the right side. He was wishful to have an operation performed for a radical cure.

The same proceeding was adopted as in the former case.

15th.—Cannot say that he has suffered any pain or inconvenience

except of a very trifling nature. There is a slight discharge from the point of the exit of the needle, as well as from the plug. A circumscribed blush of inflammation is well defined around the needle, and of very limited extent. The tumefaction in this case is not so much as it was in the former one.

18th.—Increased discharge from the exit of the needle with a strong tendency to slough, which is about half an inch in diameter; the greater size of the slough is owing to the skin of this patient being of much more delicate structure than was the case in the last patient; also the pressure exerted on this part by the plate was greater than in the cases I had previously seen. The discharge from the involuted scrotum diminished, in consequence of which I thought it necessary to introduce a little emp. lyttae. As in the former case, it was requisite to pay strict attention to the bowels; keep them in a free condition, and avoid constipation, as it is very liable to follow indolence in patients accustomed to active exertion.

Discontinue the instrumental treatment, which was maintained for ten days; the discharge from the puncture appears earlier than is generally the case, in consequence of the delicate skin of this patient.

When suffering from cough, the bubonocoele used to protrude very much, so much so that he was frequently afraid it would come down into the scrotum.

The patient has never worn a truss; he does not feel the same symptoms when the cough comes on, as he formerly did; he can only compare it to a slight pressure against the aperture where the hernia formerly protruded.

The after-treatment consisted, as in the former case, of linsced poultice, suspensory T bandage, and perfect rest. The time requisite for perfect organization and identification of the plug with the adjacent parts will be proportionate to the age and condition of the patients. The removal of the instrument before the adhesions were sufficiently organized has been the cause of relapse in some senile cases; and, as Mr. Richard Jones observes in his paper in the Medical Times, "if the upper plate is relaxed there is no danger in prolonging the instrumental treatment even to sixteen days."

There are several points in the performance of this simple operation which require great consideration. 1. The needles in present use appear to be much too short, and are apt to slip, as occurred in the case of Welsh. They should be at least six inches long. 2. The bowels should be strictly attended to, and kept regularly and freely open, so as to do away with all physical force. 3. The instrument should not be withdrawn at an earlier period than eight days, and not later than fourteen days; and the time should be proportionate to the age and condition of the patient, and other incidental circumstances; the older the patient the longer should be the time of instrumental treatment. 4. The greatest care should be observed in getting the instrument within the external abdominal ring before transfixing the needle; and when it is properly adapted, it will require some pressure to prevent it slipping above the tendon of the external oblique. 5. To get the intestines completely out of the way of the needle. 6. To keep the patient in the recumbent position, so as to allow of the least

possible motion. 7. If no discharge of epithelium, fat, or serum appears from the invaginated scrotum within five to seven days, it will be necessary to introduce some preparation of cantharides, as the emp. or acet. lyttæ. I may here mention that I think the instrument might be made of much lighter material than that in present use—as light wood (the plug being made of different sizes), malacca, or bamboo cane; or metallic cylinders, with a central tube for the needle. *Med. Times and Gaz.*

*Case.*—John C., æt. 25, a printer, at the hospital, with a direct inguinal hernia on the right side, to which he had been subject eighteen months, with frequent obstructions and constipation of the bowels, and after meals had suffered from great pain and distress in the part. He has tried seven trusses, none of which had power to retain the bowel. The last he tried produced such irritation as to give rise to a series of abscesses in the groin, which was the direct cause of his application. The abscesses being healed, and the patient disposed to submit to an operation for permanent cure, the following was performed:

On examination, he was found to have a hernia projecting into the scrotum, producing a tumor, which he said sometimes reached the size of his two fists. On reducing it, the external ring and the internal opening were found to be enlarged, so as to admit the ends of three fingers, the margin being lax and loose. On the slightest cough or exertion the bowel immediately dropped down into the scrotum.

*Description of Instruments.*—The instruments used in this operation consist of: 1st, a tube two inches and a half long, mounted on a strong handle, about three inches and a half in length, curved in a circle of an inch and a half radius, and flattened into an oval at one end, and forming a linear aperture a quarter of an inch wide at the point; 2d, a strong needle having a corresponding curve, with a perforated point, projecting a full inch beyond the end of the tube when passed through it, and mounted on a strong handle; 3d, a box-wood pad or compress, two inches by one and a quarter, perforated by a hole at half an inch from one end, and crossed longitudinally by a bar of iron wire screwed on to the upper surface; 4th, a subcutaneous section-knife, with a sharp point, a narrow blade, and an inch of cutting edge.

*Operation.*—The patient being laid on his back, with the legs a little drawn up, and the hernia returned, an incision, about three-eighths of an inch long, was made through the skin only, over the cord, about an inch and half below the external ring, with the subcutaneous knife, which was then carried close under the skin, so as to separate a circle of the superficial fascia around the opening of two inches in diameter. The detached fascia was pushed up into the inguinal canal by means of the curved tube, the end being placed through the opening in the skin. The extremity of the tube was then carried behind and close to Poupart's ligament, or the external pillar, to the extent of an inch and a half from the pubic spine. It was then felt, by depressing the handle, to raise the external pillar

upon the extremity. The needle carrying the thickest silk ligature was then protruded through the tube, and pushed through the external pillar and the skin, the latter being previously drawn considerably downwards and outwards. The needle was then withdrawn, leaving one end of the ligature on the surface. The end of the tube was next shifted upwards and inwards, and made to protrude behind the internal pillar, as far as possible from its margin. Through this the needle was then passed, and the skin moved upwards and inwards till the point appeared at the opening previously made. The ligature was then freed, and the needle withdrawn, the tube being still held firmly in its position. The ends of the ligature were next passed through the hole in the box-wood compress, one on each side of the wire bar, over which they were then drawn close and tied firmly, so as to retain the pad close down to the end of the tube in the canal, which was then withdrawn. It will thus be seen that the first passing of the needle pierces not only through the external pillar of the external ring, but through the origin of the internal oblique and cremaster muscles, from Poupert's ligament behind it; and at the second passing it goes through not only the internal pillar, but also the conjoined tendon of the internal oblique and transversalis muscles, which is placed behind it, so that the sides of the inguinal canal are drawn together from end to end by the ligature. The wound in the skin of the scrotum was drawn firmly together by plaster, a fold of linen placed upon it, and the whole secured by a spica bandage; the patient being ordered to remain in bed, and to use no exertion whatever.

The bandage was removed on the third day, when the subcutaneous puncture was found completely healed by the first intention. The compressed ligatures were retained till the fifth day, some œdema and suppuration having by this time appeared around them, with considerable soreness and pain in the groin. The opening for the ligature looked red, healthy, and suppurating. It was dressed with wet lint, oil-silk, a large compress, and spica bandage. In a fortnight it was completely healed. No irritation nor swelling of the testicle occurred in the course of treatment, nor any symptom of peritonitis.

At present, three weeks after the operation, the external ring is felt by the finger, pushed up beside the cord, to be completely blocked up by a broad band of fascia passing across it and up along the canal, with the cord passing by the lower part. The sides of the canal are felt adherent and consolidated. There is no *ballotement* whatever felt on coughing; the groin of the side operated upon being more firm and resistant, in fact, than the opposite, which has always been a little weak. The patient was ordered to wear a truss for some months, to consolidate the part and protect the newly-formed tissues. The skin is firmly adherent to the subjacent structures at the point of the punctures and subcutaneous separation. He has since undergone a severe test to the efficacy of the cure, in an attack of bronchitis, from which he has completely recovered, without in the least affecting the site of the hernia.—*Abstract Med. Sci.*, 1859. *From the Lancet.*

ART. IV.—*A Case of Labor with Occlusion of the Vagina*: By W. A. COCHRAN, M. D., Cambridge, Dallas County, Alabama.

I WAS called, December 26, 1858, about noon, to see Mrs. W., aged twenty-four years, of small stature, inclined to be dwarfish; scrofulous habit, sallow appearance; married fourteen months, and, according to her statement, six and a half months advanced in her first pregnancy.

I found her wrapped up in bed, coughing, weeping, and groaning; and on interrogating her in relation to her case, the following history was elicited: She had her house scoured on the 23d, and from exposure to the dampness of the rooms, caught cold; had some fever on the 24th, with slight pains in the region of her uterus, but they passed off with the fever; slept well at night, and felt better in the morning. On the evening of the 25th, she rode out several miles from home in a carriage and came back in the rain; had fever and cough at night, and suffered a good deal with pains in her head, back, limbs, and uterine region, the latter resembling labor pains. Not getting any better as the morning wore away, I was sent for.

I found her a good deal excited both in body and mind; and beside laboring under an attack of influenza, was strongly threatened with miscarriage.

The pains, although pretty sharp, were evidently of the earlier stage of labor, and such as I had frequently seen arrested under circumstances somewhat similar. I gave her immediately forty drops of laudanum and ten grains of calomel, and as she complained of chilliness, ordered a mustard foot-bath to be given, and warm bricks to be placed about her back and feet. I remained with her to watch the result.

The laudanum not having the desired effect, sixty drops more were administered at the expiration of a couple of hours, under the influence of which she became quiet, and slept a little. Towards night, however, her pains returned, increased in violence, and assuming a bearing down nature, it was evident her labor was setting in in earnest, when an examination was instituted to ascertain the condition of the os uteri.

On introducing the finger into the vagina, I was not a little surprised to find it arrested about two inches from its entrance by a resisting *cul de sac*, which immediately suggested the idea of an *imperforated hymen*. I examined carefully with the point of the finger in every direction to

ascertain the existence of an opening, but none could be detected. By moving the finger freely around, the pelvis was found to be well formed; and by pressing steadily upon the farthest point of resistance, and giving the point of the finger an upward inclination, using at the same time counter-pressure upon the fundus with the other hand during a pain, the mouth of the womb could be felt about half dilated with the membranes slightly protruding, and the head of the fœtus within.

After stating the nature of the case to her husband, he told me there was an obstruction, that it had always existed, but was not discovered till after marriage; that there was an opening in the upper portion of it through which the menses had escaped, and he thought the labor would overcome the difficulty.

I again examined for the opening, and found it to be the urethra, dilated to a size sufficient to admit the point of the forefinger nearly up to the neck of the bladder, which was quite sensitive, but no opening could be traced into the vagina.

A specular examination revealed a firm and resisting fibrous membrane, traversing the vagina obliquely, attached above to the urethra and margin of the vaginal orifice, and passing backwards was inserted into the posterior wall about two inches from the entrance, completely closing the passage. The upper half of the membrane presented a livid or purple appearance, as if it had been bruised, or was the seat of venous congestion from dilated capillaries, while the other half showed only a slight degree of inflammation, and was only slightly sensitive—being wanting in that morbid degree of sensibility which characterizes such obstructions. The carunculæ myrtiformis were well developed upon the lower margin of the vaginal orifice, thus corroborating the view entertained by many anatomists that these bodies are not the relics of the ruptured hymen, but are independent formations.

I pressed every portion of the membrane with the point of a probe, and at last detected a small valvular opening in its central portion, which freely admitted the probe into a spacious vagina beyond. This was the opening through which the menses found an exit, and not the urethral passage—the latter being intact. As the labor was progressing favorably, and there was no reason for expediting it, I concluded to wait till the sac containing the waters should press upon and distend the obstruction so as to favor the opportunity of making an incision.

On mentioning the necessity for such an operation, notwithstanding it was done in the gentlest possible way, and treated as a small matter, yet she became refractory, and refused to submit to any thing of the kind. Kind admonitions, however, served to reconcile her, and my friend Dr. Benjamin E. Cobb was requested to see her, who soon arrived, and after a careful and minute examination, concurred in waiting for the distention of the obstruction by the bag of waters. At length the head descended, and the membranes before it, till the partition was put fully on the stretch—though visible only through the speculum—when I withdrew the instrument, and dilating the vagina with two fingers of the left hand, made a careful crucial incision in the membrane.

The wall was very tough and vascular, about an eighth of an inch in thickness, and bled profusely. The hæmorrhage was so profuse from the cut edges of the incision that it was not carried quite to its attachment either above, below, or laterally, but only sufficiently far to aid the passage of the child, which was very small, and would not have exceeded three and a half pounds in weight. She became quite exhausted from the loss of blood together with the discharge of the waters, so much so indeed, that her head had to be lowered and stimulants given in order to accomplish the delivery, which was done by the natural powers. An examination afterwards revealed a healthy state of the organs beyond.

The hæmorrhage soon ceased, and after reâction was established she was put to bed and an anodyne given.

Dec. 27th.—Rested well last night ; had a bilious operation this morning, and passed a large quantity of urine at the same time without any pain ; complains of no pain, but a good deal of soreness ; has some fever. Ordered her to take some quinine and Dover's powders to-day, and use tepid vaginal injections to remove coagula, and a lincn bougie to be introduced daily to prevent union of the incision.

She was carefully watched for several days, lest puerpural symptoms should arise, but nothing of the kind occurred, and she improved rapidly. The child was very feeble, refused the breast, and took but very little nourishment ; it became emaciated and died on the seventeenth day.

Conception with imperforation of the hymen is a rare phenomenon, though cases of such are recorded. The obstruction, when it exists,



is frequently detected at or early after the age of puberty by its partially or wholly arresting the menstrual flow; but it sometimes happens that it is not discovered till after marriage.

The subject of the above case was always a delicate and sickly girl till menstruation was established, which did not take place until she was eighteen years of age, having suffered for several years previously from all the symptoms of difficult menstruation, minus the flow; but, when once established, the process was carried on without the least pain or inconvenience. She would have uterine and lumbar pains, with a slight enlargement of the abdomen at each menstrual epoch, but it would subside in the interval to be renewed again at the next period. The fluid, if effused, must have been absorbed.

The features of interest in this case, are, first, the phenomenon of conception with the imperforation; secondly, the extreme vascularity of the septum, causing a loss of nearly sixteen ounces of blood from an incision sufficient to admit the passage of a small child; and, thirdly, its abnormal attachment. Instead of being attached to the margin of the vaginal orifice below, its normal site, it was united to the posterior or inferior wall of the vagina two inches from its orifice.

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ART. V.—*On Hydrophobia*: By F. E. GORDON, M. D., of Mobile, Ala.

THIS name derived from two Greek words, signifying water-dread, is the common name in England and America for canine madness; and although in France efforts have been made by the writers of the *Dict. des Sci. Méd.* to apply the term *rage* to it, *hydrophobia* still retains its place in the popular vocabulary of that country. It is a very old disease. The learned Dr. Mason Good quotes a passage from Homer in which Ulysses, speaking of Hector, says: "So with a furious LYSSA was he stung;" and from this he has ventured to restore the Greek term (Lyssa) to this disease, not only as being more classical, but far more correct than the technical term of the present day.

The old writers used the terms *aërophobia* or dread of air, and *pantophobia* or a fear of all things. Others called it *phobodipsia*

(literally fear of thirst) because the patient is thirsty yet fears to drink, and Dr. Hartshorne, of Pennsylvania, has suggested the name of potophobia or fear of drink.

“With respect to a name,” says Cooper in his Surgical Dictionary, “for the disorder, as the patient does not commonly betray any tendency to fury, while the dread of water is really a customary attendant on the complaint, the terms *rabies* and *la rage* seem strictly even more exceptionable than the word hydrophobia.”

Dr. J. L. Bardsley, in his elaborate article in the Cyclopædia of Practical Medicine says, “notwithstanding this profuse luxuriance of nomenclature, the term hydrophobia has always been and still is that by which this affection is distinguished by the rest of mankind as well as by the best medical authors. It is the name applied by Cælius Aurelianus, Galen, Boerhaave, Sauvages, Dessault, Linnæus, Vogel, Sagar, Cullen and Pinel,” and I may add by all the standard English writers down to Watson.

There have been many attempts made to sub-divide this disease, not, however, with much success. Good treats of Rabies Felina and Rabies Canina. Cooper makes two grand divisions, one comprising all cases not ascribable to the bite of a rabid animal, and the other comprehending those cases preceded by a bite. He then sub-divides the first class into the symptomatic and the idiopathic or spontaneous.

By the former is understood an aversion to water occurring as a symptom in any disease, as in hysteria, epilepsy, fevers, etc.; while the latter or spontaneous hydrophobia is a questionable disease supposed to be induced by fright, anger, or other violent passion. This accords in substance with the arrangement of Chomel in his article, *Hydrophobie, Dict. de Médecine*.

Dr. Bardsley comments on the simple arrangement of Cullen into hydrophobia simplex and hydrophobia rabiosa, in the former of which are included the spontaneous and symptomatic, or all cases not traceable to the bite of a dog, and proposes the following arrangement as likely to settle all difficulties: *Genus Hydrophobia*. Species 1.—Hydrophobia Lyssodes—succeeding within the period of two years to the bite of an animal supposed to be rapid. Species 2.—Hydrophobia Paralyssode, not preceded within the period of two years by the bite of a rabid animal, or by any other than anomalous causes.

Hydrophobia Lyssodes: Varieties, canina, felina, avicularis, insectorum, fomitum, saliva. Hydrophobia Paralyssodes: Varieties,

traumatica, inflammatoria, phrenitica, meticulosa, nervosa, pathetica, febrilis. The symptomatic species are hysterica, febrilis, hypochondriaca, maniaca, melancholica, cynanchica, venenata. Here is verge and scope enough under which to arrange all debatable cases.

“Hydrophobia as it appears in the dog is marked by some singular departure from his ordinary habits, such as picking straws, threads, or small bits of paper from the floor and swallowing them, licking the noses of other dogs, or other cold surfaces, as stone or iron. Besides this, he is observed to be lonely, shy and irritable, is less eager for his food, or refuses it altogether. His ears, also, and tail drop, his look is suspicious and haggard, and, sometimes, from the very commencement there is a slight redness and watering of the eyes. In a short time saliva begins to flow from his mouth and passing by degrees into a viscid foam, impedes his respiration, which becomes difficult and panting and his tongue hangs out of his mouth. Fever comes on, and his fauces are said to be inflamed, but although irritable and easily provoked to snap at objects, he still obeys the voice of his master. “It is remarkable also,” says Mr. Youatt, “that the dread of fluids, and even the sight of them, so striking a symptom in man, is often wanting in dogs and other animals, for many dogs lap water during the disease.” In many dogs the symptoms never rise higher than these, but in others, there is a repugnancy to control, and readiness to be aroused to extreme rage on the appearance of a stick, whip or other instrument of punishment, or at any attempt at intimidation, which strikingly characterizes the disease. Even in this state, however, he seldom fights a determined battle, but bites and runs away. This irascibility occasionally ends in indiscriminate aggression and unprovoked fury; and the rabid dog now flies at every creature he meets, bites all other dogs on his way, or if tied up gnaws the wood of his kennel, or should he gain admission into a field where sheep, goats, cattle, hogs, geese, or other timid unoffending animals are collected, he tears and pursues every individual of the flock he approaches. The cat is an early object of his rage, and even the horse and elephant have sometimes sunk under the baneful effects of his bite. The animal at length dies, and apparently by convulsions.” This lengthened description given by Williams, in his book on morbid poisons, is corroborated by the graphic picture condensed by Watson from Youatt, so familiar to all. Twitchings and slight strabismus are here added to the other symptoms as coming on early;

and later, paralysis of the lower jaw, and finally of the loins and lower extremities. On the fourth, fifth, or sixth day of the disease he dies; occasionally in slight convulsions; but oftener without a struggle.

Most writers contend that hydrophobia originates in the dog, fox, jackal, wolf and cat spontaneously; but Youatt declares it to be the result of contagion in every instance. They generally concur, too, that though it may be propagated in man and most of our domestic animals, these are incapable of extending it. There is a case on record (*Amer. Jour. Med. Sci.*) of a gentleman having died hydrophobic in Massachusetts from the bite of a mad raccoon, and one of our Spanish Creoles is said to have fallen a victim to the same disease about fifty years ago, from the bites and scratches inflicted by a rabid panther, which entered his yard at night, and discomfited first his dogs, and then the master, in spite of his torches and musket.

There has been much speculation about the remote causes of hydrophobia, and it has been attributed to heat, eating of putrid meat, starvation, want of water, venereal excitement, climate, etc.; but numerous experiments made by the most careful observers have failed to produce it artificially by any of these causes. Dogs kept in the most disgusting filth and exposed to the hottest rays of the sun, starved to death, or fed with salt meat, and deprived of water, under the directions of Magendie and Breschet, have not in a single instance gone mad.

Hydrophobia prevails in Europe, Asia, and America, and probably in Africa, though not equally in all parts of these continents. Volncy first reported to us its absence in Egypt, and Baron Larrey has since confirmed his statement; but other travellers seem to think it a rare disease, but not entirely wanting there. It is said that the dogs of that country are singularly alike and inoffensive and not given to biting.

In South America it is said to be unknown, and I do not remember to have seen any incidental mention of it by Humboldt, or by the late lamented Herndon, in his explorations of the Amazon; but I fear that more careful investigations will show its occasional presence even in the land of luxuriant vegetation, and of the magnificent Cordilleras.

In the West Indies it prevails at times, though not so badly as in Europe. In Jamaica, Dr. J. Hunter says, "forty years have elapsed without a single dog being known to go mad;" but not long after this boast, it was introduced, and became endemic on the island.

Dr. Kane, whose recent death produced such universal regret among his countrymen, describes the loss of a favorite bitch, from what he supposes was hydrophobia, in the following language: "Yesterday, the mother of one batch, a pair of fine white pups, showed peculiar symptoms. We recalled the fact that for two days past she had avoided water, or had drunk with spasm and evident aversion; but hydrophobia, which is unknown north of 70°, never occurred to us. The animal was noticed this morning walking up and down the deck with a staggering gait, her head depressed, and her mouth frothing and tumid. Finally, she snapped at Petersen, and fell foaming and biting at his feet. He reluctantly pronounced it hydrophobia, and advised me to shoot her. The advice was well-timed. I had hardly cleared the deck before she snapped at Hans, the Esquimaux, and recommenced her walking trot. We shot her of course." (Arctic Explorations, vol. I, p. 123.) He in the same volume describes "the effect of long-continued darkness on the dogs, which produced disease ending fatally with symptoms resembling lock-jaw in thirty-six hours after the first attack."

In the United States, the disease is not uncommon among dogs, according to popular belief; but from the variety of cases reported in our medical journals as occurring in the human subject, I am disposed to think that many of the canine cases are spurious. From Maryland to South Carolina everybody speaks of hydrophobia as a thing known and present, and I presume this to be true of most of the Atlantic States; and, hence its existence cannot be questioned, however its prevalence may be exaggerated.

Here, in Mobile, until recently, the belief of our citizens in its entire absence was unshaken. The oldest physician had never seen a case in dog or man, and the public mind rested in tranquillity.

I have been able, however, to collect a few traditional cases from one of our early settlers, who is well known in this community as a man of character and influence. One of these produced by injuries inflicted by a panther on the eastern shore of Mobile Bay, was alluded to above, and derives additional credibility from the fact that the man was taken to the nearest U. S. garrison, and put under the care of the surgeon there. Another case is narrated as happening in the person of a negro man belonging to the parents of Mr. Joseph Murrell, of this city, who was attacked at the wood-pile and bitten in the face by a wolf. This, as usual, terminated fatally. A third case was an

Indian bitten by a mad dog, who perished about the same period or nearly half a century ago.

A case which occurred here last summer (1858), gave rise to some controversy among medical men, and to much public comment. It presented the symptoms of genuine hydrophobia, with death on the third day, but we were unable to trace it to the bite of a rabid dog. Dr. Hicklin of this city, narrates the case of an Irish woman bitten in New Orleans, who died here a few years ago with this disease.

Prof. Jones, of New Orleans, informs Dr. Nott that he had seen but one undoubted case of Hydrophobia in the human subject during a practice of twenty-five years in that city. He has, however, dissected a dog which died of rabies. Dr. David C. Kerr, whose experience extended back forty years, had not seen a case.

From a review of these facts, we are persuaded that the disease is exceedingly rare in this locality, but may be introduced as it was in Jamaica, or as in Boston, according to the following statement: Dr. J. M. Warren remarked in 1854, that some years since, at a very full meeting of the Medical Society, he had asked the question, whether a case of hydrophobia had ever occurred in Boston? No one had then seen or heard of a case. Since that time there have been six cases reported to the Society.

This is curious, as tending to show that the disease disappears for a time in a given locality, to rouse up again or be reproduced by fresh contagion.

Rabies is not equally easy of communication to man as among dogs and wolves. The latter are more dangerous from their habit of biting at the face. Herbivorous animals, though they take the disease readily, do not propagate it. Mr. Youatt, whose opinion is entitled to great respect, differs on this point as well as several others, from most observers. He has seen a horse communicate the disease. Rabid sheep are said to bite each other with impunity, even in places where the wool is shorn. A question of much greater importance, is, whether man can communicate the disease to his fellow-man? There is not a case of this kind on record in any respectable journal or book of authority. Among the many experiments made to settle the contagious property of the hydrophobia virus in man, but one has tended to shake the generally received opinion. MM. Magendie and Breschet saturated some strings with the saliva of a man who died of hydrophobia on the 19th of June, 1813, inserted them

into the polls of two dogs, one of which went mad on the 27th July following, and bit several dogs who had rabies in due course of time. As so many years have elapsed since these experiments were made, and as they have not been confirmed by more recent ones, and are at variance with all others, it is but fair to argue that the dog went mad from a bite previously inflicted, which had escaped notice, or as some think spontaneously. Among the attendants upon hydrophobia patients many have been bitten from time to time, but they have invariably escaped, which is the more strange when it is remembered that a man's bite is ordinarily more poisonous than that of any of our domestic animals.

The poison is usually absorbed directly through a wound in the skin, and occasionally by the mucous membrane of the lip, though I am inclined to think only when abraded.

The period of incubation may be said to be between six weeks and six months, though cases are cited of ten days' and twenty years' latency. The more common opinion seems to be that the poison lurks in the wound until the period of recrudescence, when the germs being hatched, they are thrown into the body at large, to cause those characteristic symptoms which so fearfully mark the disease. Others think that the virus is at once absorbed and infects the blood and that after an uncertain period it produces functional derangement of the brain, and subsequently organic alteration of the structures supplied by the eighth pair of nerves.

Post-mortem researches have shed but little light on the pathology of hydrophobia. The changes in the brain and spinal marrow have been *found* variable; and the fauces, stomach, and bronchial tubes though generally inflamed, are not always so.

It is true of this as of most morbid poisons, that the structural changes resulting from their introduction into the system, vary with the duration of the life of the victims.

The treatment of hydrophobia is as despairing and ineffectual now, as two thousand years ago. About one hundred and fifty remedies have been enumerated, among which are prussic-acid, arsenic, mercury, etc., but as there is no undoubted case of recovery on record to cheer us on, we shall have little to say on the subject.

Chloroform has in some cases mitigated the severity of the suffering; injections of warm water and narcotics into the veins have relieved temporarily, to be followed by more protracted distress; and

laryngotomy as suggested by Marshall Hall, may save from death by asphyxia, but death still marches on secure of his conquest, having more slowly poisoned all the humors of the body.

Prevention is thought to be the only available remedy for hydrophobia. The excision of the wounded part is highly recommended even up to the period of recrudescence, but the sooner it is done the better. It has failed in one instance though but a quarter of an hour had elapsed ; but in nearly every case succeeds, when done in a day or two after the bite.

Cauterizing with nitrate of silver is preferred by Youatt who has operated four hundred times without a fatal result.

The wound inflicted by a mad dog heals kindly, but after a month or more some tingling or numbness or unusual feeling is perhaps felt in the cicatrix, and it reddens and begins to discharge again. Very soon the difficulty of swallowing comes on, and the hurry of mind and other fearful symptoms which painfully mark this disease.

I will give a brief sketch of the case of young Julia, alluded to before, which forcibly impressed itself upon my mind at the time as one of genuine hydrophobia : On Sunday, the 1st day of August, he complained of his head being stopped up with cold, to relieve which he took snuff and sneezed violently all day. On Monday, election-day, he was out, though not well—took supper at night, and again went out. On Tuesday he was more complaining, and remained about the house. At night his mother proposed to bathe his feet, but when the water was brought he shuddered at it, and complained that it took his breath. This aroused her suspicions for the first time. A neighboring lad, however, prevailed on him to submit to the washing, though it caused him great distress in breathing. On Wednesday Dr. Tuille saw him first, and shortly afterwards I was invited to visit him in company with the Doctor.

We found him walking in a hurried manner about the room, occasionally catching his breath as if cold water had been thrown upon him ; his eye wild and complexion sallow. He hawked up incessantly thick white *cottony* sputa.

He seemed to be excited by our visit and to evince an indefinable dread of something to happen. Upon my asking for a spoon to examine his throat, he was thrown into great agitation, and it was some time before I could calm him sufficiently to be allowed to look at his mouth, with the assurance that I would not use the spoon.



After much persuasion he attempted to drink a glass of water. Upon every effort to bring the glass to his lips he would draw back from it spasmodically ; at last, however, he seemed desperately to gulp a mouthful when it was instantly ejected to the other side of the room. Upon insisting on bandaging his eyes to repeat the trial, and to allow us to administer chloroform, he seemed more than ever excited and appealed to his family for protection in a sobbing kind of speech. At last by the aid and persuasions of his mother he got on the bed and I attempted to exhibit the chloroform. As soon as the handkerchief was brought to his lips he caught it with both hands, and before a partiele of it was inhaled was siezed with a spasm of the glottis, followed by a slight epileptic convulsion. As soon as this subsided, which was less than five minutes, we left, and I never saw him again. I learned from his family that he continued in the same restless way until some time in the night, when he rushed out to the well and mounted on the platform around it, apparently with the design of quenehing his thirst, which was exeessive to the last. Immediately upon being brought back he fell into a convulsion and died.

The other facts connected with the history of this case that I have been able to glean, only serve to perplex us. Julia's younger brother, a lad of fifteen, was born blind, and did not walk until three years old. He is now seemingly idiotic, with half-paralyzed limbs. The other brother and sisters are in good health. He himself is represented to have been of a nervous temperament, though maintaining his health. His mother reports him to have had a fever some four years ago, from which he recovered without a physician. This was all the sickness he ever had. He had been bitten on the leg very slightly some two years before, but no scar could be found, and the dog that inflicted the wound was alive and well on the premises. A small poodle had died in the house a few weeks previous to Julia's-attack. It was siek about a week, eat grass, and bit the cloth on which it lay into shreds; it was not known to have swallowed them, however. It refused to eat, but drank ravenously to the last, and died in convulsions, foaming at the mouth. Julia carried this dog from the house to the yard, and put it into a box for burial; but handled it less than his mother and sisters, who endeavored to feed it during its illness. It should be added, that soon after this, some eight or more persons were bitten by dogs, mostly in the streets, in the course of about a week. Whether this was the result of the per-

secution to which dogs were subjected on suspicion—to being tied up and muzzled until they could break away—or to some disease prevailing among them, certain it is that no case of hydrophobia has resulted from these bites.

In the absence of proof to show the origin of Julia's case, it is not too much to say that we daily pronounce diseases small pox, yellow fever, etc., on less evidence. We glance at the peculiar physiognomy, group the symptoms, and make a diagnosis. But with hydrophobia all this is reversed. We infer nothing. We must produce the virus or the case is not genuine.

A very extraordinary case of hydrophobia is related by Dr. Jackson, of Philadelphia, in the *American Journal of Medical Sciences*, as occurring in the person of a Mrs. Burrows, about thirty years of age. In July, 1848, she was bitten by a dog running at large in the streets of Camden, N. J., of which nothing more was ever heard. Early in October, the slight wounds which had immediately healed, began to swell and become painful; one of them suppurated and healed again, but from the other the inflammation spread up to the axilla. By the last of the month all the horrible symptoms of hydrophobia were developed; spasms were produced by the sight of water, by the glitter of a teaspoon, by the waving of the hand unseen back of her neck. This state of things continued with variations all through the month of November, in spite of calomel, Dover's powder, chloroform inhalations, excision of the wound and cauterization. As the wound suppurated more freely, the pain which extended from it as a starting point lessened, and by the second week of December, when it had entirely healed, she was fairly convalescent. Her shoulder and axilla were, however, sensitive and occasionally painful till January.

This case which is detailed with much minuteness and unctiousness by the learned Professor, is pronounced one of nervous spasm; but it only varies from the most striking cases of hydrophobia by its duration, and the final recovery of the patient. If an individual should luckily survive this disease, it would seem most probable that the virus would be eliminated in this way, by prolonged suppuration of the wound. At all events it is a most strange coincidence that hysterical spasms supervening on the bite of a dog, should assume so characteristic a form.

ADDENDA BY B. DOWLER.

Gibbon signalizes as characteristic of the decline of the Roman

Empire and literature the following circumstances: "A cloud of eritics, of compilers, of commentators, darkened the face of learning." It is hoped that the Mobile and other contributors to the New Orleans Medical and Surgical Journal, will not consider occasional addenda to their articles as implying imperfection requiring commentaries, or an ambition to "gild fine gold."

The obscurity which still reigns as to ætiology, pathology, and treatment of hydrophobia, after two thousand years of investigation, is truly discouraging, but affords no valid reason why this inquiry should be abandoned in despair.

Plutarch says that "Athenodorus in the first book of his epidemial or popular diseases, writeth, that not only leprosy but also hydrophobia, that is to say *the fear of water* occasioned by the biting of a mad dog, were first discovered in the days of the Asclepiades." Celsus (*de Medicina*, lib. v.) alludes to the prominent symptoms of rabies, quoting the Greek term, hydrophobia. He recommends the cupping of the wound and cauterization; hot baths; the immersion of the patient in a pond in order to force him to drink and cure his dread of water and remove thirst, and after this temporary submersion, which he thinks may bring on convulsions, he directs a warm oil bath, and the taking of an antidote, etc., means very little better than those now favored by popular opinion.

The uncertain extension allowed to the period of hydrophobic incubation, varying from a few days to several years, as well as the variant symptoms supposed to be pathognomonic of this disease, not excepting the suffocative spasms which sometimes occur in affections of the throat without any antecedent bites, must often leave the rigid diagnostician in a very undesirable state of doubt. Moreover, records exist showing that persons bitten by animals which at no time showed signs of madness, have died from hydrophobia!

Is not hydrophobia oftener idiopathic than traumatic? Of the latter, I have no experience except what must be reckoned conjectural, that is to say, surgical operations recommended as preventive of the development of the disease. In my practice I have met with both fatal and non-fatal cases of what appeared to be idiopathic or spontaneous hydrophobia, in which, neither patient nor friends could trace the disease to bites or other wounds.

Many descriptions of hydrophobia will apply, with little variation, to some cases of tetanus, as mental inquietude, terror, tremors viscid

salivary or frothy secretion, dysphagia, dyspnœa, paroxysmal rigidity in the muscles, particularly those of the throat, etc.

Without stopping to give details of these cases, I may remark that in some cases of yellow fever, the symptoms often characterized as hydrophobic, have been, to some extent, witnessed in the closing scenes of fatal cases; as the fascial consternation; the apparent dread of swallowing drinks necessary to quench intense thirst. During the act of swallowing, the water is sometimes regurgitated, or causes not only an almost complete temporary suffocation, but violent shuddering, and also spasmodic actions in the muscles of deglutition, with universal muscular agitation little short of a convulsion.

It is no exaggeration to say that, in some cases of hysteria, the muscular disturbances, tetanic spasms, the choking dyspnœa, the sobbing, the glottidian, laryngeal, and general respiratory embarrassment; the foaming at the mouth; the spitting, biting, and howling; the fascial expression of fear and fright; the difficulty, dread, or impossibility of swallowing—all approximate the diagnostics of hydrophobia, though the eventualities of the former are as fortunate, as those of the latter are disastrous. The antecedent bite of a dog rabid or well, and a fatal termination, would be usually accepted as explanations and examples of hydrophobia in not a few convulsive diseases, and the more so, because the diagnosis of the former is as yet vague and uncertain. Nevertheless, the testimony of competent witnesses for two thousand years establishes the reality of this disease, although, perhaps, no one person has seen it often enough to furnish a satisfactory history of the malady in all its essential features. According to the U. S. Mortality Statistics for the year ending June 1, 1850, twenty-one whites and five blacks died of hydrophobia. Of this number, the geographical distribution is as follows: Rhode Island, four; Virginia, four, New York, three; Territory of New Mexico, three; Vermont, two; Pennsylvania, two; Illinois, Maryland, Michigan, New Hampshire, New Jersey, North Carolina, Georgia, and Louisiana, one each. From this geographical tableau, it would appear, that the numerical history of the year aforesaid, shows that south of Virginia, the Atlantic and Gulf States furnish but three cases, a smaller number than the tiny State of Rhode Island, the smallest State of the Republic, with a population scarcely exceeding that of New Orleans.

According to the Committee on Hydrophobia (Drs. Blatchford and

Spoor), whose report appeared in the transactions of the American Medical Association (vol ix), it appears that "hydrophobia is of frequent occurrence in France and Northern Europe, as well as in the Northern States of this Union. On the Continent of Europe especially is it so, as will appear from the following curious statistical table of its ravages in Prussia: In 1810, there were one hundred and four deaths from hydrophobia in that kingdom; in 1811, one hundred and seventeen; 1812, one hundred and one; 1813, eighty-five; 1814, one hundred and twenty-seven; 1815, seventy-nine; 1816, two hundred and one; 1817, two hundred and twenty-eight; 1818, two hundred and sixty; 1819, three hundred and fifty-six; total, one thousand six hundred and fifty-eight."

This Committee has published, for the first time, one hundred and six histories of hydrophobia by American physicians who had witnessed the cases which they report.

In 1852, the deaths from hydrophobia, as reported in France, amounted to forty-eight.

Dr. Gordon has referred to some experiments by the late M. Magendie, made in 1813. The following additional experiments reported in his Journal of Physiology, Jan., 1821, (*Med. Rec.*) will be subjoined.

"*Experiments on Hydrophobia, by M. Magendie, M. D.*—M. Magendie remarks that, both in animals and men laboring under hydrophobia, the most active substances, the most powerful narcotics, have no perceptible operation. This holds good, not only as to matters taken into the stomach, but injected into the veins. For instance, he has injected into the veins of dogs that were hydrophobic, large doses of opium (ten grains), without any perceptible narcotic effect; while a single grain produced eight or ten hours of somnolency in a healthy animal of the same species. It was the same in man. M. Dupuytren and our author injected into the radial vein of a young man, in rabies canina, about eight grains of the gummy extract of opium, without any apparent result. Prussic acid was also injected into the vessels of dogs, with the same want of effect. MM. Magendie and Breschet inoculated a healthy dog with the saliva of the young man above-mentioned, by inserting some of the fluid under the skin of the forehead. The animal became mad at the end of a month. Two dogs bitten by the latter, became affected with hydrophobia in forty days. These last bit several other dogs, but without effect. So that, accord-

ing to these experiments, the virus becomes innocuous in the third inoculation or generation. But to come to the main experiment of this paper.

“The proprietor of a kind of menagerie, in Paris (*le combat des animaux*), sent for M. Magendie to see a very large and strong bitch, in a high state of rabies. The constant agitation of the animal—hoarse and short barkings—and fierce expression of the eye, convinced our author that the animal was hydrophobic. Early next morning, M. Magendie, attended by several of his most zealous pupils, secured the animal, with some difficulty and hazard. M. Magendie then opened the left jugular vein, and drew off about sixteen ounces of blood; after which, he injected nearly forty ounces of water; during the latter part of the operation, however, permitting ten or twelve ounces of blood and water to flow from the upper part of the orifice. The injection finished, the dog was let loose into her den; and to their great surprise, coiled herself up and lay down, as if to sleep, in the most perfect state of calmness. The fierce expression of the eye was entirely gone—she did not bark, and only ground the teeth, when a stick was put into her cage. M. Magendie waited an hour, during which the animal lay perfectly quiet. Some pupils were left to watch her. About five hours afterwards, she was seized with a difficulty of breathing, which increased, and killed her in half an hour more. On dissection, the brain, spinal marrow, and all the organs, excepting the lungs were sound. The lungs were gorged with watery blood, and the mucous membrane appeared inflamed. M. Magendie, before the fatal termination of this case, accused himself of having injected too much water, and anticipated effusion in the lungs. What led him to this experiment? It was from observing that, in artificial aqueous plethora, the various functions of the animal, especially those of the nervous system, were very evidently enfeebled. Now, in rabies, the excitement of the nervous system is carried to its utmost limit; and hence he was naturally enough led to try the sedative effects of aqueous injection and bleeding. Moreover, from the time an animal becomes mad, he ceases to drink; while the pulmonary and cutaneous transpirations are in full force. Hence, he found the blood of rabid animals thick, and apparently without serum. Upon the whole, this experiment, though unsuccessful, holds out a ray of hope in this hitherto incurable affliction.”

The literature of hydrophobia scarcely affords a more interesting

case of this malady than that found in Dr. Rush's essay on the same. (*Inq.* ii, 193.) The book being out of print, the republication of this suggestive case will doubtlessly be acceptable to the reader, more especially as some later writers have obtained the credit of suggesting tracheotomy in hydrophobia as something quite original growing out of the incomprehensibilities of the excito-motory system.

“ Since the first edition of the foregoing observations, I have seen a communication to the editors of the Medical Repository, by Dr. Physick, which has thrown new light upon this obscure disease, and which, I hope, will aid the remedies that have been proposed, in rendering them more effectual for its cure. The doctor supposes death from hydrophobia to be the effect of a sudden and spasmodic constriction of the glottis, inducing suffocation, and that it might be prevented by creating an artificial passage for air into the lungs, whereby life might be continued long enough to admit of the disease being cured by other remedies. The following account of a dissection is intended to show the probability of the doctor's proposal being attended with success.

“ On the 13th of September, 1802, I was called with Dr. Physick, to visit, in consultation with Dr. Griffiths, the son of William Todd, Esq., aged five years, who was ill with the disease called hydrophobia, brought on by the bite of a mad dog, on the 6th of the preceding month. The wound was small, and on his cheek, near his mouth, two circumstances which are said at all times to increase the danger of wounds from rabid animals. From the time he was bitten, he used the cold bath daily, and took the infusion, powder, and seeds of the anagallis, in succession until the ninth of September, when he was seized with a fever which at first resembled the remittent of the season. Bleeding, purging, blisters, and the warm bath were prescribed for him, but without success. The last named remedy appeared to afford him some relief, which he manifested by paddling and playing in the water. At the time I saw him he was much agitated, had frequent twitches, laughed often, but, with this uncommon excitement in his muscles and nerves, his mind was unusually correct in all its operations.

“ He discovered no dread of water, except in one instance, when he turned from it with horror. He swallowed occasionally about a spoonful of it at a time, holding the cup in his own hand as if to prevent too great a quantity being poured at once into his throat. The quick manner of his swallowing, and the intervals between each time of his doing so were such as we sometimes observe in persons in the act of dying of acute diseases. Immediately after swallowing water, he looked pale and panted for breath. He spoke rapidly and with much difficulty. This was more remarkably the case when he attempted to pronounce the words *carriage*, *water*, and *river*. After speaking he panted for breath in the same manner that he did after drinking. He coughed and breathed as patients do in the moderate grade of the cynanche trachealis. The dog that had bitten him, Mr. Todd informed me, made a similar noise in attempting to bark, a day

or two before he was killed. We proposed making an opening into his windpipe. To this his parents readily consented; but while we were preparing for the operation, such a change for the worse took place, that we concluded not to perform it. A cold sweat, with a feeble and quick pulse came on; and he died suddenly, at 12 o'clock at night, about six hours after I first saw him. He retained his reason, and a playful humor, till the last minute of his life. An instance of the latter appeared in his throwing his handkerchief at his father just before he expired. The parents consented to our united request to examine his body. Dr. Griffiths being obliged to go into the country, and Dr. Physick being indisposed, I undertook this business the next morning; and in the presence of Dr. John S. Dorsey (to whom I gave the dissecting knife), and my pupil Mr. Murduck, I discovered the following appearances. All the muscles of the neck had a livid color, such as we sometimes observe after death, in persons who died of the sore throat. The muscles employed in deglutition and speech were suffused with blood. The epiglottis was inflamed, and the glottis so thickened and contracted, as barely to admit a probe of the common size. The trachea below it was likewise inflamed, and thickened, and contained a quantity of mucus in it, such as we observe, now and then, after death from cynanche trachealis. The œsophagus exhibited no marks of disease; but the stomach had several inflamed spots upon it, and contained a matter of a brown appearance, and which emitted an offensive odor.

“From the history of this dissection, and of many others in which much fewer marks appeared of violent disease, in parts whose actions are essential to life, it is highly probable death is not induced in the ordinary manner in which malignant fevers produce it, but by a sudden or gradual suffocation. It is the temporary closure of this aperture which produces the dread of swallowing liquids; hence the reason why they are swallowed suddenly, and with intervals, in the manner that has been described; for, should the glottis be closed during the time of two swallows; in the highly diseased state of the system which takes place in this disease, suffocation would be the immediate and certain consequence. The same difficulty and danger attend the swallowing saliva, and hence the symptom of spitting, which has been so often taken notice of in hydrophobia. Solids are swallowed more easily than fluids, only because they descend by intervals, and because a less closure of the glottis is sufficient to favor their passage into the stomach. This remark is confirmed by the frequent occurrence of death in the very act of swallowing, and that too with the common symptoms of suffocation. To account for death from this cause, and in the manner that has been described, it will be necessary to recollect, that fresh air is more necessary to the action of the lungs in fever than in health, and much more so in a fever of a malignant character, such as the hydrophobia appears to be, than in fevers of a milder nature. An aversion from swallowing liquids is not peculiar to this disease. It occurs occasionally in the yellow fever. It occurs likewise in the disease which has prevailed among the cats, both in Europe and America, and probably, in both instances from a dread of suffocation in consequence of the closure of the glottis, and sudden abstraction of fresh air.



“The seat of the disease, and cause of death, being, I hope, thus ascertained, the means of preventing death come next under our consideration. Tonic remedies, in all their forms, have been administered to no purpose. The theory of the disease would lead us to expect a remedy for it in blood letting. But this though now and then used with success is not its cure, owing, as we now see, to the mortal seat of the disease being so far removed from the circulation, as not to be affected by the loss of blood in the most liberal quantity. As well might we expect the inflammation and pain of a paronychia, or what is called a felon on the finger, to be removed by the same remedy. Purging and sweating, though occasionally successful, have failed in many instances; and even a salivation, when excited (which is rarely the case), has not cured it. An artificial aperture into the windpipe alone bids fair to arrest its tendency to death, by removing the symptom which generally induces it, and thereby giving time for other remedies, which have hitherto been unsuccessful, to produce their usual salutary effects in similar diseases.\* In removing faintness, in drawing off the water in ischuria, in composing convulsions, and in stopping hæmorrhages in malignant fever, we do not cure the disease, but we prevent death, and thereby gain time for the use of the remedies which are properly to cure it. Laryngotomy, according to Fourcroy’s advice, in diseases of the throat which obstruct respiration, should be preferred to tracheotomy, and the incision should be made in the triangular space between the thyroid and cricoid cartilages. Should this operation be adopted, in order to save life, it will not offer near so much violence to humanity as many other operations. We cut through a large mass of flesh into the bladder in extracting a stone. We cut into the cavity of the thorax in the operation for the empyema. We perforate the bones of the head in trepanning; and we cut through the uterus, in performing the Cæsarian operation, in order to save life. The operation of laryngotomy is much less painful and dangerous than any of them; and besides permitting the patient to breathe and to swallow, it is calculated to serve the inferior purpose of lessening the disease of the glottis by means of local depletion. After an aperture has been thus made through the larynx, the remedies should be such as are indicated by the state of the system, particularly by the state of the pulse. In hot climates it is, I believe, generally a disease of feeble reaction, and requires tonic remedies; but in the middle and northern States of America it is more commonly attended with so much activity and excitement of the blood-vessels, as to require copious blood-letting and other depleting remedies.

“Should this new method of attacking this furious disease be adopted, and become generally successful, the discovery will place the ingenious gentleman who suggested it in the first rank of the medical benefactors of mankind.

“I have only to add a fact upon this subject which may tend to increase confidence in a mode of preventing the disease, which has

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\* The hoarse barking or the total inability of mad dogs to bark, favors still further the idea that the mortal seat of the disease is in the glottis, and that the remedy which has been proposed is a rational one.

been recommended by Dr. Haygarth, and used with success in several instances. The same dog which bit Mr. Todd's son, bit at the same time, a cow, a pig a dog, and a black servant of Mr. Todd's. The cow and pig died; the dog became mad, and was killed by his master. The black man, who was bitten on one of his fingers, exposed the wound for some time, immediately after he received it, to a stream of pump water, washed it likewise with soap and water. He happily escaped the disease and is now in good health. That his wound was poisoned is highly probable, from its having been made eight hours after the last of the above animals was bitten, in which time there can be but little doubt of such a fresh secretion of saliva having taken place as would have produced the hydrophobia, had it not been prevented by the above simple remedy. I am not, however, so much encouraged by its happy issue in this case as to advise it in preference to cutting out the wounded part. It should only be resorted to where the fears of patient, or his distance from a surgeon render it impossible to use the knife."

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ART. VI.—*Are Overflows Preventive of Yellow Fever?*

SOME intelligent observers have thought that yellow fever never occurs in those parts of Louisiana subject to inundation, during those years in which they are overflowed. With no desire to build up or tear down any particular theory, we have attempted to elicit the simple facts bearing on this point, in order that they might be recorded for comparison with any future observations. The following correspondence furnishes all of these facts which we have been able to procure, except such as relate to New Orleans. In regard to the latter, New Orleans was inundated in 1849, during which year there was no epidemic of yellow fever. In 1858, the villages of Gretna, McDonoghville, and Algiers, situated on the banks of the Mississippi directly opposite this city, were partially inundated, the water extending to within a few squares of the levees. Yellow fever, however prevailed in these places equally as in the city, although we are credibly informed that its ravages were severer in those squares which were not overflowed. The constant intercourse of all the inhabitants of these villages with New Orleans, renders it difficult to deduce satisfactory conclusions from the facts stated.

"BAYOU SARA, LA., Feb. 16, 1859.

"S. E. CHAILLÉ, M. D.: *Dear Sir*—I take pleasure in complying with your request, in furnishing you with the facts in relation to the exemption of Bayou Sara and St. Francisville, from the epidemic of yellow fever of 1858, for record in your Journal

"For the information of your distant readers, it may be well to state that Bayou Sara is situated immediately upon the bank of the Mississippi river, on an alluvial bottom, and St. Francisville upon a high bluff, that rises abruptly, about half a mile in the rear of Bayou Sara. The river overflowed Bayou Sara in May, 1858, and it remained submerged to the depth, in many places, of five feet, until the middle of August. During the months of May and June, and the first two weeks of July, both towns were unusually exempt from disease. Thereafter, fevers became very prevalent in both towns, and in all the country, for miles around. These were mostly of an intermittent or remittent type; but with the supervention, to an unusual degree, of congestion of the brain, and of the thoracic and abdominal viscera. Some cases of typhoid also appeared. The waters having subsided late in August, and left the town, the streets, yards, and commons were covered with a slimy mud, and the decaying animal and vegetable matter gave rise to a horrible stench. Much apprehension was felt and expressed, of a fatal prevalence of yellow fever, which, at this time, was committing such ravages in New Orleans. The facts are, however, that notwithstanding boats landed daily and almost hourly at our wharf, not a single citizen of either town took the disease. Several cases presented, indeed, of strangers, who came well into Bayou Sara, from the infected districts of New Orleans and Woodville, and who afterwards took the disease. Dr. Whicher informs me of several cases in his practice, and in my own there were two. Although there were no attempts at isolation, the disease did not spread, either in Bayou Sara, St. Francisville, or on the neighboring plantations. I believe that every other town on the river between New Orleans and Vicksburg, was, more or less, scourged by this disease; and this, notwithstanding the enforcement of quarantine regulations at several places, as strict as is, in this country, possible. Bayou Sara has never instituted quarantine, but, as before stated, has almost hourly intercourse with the city.

"These facts are singular. Their value, isolated as they are, is not much; yet should they be gathered into the great storehouse of

knowledge for future use and comparison, should any such occur, with like facts in the future.

"Whether the popular belief that this exemption was the result of the overflow, has any foundation, is more than can be determined. I have inquired among the old citizens, and sought after old registers to ascertain if the like coincidence has ever before occurred. But I could discover nothing authentic or reliable. One swallow does not make a summer. One fact of this kind will not suffice whereon to build an hypothesis.

"Very truly,

"L. FAIRFAX DASHIELL."

"ALEXANDRIA, LA., April 1st, 1859.

"DR. CHAILLÉ: *Dear Sir*—Your interrogatories in regard to the history of yellow fever at this place, and its connections, if any, with its inundations, received my particular attention, and I have elicited the following reliable information:

"There have been ten epidemics of this disease in Alexandria, the first occurring in 1819; since which time Alexandria has been six times inundated. The years of yellow fever were 1819, '22, '27, '31, '37, '39, '47, '53, '54, '55. The six years in which overflows occurred were 1828, in May; 1835, (partial overflow) in May; 1844, in September; 1849, in August; 1851, in March and April; 1858, in February, March and partially in April.

"The epidemic of 1839 was considered by Dr. Dudley an epidemic of violent gastritis and not of yellow fever. And Dr. S. A. Smith, one of our ablest physicians, thought the disease which prevailed in 1854 was dengue, in which he was, however, at variance with his professional colleagues here.

"In 1819 the population of Alexandria was about 500, and is at present three times as great. I am not aware that any meteorological observations were made during the prevalence of our epidemics. In 1847, yellow fever for the first time attacked negroes, among whom there was one death; and the epidemic of 1853 differed from its predecessors in that it spread from the town into the country, prevailing in the pine woods as well as in the low or "bottom" lands. In one family, living in what is here known as the "Cotile Pine-woods," there were sixteen cases. The disease, did not, however, in this case spread to the neighboring houses.

" In 1855, there occurred some interesting facts which deserve a record in the history of this disease. In this year it prevailed at the house of Jno. J. Irwin, who resided in the pine woods of this parish (Rapides), on Gray's Creek, some seventeen miles north of Alexandria. On the night of the 12th of September, 1855, a Mr. Griffin, accompanied by his wife, called at Mr. Irwin's. They had been on a visit to their son, who was ill of yellow fever at Point Meagher, about twenty-five miles below Alexandria, on Red River. When they arrived at Mr. Irwin's, Mrs. G. complained of being very much fatigued (having ridden about forty miles), but was not otherwise indisposed. The next morning she had fever, and was so unwell that she had to be conveyed home (about fifteen miles) in a carriage. She did not leave Mr. Irwin's until about 10 o'clock, A. M. In the mean time she had a pallet spread on the floor. This pallet was not the bed or mattress, on which she had slept the previous night. A short time after Mrs. Griffin had left Mr. Irwin's, O. P. Alexander, the son-in-law of Mr. Irwin, called at Mr. I.'s house. The pallet on which Mrs. G. had been lying was still on the floor. Without knowing that Mrs. G. had been lying there, he laid down on the pallet to rest for a short time. He went home, a distance of about one and a half miles, and did not complain of being indisposed until the morning of the 29th of September, when, on rising from his bed, he said he felt tired. About 11 o'clock, A. M., this same day, he had a chill. This was followed by violent fever. On Tuesday, October 4th, his wife observed small dark "spots or specks" in the matter vomited. On the following night (the 5th of October), after having thrown up considerable quantities of black vomit, he died. About the same time Mrs. Griffin died at her own house.

" On the morning of the 27th of September, one of Mrs. Irwin's daughters, about twelve years old was taken ill, and on the following day a younger sister was also taken with the same disease. Every member of Mr. Irwin's family (eight in number) were now successively attacked. In addition, his son-in-law, B. J. Goree, his wife, and father, E. Evans, a neighbor, a man by the name of Wilkins, who was living with Mr. I. and his mother, and Mrs. Alexander and child. The whole number of cases was nineteen. Of these, five—that is to say, Mr. Irwin and wife, Mr. Alexander, Mr. Wilkins, and Mrs. Griffins, terminated fatally.

" What is remarkable in this epidemic is, that it did not spread from

Mr. Irwin's house. None took it but those who visited Irwin's. Mrs. Griffin went home and died, without any of her attendants taking it. Mr. Alexander went home and died, none of his attendants taking it, except those who also visited his father-in-law Irwin. Mr. Evans went home, a distance of about three quarters of a mile, no other member of his family took it. Mr. Goree Sr., went home, a distance of four miles, after taking it, but no other member of his family took it. Mrs. Alexander and child both had it at Mr. Irwin's.

"It will be seen that the disease was confined to one particular spot, and this is *prima facie* evidence that there must be a peculiar state of the atmosphere for the propagation of the poison.

"Why this epidemic should thus have been confined to this one spot—why it did not spread to others where apparently there were all the necessary conditions for the propagation of the virus, is a problem we leave to medical philosophers to solve.

"Hoping the above may be interesting to yourself as well as to the readers of the Journal, I remain,

"Respectfully,

"G. S. D. ANDERSON, M. D.

ADDENDUM, BY B. DÖWLER.

At the request of my friend, Dr. Chaillé, I have added a few data relating to the query which heads this article. The paucity of these data is partly owing to a want of leisure to examine my MSS, and also, to the absence of literary, and scientific records in Louisiana. Upon former occasions I had suggested the historical coincidence or parallelism between the construction of levees for the prevention of overflows, and the rise, progress, and perpetuation of yellow fever upon the Lower Mississippi, without pretending to trace any necessary connection between these events as cause and effect.

Yellow fever is a disease of the hot season. Heat is one of its most essential known conditions. An overflow during the highest stage of the river, extending over from ten to twelve thousand square miles, from a few inches to eight or ten feet in depth, during the hot season, must produce great refrigeration by evaporation, and, consequently diminish one apparent cause of yellow fever, that is, heat. New Orleans is, with its environs, least subject to overflow as compared with other portions of Lower Louisiana, but is most subject to yellow fever.

In other lands, however, where there are no inundations, yellow fever prevails, a fact which weakens, without wholly destroying the presumption that overflows may be beneficial in preventing this disease in New Orleans, and the more so, as the essential cause of this disease is unknown, while some of its conditions are obvious, as presence of strangers, the hot season, etc.

In this state of uncertainty as to the essential cause of yellow fever, and taking into view the diminished or total absence of yellow fever epidemics hitherto witnessed during several inundations of the city, as in 1816, 1849-50, etc., a reasonable presumption arises favorable to the expediency of at least irrigating or washing out every street of the city by constant streams of river water day and night during the hot season. This could readily be done for several months, by locks placed in the levee; and when the river recedes, by simple means well known to engineers. The tapping of the river for seven miles in front of the city so as to wash every street, from April to November, could not fail to purify and cool the city, and would probably diminish, if not wholly prevent the ravages of yellow fever. An experiment of this kind no where so easy of execution as in this city, is, independently of its bearings on yellow fever, the most important one which rational sanitarians have in their power to accomplish for this city—one which deserves a trial of ten or more years' duration.

Yellow fever did not appear in Louisiana until 1796. Anterior to this æra, the annual inundations were little restricted by artificial levees; since that time the latter have progressed with great rapidity. The sugar cane was introduced in the latter half of last century. The whole amount of cotton exported from Louisiana in 1786 was six bales, and in 1792 it amounted to only 138,328 lbs. The production of sugar and cotton required levees to reclaim the alluvial lands from annual inundation.

Dr. Chaillé has already mentioned the comparative salubrity of 1849, '50, when crevasses and inundations abounded.

The next year (1719) after Bienville founded New Orleans, its site and buildings were so completely inundated that they were temporarily abandoned, not from a fear of sickness from the inundation, but from a dread of "too much water," by which "poor Ophelia was drowned."

In the year 1816—a year remarkable for salubrity in this city, a crevasse took place early in May, three or four miles above New

Orleans, which poured a flood into the rear or lower portion of the city several feet in depth, and gave rise, as usual, to alarming but false prognostications of an epidemic as the natural result of the inundation. On the 25th of May, *The Friend of the Laws*, a Franco-American newspaper, in view of the prevalent alarm, says :

“ We announce with satisfaction, that according to the documents which we have procured, the fear of an epidemic appears to be absolutely chimerical. Here are the facts which we guarantee, and to which we invite the attention of the citizens, especially such as are from the north, because they are more exposed to the dangers of the climate, and consequently more liable to be struck down. Old authorities recount three inundations caused by crevasses at this same place. One occurred in 1785, and another in 1791. There was not a greater mortality upon these two years than others. Those who doubt may go and consult the mortuary register kept by our venerable pastor. A third inundation occurred in 1799. There died this year more or less ; but not in consequence of the inundation. A vessel arrived from the north, where yellow fever had been committing severe ravages for a long time, which introduced into this city patients and the disease. This circumstance is well known to many respectable persons, and let their intelligence and information dispel all the apprehensions which have sprung out of the popular tales.”

Subsequently, the Medical Society (French) of New Orleans referred to this inundation as having had a salutary influence upon the health of the city in 1816.

Dr. J. W. Heustis, of the U. S. Army, whose “ residence in Louisiana, amidst the professional avocations of a military life,” and whose book on the “ topography and diseases of Louisiana,” was published in 1817, says, “ that a rainy season renders the city of New Orleans healthy, by keeping the ponds filled with water.” (39.) This statement is not given with a view of affirming or denying its justness, but for the purpose of showing what was the opinion entertained fifty years ago concerning the influence of inundations, rains, etc., upon the sanitary condition of the City. Certain it is, that the last great inundation was followed by an extraordinary and prolonged exemption from yellow fever anterior to 1853.



ART. VII.—*Case of Frambæsia and Vesico-Recto Vaginal Fistula*: By  
SAMUEL A. CARTWRIGHT, M. D.

IN the Autumn of 1856, Mr. G. More, of St. James's Parish, Louisiana, sent a negro woman to the city to be put under my care, who had been afflicted with vesico-vaginal fistula for a number of years. The rupture in the bladder and rectum occurred in labor. She was the wife of his engineer, and had a number of children. Mr. M. told me he wanted her to have the full benefit of medical science, and was willing to pay for it. He authorized me to call in consulting physicians, and to procure for her all the comforts and conveniences necessary in the treatment of her case. He said that the patient had been pronounced incurable by a number of physicians, but if impossible to effect a cure, he hoped that it was in the power of science to afford some relief to her intolerable sufferings.

On examination, it proved to be a case of vesico-recto vaginal fistula, associated with frambæsia or yaws. The integuments of the pudendic region were piebald. The spots were white and varying in size from a mere point to an inch or more in diameter. So profoundly altered was that portion of the skin occupied by the maculæ, that when they were seated on the pilous portion of the pudendic integuments, the hirsuteness was metamorphosed into pubescence—a short, white down succeeding the usual crinigerous appearance of the parts.

The *eruption pianique*, as these spots are called in the West Indies, where the frambæsia or pian is very common, extended as high up as the umbilicus, and all around the nates. Hard excrescences had formed on the spots occupying the integuments of the labia, and through fissures in these excrescences granulated pullulations, resembling mulberries, had protruded, distilling a very fetid, ichorous humor. The *mamapian* or mother yaw was situated near the upper commissure of the right labium majorum. It so irritated the urethra as to constrict it, and to render the passage of a catheter into it both difficult and painful. A catheter however, was passed into the bladder, and from the latter organ into the vagina and rectum. The entire mucous membrane lining the vagina and neck of the uterus was covered with fungous excrescences distilling a thick, greenish humor mixed with blood. There was a constant stillicidium of urine mixed with blood. The hæmorrhagic flow all the time occurring was fully equal to that which usually takes place in the menstrual period. It

greatly reduced the flesh and strength of the patient. She, moreover, suffered the most torturing kind of pains by day and by night; she had also inguinal swellings, which were very painful and hard, but had no tendency, like buboes, to suppurate; they were damp with a foetid exudation.

I put the patient on a regular course of treatment for yaws: The bi-chloride of mercury with quinine, sassafras, saffron and nitrate of potash, with various remedies to determine to the surface, were the internal remedies mostly relied on. The iodide of potassium, dissolved in water was applied all over the maculated portion of the integuments by means of diaper cloths, which were kept in place by a jacket made to fit the hips, and rendered impervious to water by an oiled silk lining. The jacket greatly relieved the bearing down sensation by the mechanical support it afforded to the engorged abdominal viscera. The diaper cloths wrung out of a weak solution of the iodide of potash in water, which were placed under the jacket, being frequently renewed, whenever they became uncomfortably warm, greatly tended to arrest the molimen hæmorrhagicum, to which the constant stillicidium of blood owed its cause. The nitrate of silver was repeatedly applied to the fungous substances within the vagina. The Peruvian balsam was used after the caustic had destroyed the more prominent fungi, and it made the best dressing for the mamapian or mother yaw. Frequent ablutions in warm water, a generous diet and occasionally tonics and cod liver oil, were also bought into requisition in the treatment.

By the end of the year 1856, the frambœsial affection was nearly cured, the pain had ceased, and the hæmorrhagic flow was entirely arrested. In consultation it was decided that every thing had been done for the woman that could be done, and that she had better go home. I accordingly sent her home. Her master, however, sent her back to me again early in the year 1857, as the yaws had not entirely disappeared, and she occasionally suffered pain from them. I got the council of Dr. B. Dowler in her case, in the month of May, 1857. The inguinal tumors had not then entirely disappeared, and the lining membrane of the neck of the uterus was still covered with aphthous incrustations when Dr. Dowler examined her.

I continued to treat her until the tumors disappeared, and all the vestiges of the yaws, except a few of the white spots, mostly on the labia and nates.

The next year (1858) I handed the case over to Dr. Bozeman, of Montgomery, Alabama. He operated on her for the vesico-recto-vaginal fistula ; but owing, I suppose, to her not having entirely recovered from the effects of the frambœsia, his first operation was unsuccessful. He, however, kept her under treatment, and some six months after the first operation, he operated again with entire success.

In March last, he returned the woman to me entirely cured. I examined her particularly, and found the cure complete and perfect. There had been not only a rent between the bladder and vagina, but also another communicating with the rectum. Both rents were entirely closed, and the parts in their natural condition. The general health of the woman was entirely restored. She came tripping into my office in fine health and spirits, active and lively, with a letter from Dr. Bozeman in her hand. I never saw a greater triumph of science.

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ART. VIII.—*Case of Acute Pemphigus* : Reported by JOHN E. DAFFEL, Resident Student, Charity Hospital.

LIZZY MIEER, a Geiman girl, aged fifteen years, a resident of New Orleans for four months, was admitted into ward thirty-four, Charity Hospital, on the 15th of February. At this time she was suffering with an attack of acute rheumatism, which readily yielded to the usual remedies. Such was her condition on the 26th of February, eleven days subsequent to her coming, that she was permitted to leave the hospital.

She returned on the 8th of April. Being questioned as to her occupation and state of health, since withdrawing from the hospital, it was found that she had resided in the rear of the city, near the swamp, where she pursued her task as a washerwoman. During her sojourn in this unhealthy locality, her diet consisted chiefly of salt provisions. In reference to the ailment that afflicted her, she stated, that three days before coming to the hospital, she was attacked with

high fever and violent pains across her back and temples. On the fourth day, the fever subsided, and a peculiar eruption, characterized by rounded elevations of epidermis, containing a transparent serous fluid, made its appearance on the lower extremities. Such was the picture she presented at the hospital gate when seeking admission, otherwise she seemed robust, with a normal pulse, and at a casual glance had the look of health.

On examination, the attending physician, Dr. G. A. Nott, pronounced the disease to be pemphigus. The eruption that covered the lower extremities of the patient resembled in a great degree the vesicles commonly produced by a burn. These water-blebs increased in number, and on the day following her admission, they had extended to the arms, chest, and face, whilst those on the lower extremities had already attained the size of a pea, and were distended with a thick, transparent fluid.

During the first four days of her confinement to the hospital, no medicine was ordered excepting an ounce of castor oil, which was given on the day of her admission. On the fifth day she was ordered the following:  $\mathcal{R}$  potass. iodid.  $\mathfrak{z}$ i; syrup. sarsa.  $\mathfrak{z}$ iv. Dose, a table-spoonful three times daily.

One week subsequent to her admission, the vesicles situated on various parts of her body, had attained their full size, some, especially those of the extremities, having reached the enormous size of a hen's egg. At this period of the disease, the soreness was excessive; and the itching was so intolerable, that the patient, in the vain hope of securing relief, could scarcely refrain from tearing away the vesicles. The fluid from a vesicle was found, on examination, to be highly albuminous, coagulating when exposed to heat. It was, moreover, observed, that whenever a vesicle was accidentally ruptured, it healed slowly and with greater difficulty than when bursting spontaneously—manifesting a tendency to ulceration. The first crops of vesicles eventually healed with slight epidermic desquamation; but the duration of the disease was prolonged by a second crop that shot up on the exact site of these that had already faded away.

The constitutional treatment first adopted with this patient, was continued until a few days previous to her dismissal from the ward. Her bowels were kept in a laxative state throughout the disease by the administration of a saline aperient on each alternate morning.

A fortnight previous to her departure, the vesicles gradually ruptured and dried, the scabs falling off in the course of five or six days. She finally left the hospital on the 4th of May, entirely relieved, and has since enjoyed good health.

[As pemphigus has been seldom alluded to in this Journal, it is intended to add to the above case, a small part of Dr. J. Bell's researches upon this malady, published in the Glasgow Medical Journal, in an essay which teems with the learning of the Ancients. The more practical, pathological and therapeutical portions of this elaborate paper will be here reproduced. B. D.]

*Pathology.*—When we come to inquire into this most important point, we have to lament our great ignorance—indeed, I may say our almost total want of any real knowledge. From the quotations which I have made, the reader will perceive that some authors have regarded the disease as contagious; consequently this leads to the conclusion that it depends on the presence of a morbid poison in the blood. That a diseased alteration takes place in the blood there can be no doubt. But it is a totally different question to decide, that the poison is capable of emanating from the body of the patient, and originating the disease in others, as happens in small-pox, measles, etc. The epidemic character of pemphigus in bygone days gives considerable support to the doctrine of its contagious nature. Besides this, the following observations tend to confirm the same views: Blagden, at page 105 of the first volume of 'Medical Facts,' says: 'In January, 1790, was called to see a little girl; she had fever. I gave an emetic. The following day the fever was stronger, and she had delirium. A blister was applied between the shoulders, and the bowels were opened. In the evening pustules appeared first on the chest, and afterwards on the hairy scalp and other parts of the body in three days; they were filled with a yellowish fluid, and had the appearance of little bladders; the greater part were of the size of an almond, but those of the forehead and chest were as large as a walnut. Five days after the appearance of the eruption, another child, aged three months and a half old, belonging to the same person, was seized with the same affection. Both children recovered.' In the month of January, 1847, I observed two cases in the same family very similar to those narrated by Blagden. An infant, ten months old, was observed to become feverish and unwell. This was ascribed to disorder of its stomach, being nursed by the bottle. On the third day an eruption of large vesicles, about the size of almonds, appeared on the thighs and the abdomen, and extended to the chest; they burst, some ulcerated slightly, and others healed rapidly. Several days after these bullæ appeared, a boy, aged five years, was attacked with febrile symptoms; two days afterwards the same vesicular eruption appeared, and extended over the greater part of the surface of his body—some attaining the size of a small apple. Dr. Plieninger relates two very important and interesting observations: in the one,

an infant ten days old, was attacked with the disease, which commenced on the abdomen, and extended to the rest of the body. The child died; two days after death, the mother, a sister and brother were all successively seized. In the other instance, an infant was first attacked; shortly afterwards the mother and three sisters were affected by the same disease. Such facts as these strongly confirm the doctrine of contagion; at least render it very improper to hazard the opinion that the disease has never been propagated in this manner.

“But in whatever manner the poisonous principle makes its exit from the body, it seems to have been demonstrated by the experiments of Gaitskell, Martin, and Hussen, that the contents of the bullæ are incapable of exciting the disease. Inoculations made with this fluid were perfectly innocuous, the punctures healing rapidly. Such experiments, however, by no means disprove the doctrine of contagion. But whatever opinion we may adopt regarding this question, we must admit that the cutaneous affection arises from the presence of some specific poison in the blood, capable of exciting a prior constitutional disturbance. This morbid matter may arise spontaneously in the body, may emanate from the patient, and become imbibed into the systems of the attendants and others, and thus propagate the disease.

“We have next to advert to the pathological state of the skin. There can be no doubt but that some amount of inflammation exists at the site of the vesicles, and that the effusion of serum is the result of the inflammatory process. The chemical character of the fluid has been carefully examined by several observers, especially Simon. Heat and nitric acid caused coagulation. It has an acid reaction, and a specific gravity of 1018. When allowed to remain at rest, a deposit of mucous and pus corpuscles takes place, in some of which a nucleus can be detected. In 1000 parts of the fluid, Simon found 940 of water; 60 of solid constituents, of which albumen and earthy phosphates formed 48 parts; cholesterine, 2·6 parts; extractive matter, iodate of soda, and chlorides of sodium and potassium, 6·5 parts; a substance resembling ptyalin, 1·9 parts; and traces of acetic acid. From this analysis it appears, that the only difference between this fluid and healthy serum consists in the amount of cholesterine, and the presence of a trace of acetic acid, and a small quantity of ptyalin.

“I am afraid that we are not in a position to deduce from these results any conclusion regarding the pathology of the disease. Dr. A. T. Thomson has furnished us with a table showing the difference between Heller's analysis of the urine of a female, who died from pemphigus, and that of a healthy person as given by Simon. The chief difference consists in the increase of urea, and the decrease of fixed salts. In the meantime from a single analysis, we are not prepared to make any deductions regarding the relation which these circumstances hold to the disease.

“The associated pathological conditions which have been detected

are principally confined to the gastro-intestinal mucous membrane, and consist of redness, softening, and ulceration; or, in other words, the results of inflammatory action. Alibert, and some others state, that occasionally bullæ also exist in the stomach and intestines. Rayer, however, has never witnessed any such appearance in his inspections; but he found the liver in a state of fatty degeneration. This has also been detected by Biett. In Dr. Burne's case, 'the bronchial ramifications were found full of muco-purulent matter, evidently generated by the inflamed mucous membrane. In the abdomen, a large part of the mucous membrane of the small intestines, particularly that of the ileum, was inflamed, but no ulceration was detected.' Dr. Roots, in the post-mortem examination, could find no appearance to account for death. He thought that the fatal result arose in the same way as it sometimes occurs after extensive burns. I regret that I was not able to obtain a post-mortem inspection in my case.

"*Treatment.*—The exhausted condition of my patient precluded any other than a nourishing, stimulating, and soothing treatment. I ordered the chlorate of potass, in consequence of the benefit which I have seen following its use in cases of cancrum oris, purpura hæmorrhagica, and other allied affections. In such an extensive destruction of skin, I could not think of any more suitable external application than cotton wool, moistened with the linimentum aquæ calcis.

"In conclusion, I may remark, that all the milder cases that I have seen were associated with debility, and recovered favorably under a generous diet. I generally ordered, as a tonic, the nitro-muriatic acid, along with a vegetable bitter, and ordered the linimentum aquæ calcis, externally. Some recommend puncturing the bullæ, and applying dilute nitric acid. Dr. Graves, of Dublin, advised the denuded surface to be touched with the nitrate of silver. At one time anti-phlogistics were employed, but such remedies were perfectly unsuitable to any case which I have ever seen. Quinine, and other preparations of bark, have been much used and highly praised by many medical men, both in this country and on the continent. But as it is not my intention to discuss the general treatment of the disease, I will not enter further on the subject, except to remind the reader of the favorable manner in which Thierry speaks of the internal use of vinegar.

"I have entered into the history of this disease with the view of attracting attention to the study of ancient medical literature, a field extremely rich in most profitable instruction. No man can rise from the perusal of the works of the old masters of our art, without experiencing feelings of amazement at the vast amount of labor, research and erudition, which their writings contain—feelings of admiration for the spirit of devotion which they exhibited in the pursuit of professional knowledge."

ART. IX.—*Remarks on the Proceedings of the late Quarantine Convention, held at New York*: By B. DOWLER, M. D.

" NATIONAL QUARANTINE AND SANITARY CONVENTION, COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK, April 30, 1859.—On motion of P. M. Wetmore, amended by J. McNulty, M. D., it was ordered—That the Secretary be authorized to publish the record of the resolution of A. H. Stevens, M. D., and the vote thereon, as adopted at the preceding session of the Convention.

" *Resolved*, That in the absence of any evidence establishing the conclusion that yellow fever has ever been conveyed by one person to another, it is in the opinion of this Convention that personal quarantine of cases of yellow fever may be safely abolished; *Provided*, That *formites* of every kind be rigidly restricted."

This resolution, with its proviso, is the great result arrived at by the late Annual National Convention with an almost complete unanimity, the affirmative vote being eighty-five, the negative six only.\* This adjudication has been hailed as definitive! Altogether satisfactory! A new foundation for sanitary legislation.

In no spirit of contradiction—with no wish to dissent from the opinions of the highly respectable National Convention—with no purpose to lessen the internal or external influence of the medical profession in the opinion of the public, by exposing its dissensions, a journalist may sink expediency in that of the well-being of society; he may be allowed to examine the dicta of an influential organization in matters which concern health, life, commerce, sanitary legislation and police. He may claim, until the contrary appears, the same honorable motives which govern those from whom he may reluctantly venture to differ in regard to unsettled questions of science, especially such as are supposed to be well or ill-adapted to promote health and prevent epidemics. Unawed and unbiassed by great names and official proclamations, his duty requires neither silence nor acceptance contrary to his conscience and reason. The majority is not always right, nor the minority always wrong.

\* AFFIRMATIVE.—The President, Dr. J. H. Griscom, Drs. A. H. Stevens, E. Harris, Stephen Smith, D. B. Reid, A. S. Jones, J. S. Cooper, S. T. Hubbard, H. D. Buckley, A. Underhill, Wm. Rockwell, J. Miller, Wm. H. Williams, H. Guernsey, James R. Wood, Frank Tutthill, John Watson, J. McNulty, W. C. Anderson, J. W. Sterling, S. S. Purild, Joel Foster, James D. Pond, T. W. Johnston, T. C. Finnell, E. Lee Jones, Samuel Boyd, J. H. Perome, W. R. Donaghe, A. N. Bell, J. C. Hutchinson, New York; George B. Wood, R. La Roche, J. F. Lamb, L. U. Buffington, U. A. Piper, Penn; H. C. Clark, J. M. Moriarity, D. H. Storer, Mass.; J. A. Nichols, M. Baldwin, S. J. Southard, J. P. Trimble, H. D. Holt, Gabriel Grant, S. A. Cross, E. T. Whittingham, J. M. Cormelison, C. F. J. Leback, New Jersey; C. B. Guthrie, Tenn; Wm. M. Kemp, D. J. McKew, J. Gilman, Maine; J. F. Wilson, Delaware; G. W. Cowdrey, W. M. Wilson, Va.; John Darby, Ala.; C. F. Force, D. C.; E. M. Snow, Jos Mauran, F. H. Peekham, Rhode Island, Messrs. W. L. Bladen, Penn.; J. H. Henderson, Conway Whittle, Va.; W. McPhail, Maine; F. E. Mahar, Chas. H. Haswell, J. P. Batchelder, P. M. Wetmore, C. C. Savage, H. O. Reilly, Wm. Nelson, Joseph Prunt, New York; Hon. F. W. Lincoln, Jr., S. D. Craven, George A. Curtis, Jos. S. Baily, T. C. Amory, Jr., Silas Pierce, George Dennis, Ebert Atkins, Clement Willis, Mass.; Hon. D. S. Gregory, Hon. M. Bigelow, New Jersey.—85.

NEGATIVE.—Dr. J. W. Francis, New York; Dr. E. P. Nichols, New Jersey. Messrs. H. A. Parkhurst, N. J.; S. B. Halliday, New York; Thomas H. Town, Wm. H. Taylor, Penn.—5.

C. H. HASWELL,	A. N. BELL, M. D.,	GABRIEL GRANT, M. D.,
H. G. CLARK, M. D.,	E. D. HINCLE, M. D.,	H. ST. CLAIR ASH, M. D.,

*Secretaries.*



From the resolution with its proviso as above quoted, and from the debates of the Convention as already published, it appears that while this distinguished congress voted yellow fever to be non-contagious, goods or their fomites are voted to be contagious, "things, not persons, being the cause of the communicability of yellow fever."

Among the ninety-one votes cast on this memorable occasion, it is highly probable that not ten of the voters ever witnessed an epidemic of yellow fever, although some of the delegates professed to have read as high as three hundred volumes on this disease!

"*Prince Henry.* O, monstrous! but one half-penny worth of bread to this intolerable deal of sack! What there is else keep close; we'll read it at more advantage." "One half-penny worth" of logic, will show that the proposition of the proviso stultifies and even virtually contradicts that of the resolution, as well as the fundamental ideas of nearly all that portion of mankind not members of the National Quarantine Convention.

"Chief Justice Parker

Made that darker,

Which was dark enough before."

It is not intended on the present occasion to write a polemic for or against the contagiousness of yellow fever. Nor is it intended to attempt an exposition of the history, mystery, and expediency of quarantine in general. A few remarks, however, upon the supreme result of the late Quarantine Convention held at New York (which is one of high import to the well-being of Society, and deserves a closer scrutiny than can now be given), may be allowable.

Yellow fever, it is said, originates from fomites; exclude fomites and yellow fever is thereby excluded. This truism is as evident as that a part is less than the whole. If Latin be more potent than the mother tongue, here it is: "*Sublatâ causâ, tollitur effectus.* Fomites, says the Convention, are in *things*, not in *persons*, therefore exclude things, not persons, and yellow fever is thereby excluded. This is self-evident if the premises be true. Eighty-five votes were, in this case, wasteful and unnecessary, even though the members had neither witnessed an epidemic yellow fever, nor read a book upon it. Their mothers would have told them so. But what are fomites? Definitions, typical examples, creteria, or some recognized standard must

be appealed to and universally admitted outside of the mad-house, else like the builders of the tower of Babel, no one will understand the other.

“*Fomites*. A term applied to substances which are supposed to contain contagious effluvia; as woollen goods, feathers, cotton, etc.” Dr. Dunglison’s Dictionary.

Fomites are not mere words. Yet even in New Orleans where the *ens epidemicum* ought to be known, nothing is yet known but such hard words. The people, however, are in advance of the doctors in nomenclature, and withal more poetical; as they usually personify the great unknown ENS by calling it YELLOW JACK, instead of *Fomites*. Mother wit against abstractions and hard words!

The popular mode of dealing with Yellow Jack among the unacclimated, is, not by fumigation, but by running away to places where his presence, for the time being, is not indicated by symptoms. As they have not discovered fomites in things, they carry their things, clothing, boxes and baggage with them. When the yellow fever declines, they return without fear to houses which sometimes had been totally depopulated by death from this fever—houses shut up for weeks after the last tenant had gone to the graveyard—houses from which no one had attempted to remove the unknown fomites. Thus immediately after the subsidence of the great epidemic early in the season of 1853, a vast number of persons returned to or arrived at New Orleans without having suffered from fomites in the things or houses of the dead.

The Board of Health of New Orleans, in an official announcement, shows, for the month beginning with the 26th of November, 1853, that 6,707 passengers from foreign ports, chiefly immigrants, had arrived at our wharves in forty-seven sea-going vessels, by the river route. Now, if we add the number which had previously arrived, to the number which afterwards arrived from sea, the aggregate will scarcely fall below 10,000, while by other routes, chiefly by the river, the emigrants, absentees, and other unacclimated persons, as the steamboat population coming to the city, in September, October, November and December, forty thousand more may be added, making fifty thousand—fifty thousand living experiments against contagion—fifty thousand exposures to fomites—the houses, goods, etc., of persons recently dead, including emanations from the few still sick, and yet, so far as the most careful observation could determine, the new

comers escaped all danger from yellow fever, although the weather was considered favorable for the extension of the disease. After the end of October, the deaths from this fever did not amount to thirty, although as already stated, thousands of unacclimated persons were arriving, or soon after arrived, numbering about fifty thousand exposures to the assumed fomites of things. Medical history probably furnishes no example parallel with this in significance and extent.

Fomites exist or they do not. If they are positive entities what are they? whence are they? where are they? how many? In the name of fourscore and five witnesses, what are their sensible properties? Do they exist in all parts of space except such as are specially occupied by human bodies? Give their qualitative and quantitative analyses independently of the personality. Say, ye fourscore and five witnesses, who know and who vouch for these positive entities or fomites in *things*, which cotton, wool, and feathers, ought to sweep in a few weeks faster than and even against the winds over the continents of Asia, Africa, Europe and America, say, what are the characteristics of cholera fomites? influenza fomites? dengue fomites? typhus fomites? diphtheritic fomites? plague fomites? yellow fever fomites? Are all these in things, not in persons? Will they succumb to quarantine? or can they be "*restricted*" by it? Can fourscore and five restrict fomites without knowing them, without being able to point them out, as well the cotton and other *things* which they have actually entered and possess?

The constituents of the convention, doubtlessly expected, as usual, that this assembly would explain the *ens epidemicum*, and point out the means of prevention—a feat never yet performed so far as is known in regard to a single epidemic. A short and plain route is open to such as admit that they are not born to solve the problems of the universe—namely, an honest confession of ignorance. To such as study the laws of Nature, without affecting to know the essential origin and connection of cause and effect, this confession is easy because it is true. Newton, for example, investigated the laws of gravitation—the cause of gravitation was entirely beyond his gigantic grasp; indeed he looked upon the cause as a perpetual volition of the Deity.

No one is bound to testify to what he does not know in order to please the public. If the cause of yellow fever does not originate in the human body, it does not follow that it must, as the only possible

remaining alternative, originate in the air. The stars, the earth, metals, water, insects—in a word everything known and unknown, may be a possible cause or antecedent, so long as nothing is positively known in the premises. No one can say that *things*, cotton, wool, feathers, cloths, lemons, etc., will not originate or carry the cause of yellow fever. All that can be said is, that this assumed origin and conveyance of fomites have not been proven by fourscore and five votes or otherwise.

So long, therefore, as the essential cause of yellow fever is unknown, no convention, least of all one not experimentally acquainted with yellow fever, can vote intelligently whether it originate in any one thing in the heaven above or in the earth beneath, without having carried out a two-fold exhaustive process, namely, the synthesis and the analysis of all things in the universe ; for, if one be left out, that may be the *causa vera*.

While, therefore, the personal contagiousness of yellow fever is scarcely probable in any well accepted meaning of contagion, it is not a thing to be voted upon absolutely by yeas and nays. At least, those who vote against contagion, and yet know the dreadful fomites very well, as something very different from personal contagion, ought to sustain the *onus probandi* of their anomalous position. They ought, at least, to give some analogical evidence, derived from some indisputably contagious disease, as that of measles or small-pox, in order to show that yellow fever, itself perfectly innocuous *in persons*, will, nevertheless, communicate fomites to *things*, and thereby cause epidemics. The small-pox is communicated personally, and by things; by a direct contact, by a direct emanation, or by garments sent to a distance, or by the lymph or scab as in inoculation, or by other less obvious means, always, however, more energetically by the near presence of the patient.

Admitting the very thing to be proven, namely, the contagiousness of the yellow fever thing, that is to say, its communicability by fomites contained in clothing, wool, cotton, etc., still the contradictory proposition, that the person affected with this disease cannot communicate it to another person otherwise than by means of goods, wares, merchandize, seems fundamentally contrary to the common-sense idea and all the known analogies of contagion. Yellow fever or yellow fever fomites (fomites, literally any thing that kindles or keeps the fire in) cannot exist without the subject in which it inheres. If there

be any positive or sensible entity known as yellow fever fomites originating without the human body, it must be, according to the prevalent, yet *untenable* theory, an aerial infection, not an infection originating in articles of merchandize, as feathers, wool, cotton, mail matter, etc. If it be aerial and exist independently of the yellow-fever patient, no method has yet been devised for quarantining the wind. If the fomites originate with or come from the yellow-fever patient, and be communicable by cotton and other merchandize, it must *à fortiori*, be far more communicable by, with, and from the patient. If there be no fomites or harmful substance in or emanating from the patient, he can communicate none to his clothes, goods, chattels, wares, or merchandize. A contrary supposition is at war with facts, physics and metaphysics. The fomites in goods, which preëxisted in the patient, must be, judging from analogy in the latter, of far greater intensity. Small-pox or measles may sometimes be transmitted by goods, but far more by the presence of patients suffering from that malady as already mentioned. Perhaps a single person might communicate small-pox to the majority of the human race, not already protected, could all be brought within the circle of the contagious emanation. It would, however, reverse all that is known as fundamental in contagion or fomites, to assume either that a cotton bale which had never imbibed the contagion or fomites of small pox, could nevertheless originate or communicate that disease; or, that while the garments of a small-pox patient are dangerous, and should be detained, washed, fumigated, or burned at quarantine stations, the patient himself should be sent into the bosom of society without detention, as being incapable of fomites. The same reasoning applies to the assumed fomites of yellow fever, in order to give consistency to this conventional logic.

It is reasonable to assume that the fomites of yellow fever, when discovered, will not prove to be either ordinary filth or simply impure air, but some specific, peculiar local agent, which produces a specific or peculiar disease—not any disease indifferently, as cholera, intermittent, rheumatism, small-pox, typhus, etc. If the material cause or fomites of yellow fever (the auxiliary influences of locality being unknown) were appreciable and could be readily identified by the naked eye, by the microscope, by chemical or other criteria, as a concrete, gaseous or mixed substance or ens, whether vegetable, animal, mineral, or something else, it might happen that these fomites, thus

occasionally or periodically developed in one locality, would not flourish, though transported to another, as is known of many plants and animals. A remarkable fact adverse to the doctrine of transmissible fomites (of which cotton is alleged to be one of the most certain carriers), is witnessed in every year, namely, the continued shipment of cotton from New Orleans to the great *emporia* of the world, where this cotton from this port is manipulated and worn by, perhaps, one-half of Christendom, without having as yet carried yellow fever in that direction. As the exportation of cotton increased yellow fever declined in peninsular Europe, where it formerly prevailed with unexampled violence. It is not known that cotton extinguished those great epidemics; but it is known that they have not increased at the ports where New Orleans cotton has been sent in a constantly increasing quantity (as at Liverpool, Havre, etc.), and that, too, during the period in which yellow fever has been most active and deadly in this city. Fomites thus harmless need no fumigation, not even resolutions nor provisos by which useless quarantines are maintained, for the sole benefit of their officers, dependants and appointees in convention assembled, or elsewhere. Long ago I alluded to another remarkable circumstance or coincidence which has a bearing on the fomites of yellow fever, namely, that during the most stringent enforcement of quarantine, as in the peninsula of Europe and in Louisiana, this disease prevailed most. This may have been wholly accidental; but it shows that quarantine, as it regards things as well as persons, is not known to be useful. This accidental parallelism between quarantine and yellow fever in New Orleans continues.

Hypothetical phrases, as fomites, constitution of the air, epidemic constitution, malaria, *ens epidemicum*, etc., are worse in a scientific point of view than the personification of yellow fever under the name, YELLOW JACK, as the former terms virtually assert that the cause is known substantively having physical, chemical, or sensible properties, as if it were an animal, vegetable, mineral, gaseous or liquid substance. Such evidence would be rejected at once in a court of jurisprudence, and, *à fortiori*, in a court of science. Analogy, it is true, teaches that yellow fever, as well as small pox, has a cause, and that this cause may possibly be discovered and prevented. Among the many causes to which this fever has been ascribed, some may be the *causa vera*, but until the latter shall be proven, nothing

will be discovered, even though both houses of Congress and the Convention should unanimously pass the strongest resolution to the contrary. Physical causes must be established by physical evidence, and not by a majority of votes.

Of the great majority of diseases of a purely epidemic character which sweep over the earth rapidly, from the rising to the setting sun, even the antecedents or forerunners are wholly unknown, and, consequently, cannot be determined by pathogenics consisting of resolutions or words only. Many of the ancients, and moderns too, ascribe epidemics to Divine wrath, to human passions, to astral conjunctions, to earthquakes, etc.; some to saline, sulphurous, alkaline, acidulous and the like matters diffused in the air; to predispositions not known; while the best of modern meteorologists utterly fail not only in determining the causes, but even the precursors of an epidemic. Hippocrates has not been surpassed. He assumed an epidemic constitution to account for epidemics—a proposition which it is not safe to deny, seeing it is generally conceded, that effects have causes though they may not be discovered in all cases, not even by an unanimous vote. Spontaneous epidemics, spontaneous generation and the like, find few advocates.

The rise, extension, and decline of these yellow fever fomites in things do not run parallel with or conform to any other variety of fomites yet known, at least in New Orleans, where, for the most part, epidemics attack the unacclimated in all parts of the city simultaneously. It exceeds the bounds of probability that the things imported, which are supposed to be the exclusive carriers of fomites, could thus suddenly be diffused for seven miles in the city. Things other than mail-matter are usually stored in warehouses, and the quantities which arrive on the approach of the yellow fever season of the year are inconsiderable.

A truce to logomachy and verbal dialectics. History, that true logician, indicates an æra not long past, when New Orleans was exempt and New York suffered from yellow fever. Passion and interest, all powerful in swaying the present are often but too forgetful of the landmarks of the past. There is nothing known in the sanitary conditions of these two great *emporìa* of this Republic, which can give any assurance that what has happened may not happen again. One advantage New Orleans may claim. She has a large

resident population not susceptible to yellow fever. Should the northern cities be again visited with this disease, as were Norfolk and Lisbon lately, and the peninsula of Europe at the beginning of the present century, the mortality would probably be far greater than ever occurred in New Orleans. What is called acclimation does not appear to exist, so far as exemption from this fever is concerned, only within or near the tropics.

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## PROGRESS OF MEDICINE.

### ART. I.—*Treatment of Burns by the Permanent Warm Bath.*

THE observations upon which these conclusions are based, were made on the occasion of an explosion which destroyed some fourteen persons, in a manufactory of firearms, at Frankfort. Thirteen cases were taken to the hospital, with burns of every degree; all were treated with continued warm baths, or when this was inapplicable, by fomentations of warm water. The water, which was renewed twice a day, or more frequently when the suppuration was excessive, was maintained at the temperature of 27° Réaumur (= 93° Fahrenheit). An apparatus was employed similar to that used by M. Langenbeck in the treatment of amputations; and when at the end of some weeks this became fatiguing to the patient, warm fomentations were substituted.

The first effect produced by the bath was a considerable relief, and soon the pain, which was at first excessive, ceased completely. The desiccated and hardened tissues were penetrated by the water and softened, and the eschars were more easily detached. This method of treatment protects the injured surfaces completely from all sources of irritation, and diminishes the chances of purulent resorption. Cicatrization progresses more rapidly in consequence of the maintenance of an uniform temperature, and of the slight compression exercised by the water on the injured tissues. (*Translated from the Archives Générales, quoting from the Deutsche Klinik. S. E. C.*)



ART. II.—*Croup of Animals.*

M. DELAFOND read a detailed work (before the Academy of Medicine, Paris) upon the croup of animals. He concludes from the numerous facts reported by him, "that croup prevails in an epizootic and enzoötic state among domestic animals, and that it assumes then an extreme gravity; the coincidence of these epizootics with epidemics of croup in the human species, has been several times observed.

"Sporadic croup is more frequent with domestic animals than epizootic croup; in both cases it attacks by preference the young animals. By the aid of various irritating agents, a croupal inflammation may be produced in animals; but this inflammation assumes a particular type, and one which differs from that produced by spontaneous croup. It may be inferred that croup is a specific malady, and numerous facts seem to prove, that with animals at least, it is only a local expression of a specific disease, which, primitively, was general. The manner in which croup appears, and extends itself, is the same in animals as in man; and it is upon the distinction of its forms, of its generalization or localization, and of the places where it appears, that the indications for the medical or surgical treatment of this disease should be based.

"This constitutes the principal medical treatment: cauterization with chloro-hydric acid, or any other caustic, insufflation of alum or calomel and cinchona; administration internally of soda salts, and of antimonials, not as purgatives, but as alterants; copious venesections repeated according to the indications drawn from the difficulty of respiration; powerful revulsives in the neighborhood of the pharynx and trachea; in conclusion, irritating enemata.

"The employment of these means is never followed in animals, with those serious consequences which have been observed by M. Trousseau to succeed local venesections, etc., in children. As to tracheotomy, veterinary surgeons resort to it, so soon as the medical treatment appears incapable of arresting the march of the croup, and before the attacks of suffocation are well manifested, and announce the imminence of asphxia. This *preventive* tracheotomy counts from seventy-six to eighty successful cases in the hundred.

"In that which concerns the application to man of that which precedes, M. Delafond concludes in these terms: Is it not calculated to compromise the success of this operation, to delay it (in children) until the false membranes have become so numerous, so thick, and so

accumulated in the larynx and trachea, as to render the respiration difficult; until the attacks of suffocation rapidly succeed each other, and asphyxia has begun; until the blood has commenced to undergo a serious alteration; and until the child has been exhausted not only by the disease, which in itself is so serious, but also by the commencement of an hæmatisia?"

The above translation from the *Archives Générales*, appears in the report of the long and interesting discussion which has recently taken place in the Academy of Medicine, Paris, on the subject of croup.

This discussion was originated by the proposition of M. Bouchut, the distinguished French practitioner in the diseases of children, to introduce a tube into the larynx in those cases of croup where suffocation was impending, and as a substitute in many, if not in all cases, for the operation of tracheotomy.

After a protracted and brilliant debate, the Academy concluded that, "tubage of the larynx, such as it has been applied so far, has appeared to us neither sufficiently useful, nor sufficiently exempt from danger to merit the approbation of the Academy;" that "tracheotomy, in the actual state of science, is the only remedy to employ, when there no longer remains any hope from the employment of medical means."

S. E. C.

### ART. III.—*Atropine in Epilepsy.*

DR. MAX. MARESCH, availing himself of his position as physician of an establishment for the insane, at Vienna, has submitted some epileptic patients to the influence of atropine; and has published the results in the Vienna Journal of Medicine. Dr. Maresch's experiments were made upon eight patients in the female department, and ten more in the department of the incurable insane, four of these were men and six women.

Of the eight first patients, three were completely cured, and the condition of the five others was so notably ameliorated, that it was

impossible to deny the beneficial effect of the atropine. Of the ten patients belonging to the class of incurables, eight experienced a marked diminution in the violence and frequency of their epileptic attacks, as well as in the exacerbations of their physical troubles. These results, united to those obtained by others in the treatment of epilepsy by atropine, merit serious attention.

Maresch has carefully noted the therapeutic phenomena which have arisen during the administration of this remedy. One-fiftieth of a grain of atropine gave rise, in every case, to effects which habitually follow the administration of this agent, such as dryness of the throat, difficulty in articulating, visual aberration, dilatation of the pupils, etc., phenomena to which by degrees the patients become habituated, which, however, do not cease during the entire treatment. In every case the pulse lost eight or twelve pulsations during the first hour after taking the remedy, but the pulse resumed its normal frequency so soon as the other therapeutic phenomena manifested themselves. There did not occur, in any case, a marked and permanent acceleration of the pulse under the influence of the dose above indicated. As special phenomena, Maresch observed in those cases while under the influence of atropine, an exanthem analogous to roseola, which soon disappeared under the influence of warm baths and the discontinuance of the remedy. It is, besides, worthy of remark that atropine did not give rise in any case to digestive derangement, or any other unfavorable symptom.

The administration of this remedy has not proved beneficial in the other forms of mental diseases. Dr. Maresch administers atropine as follows : he dissolves a grain of it in five hundred drops of rectified alcohol, and of this solution he gives from five to ten drops, (from one-hundredth to one-thirtieth of a grain). This dose is administered once, in the morning before breakfast, from which coffee, tea, and chocolate must be excluded, as these substances interfere with its action. This is continued from sixty to ninety days without interruption, then is to be resumed after an interval of from thirty to forty-five days. With women, there is no need to suspend its administration during menstruation, as it favors and augments this discharge. Rarely does atropine give rise to constipation, more frequently to diarrhœa, which necessitates, when it becomes severe, a suspension of its administration for some days. (*Translated from L'Union Médicale, by S. E. C.*)

ART. IV.—*A Pustulous Eruption Produced by the presence of the Dermanyssus Avium (a Parasite of Chickens, etc).*

A WOMAN aged about seventy, otherwise very vigorous, consulted Dr. Itzigshon, for an eruption, which in a short time had invaded the skin of the neck, and of the superior parts of the trunk. This eruption was formed by numerous confluent pustules, having some analogy to those of variola. They were accompanied by redness, and an excessive itching, which was particularly aggravated at night. Soon after, a girl aged twenty-three, who slept in the same room as the woman, and a short time after the servant girl, were attacked with the same affection, which they considered was nothing else but the itch. It was not so however; the relative large size of the pustules, their seat at first and almost exclusively limited to the trunk, in fine, the presence of peculiar animals which were readily distinguished by the naked eye, would serve to cause this idea to be at once rejected. A microscopic examination demonstrated that these animals were acari very much larger than the acarus scabiei. The length of the palpi, the ovular and elongated form of the body particularly distinguished these from those acari which are proper to man and mammiferous animals. A glance at the treatise of Küchenmeister upon parasites induces us to state that this was the *dermanyssus avium*. It was soon discovered that some chicken inhabiting a hen-house located beneath the apartment occupied by the old woman, were infested with the acari in question; that these acari were even spread over the floor and walls of the hen-house, and that it was probable that they had penetrated into the apartment above by the seat of a water-closet. Baths were prescribed, with unctions of ointment composed of lard, white precipitate and oil of rosemary. A cure was promptly effected. (*Virchow's Archiv.*)

The facts in regard to the transmission of the *dermanyssus* of birds to man appear sufficiently rare; however, Alte, Bory, Simon and others have reported some instances. It results from their observations, that it is chiefly old and cachectic women who are most liable to become infested. The animal establishes its seat almost exclusively upon the skin of the trunk; rarely it spreads upon the extremities. The pustules which indicate its presence are voluminous, sometimes almost as large as a boil; they generally rest upon a very inflamed base. (*Translated from the Gaz. Hebdom. de Méd. et de Chir., by S. E. C.*)

ART. V.—*Treatment of Ulcer of the Stomach.*

M. FLOURENS, in presenting (to the Academy of Sciences) in the name of its author, M. D. de Luca, Surgeon of the Hospital of Incurables, at Naples, a printed memoir entitled: "Diagnosis and Treatment of an Ulcer of the Stomach," called attention to the mode by which the cure was effected.

The existence of the ulcer of the stomach being proved, M. de Luca, who had already collected several cases of the good effects of lime water in the treatment of ulcerations of the intestinal mucous membrane, had recourse to the same means. The patient was placed first upon a diet of the milk of the ass, and then upon goat's milk, combined with lime water. At the end of fifteen days, the pains were much diminished; at the end of a month and a half the patient was cured. In this case there was no doubt as to the diagnosis; for independently of the local pains in the stomach, the patient has passed pus in the excrements. (*Translated from the G z. Hebdom. de Méd. et de Chir., by S. E. C.*)

ART. VI.—*Ready Means of Extracting Foreign Bodies from the Eyelids.*

MR. EDITOR—The reading of an article inserted in the last number of *L'Union Médicale*, and entitled: "An easy means of extracting foreign bodies engaged beneath the lids," has induced me to communicate to you a very simple means, and I believe little known, of extracting without the aid of any instrument, the small foreign bodies which become engaged under the superior lid. This means applies only to those little *movable* foreign bodies which are so often introduced beneath the superior lid, and are sometimes long retained in the cul-de-sac formed by the reflection of the conjunctiva. Upon the occurrence of such an accident, it often happens that in spite of the searches made by elevating and everting the upper lid, no benefit follows, the foreign body being almost imperceptible, and situated deeply in the superior conjunctival furrow. Instead of making useless searches, and injections, instead of introducing a ring or some other instrument,

often unsuccessfully, nothing is more simple than to try the following means which will generally render unnecessary any other. The upper lid being seized near its angles by the thumb and index of each hand, draw it gently forward, then immediately down as low as possible upon the inferior lid, and retain it thus for about a minute, taking good care to prevent the escape of the tears. When, after this time, the superior lid is allowed to resume its position, a flow of tears has washed out the little foreign body, and it will be found upon the far border of the lower lid, or upon an eyelash, or upon the skin of the lid, or of the cheek. . . . It often happens that nothing is found, but of what consequence is this, provided the patient says, "I no longer suffer." Since I have been acquainted with this process, I have had occasion to employ it twenty times, and always with success. . . . This process ought not to be new; I believe it as old as the world, but I also believe it to have been too much abandoned, even if it has ever been generally employed by surgeons.

Old medicine, or traditional medicine as it is at present styled, despises no means of relieving suffering man, and seizes these means if good, where it finds them, or rather refinds them. *Nil novum sub sole. Multa renascentur quae jam cecidère.* No operative procedure offers in so high a degree as this the true advantages sought for in every operation. *Tutò, citò, et jucundè.*

DR. LEON RENARD.

(Translated from *L'Union Médicale*, by S. E. C.)

ART. VII.—*Hypertrophic Lengthening of the Neck of the Womb.*

A MEMOIR on this subject, by M. Huguier, read before the Imperial Academy of Medicine, in Paris, has recently excited some interesting discussion before that body. M. Huguier concludes that "falling of the womb, whether it be complete or incomplete, is not a single malady, but an assemblage of several affections designated by a single name; and that when the uterus has prolapsed, even when the

vagina is completely inverted, and when the womb from the volume of the tumor, in the centre of which it lies, appears entirely precipitated between the thighs, it is not, in the great majority of cases, because that the womb has fallen down, and completely passed out of the pelvic cavity, but indeed because it has undergone an hypertrophic lengthening both partial and general. In the affection designated as falling of the womb (*précipitation*), the hypertrophic lengthening is not the exception, but very generally the rule. Two principal varieties of longitudinal hypertrophy, the intra and extra-vaginal, which constitute two different diseases, may simulate the falling and prolapse of the womb." As to the first, "medicinal remedies and different kinds of cauterization are applicable only to slight hypertrophies, and to those complicated with inflammation and engorgement. Pessaries are generally useless or dangerous. But when an hypertrophic lengthening of the vaginal orifice of the womb causes serious accidents, and measures from five to seven centimetres (about two to three inches), there is only one truly efficacious and curative means to employ, which is the resection of the neck of the womb about half a centimetre (one-fifth of an inch) below the insertion of the vagina."

As to the second variety, the extra-vaginal, M. Huguier says that "the disease which to the present time has been designated by the name of prolapsus, precipitation, or complete falling of the womb, is generally nothing else but a longitudinal hypertrophy of the extra-vaginal portion, the body and fundus of which has remained in the pelvic cavity, and this is true even if the vagina be entirely inverted, and the tumor hanging between the thighs be equal or superior in length to that of the normal uterus. In the treatment of this affection, recourse ought not to be had to a bloody, or properly speaking a surgical operation, unless serious accidents arise, and it be certain that medicinal and *prothetic* remedies are insufficient. All those operations which have thus far been invented to fulfill the therapeutic indications are insufficient. They may be useful in simple falling of the womb, without hypertrophic lengthening, and as applied to these, they should remain in science. But in this hypertrophic lengthening of the neck, followed by precipitation of this part and inversion of the vagina, the only operation which fulfills the principal indications and which can be practised with success, is the amputation of the neck above the insertion of the vagina, more or less near the body of

the womb according to the degree of lengthening. But this operation ought never to be performed without having taken precautions beforehand against consecutive inflammations, which should be continued with the greatest care for fifteen or twenty days after the operation. The arteries of the uterine tissue are very difficult to seize and ligate, and to do this promptly and surely a species of tenaculum must be used which is left in place until it falls off spontaneously."

"The linear *écraseur* has appeared to us useful in terminating the section of the neck, especially if it be very vascular. The operation is contra-indicated when there exists at the same time a very large pelvic cavity and vulvar opening, a lacerated perineum, and any considerable weakening of the soft parts forming the floor of the pelvis. If this operation be performed in the conditions noticed, the patient will not relapse, and the health will become as flourishing as it was before the disease was developed."

These conclusions of M. Huguier found a warm and able opponent in M. Depaul, from whose criticism are selected the following passages: "Our honorable colleague (Huguier) has reproached even the most distinguished surgeons, that they were content with looking at the vulva and judging that there was a prolapsus uteri so soon as they perceived the vaginal orifice of the womb between the labia. Who is the one among us who proceeds with such inconsiderateness, and is contented with an examination so superficial? It is not thus I have seen my masters act, nor was it in such a manner I was taught by them to proceed.

"In regard to the causes of this affection, I believe it important to take into serious account individual dispositions, or better, original, native temperaments. In fact nothing is so variable as the dimensions and physiognomy of the uterine neck, as I have become well convinced from the examination of many young children who have died some days after birth. There are some women, the neck of whose womb is scarcely distinguishable; in others it is entirely wanting. On the other hand some women have this portion of the womb largely developed, sometimes congenitally, sometimes pathologically, as the result of some chronic disease of the womb.

"As to the diagnosis, I believe, contrary to M. Huguier, that nothing is more easy than to discover an excess of length in the uterine neck. Often the vaginal touch (*toucher*) will suffice; but combined



with hypogastric palpation, it will furnish most precise information, since by these means the womb may be seized between the two hands, and its dimensions be thus very accurately appreciated. I know of no better process of mensuration. If this do not suffice, recourse may be had at the same time to a rectal examination, and to the introduction of a sound into the bladder.

“Our colleague also extols catheterism of the uterine. In my opinion this is a process often useless, always dangerous, and frequently deceptive. In many women it gives rise to acute pains or to menorrhagias; it exposes them to severe and often fatal peritonitis; in fine, it is calculated to excite abortion, always followed in such case by formidable accidents, as a recent example has enabled me to observe. Will it be said that this is the only means of mathematically measuring the length of the uterine cavity? And of what importance is a centimetre more or less in length? Is it not better to commit a slight error on this point, rather than expose the patient to the danger of death? I consider then, that in the immense majority of cases the ordinary processes of measurement are quite sufficient, and would restrain the use of the uterine sound to those very exceptional cases, where the dimensions of the uterus cannot be appreciated by the ordinary means.

“I come now to the treatment. M. Huguier declares that the amputation of the neck of the uterus below the insertion of the vagina, is an operation without gravity. It is true that M. Huguier saw none of those patients succumb whose cases he has reported; but the most part of them experienced grave accidents, and particularly abundant menorrhagias which required *tamponing* of the vagina. For my part, I cannot deem this operation as inoffensive. I remember having assisted some fourteen or fifteen years ago my master, P. Dubois, in the amputation of the neck of the womb, which was soon followed by the most fatal results. M. Giraldés also lost at the Charity a woman upon which he had performed the same operation. During the time I was an externe in the service of Lisfranc I saw several women die from this operation; and I am convinced that more than one surgeon in this academy can cite grave accidents or deaths caused by ablation of the neck of the womb. I contend then that this is a most serious operation which may compromise the life of the patient by peritonitis or hemorrhage. Recourse should rarely be had to it, and only in cases of absolute necessity, after the entire

series of remedies ordinarily proposed in hypertrophy of the neck of the womb has been exhausted; such as repose, astringents, palliative remedies, different modes of cauterization, and particularly cauterization with the red-hot iron. Let it be added that women are not always much incommoded by this infirmity, that generally they are able to go about their occupations, and even to have children, as one will be convinced by reading the facts reported by Dugès, Boivin, and by M. Herpin (of Geneva).

“M. Huguier affirms that that which has been taken for falling of the womb, is an hypertrophy of the extra-vaginal portion of the neck; he affirms this, but demonstrates it no where, not even on those preparations which he has placed under our eyes. I have seen these preparations, and it has appeared to me that the entire uterus was lengthened, not alone the extra-vaginal portion of the neck, as M. Huguier believes, who in truth tells us so, and shows a depth of fourteen centimetres (five to six inches), measured by his uterine sound. But these fourteen centimetres, in place of the seven to nine which the uterus measures in its normal state, M. Huguier affirms should be carried to the account of the neck only. Why not to that of the whole uterus?

“I approach now, gentlemen, that part of M. Huguier's memoir which relates to the mechanism by which these uterine descents are produced. Until the present time these have been considered passive; he makes them active; he has placed in the extra-vaginal portion of the neck, in this length of a centimetre and a half which forms it, an active force which pushes the neck without, drawing with it at the same time the bladder and rectum. But this hypothesis is absolutely incomprehensible. It is very difficult thus to invert the vagina with the womb, and upon the anatomical preparation which I will now submit to you, three persons exerted for some time great force to succeed in simulating this inversion. Besides, in supposing this active pushing by the extra-vaginal portion, why is this effect produced downwards where there are so many obstacles, and not upwards in the pelvic cavity where the uterus floats freely, and where nothing opposes its ascent? Is it not thus that matters occur during pregnancy?

“M. Huguier recommends not to operate during menstruation, to keep the woman reclining long after the operation, to use frictions of croton oil upon the thighs, not to make use of chloroform, etc., etc.

It is well; but he also recommends to cut off the vagina at the origin of its insertions upon the neck. Here, a difficulty arises; the vagina has no insertions; the vagina is but a prolongation of the uterus and nothing else. In truth then, nothing is then cut off, but the tissue of the uterus is cut into. This is no great matter in front, but behind great risk is run of opening the peritoneum. Instead, then, of advising the cutting off (*décollement*) of the vagina, it should be said: touch not the vagina. Let it be added, that the operation as described by M. Huguier has nothing absolutely new; it is in like manner that Lisfrane performed it."

M. Huguier's reply to M. Depaul is an able defense, from which the following passages are extracted: "As to his (Depaul's) diatribe against the uterine sound, it also is a *hors d'œuvre*, out of place in this discussion; but let him be patient, for in a little while I will furnish him cause to pronounce a third discourse on this subject, since I am about to read before this Academy, a work upon this method of exploration, a work in which I will expose with as much independence and conscientiousness as it is possible to do, all the advantages and inconveniences of this new means of diagnosis. Until then, let his time be occupied in searching throughout France and foreign lands for all the victims of the uterine sound. But let him understand that it is neither in his or my power to prevent this instrument making the tour of the medical world; and let him be assured that it will not die from fatigue after its long travels. I know several practitioners, who, in learned societies raise their voices against the uterine sound, while they hold one in the bottom of their pockets. Great, indeed, was my surprise at the last meeting, to see M. Depaul extract one from his; I thought it at first a modified laryngeal tube! But no, it was in truth a uterine sound, which he uses remarkably well I assure you, and which proves that he uses it often. Who knows, perhaps, on that side of the Academy there are still more of them lost in the folds of deep pockets?"

"Let us now enter into the heart of this question, and occupy ourselves with the treatment. M. Depaul prefers to amputation, the cauterization with the red-hot iron, which no one says he will compare in danger to amputation of the neck. And I also prefer this cauterization in certain cases of hypertrophy, in those for example which have an extent of two or three centimetres only, and when at the same time there exists an engorgement, a softening, a fungous or

varicose state of the neck. . . . . But for those cases in which the neck measures from five to seven centimetres, cauterization is useless, and amputation should be resorted to as soon as accidents arise. In such cases I prefer amputation to cauterization, because it disembarasses the patient at once, and surely, because it frightens her less; because, although cauterization be not very painful, amputation is still less so; because the cure is much more prompt; because the patient is not so much exposed to peritoneal and uterine inflammations; because that, in cases of hypertrophy, this operation is less grave than when it is performed for scirrhus, encephaloid, or for an epithelial ulcer of this part, circumstances in which it is often necessary to remove the entire disease, and to ascend sufficiently high to cut very near the vaginal insertion, or even into this insertion; whilst in hypertrophy the incision should be made about one centimetre below this.

“The hæmorrhage, which has been described to you as so terrible, and represented as an accident of so much gravity, can now be guarded against almost at will by the surgeon, whether he makes use of the linear *écraseur*, or covers the wound with pledgets of charpie, moistened in a solution of perchloride of iron, or tampons at once and by way of precaution the vulvo-vaginal opening; the tamponnement in these cases not having those inconveniences which it may have when recourse is had to it after an operation for some organic lesion of the womb. But it is better at first to dispense with these things, allowing a certain quantity of blood to escape, and not arresting this flow unless it becomes very abundant, and farther, if an experienced assistant be left with the patient, as should be done, the great objection from fear of hæmorrhage falls to the ground. Farther, and on this point I may speak with some knowledge, for apart from the fourteen amputations of the extra-vaginal portion of the neck which I have reported in my work, I have performed, at least thirty times, amputation of the intra-vaginal portion for different lesions of this part, and I have yet to see a single fatal hæmorrhage, or one which could not be readily arrested. Farther, the harmlessness or gravity of an operation should be judged not only from the operation itself, but above all from comparison with the affections for which it is performed, and the number of successful or unsuccessful cases which have attended each operation. In regard to the one we are now occupied with, I see, that setting aside my

seven cases, which were attended with complete success, that Follin, Broca, Marchal (de Calvi), Bertet (de Cereoux), have each performed it once, and Ph. Boyer twice, with the same success, and cured their patients of the accidents which they suffered. One of the most distinguished men of Germany, Professor Scanzoni, whose work has recently appeared, says, p. 65, 'Concerning the treatment (of this hypertrophy), we have so often stated the inefficiency of therapeutic means, general as well as local, that now we no longer resort to anything, except amputation of the hypertrophied part.' So, gentlemen, you will see, that if I be wrong in acting, and counselling others to act thus, I am at least in good company." (*Translated from the Gaz. Hebdom., and L'Union Méd., by S. E. C.*)

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ART. VIII.—*Raw Meat in Diarrhœa.*

i. *Reflections upon the Use of Raw Meat in the Colliquative Diarrhœa of Infants at the Breast*: By DR. J. F. WEISSE, Director of the Hospital for Children, at St. Petersburg.

A period of seventeen years has elapsed since the attention of my confrères was called to the great value of this remedy in this disease, but it did not come into general use until five years later, and after the publication of more extensive works upon the subject. About this time Dr. Behrend, of Berlin, addressed me a letter which contained the following passage: "You cannot imagine how much interest your communication upon the treatment of the colliquative diarrhœa of infants at the breast, by raw meat, has excited; we now use it exclusively in this disease. Soon after, Dr. Behrend inserted in the sixth volume of his journal, a letter from M. Marotte, Physician of the Central Office of the Hospitals of Paris, addressed to Professor Trousseau, in which the author gave a theory to account for the results which I had obtained. From this latter time, the treatment by raw meat has been generally admitted everywhere, and its utility has become incontestable.

Among the numerous favorable reports recently published, I will cite that of Dr. Eichelberg: "In consequence of the short time which has passed since this treatment was recommended, I have by me only a limited number of observations (twenty), but all prove its efficiency. The cases in which infants refuse raw meat are very rare, the majority swallow it with avidity. I have observed two cases which were very striking; in these the children took the meat for several weeks without repugnance; a very great amelioration was felt in the general system; all at once they refused it as if guided by a natural instinct. The want of osmazome induced the infants to devour the meat, but as soon as the equilibrium was established in the economy, this want disappeared."

Dr. Eichelberg makes use of raw meat only in the treatment of diarrhœa which attacks children fifteen days or three weeks after they have begun to take the breast; the cure is then certain; for myself, in ordering this regimen, I also have employed it in this affection only; and now, after twenty years of experience, I maintain that raw beef reduced to a pap by scraping, is to the exclusion of all other medication, the veritable specific for this diarrhœa which causes so much ravage. I am not able to admit the assertion of Mr. Charles Hogg, in recommending beef tea, so well known in England: "Beef tea is an excellent aliment, very nutritious and easy to digest; it replaces completely the juice of the meat obtained by scraping, which is so lauded by Dr. Weisse, of St. Petersburg." I have found raw meat not an aliment for infants, but a remedy for diarrhœa; farther, I have never spoken of the juice of meat, but have recommended the muscular substance itself, hashed or grated in such a manner as to be easily swallowed and digested. The end proposed is to cause the muscular substance itself to pass into the digestive tube, and beef tea has no more effect upon the diarrhœa of children at the breast, than the excellent decoction of meat, praised by Liebig. These two liquid aliments, from the simple fact of their fluidity, traverse too rapidly the intestinal canal. By giving the meat in pap, the solid parts sojourn longer in the intestine, acting by contact, and being able, by exciting the intestinal mucous membrane, to stimulate absorption; it is also probable that this means contributes to neutralize the acidity of the gastric juice. I cannot partake of the hope expressed by Dr. Beer, of seeing cod liver oil replaced in the *Materia Medica*, by hashed raw meat. Each of these excellent remedies has its sphere of

action marked out in the treatment of the diseases of infants; raw meat combats the diarrhœa of the new-born; cod liver oil triumphs over rachitic affections with or without atrophy.

The treatment of infants by raw meat has come into general use at St. Petersburg, and this use has been propogated rather by proofs of its good effects, than by special writings. The most part of our confrères, employing it when the ordinary means have failed, have also verified the good results obtained by myself. So far as it concerns me, I have employed this treatment in nearly two hundred cases, and the result has been always satisfactory when the disease was taken in time. I say when the disease was taken in time; for, if the malady is too far advanced and has assumed the characteristics of gastro-malacia (softening of the stomach) a cure is rarely obtained. Sometimes even in these circumstances, we may succeed in calming some symptoms so fatiguing to the little patient, such as the inextinguishable thirst, and the vomiting.

In a large number of patients cured by raw meat, verminous affections have been observed, and particularly tœnia, otherwise very rare at St. Petersburg. Dr. Braun stated this as his opinion, and two years after, a high authority, Prof. Von Siebold, of Munich, expressed the same idea, in the last paper of his interesting work, "*Weber die Band und Blasenwürmer*" Leipsic, 1854: "we should no longer be surprised at the fact, and must credit those physicians who have declared that they have found in many patients submitted to a diet of raw meat, the tœnia. And Herr Von Siebold has remarked that in every case the tœnia solium was found, and he believes that in all probability, this worm, which is not indigenous to St. Petersburg, has been brought there by the cattle driven from Tocherkask and Podolia. Some weeks before my departure from St. Petersburg, in the month of June of the present year, a tœnia more than ten feet in length, was sent to me by a confrère, to whom I had warmly recommended meat, in the case of an infant aged 18 months. The tœnia was expelled after the administration of the ethereal oil of male fern. This remedy was employed because the child having been cured of its diarrhœa by the raw meat, had several times passed parts of tœnia.

I ought not to forget to say that in the Hospital for children, which is in my charge, I have often tried, but unsuccessfully, the raw meat in the diarrhœa of children more aged, who were sick from no cause

connected with dentition. More frequently this diarrhœa was caused by ulcerations of the intestinal canal. [Excerpta translated from *L'Union Médicale*, by S. E. C.]

ii. *Exhibition of Raw Meat in Diarrhœa.*

We have on more than one occasion, alluded to this mode of treating diarrhœa, particularly the *Diarrhœa Ablactorum*. We copy the following, as it enters more fully into the best means for administering this valuable remedy :

Our readers have, doubtless, not forgotten the interesting history of the two little twin daughters of a wealthy Mulhouse merchant, who had been reduced by unconquerable diarrhœa to the last gasp of life, and who fed with the pulp of raw meat, returned in a few months to a state of perfect and robust health. Many facts have, since then, confirmed our confidence in the value of this Russian mode of treatment. Mr. Trousseau never allows an opportunity to escape of recommending it, and of pointing out the best manner of rendering it both useful and acceptable.

The meat best adapted to the purpose is the fillet of beef; some patients, however, prefer the centre part of mutton chops. It should be cut fine, pounded in a mortar, and strained through a sieve or a cullender. The pulp, thus separated from the cellular texture of the muscular substance, is then gathered with a knife, and rolled in salt or powdered sugar, or mixed with current-jam.

One of Mr. Trousseau's grand-children would take it only when mixed with racahout, a farinaceous compound of cocoa, ground rice, and potato-flour, sweetened and flavored with vanilla. Mr. Trousseau causes it sometimes to be rolled into small salted balls, of the size of a hazel-nut, or in little oblong gobbets, which may be administered in soup, to the number of thirty or forty, equivalent to four or five ounces of meat-pulp. In grown persons, and particularly with ladies, the physician will probably meet with a repugnance, which he must overcome by concealing the repugnant character of the medication. For this purpose some appearance of cooking may be imparted to the food, by exposing a thick slice of the meat, for twenty minutes, to the action of a brisk fire; its surface is thus roasted, the interior parts remaining raw, and being then treated as we have said. Mr. Trousseau has thus caused to be prepared by Mr. Mialhe (one of the principal apothecaries of Paris), meat-pulp, combined with confecton of roses, destined for delicate stomachs, which is taken without disgust, and even with pleasure, under the agreeable denomination of Damascene Preserve.

In children, the dose of raw meat, the first day, should not exceed  $2\frac{1}{2}$  drachms in four meals. It may be doubled on the second day, and on the third attain eight drachms; and so on without any other additional food than albuminous water. It is easy to measure with precision the quantity administered daily, by means of a small balance and the current coins, the weight of which is well known—the franc being equivalent to one drachm, and the five-franc piece to six drachms.



The dose may be carried as far as ten or twelve ounces, and the children gradually recover their good looks, their plumpness and spirits. At the end of a month or six weeks, when diarrhœa has entirely ceased, the quantity of raw meat can be gradually decreased, and broth or underdone eggs can be substituted, so as to reduce the dose of meat to three or four ounces daily.

It is necessary to be aware that, at first, when already the nature and abundance of the diarrhœa has undergone a favorable change, the motions are red and fetid. In one of the little Mulhouse patients we above referred to, this animal diet appeared to have occasioned the development of tape-worm, a parasite commonly met with in Abyssinia, where the natives feed on raw meat; but this kind of nutriment, not being so long persevered in, generally, as was the case in the instance of the little girl alluded to, this circumstance must be considered exceptional, and cannot counterbalance the decided benefits yielded by the Russian method of treatment, in cases of chronic disturbance of the bowels, and especially in the unconquerable diarrhœa which children are subject to in their second year. *Journal Prac. Med. and Surg., Paris.—Dublin Hospital Gazette.*

#### ART. IX.—*Diphtheria.*

##### i. *Observations on Diphtheria*, by STEPHEN MONCKTON, M. D.

1. Diphtheria is a distinct disease, easily recognized, and not to be dreaded till such changes have occurred about the fauces and tonsils, as it is impossible to overlook. A remote kinship there certainly is between it and scarlet fever, but identical they are not; about here, as before mentioned, they have coëxisted largely; and in the same little hamlet children have been lying dead at the same time, some of scarlatina and some of diphtheria, not very many yards apart; but still it has never been for a moment difficult to determine of any individual case to which category it belonged; three times, at least, I have seen diphtheria in individuals where scarlet fever had been previously suffered. Again, though as the diphtheritic membrane loosens and separates from the surface of the throat and tonsil, sloughing ulceration may ensue, I feel at present fully persuaded that diphtheria and cyananche maligna are not the same thing.

2. Premonitory symptoms are often absent, and so far as I have seen, always very mild, *i. e.* the first visible deposition of patchy white, putty-form membrane is not preceded by acute fever, sore throat, prostration, or marked constitutional symptoms. Immediately after a suspicion has arisen that the patient ails, a spot as of soft

Pâte de Guimauve, and more or less extensive, becomes distinctly visible about the isthmus faucium, most frequently, I think, occupying the sulcus between the anterior pillar and the tonsil; neither patient nor practitioner, therefore, need timidly apprehend the existence of this malady from either moderate or severe symptoms or disturbances of any sort: in real diphtheria the first suspicions are in a very few hours (from seven or eight to twenty-four) confirmed by the appearance of undoubted deposit in the throat.

3. The order of appearance in the local phenomena seems to be this—first, upon the mucous surface is formed a soft, reasonably coherent cake of epithelial cells, which quickly extends in dimensions, and rather solidifies in substance; underneath this there is not at first ulceration or breach of surface, the cake consists wholly of epithelial cells without pus or fibres, and if removed leaves behind it a raw but unbroken basement membrane rather shining, and with bloody points, exceedingly like the skin from which an eczematous scab has been prematurely torn; this is best seen in cases where a large thick patch has formed upon the under surface of the velum, and begins to separate by a thick crescentic margin across the back of the palate about the eighth or ninth day. It would seem, however, that before the separation by natural process takes place, ulceration does begin, and with it some change in the character of the false membrane; as the glands often, but not always, enlarge, a few pus globules can be found in the deposit, and some distinct sloughy shreds of basement membrane are to be distinguished; but even when glandular swelling constitutes a painful and prominent feature, the ulceration and sloughing, as I have proved after death, is on a very limited scale.

4. Besides the specially characteristic false membrane, other points are to be noticed; the cervical glands commonly begin to enlarge about the fourth day, and occasionally swell to a very considerable extent, rendering it difficult to mediate the throat topically, or to get down the proper amount of medicine and food. I have also seen two deaths in which the fatal event did seem to be so largely resulting from obstructed breathing, as to bring me to the very eve of tracheotomising; but in one of these cases a *post-mortem* enabled us to attribute much of the dyspnoea to a clearly-developed, but thin, layer of the false membrane down larynx, windpipe, and larger bronchi. As a general rule, I should say that the glands swell in diphtheria more early, more constantly, and less considerably than in scarlet fever. The adjacent tract of mucous membrane is also disordered, tongue foul, but not the clear white coat and punctuation of scarlet fever; nose full and tumid, and sometimes a slight acrid discharge from the nostrils is to be observed.

5. The constitutional symptoms at first altogether slight (the pyrexia, headache and sickness of early scarlatina being altogether wanting), become very real as the disease advances. The main feature is prostration; not typhoid at all, no coma, no sensorial disturbance throughout, no sordes, no heavy lurid look, no suggestion of a frame burdened with an animal poison; and in many cases the practitioner, if not warned by previous experience, or a careful observation of the pulse, is surprised to learn that the patient he left with

clear countenance, cheerful manner, and little suffering a few hours ago, has just gone off, while casually sitting upright, in a fatal syncope. The theory seems almost warranted, that, whereas in typhus a poisonous nescio quid is generated in the blood, we have here by the growth of the false membrane, or in some other way, an essential ingredient exhausted from it, precisely as the atoms constituting the difference between sugar and spirit are removed by the growth and vegetation of the yeast plant. The practical fact is, however, this, that after the fourth or fifth day a diphtheritic patient becomes the subject of very real asthenia, not so much perceived by the patient, as discoverable by the lax pupil and feeble pulse, and that this state is the one which about the eighth day is too apt to terminate in death. I have sometimes found albumen in the urine—sometimes not; more frequently in the late stages than the earlier ones.

6. *Treatment.*—Directly patches appear in the throat destroy them with caustic, and, despite biting, scratching, shouting, or remonstrance, let there be no mistake about the *fortiter in re*. I can hardly conceive that for this purpose anything can equal the solid nitrate of silver. In the majority of instances it would be right to administer with your own hand at the same time a large dose of calomel. There is no depression now, and the amount of that which is to come will depend on the progress of the throat changes, which the caustic and calomel are designed to arrest. One more vigorous application of the caustic will be doubtless needed in from twelve to twenty-four hours; after that, unless the disease spreads around, discontinue it for a time; and should some large and tender glands begin to show, pencil them over freely on the outside with nitrate of silver; it is a quick, certain, manageable, counter-irritant, less annoying and depressing than a blister. Follow the first dose of calomel with aperients till the evacuations be satisfactory as to frequency and character. The throat mischief often yields as the edges of the tongue clean, and attention of this sort is, therefore, not to be dispensed with. Supply the patient with fresh air, abundant food and stimulus, and, especially after the sixth day, *keep him flat*.

As regards medicine, I give either muriatic acid, with chloric æther, every few hours, or pretty full doses of ammon. sesquicarb.—*Medical Times and Gazette*.

## ii. *Clinical Lecture on Diphtheria*: By DR. CORRIGAN.

[*The Dublin Hospital Gazette* (Feb. 15, 1859), contains a lecture founded on a fatal case of *diphtheria*, the concluding portion of which is subjoined:] On the *post mortem* examination, the appearances were such as to leave no doubt as to the nature of the disease. The root of the tongue, the soft palate, arches of the palate, the uvula on both surfaces, and the back of the pharynx, were covered with the pasty exudation described. It did not extend upwards into the nasal fossæ nor downwards into the œsophagus. The tonsils and lymphatic glands were unaffected. There was no appearance of gangrene, and the mucous membrane underneath was merely red and congested. The epiglottis was not swollen. There was no sub-mucous inflamma-

tion—at least none worth noticing—but the pasty exudation was thick and jagged on the mucous membrane. The exudation did not extend into the larynx. The chordæ-voeales were slightly vascular, but otherwise free, as likewise were the ventricles of the larynx.

The two important questions now before us are—Is this a new disease? and if so, how are we to distinguish it from other affections of the throat which resemble it? On the answers to these questions, or on accurate diagnosis of it, must rest, as the only sure foundation, our after observation as to treatment.

I believe it is a new disease in this sense, that whether scattered instances of it have or have not occurred previously, its visitation as an endemic is new to us; and all that we know of it is sufficient to stimulate us to lose no time in acquiring as rapidly as possible an intimate knowledge of it.

The first point to which I wish to draw your attention is the peculiar oval swelling of the neck, not extending to fill up the angles of the jaw. When the post-mortem was made, the deep-seated muscular tissues were found infiltrated with a yellowish gelatinous serum, and this infiltration will explain the peculiar shape of the swelling, when we bear in mind the anatomy of the cervical fascia. The inner layer of this fascia, dipping from the anterior edge of the sterno-cleido-mastoid muscle, bends inwards as it ascends, until it reaches the styloid process and inner angle of the jaw, and will give to an infiltration under it the oval form of the tumefaction, as presented in this case; while in the effusion accompanying scarlatina, the fluid is poured out equally under the subcutaneous layer of the fascia, which spreads upwards, to be lost in the skin and cellular tissue of the cheek, and thus gives the collared or rounded swelling to the jaws so characteristic of scarlatina. That the disease was not scarlatina was also sufficiently evident from the number of days that elapsed before the throat became engaged. We must therefore dismiss all idea of this disease being scarlatina throat of any kind. The perfect freedom of the tonsils from all disease removes it equally from tonsillitis and all its subdivisions. Croup has occasionally attacked adults; I am not aware that it has ever attacked persons of advanced life; but the freedom of trachea and larynx from disease distinguishes it from that malady.

Acute laryngitis in the adult is a very rapid disease, but neither in symptoms nor in post-mortem appearance does it even nearly resemble that disease. There was no whispering voice, no struggles for breath, no congested countenance, no staring eyeball, such as we see in acute laryngitis presenting mechanical obstruction to the passage of air, nor, on examination after death, was the larynx affected to any degree worth noticing. In acute laryngitis too, when the epiglottis is affected, it is with submucous effusion, not, as in this case, with pasty exudation on both its surfaces.

Reviewing, then, the whole case in its symptoms and pathology, we must, I think, look upon the case before us as diphtheria, and practically as a new and terrible disease.

There still remains an important question for us; was the fever under which F. labored for several days one peculiar to diphtheria,

or was it merely our non-maculated typhus—diphtheria terminating life, as an accompaniment or sequelæ—as we frequently see occur in other instances, whatever local disease is prevalent, having a tendency to be developed towards the termination of any of our depressing fevers? The latter was probably the case in the instance before us, but this cannot diminish our fears about the disease—whether as an idiopathic disease or as a sequela of fever, there is much to be dreaded about it.

On treatment I do not at present intend to make almost any observation. The indications clearly were, up to this period of the local affection of the throat, to treat the attack as one of adynamic fever. Other observations for treatment must be reserved for future cases, of which I fear we shall have more than we desire, for our worst epidemics are generally ushered in by dropping cases, such as this. As to the influence of bad food, bad air, or contagion in generating this disease, I have to observe that this man was well fed, being in the police force, that he lived in a high and healthy part of the city, and in the part of the city most remote from all communication with shipping, or with persons passing through from England.

This accords with the circumstances of the first case of cholera which appeared in Dublin in the last outbreak, in 1849-50. The first case occurred in the Convict Depôt, in Smithfield, in an inmate that had been in the depôt for months, and barred in from all the possibilities that could support the idea of contagion. That case is given in the report of the Central Board of Ireland on the epidemic fever and cholera of that period.

It is better for the advancement of medical knowledge that we should admit we know little or nothing of the propagating causes of epidemics, than impede further research by advancing ill-founded hypotheses, and call them scientific theories.

iii. *Report of the Lancet Sanitary Commission on Diphtheria: Its History, Progress, Symptoms, and Treatment.*

Three distinct forms have prevailed in this country of diphtheric angina, or more briefly, of diphtheria. The first may be properly called simple diphtheric angina, or simple diphtheria; the second, croupal diphtheric angina, or croupal diphtheria; the third, malignant diphtheric angina, or malignant diphtheria.

1. Simple diphtheria is the mildest and most frequent form of the disease. It is preceded by more or less of fever, and by headache; the tongue is coated by a thick creamy deposit; some discomfort is complained of in the fauces, perhaps a slight difficulty in deglutition. It is usually at this time that the medical man has the opportunity of seeing the throat, and now (from twelve to thirty-six hours after the first invasion) one tonsil—rarely both—is covered by a small patch of white membranous deposit. This may extend and cover the whole of the soft palate, and the pharynx but *rarely*. It commonly, in this form of the affection, remains stationary, or extends but little; it does not blacken or putrefy, neither does it exhale the fœtid odor of putrescence. The surrounding mucous membrane is swollen, purple, and

projecting; the subjacent tissue not uncommonly betrays a breach of surface, partly due to the injurious surrounding pressure. The sub-maxillary glands are somewhat tumefied, but neither the parotid nor the cervical glands are implicated.

The duration of this affection varies from five to nine days. It has been observed in nearly every district where the diphtheric type has shown itself. The prognosis is favorable. The treatment which succeeds best is the local application of a solution of nitrate of silver, thirty grains to the ounce, and the ferrochloric mixture, containing the tincture of sesquichloride of iron, in combination with chlorate of potash, with a judicious and sparing use of evacuates.

2. Croupal diphtheria, or croupal diphtheria angina, is a more severe manifestation of the diphtheric type, and is undoubtedly that by which the greatest number of deaths have been occasioned in this country. It is more frequent in children than in adults. Its precursive symptoms are active fever, intense headache, hot skin, engorgement of the glands behind the jaw, and perceptible difficulty of deglutition. The parents are only now aroused to the existence of a morbid condition. When the surgeon is summoned, he finds the throat and mouth covered with yellow or brownish leathery exudation. Within a few hours a hoarse, barking cough, and a change in the tone of the voice are marked; oppression of the breathing supervenes; then paroxysms of suffocation, more and more frequent; the cough is stifled, and the voice also dies out. As the access of suffocation is felt, the poor child turns from side to side, throws its arms into the air, clutches its mother violently, and struggles furiously to gain breath, then falls exhausted in the bed, and gaining strength from momentary repose, renews the hopeless struggle to the end. Perhaps in a violent fit of coughing, it expels a false membrane from the air-tubes, which has extended down to the fifth division of the bronchi; then it breathes easily, smiles again, and sleeps, but soon wakes to resume its struggle with death—it may be again to expel the membrane, and finally to triumph. But such a happy victory is wholly exceptional, and when once the grip of the disease has closed upon the air-tubes, death claims its prey.

The drama is of another, but not less tragic character, if the sufferer be an adult. It is more prolonged; for the larynx and tracheal tube are more capacious, and the membrane advances far down the bronchi before the scene closes and the black curtain drops that shuts out the future from the gaze of straining human eyes. As the oppression of breathing, the piping tone of the voice, the stifled cough, and the agony of suffocation accede, the patient fights against them with all the energy which the intelligent perception of danger, the earnest desire for life, and the despairing sense of approaching fate can inspire. He has an heroic endurance, and does not murmur at the most barbarous cauterizations, if they afford only a temporary relief; nor must the surgeon shrink from them, but with deliberate and benevolent cruelty, thrusting back the epiglottis with a spatula, he must follow the disease into its home in the larynx, and freely brush the surface with effective solutions of nitrate of silver, or of hydro-chloric acid.

As to the treatment of this croupal variety, empiricism has led to results widely differing from those which rational medicine had counselled. It is a disease characterized by rapid and excessive exudation. Therefore rational medicine counselled every form of antiphlogistic medicine. Calomel, leeches, phlebotomy, and blisters, have been lavished on the sufferers with an unfortunate prodigality. When these failed, the alkaline remedies were strongly counselled, and especially the bicarbonate of soda. M. Marchal de Calvi and M. Lemaire contested the honor of recommending it.\* Others have advocated with equal warmth the employment of full doses of bicarbonate of potash.† But these remedies, having been fully tested, may be pronounced useless here, or of exceptional utility only. "La rationalisme," says Trousseau in an admirable series of papers on this subject, which we have consulted with great interest and profit, "ne conduit en médecine qu'à des sottises." Croupal diphtheria is, indeed, a phlegmasia, but it is a specific phlegmasia, and in employing antiphlogistic remedies, you would arrive at results parallel to those of that unfortunate physician of Chapelle Véroux, who lost sixty patients out of sixty cases which he treated with blisters, leeches, and bleeding—bleeding, blisters and leeches. Thus, Mr. Stiles, of Pinchbeck, who, in the short space of sixteen days, had not less than three hundred diphtheric patients under his care (and to whom we are indebted for a very important study of the epidemic, which he witnessed, and by his intelligent efforts succeeded in greatly ameliorating), had the annoyance, in two out of the only three cases in which he was induced to apply blisters, to see the vesicated surfaces quickly covered with a diphtheric deposit, followed by sloughing; and thus, also, in the epidemic at Launceston, Mr. Thompson found that blistered surfaces were attacked with diphtheric inflammation. So also with leech-bites.

Clinical experience teaches that the first importance is to be attached, in these cases, to local applications. The choice involves the exercise of judicious discretion. Unable to discuss here at length the cases on which we found our practice, we are compelled to adopt a mild dogmatism. In the earliest stage, a solution of nitrate of silver should be employed, of the strength of thirty grains to the ounce of distilled water. This is best applied to adults by the curved whalebone probang and sponge now in general use, and to the child with a full-sized camel-hair brush. For the adult, Dr. Richard Quain's tongue-depressor should be employed, if at hand, or the handle of a tea-spoon. In the case of a child, he should be placed on the knee of an attendant—not the mother—and the head fixed. If he will not open the mouth—and gentleness is very desirable—the nostrils should be closed for a moment; as he opens the mouth for breath, the jaw should be firmly depressed, the lower lip being folded over the teeth to prevent the operator from being bitten, and then the tongue being kept down, the whole of the fauces are fairly

\* The priority appears to be with M. Lemaire. See his paper "On the Employment of Bicarbonate of Soda as an Antiphlogistic," in the "Lancette Française," vol. xii., p. 1416.

† See further Marchal de Calvi, "L'Union Médicale," 1855; Dr. Laségue, "Thèses de Paris," 1856.

brought into view, and may be thoroughly washed with the solution. This is no unimportant detail, for upon the effectual accomplishment of this manœuvre the success of the treatment will greatly depend. It should be repeated three or four times in the twenty-four hours.

If the exudation continues to extend, a collutory of hydrochloric acid should be applied in like manner, in the case of an adult. With children the addition of honey to the hydrochloric acid is desirable. But this is not only a much more painful application than the solution of nitrate of silver, but it has the disadvantage of creating a superficial eschar which simulates the diphtheric exudation, and hinders the perception of the progress of the disease. The solid nitrate of silver labors under similar defects, and has a danger of its own, from the risk of a fracture of the pencil in the mouth, which if swallowed, might be fatal—an accident which has happened, and which has come near to such a result in more than one case.

The constitutional treatment in croupal diphtheria should commence with the employment of an active emetic; it matters little of what nature, so that it be immediate in its action. Ipecacuanha is to be preferred, as being less depressing than most others. If the symptoms should give evidence of arrest, the ferrocchloric mixture may be employed with confidence. The valuable influence of the tincture of sesquichloride of iron has been fully tested in the English epidemics. Not only do Dr. Heslop, of Birmingham; Dr. Kingsford, of Boston; Mr. Stiles, of Pinchbeck, and other competent observers of extensive epidemics, speak in the most confident terms of its value, but we have confirmation on every side of the results which they announce. The combination of chlorate of potash and hydrochloric acid, with the tincture of sesquichloride of iron, is strongly to be recommended, especially in these croupal cases the chlorate of potash having an undoubtedly anti-diphtheric influence, where time exists to bring it into play.

When, in spite of these measures, the diphtheric inflammation, traveling onwards, reaches the larynx, when the altered voice, the oppressed breathing, and the stifled and less frequent cough give warning that the vocal chords themselves are affected by the exudation, in spite of courageous and energetic cauterization, then the question of tracheotomy must be entertained. For us this is a settled point. There is no longer any need to recapitulate the arguments on the one side or the other: tracheotomy is a resource which surgery is bound to employ for the salvation of the patient under such circumstances, and in the view of what experience teaches is otherwise certain death. The statistics of tracheotomy at the Hôpital des Enfants in 1855, showed ten cures and thirty eight deaths out of forty-eight cases, or one patient saved in five. But the statistics vary with the character of the epidemic, and with the courage of the surgeon who is so wise as to counsel or to perform the operation when the indications we have given are established, or hesitates until the most valuable time is lost, and eternity is at hand for his patient. We would give only one counsel as to the performance of the operation: it is, that the trachea should be fixed with an ordinary tenaculum, or one grooved on its convexity. It facilitates the operation by



rendering it more secure and speedy. The most important precautions are those connected with the subsequent care of the patient, which includes the employment of a double tube, of which the inner may be frequently removed and cleansed; the placing a light gauze around the neck; the impregnation of the air with vapor of water (from a kettle or otherwise); and the cauterization of the edges of the wound.

III. Malignant diphtheria, or malignant diphtheric angina, is that form which has attracted the most careful attention, and has impressed medical observers in this country most strongly with the active and fatal character of the diphtheric poison. We have ample materials for the description of this most severe form. Its prodromata are, intense headache, severe febrile condition, vomiting, or occasionally sudden nasal flux, or (as at Walsall) "hæmorrhage from the nose, mouth, rectum, or all the mucous canals;" the skin is hot and pungent; the tongue thickly coated. The throat soon becomes painful, deglutition difficult, and considerable engorgement occurs of the submaxillary, parotid, and cervical glands. This characteristic engorgement increases to a surprising extent, the glands often projecting far beyond the jaw; and the cellular tissues become deeply infiltrated and doughy to the touch. The throat, tonsils, and soft palate are covered with a yellow, leathery deposit, which early exhales a fœtid odor that presently becomes intolerable. The patient is now in a condition of intense adynamia; the pulse is rapid beyond limit, the face of vivid pallor, the lips congested, the eyes lachrymose, the mouth slobbering, deglutition difficult, perhaps almost impossible; from the nostril often a fœtid ichor distils, showing that the exudation has also appeared on the walls of this cavity, where it may be seen if the nostril be expanded by an ear-speculum. Coma and extreme prostration follow; and if a fatal termination ensue, the patient dies in a state of somnolent quietude which strongly contrasts with the agitation preceding croupal suffocation.

We need not to adduce examples of a type which has been but two commonly witnessed throughout the country. Many instances have been seen of an exceptional and much earlier termination by death. Thus, at Bagshot, death occurred in many cases in the first stage through sudden and extreme adynamia, before the exudation had fully formed. Dr. Semple exhibited the parts in one such case at the Pathological Society; there was only a slight effusion into the larynx; the tonsils and neighboring parts were extensively congested, and it would appear that the patient had died from suffocation before there was time for further exudation to take place. Dr. C. J. B. Williams was called upon to treat cases in which the toxic influence had been equally apparent. Dr. Blount, of Bagshot, had added his experience in confirmation. In the fen district of Cambridge a series of cases occurred in May, in which death ensued from the intense depression of the vital powers.

The treatment of these cases must necessarily be energetic; it needs to be essentially tonic. Of the various local applications, none equal in efficacy hydrochloric acid freely applied, or Beaufoy's concentrated solution of chloride of soda. This was used by Mr. Davey,

of Rumford, in Essex; by Mr. Stiles, of Pinchbeck; by Mr. Wilkinson, of Spalding; by Dr. Cammack, of Spalding; and by many other practitioners who were seated amidst wide-spread epidemics, and their testimony is strongly in its favor. Where other local applications are employed, a gargle composed of two drachms of Beaufoy's solution to eight ounces of water should be used concurrently. Dr. Cammack recommends that two ounces of glycerine should be added to the gargle. When the nasal fossæ are implicated, such a solution should be injected through the nostril; it is preferable to the solution of alumen, or the insufflation of powdered alum, recommended by M. Bretonneau and M. Trousseau.

Of other local applications, alumen is too feeble; sulphate of copper not very efficient, and more poisonous; bromide of potassium, recommended by M. Ozaman\* as a solvent of diphtheric exudation, theoretical and untried. Of the tincture of iodine, recommended by Dr. Marzel, we have no experience. Of the actual cautery, or of the cautery major dipped in boiling water, advised by Dr. Dauvin,† we do not desire to have any practical knowledge.

Of the many internal remedies which have been advised, we do not know of any on which so much reliance can be placed as on the tincture of sesquichloride of iron, with chlorate of potass, chloric ether, and hydrochloric acid, in the form of mixture, sweetened with syrup, full doses being employed according to the age of the patient, and frequently repeated. A free use should be made of generous wine, beef-tea, coffee, eggs in combination with brandy and wine, milk, and whatever other form of nutriment the ingenuity of the surgeon or the fancy of the patient can suggest. When food is refused, then enemata similarly composed must be administered frequently, in small quantities of two ounces and upwards, that they may not be rejected; for it is of the first importance that inanition should not open the last portals of life to the advancing disease.

If the medical attendant should have the good fortune to vanquish the disease, he must be prepared to meet with long-enduring debility and adynamia—with a loathing for food, which will tax all his ingenuity and patience—and with various complications which call for a brief notice.

The most frequent sequence retarding convalescence is *paralysis of the soft palate*. This has been frequently noticed by various observers. The symptoms are, a nasal twang in the speech, incapacity for suction, and the regurgitation of fluids by the nostrils. The treatment is by local application of astringents, feeble cauterization, or by the employment of Duchenne's Faradization: one conductor to be applied to the soft palate, and the other over the mastoid process; or, as Duchenne himself recommends, both in one handle to the soft palate.‡ This paralysis may be of local origin; and Dr. Gull and Prof. Trousseau have held this opinion from cases under their observation. But

\* Académie des Sciences, May 19th, 1856.

† L'Union Médicale, 1855.

‡ See "L'Union Médicale," 1857, Trousseau and Lasegue: Dr. Moriceau, *ibid.*, 1857; Dr. Maingault, 'Thèses de Paris,' 1854, where an excellent monograph will be found on this subject.

a general paralysis has also frequently supervened, which is evidently the result of the general toxic influence of the diphtheric poison on the blood. M. Trousseau had such a case lately under treatment at the Hotel Dieu, and it has induced him to change his opinion as to the local origin of the paralysis. Dr. Gull and Dr. Kingsford also relate instances. M. Herpin, the surgeon of Tours, was so affected during several months, and has given a graphic account of his symptoms. Other sequences, more frequently observed in this country, have been severe otalgia, amaurosis and headache. Diphtheritic ophthalmia, of which epidemics were described in Germany, many by Græfe, in 1853-54;\* and by M. Jobert, at the Hôpital St. Eugénie, Paris, in 1857,† has been very little noted in this country.

In terminating this Report, we may offer some concluding propositions by way of summary:

I. Diphtheria is a *specific* disease. This is seen in its origin, march and mode of extension; in the character of its exudation; in its local manifestation; in its seats of predilection; in its toxic influence; in its prodromata; its manner of termination and its sequences.

II. It is often confounded with scarlatinal angina, and with gangrenous cynanche. We have sufficiently indicated the diagnosis.

III. It is propagated by infection and by contagion. It is both epidemic and sporadic in its manner of invasion, and is remarkable for the severity with which it is developed in limited centres of population.

IV. Diphtheric angina presents three varieties, which may be designated—1. Simple diphtheric angina. 2. Croupal diphtheric angina. 3. Malignant diphtheric angina. The prognosis of the first is favorable; of the second unfavorable; and of the third, most unfavorable.

V. The treatment should include the local application of a solution of nitrate of silver, Beaufoy's concentrated solution of chloride of sodium, or hydrochloric acid, according to the circumstances also indicated. The internal remedies most useful are, emetics in the early stage of croupal diphtheria, and the tincture of sesqui-chloride of iron with chlorate of potash.

VI. Tracheotomy should be resorted to in the second or third stage of croupal diphtheria; leeching, blistering, and bleeding should always be avoided.

VII. The means of prevention, besides careful hygienic measures—as ventilation, etc.—must also include the daily examination of the throat where the epidemic type presides—a matter of the greatest importance, as experience has very fully shown, and the early isolation of the patient as soon as attacked—a precaution hardly less necessary.

\* Archives für Ophthalmologie; see also "Gazette Hebdomadaire, 1856, Warlomont et Test etin.

† Archives Générale de Méd., 1857.

ART. X.—*Note on Diphtheria.*

ARÆTÆUS, who lived nearly eighteen centuries ago, in his chapter on *ulcers of the tonsils*, has described a malignant variety of sore throat and mouth, which, to a great extent, agrees with those of the moderns, as being characteristic of malignant sore throat, diphtheria, black tongue, membranous angina, etc. This malignant form has ulcerated surfaces, which “are broad, deep, foul, surrounded with a white lived or black crust. The parts around the crust resemble carbuncle,” etc. He describes this disease as also involving the tongue, neck, trachea: “Patients thus affected,” says he, “die in a few days of fever and inflammation, accompanied with fœtor, ichor, cough, dyspnœa, etc. The mode of death is most miserable; there is a pungent, burning pain, like that of a carbuncle, with a fœtid breath—a putrid stench, which they reïnspire into the chest, and so loathsome, that they cannot endure their own fœtor. They are hoarse, lose their voice,” etc.

The history of epidemics for the last two or three centuries abounds with notices of this disease under the various names of malignant, strangulatory, pestilential, putrid, gagrenous sore throat, membranous croup, etc. These vague descriptions are not, however, accepted as diagnostic of diphtheria, as described more than thirty years ago by French pathologists, such as Gendrin, Bretonneau and others—a malady which is attended with a plastic or concrete membraniform exudation, and one occurring often among adults, generally as an epidemic, and supposed to be contagious.

The following note would seem to relate to diphtheria rather than to simple croup, as the former only has been regarded contagious.

B. D.

*Victim of Science.*—M. Sturme, health officer at Blandeneques, near Saint Omer, performed tracheotomy upon a young girl aged 16 years, who was attacked with croup; not having the canulas necessary in this operation conveniently at hand, he substituted for it a piece of elastic sound, which soon became clogged up. Mr. Sturme, in order to remove the obstacles, breathed through the tube by applying his mouth to its external extremity. This imprudent devotion was the cause of his loss. He was in turn attacked with croup, and in spite of the care of his neighboring confrères, in spite of tracheotomy, which was skilfully performed by Dr. Revil, the patient succumbed. M. Sturme leaves a widow and two children. (*Translated from L'Union Médicale*, by S. E. C.)

ART. XI.—*Preparations of Iron—Endermic Medication.*i. *On Liquor Ferri Iodidi*: By DR. ROBERT BATTEY.

Complaints are often made of the difficulty encountered in keeping and dispensing the official liquor ferri iodidi in good condition. Various suggestions have been made, by way of remedy for the evil, some proposing modifications of the formula, others the use of small ground stoppered vials of transparent or opaque glass.

In my own experience this trouble was early encountered and easily overcome by the use of clean iron wire as an additional protecting agent. I much prefer it in the form of wire to iron filings, or iron by hydrogen, which I have heard spoken of. Immediately upon the completion of the preparation, I take a piece of fine iron wire, clean it well with sandpaper, coil it around a glass rod or cedar pencil, draw out the coil to nearly the length of the bottle and drop it in. If the bottle be a large one, I use two or three of these coils. In dispensing small quantities, I have found it convenient to use a straight piece of larger wire passing through the center of the corks (I never use glass stoppered vials) and to the bottom of the vial. I do not deem it *necessary* that the wire pass to the bottom of the vial, as the charge commences at the surface, but have thought that it gave some additional security if long kept on hand.

With this expedient I have encountered no difficulty in keeping from two to five pounds of the solution in one shop bottle, and dispensing it, day by day, in small parcels. I have not found the presence of the wire in the vial to detract from its good appearance, or to be in any respect objectionable, if it be bright and either neatly coiled or passed through the cork, evidencing care and design in the arrangement. Upon several occasions has this precaution been marked and openly commended by physician and patient.

I have occasionally by this means restored (in appearance at least) deteriorated samples, but think in this, as in every thing else, an ounce of prevention is better than a pound of cure.

I would suggest that the formula be allowed to remain unaltered, and the use of the wire added to the direction following it in the Pharmacopœia.—*Am. Jour. Pharmacy.*

ROME, GEORGIA, Feb. 1859.

ii. *Hæmstatic Properties of the Perchloride of Iron.*

The solution of this persalt is now almost universally employed to arrest arterial or venous hæmorrhage, resulting either from accident, or as a consequence of surgical operations. It has also been found useful in intestinal hæmorrhage; in one case in particular, M. Demarquay, of Paris, administered, morning and evening, enemata of seven ounces of fluid with twenty drops of the concentrated solution of perchloride of iron, and a tablespoonful of the perchloride syrup (five or six drops to the tablespoonful), where the hæmorrhage from the bowels was considerable, and had resisted the ordinary remedies. The result was extremely satisfactory. The same surgeon relates a

second case of extensive abscess of the shoulder, where an injection of iodine caused severe hæmorrhage. This was arrested by throwing into the sac a lotion composed of seven ounces of water and ten drops of the perchloride.

In gonorrhœa and leucorrhœa, injections of the perchloride have been tried with success in weak and lymphatic subjects, the proportion of the perchloride being twenty drops to three ounces and a half of water.—*Lancet*.

iii. *On the Best Form to be given to certain Pharmaceutical Preparations intended for External Use.*

In an article in the *Bulletin Général de Thérapeutique*, it is remarked that greasy applications to the skin do not possess in general any great therapeutical efficacy, and it is therefore suggested that they might be advantageously replaced in many instances by saponaceous compounds. In studying the action of the latter, M. Deschamps composed a soap with iodide of potassium, and rubbed it in four times upon his epigastrium. He analyzed his urine in the intervals of the frictions, and found it to contain appreciable quantities of iodine. He afterwards washed with water the part which had been rubbed with the soap, and he found in the liquid some fatty acids arising from the decomposition of the soap by the fatty acids secreted by the skin, together with iodide of potassium in solution. These washings were continued for five days successively, and every day some iodine and fatty acids were obtained. These facts prove that the skin is easily penetrated by saponaceous compounds; that the skin rejects a part of the principles which it absorbs; and that therapeutical agents may pass through the skin, diffuse themselves into the system, produce physiological effects, and be expelled by the ordinary passages. M. Deschamps has therefore proposed to employ certain saponaceous compounds, instead of liniments and pomades, in cases where the surface of the body, where these preparations are to be applied, is not denuded. These soaps contain respectively iodide of potassium, laudanum, ammonia and laudanum, extract of belladonna, sulphuret of sodium, and digitalis, combined with an alcoholic solution of soap.—*Br. and For. Med. Chir. Review*.

iv. *Experiments relative to the Hypodermic Treatment of Disease.* (Western Med. and Surg. Society, April 1, 1859.)

Mr. Charles Hunter related some experiments relative to the hypodermic treatment of disease. The object of these experiments was to test the value of this plan of treatment, and to compare it with other modes of administering medicine. The author stated that he had in two cases of neuralgia to desist from Dr. Wood's plan, because abscess followed the constant localisation; he was therefore induced to try the injection of parts distant from the neuralgic part. He found the greatest benefit follow his plan, and local inflammation was by this means avoided. Cases of neuralgia were cited showing how

the injection of the cellular tissue (the tissue Mr. Hunter always injected) of any part would act rapidly upon and cure neuralgia in another part. Experiments were detailed which he had made to see whether those injections acted locally, or by being absorbed before they produced their effect. The conclusions were these: 1st. That they acted by absorption. 2nd. That they acted quicker than the endermic method or stomachic doses. 3rd. That they acted more effectually. 4th. That a small injected dose was equivalent to a much larger one by the stomach. Mr. Hunter then showed that for speedy action, which he supposed chiefly due to the rapidity of the absorption, this plan was superior to others. 1st. Where the immediate and decided effect of a sedative or narcotic was required. 2d. Where narcotics administered by the usual methods fail to do good; and 3rd. Where patients cannot or refuse to take medicine.—*Med. Times and Gaz.*, April 23, 1859.

ART. XII.—*Vermifuge Properties of Ailanthus.*

*On the Vermifuge Properties of Ailanthus Glandulosa, of China.*—This tree, so well known in the United States as a shade tree, and recently so much decried, owing to the strong peculiar odor of its flowers, is the subject of a notice in the *Journal de Pharmacie* for March, by Prof. Hetet, of the Ecole de Médecine of Toulon. This tree, which belongs to the natural family Xanthoxyleæ and allied to the genus Rhus, is also identical or closely related to the Japanese Varnish tree, and within a few months has acquired more importance, in view of its leaves being used as food for a species of silk worm, Bombyx cynthia, which has recently been introduced into France.

If a piece of the bark of *Ailanthus* is chewed, it gives a decided bitter taste, and soon after a general unpleasant feeling, accompanied by nausea and prostration, similar to that derived from tobacco by novices, or from henbane. These physiological effects seem to have given rise to the term *caco dendron* (Ehrh.) formerly given to the tree.

These qualities induced the author to enter on an investigation of its effects on animals, and to analyze it, but he afterwards ascertained that M. Payen *Annals de Chimie* t. xxvi. p. 239, had already given an analysis, in which it appears to contain lignin, chlorophylle, yellow coloring matter, pectin, bitter substance, aromatic resin, traces of volatile oil, nitrogenous matter, and some salts.

The powdered bark has a greenish yellow color, and when recent has a strong virous odor and very bitter taste. The coloring matter is not valuable as a dye. The pectin is very abundant. By treatment with alcohol, the resin, volatile oil and bitter principles are dissolved, and on evaporation, a soft, oleo resinous extract, strongly odorous, is obtained.

M. Hetet has experimented therapeutically and physiologically with

the powder, aqueous alcoholic extracts, and oleo resin on dogs. All have proved successful, that is to say, in all cases they have produced abundant stools, with the expulsion of tænia.

The powdered bark was given in doses of 8 grains; the aqueous extract, in doses of four grains, and the oleo resin in three grains. The pure resin proved purgative in six grain doses, but did not expel worms. The author attributes the prostrating effects of the bark to the volatile oil, as the resin did not produce them. The effects of this oil is so decided that an assistant, charged with evaporating the extracts, has been seized with vertigo, cold sweats, and vomiting every time he neglected the precautions adopted to avoid the vapors; and the author himself had experienced them to some extent, from similar emanations, and he also had observed the effects of the extract on a dog, accidentally taken.

Being satisfied of the emetico-cathartic and anthelmintic power of the ailanthus, the author sought to verify it in practice, and gives several cases in point.

1st. F——, a laborer in the arsenal, having sharp pain in his left side, stomach and abdomen, was examined in reference to the debris of tænia, and evidence of their existence noticed. The usual vermifuges, such as pomegranate bark, castor oil, etc., did not relieve him. The powder of ailanthus having been prescribed, at the instance of the author, soon expelled several fragments, and another dose of a gramme ( $15\frac{1}{2}$  grains) relieved the patient of the worm, four and a half yards in length, including the head.

Returning to the hospital ten weeks after for another affection, he states, on inquiry, that it had not returned since.

2d. The Abbe L——, having noticed portions of tape worm, came to the hospital, and, under treatment, considerable portions of the worm were expelled, but he became impatient and left before he was cured.

3. L——, aged 49, a cooper by profession, had long suffered from the tape worm, and exhausted the nostrums of charlatans. He took the powder of ailanthus, alternately with castor oil and glauber salts, during several days, the dose varying from 12 to 30 grains. After fifteen days' treatment the patient passed a complete worm,  $5\frac{1}{2}$  metres (18 feet) long.

The author believes that the ailanthus has also febrifuge properties, and that its administration is entirely safe. (*American Jour. Pharmacy, May, 1859.*)

#### ART. XIII.—*Vertigo.*

##### i. *Nervous Vertigo and its Treatment.*

[J. W. GREENE, M. D., in the *New York Jour. Med.* (March, 1859), gives a condensed account of DR. MAX SIMON'S Paper on *Nervous Vertigo and its Treatment*. Portions of this essay, which was honored with



a prize by the Academy of Medicine of Paris, will be subjoined.] Dr. Simon defines this disease to be a painful illusory turning, which seems to carry with it the person and external objects, whether they are in repose or in movement, and where the body totters and is ready to fall. This definition, which he borrows from P. Franck, he accepts, so far as it relates to exactly the vertiginous sensation conceived in an abstract manner, but he does not give it the extended signification of Franck. All modern physicians have studied this disease as a symptom and as an essential disease. Authors, who have written on this subject, have studied it and treated it as a particular perturbation of nervous function, and these have embroiled the whole question, by their tendency to see the affection as an essential instead of an accident in many cases.

He finds it impossible to localize the trouble to a limited extent. It is never dangerous—never produces death. Is the point of departure to be placed in the peripheric nerves, in the organ of vision, in the nervous cords, or in the centre of perception itself? Piorry establishes the fact, that in a certain number of cases the "migraine" is a neurosis of the iris, with an ambient iridation, more or less extended. \* \* \* In the first group he places those who, attacked by vertigo, do not offer to the observation, either from the nervous system or from any of the other systems any trouble, either static or dynamic, which can hold this accident in dependence. This is idiopathic vertigo, properly so called. In the second group are placed those in whom the vertigo starts from a purely functional derangement of an organ other than the brain, mingles itself with various accidents, but is detached, nevertheless, by the clearness of its manifestations, its intensity, the frequency of its re-appearance, and its duration. This is sympathetic vertigo. There is one function in the economy, whose perturbations frequently give rise to vertigo. This is the genital, when disturbed by masturbation, excess of coitus, spermatorrhœa, and in rare cases continence. He calls attention to the fact, that many patients are bled, blistered, and starved, because the physician does not understand the disease.

Symptomatology and preliminary considerations. The vertiginous sensation, when we see it clear and distinct, cannot declare to us the dynamic or static condition of the phenomenon. It is useful for symptomatologists to study this sensation in some of the circumstances where it is produced without being a disease, properly so called. The study of this subjective side of the phenomenon is here more necessary, for, to tell the truth, in the primitive nervous vertigo frequently the vertiginous sensation constitutes all the disease, in so much, at least, as an intermittent manifestation, producing itself at intervals, more or less long. A common cause of vertigo is mounting to an elevation and looking freely down and around. In this case the vertigo consists, ordinarily, in a sentiment of instability, provided there are safeguards to prevent the persons falling; but, should these be absent, the illusion is carried past this, to the annihilation of view, to scotodinia, and his fall will be inevitable. In such a case, it is evident that the vertigo, in the first phase of its development, is a physical phenomenon, unknown in its nature, but which, one degree more advanced in its manifestation, a physical influence, fear, comes to

mingle itself with this first impression, to give it a more decided character.

The result is not the same when the vertigo results from the action of turning rapidly, as in the waltz, or by looking at anything that is whirling rapidly itself, or by the uniform or continued movement of a large body of water. Here it is not the sentiment of instability, but the primitive character of vertigo, the visual illusion. Primitive or idiopathic nervous vertigo, like all neurotic vertigo, is submissive to the law of periodicity ; but, while in many other diseases the organism attentively interrogated presents frequently some phenomena which reveal more or less surely the morbid servitude which weighs upon the nervous system, nothing in pure vertigo betrays it outside of its access. Most of the neurotic troubles are produced again and again by the same exciting causes—nothing of this kind takes place in nervous vertigo ; once the access terminated, the impression is completely effaced ; when the vertigo is simple, it is only a passing accident of the nervous system, which receives no durable attack, or none capable of reproducing itself in the intervals of access of the disease, by any symptom which would betray it before the development of the disease itself. These irregular periodical accesses, by which nervous vertigo manifests itself, and which, in the state of science, are the sole means given to our observation, are the ones by which we intend studying the disease. The phenomena present themselves under various forms and in different degrees of intensity. These varieties of form and of intensity constitute all the symptomatology. Some persons are troubled with vertigo even when in the horizontal position and with the eyes shut, others when rising suddenly from a recumbent position—this latter can be avoided generally by rising carefully. When the vertigo is more intense, the patient becomes fearful, sees objects as through a veil, or has illusory visions, luminous sparks are seen ; sometimes he falls, but falls voluntarily, so to speak, not as in an epileptic or apoplectic fit ; he does not lose his senses. If by chance he does, it appears to be from the fear, not from the functional cerebral trouble. When the disease arrives at this point, the general circulation, which is calm in simple vertigo, is more moved, the pulse is accelerated, but the skin does not appear to increase in heat proportionately ; sometimes there is nausea, followed by vomiting, which generally puts an end to the trouble ; but these latter difficulties are rare in simple vertigo ; they are generally found in complex cases. The second article takes up the consideration of this subject of sympathetic vertigo, caused by dyspepsia. He speaks of the great sympathy existing between the stomach and other organs, and states cases where this vertigo has been produced by a few glasses of soda water, wine, or by articles of food. Article third treats of vertigo, produced by hypochondria, venereal excesses and spermatorrhœa. He states that these cases are not rare. In speaking of that caused by seminal losses, he says, before the excessive losses have thrown the organism into that grave state called tabescence, and have extinguished in the genital apparatus all force of reaction, each venereal excess, either by coitus or by masturbation, has a direct reaction upon the nervous system, which receives a more or less profound

blow, and the nervous vertigo results from this perturbation. He also cites cases produced by excessive continence. Article fourth is devoted to vertigo during the convalescence from other diseases. Article fifth treats of sea-sickness or marine vertigo.

Progress, termination and prognostic. Nothing is more irregular in its progress. It has continued with variable intermittences nearly the whole life, or it may be of very short duration. The access is also very irregular and comes on from the most varied causes. Vertigo, considered as a pure neurosis, is very generally of little account, so far as life is concerned.

*Chapter eighth* is upon the differential diagnosis, and is divided into paragraphs, the first of which treats of vertigo, produced by anemia; the second of vertigo in a plethoric condition; the third, vertigos caused by organic diseases of the brain. *Chapter ninth* is devoted to the etiology of the disease, which is classified under two heads—causes of idiopathic vertigo and the causes of sympathetic vertigo.

*Chapter tenth*, the concluding chapter, applies itself entirely to the treatment. He first speaks of the momentary vertigo, which he calls physiological vertigo. For this kind he recommends the patient to accustom himself to the things that produce the trouble; perseverance in this will cure the disease, he thinks. The next in order is idiopathic vertigo. In this disease the patient is to be placed horizontally, unless he is easier in some other position, as sometimes happens. The clothing should be loosened, he must be perfectly quiet, the room must be quite dark; when the patient loses his senses, the same means are recommended, with the addition of dry frictions—alcoholics, aromatics, coffee, or the inspiration of ammonia; during the intermissions, castoreum, camphor, or the tempering powder of Stähl; Pitcairn speaks well of opium. The vertiginous habit should be combated; this depends upon the morbid activity of the encephalon; regulate the life of the patient; let them follow hygienic laws. The intervals between the repasts must not be too long; the patients must avoid, as much as possible, all saddening emotions, and everything that will weigh upon the mind. They must avoid all drinks or food that generally disagree with them. Most usually decoctions of sage and mint, etc., are found to be very useful. Valerian and assafoetida were much used by Herz. Attention must be paid to the bowels, as there is generally constipation. The datura stramonium was used by Abercrombie with success. In many cases moral force is necessary. It is necessary that the practitioner should be careful not to confound the idiopathic form with the plethoric.

Vertigo depending upon dyspepsia is a variety more amenable to treatment than any of the other forms. This form generally depends upon a hypersecretion of acid in the stomach. Magnesia, lime water, pastilles, Vichy water, etc., are the best means of satisfying the first indication. Sometimes the intestines are so irritable that the slightest causes may produce diarrhoea. In these cases the alkaline waters are very beneficial; but this medication only removes the trouble temporarily. Trousseau recommends this to be prolonged after the patient feels well. When the patient can do it, travel, combined with the use of mineral waters, is good. This treatment is only to be

used when the disease is light. When the stomach has undergone profound alteration, or a saburral state has taken place, an emetic or purgative may be given; this done, we may then return to the alkaline treatment. The diet must be strictly attended to. Bitters, the excitant tonics, etc., may be given with success. Attend to the bowels; if they are not relieved by the common methods, 10 or 15 grammes of senna, with 10 or 15 drops of cherry laurel water may be given.

*Nervous vertigo from hypochondria, spermatorrhœa, and venereal excess.*—It is not necessary to go at length into this treatment, as it is only that of the diseases mentioned. In regard to spermatorrhœa, he recommends lupulin, which he considers the opium of the generative apparatus.

*The vertigo during the convalescence from diseases, as fevers.*—Ducher and Widel used cinnabar in these cases, and report four cures. He does not think that quinine is of benefit in this kind of vertigo, following intermittents, but prefers the general tonics.

*Nautical vertigo.*—He recapitulates many popular remedies that have been tried, but thinks that keeping in the open air, smelling salts to combat the syncopal tendency, not to look at anything movable, not to take part in arguments, eating the meals on deck, mint water, aromatized pastilles, a belt around the stomach moderately tight, which must not press on the chest. If the vomiting comes on with all these precautions, then the patient must take the horizontal position; he may then have tea, infusion aromatic, grog, etc.

*Arthritic or gouty vertigo.*—He states that it is with extreme circumspection that he expresses himself upon the reality of this variety of nervous vertigo. He cannot deny, in an absolute manner, the reality of connection in certain cases between the arthritic diathesis and the vertiginous habit. The authorities who have written on the subject suffice to place it on the nosological catalogue. He does not pretend to do it, but only to call the attention of practitioners to the facts.

ii. *Treatment of Vertigo from Gastric Derangement.* From the French of TROUSSEAU and BRETONNEAU.

In the morning the patient should take a cup of a solution of *quassia amara*, made by macerating two grammes of the chips for twelve hours in a cup of cold water. The following should also be taken:

R.—Sodæ bicarb.  
Cretæ ppt.  
Magnesiæ                    ää grs. xv.

Mix, and divide into three powders, which should be taken two hours after each meal, in a half glass of sugar water, the third being taken at the time of going to bed. As long-continued use of magnesia may produce looseness of bowels, the use of the alkaline powders should be suspended from time to time, so that they should be given for six consecutive days, and then resume their use for an equal length of time, after a cessation of eight or ten days. In the interval the patient should employ natural mineral waters, such as those

of Vichy, Pougues, Vals and Ems, which are principally active, on account of their alkaline constituents; or such as those of Bussang, Schwalbach, and Sultzbach, which owe part of their action to these substances, and most of it to the ferruginous principles they contain.

To excite the appetite, and to stimulate the contractility of the muscular fibre of the digestive apparatus, it is well to have recourse, from time to time, to the properties of strychnine. This may be given in solution, or in the form of pills of the extract of nux vomica, beginning with a dose of 0.05 centigrammes, which may afterwards be slightly increased.

The hydropathic treatment at home, with cold lotions, or wet cloth, is in a large number of cases of undoubted utility. Above all things is it necessary to insist upon a substantial tonic regimen, moderate exercise, and everything that can favor nutrition. The use of pepsin, in the form of *pastilles*, seems also indicated in such cases. L. H. S. (*Am. Med. Monthly.*)

#### ART. XIV.—*Late Researches on the Atmosphere.*

THE activity of chemistry, in its examinations of the accidental and essential constituents of the atmosphere, has been very great during the past year. We propose throwing together some gleanings from French and English journals on the subject, that will be of interest in a hygienic point of view.

*Iodine in the Atmosphere.*—Bouis has detected iodine in the rain-water of Paris during the months of April, May, June, July and August. Since it is the opinion of some medical men that the iodine contained in the air is not without some influence on public health, Bouis considered it a matter of importance to determine in what condition it existed in the atmosphere. The solution of this question is a very complicated matter, and he has made many fruitless experiments; but the results are so interesting that they are worth publication.

Being of the opinion, advanced by Chatin, that iodine is in a free state in the air, he endeavored to detect this substance, in the first products, in the distillation of rain-water; but he discovered that it remained in the residuum of the distillation, and, as rain-water always contains ammonia, he supposed that the iodine would be found in it as iodide of ammonia—a compound only slightly volatile. But the use of perchloride of iron showed that the iodine, in a large number of cases, was associated with organic substances that concealed its presence. If rain-water contained iodine in a free state, distillation alone would eliminate it; or if the iodine was in the condition of an iodide, the addition of perchloride of iron would make its elimination easy—but this is rarely the case.

In treating rain-water with perchloride of iron very slightly acid, brownish flakes are formed, resembling crenate of iron, in which case iodine cannot be determined in the products of the distillation; but if, after such treatment, the ochreous deposit be calcined, in the presence of carbonate of iron, the organic matter is destroyed, and then it is an easy matter to detect the iodine.

Bouis concludes that iodine is found in rain-water, sometimes in the condition of iodide of ammonium, but most frequently in combination with organic substances.

*Atmospheric Ozone.*—Dr. Moffat read a paper on the subject, at the meeting of the London Meteorological Society. He stated that a slip of paper moistened with iodide of potassium and starch becomes brown after exposure to the air, but after a longer exposure it lost this color; that if suspended over a cesspool the brown discoloration would not be produced, and if a brown slip were suspended over a cesspool it would also lose this color. "In these results," the author observes, "there are proofs of three distinct agents: one, ozone, which decomposes the iodide of potassium; the iodide being set free, produces the brown color. The second, sulphuretted hydrogen, the hydrogen of which removes the brown color by combining with the iodine, and forming hydriodic acid. The third, incompletely oxidized substances, the products of decomposition of animal and vegetable matter, which are more easily oxidable than the oxide of potassium." "As the products of putrefaction and combustion are found at the earth's surface," Dr. Moffat concludes that, "the quantity of ozone must be greatest in the lowest strata of the air, and that consequently the quantity of ozone must *there* be at its minimum." He also states that where the air is stagnant ozone is at its minimum; and that as the north current is the lower stratum of air in motion, it is the minimum ozonic current, while the south current, being the higher air in motion, is the maximum.

The north current is the "death current," the south that of "sporadic diseases." The deadly effects of a calm are attributed to a concentration of the products of the decomposition of animal and vegetable substances, which substances are made innocuous by ozone, since it oxidizes them. In calms, fevers and cholera prevail, and the type depends on the degree of concentration of poison. Dr. Moffat has seen "an epidemic commencing with scarlatina run into typhus, and terminate in a disease of choleraic type, rapidly decline after cleansing and draining. We have no power over the winds, but he believes that if a south or ozoniferous current could be directed into 'fever-nests,' or into cholera localities, these diseases would vanish; and in proof of the correctness of this opinion, mentions that cholera declined at Newcastle in 1853, and in London in 1854, after the setting in of the ozoniferous current."

Mr. H. S. Eaton read a paper at the same meeting of the Meteorological Society, showing, from tables, that "ozone was prevalent to the largest extent when the direction of the wind was between the south and west points of the compass, and when the amounts of rain and cloud were greatest; and that the least amount of ozone was coincident with winds having a northerly and easterly direction, and

with the least amount of cloud and rain." These results, it will be observed, agree, in the main, with those of Dr. Moffat.

*Measurement of the Variable Intensity of Ozone.*—Dr. Lankester has devised an instrument for this purpose, composed of two cylinders, contained in a box, on which is wound a band of prepared paper. The arrangement is set in motion by ordinary clock-work. As the band leaves the one cylinder it is wound up by the other, and the arrangement is so managed that but a small portion of the paper is exposed at any time to the action of the air. The whole band is divided into twenty-four parts, corresponding with the hours of the day. The quantity of ozone which the atmosphere contains at different periods of the day is thus indicated by the different coloration of the divisions. L. H. S. (*Am. Med. Monthly.*)

ART. XV.—*Death from the Passage of Vomited Matters into the Wind-pipe.*—St. Thomas's Hospital. (Case under the care of Dr. BRISTOWE)

THE following case illustrates some of the ill-effects of drunkenness, and shows the impropriety of leaving persons in this condition wholly uncared for:

J. W., a laborer, 32 years of age, was brought to the Hospital by the police at seven o'clock in the morning. He had some little time previously been taken to the station-house in a state of complete and helpless intoxication; had remained there a few hours without having much attention bestowed on him; and, at the end of that time, his condition then exciting some alarm, he was brought to the Hospital.

On admission he was found to be still somewhat comatose; but his face was livid, his breathing laborious and wheezing, his pulse almost imperceptible and rapid, his skin moist. He presented, in fact, most of the features which characterize the last stage of bronchitis. He died in the course of a few hours.

*Autopsy.*—Body in good condition, but anasarous. The superficial veins of the brain were more loaded with blood than usual, and the substance of the organ was somewhat congested. Pericardium and heart healthy; the cavities of the latter were dilated and filled with soft black coagula; pleura free from adhesions; lungs large, much inflated, but not emphysematous; they were crepitant throughout, and a little congested; but there was no œdema. On squeezing the sectional surface of any part of either organ, all the cut bronchial tubes yielded tenacious cylinders of a dirty yellowish hue; and on tracing the tubes it was found that all of them, from the largest trunks to the minutest ramifications, were filled almost completely by

a material in some places softer, in some more tenacious, but everywhere, in all essential particulars, resembling that squeezed from the cut orifices. The mucous membrane of the tubes was deeply congested throughout, and in some instances it appeared as though small extravasations of blood occupied the submucous tissue. The mucous lining of the larynx and trachea was likewise intensely congested; but it was not thickened, and there was no exudation on the surface. Under the microscope the material occupying the bronchial tubes was found to present a large quantity of free oil, cells of potato, and innumerable fragments of muscular fibre, together with a good deal of stuff of which the nature was not clear, the whole being intermixed with ciliary epithelium, and some imperfect forms of cells belonging to the mucous which cemented, as it were, the various substances into the form of tenacious cylinders. Peritoneum healthy; liver of a uniform reddish brown hue, congested, but healthy; spleen of usual size, pale, flabby, and wrinkled; pancreas, kidneys, and supra-renal capsules healthy. The mucous membrane of the cardiac end of the stomach was somewhat congested and softened; but that of the rest of the organ and of the intestines was healthy, presenting, however, a more than ordinary quantity of mucous adhering to it. Aorta healthy.

It is evident from the results of the examination, that the man while in his drunken state had been continually vomiting, or rather regurgitating the contents of his stomach; and that, owing to his unconscious and perhaps partially paralysed condition, had drawn these continually through the larynx and thence into the bronchial tubes, which had gradually become choked up. It is clear, too, that the irritation of the foreign matter had excited in the latter intense congestion and copious effusion of mucus; and that from the combined effects of the abundant accumulation of vomited material, of congestion and of effusion, the patient had been suffocated, and had developed those symptoms which had simulated bronchitis. From the complete way in which even the smallest bronchial tubes were filled; from the history, imperfect though it be, and from other circumstances, it is obvious that the process had been a gradual one; and one cannot help regretting, therefore, that he had not before been placed under competent supervision, and that such precautionary measures and such treatment as would have suggested themselves to any medical practitioner, had not been put in force at an early period.—*Med. Times and Gaz.*, April 23, 1859.

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ART. XVI.—*Osseous System, formation of Bone, etc.*

i. *On the development of the Bone-substance in Plasmatic Fibroid Tissue.*

The subject is treated by Dr. C. Ronget in a paper (*Brown-Séguard's Journal de la Physiologie*, Oct., 1858), illustrated by plates, on the



corpuscles of bone and on the development of secondary bones. After alluding to the controversy regarding the supposed method in which the bone-substance is formed where no cartilage previously existed, whether, for example, the calcareous matter be deposited in a continuous membranous blastema of which but certain parts become ossified, or in a alveolar cartilaginous framework—or whether, again, the cellules of the blastema be transformed into osseous corpuscles, or whether the latter be only cavities formed either at the expense of the interstices of the cartilaginous framework or directly by ossification—the author proceeds to detail the results of his own examination of the subject. He states that in the human embryo of ten or twelve weeks, if the fibrous membranes (the periosteum and dura mater) between which the bones of the cranium are developed, be carefully examined, a lamina partly osseous and partly membranous may be obtained, which is continuous throughout the whole extent about to be occupied by the perfectly formed bone. The membranous zone is in direct continuity with the zone in course of ossification, and, like it, is quite distinct from contiguous fibrous structures. In each lamina already four distinct regions may be recognized. Of these, two occupy the extreme limits, the one completely membranous and homogeneous in structure, the other completely osseous. Of the two middle ones, the one contiguous to the osseous region is itself in process of ossification, whilst the other, which is contiguous to the membranous region, presents certain peculiarities, and this zone the author designates the intermediate zone or region. The membranous zone is formed of a fibroid and granular connective tissue, traversed here and there by decussating fibrous bundles, and characterized by innumerable rounded or ovoid plasmatic cellules and free nuclei. In the intermediate zone the same elements exist, but in addition a network of transparent and homogeneous hyaline trabeculæ, which appears to repeat the disposition observed by the inter-crossing bundles of the membranous zone, with which it is manifestly continuous, but by a gradual fusion of the characteristics of the two elements.

Towards the zone in course of ossification the trabeculæ multiply, become thicker, and circumscribe large and small spaces. At the limit of the two regions the trabeculæ of the hyaline framework are continuous with trabeculæ quite analogous as to general disposition, but differing in appearance, being less transparent, and occupied by yellowish granulations and innumerable microscopic cavities; and a decided line of demarcation exists between the extremities of the trabeculæ in course of ossification and the hyaline trabeculæ, although both cavities belong to one and the same system. The web-work which is undergoing ossification intercepts two kinds of meshes distinguishable by their peculiar dimensions. Of these, the one forms veritable areolæ, the other small microscopic cavities, both possessing trabecular walls either hyaline or in course of ossification, and containing connective blastema and cellules. On approaching the perfectly ossified zone and the more obscure and yellowish trabeculæ, the minute cavities in the thickness of the trabeculæ become more and more small, and a hyaline network, which speedily becomes ossified, extends in the areolæ, which it closes or obliterates. Where,

also, the hyaline framework becomes involved, groups of cellules, more or less numerous, and one or two isolated cellules, insinuate themselves in the inter-cellular connective substance. Thus, when the primitive membranous layer has undergone perfect transformation, we have a transparent homogeneous blastema gradually poured out in the granular and fibrous connective substance, and in this blastema subsequently are deposited the elements of ossification, whilst during all these changes the plasmatic cellules of the connective tissue remain intact within the small irregular cavities of the new osseous substance. In this way, in the place of a membranous blastema, whose elements appear only at the period of ossification, it must be admitted that there is a preëxistence, at the spot where the osseous substance is primarily developed, of a fibrous or fibroid membrane, which at first occupies all the space possessed at a later period by osseous formation. True it is that an areolar hyaline framework gradually precedes the calcareous deposit, but there is no reason for looking upon this framework as being cartilage, for it is wanting in cartilage cellule, and the persistence of the prismatic cellules in the midst of this framework shows it to be a simple modification, a return to the primitive form of the connective inter-cellular substance. Throughout the paper reference is made to the late theories of Kölliker and Robin, and most authors who have written upon the formation of bone, as well as to the views of Reichert, Virchow, Donders, etc., as to the original identity or homology of the various plasmatic or connective tissues of cartilage, bone, fibre, medullary tissue, etc.—*Br. and For. Med. Chir. Rev.*

ii. *On the Artificial Production of Bone by means of Displacement and Transplantation of the Periosteum.* By M. L. OLLIER.

The researches of Duharnel, and more recently those of Heine and M. Flourens, have demonstrated the importance of the periosteum for the reparation and reproduction of bone. Our own experience has led to similar results, and we have endeavored by experiment still further to demonstrate its truth. We have dissected up portions of periosteum, and transplanted them in the midst of tissues normally foreign to ossification; and wherever we have engrafted them, new bone has been developed.

We have made three series of experiments. In the first series we dissected long bands of periosteum from the tibia (leaving one end adherent to the bone), which were entwined in various ways around the muscles of the leg. The result was the production of so many circular, spiral, and figure of 8 formations of bone. In a second series we excised the adherent end or pedicle of the band of periosteum three or four days after the operation, and, in spite of this interruption to the primitive circulation of blood, the transplanted periosteum continued to produce bone. In a third series of experiments we completely detached the bands of periosteum from the bone at the time of the first operation, and immediately transplanted them in regions more or less removed—under the skin in the groin, on the back, etc.; and even under such circumstances ossific secretion was obtained, and true bony tissue was the result.

These experiments were made upon rabbits of different ages. Advanced age diminishes but does not destroy this property of the periosteum. The osseous tissue thus obtained possesses the structure of every true bone: the fundamental elements consist of bone corpuscles, in every way similar to normal bone. On the surface is a layer of compact substance studded over with Haversian canals. On the interior is found, at the end of a certain time, a medullary cavity, containing a reddish substance of similar anatomical characteristics, under the microscope, of normal marrow; and one or more foramina are seen to transmit blood vessels. This new bone is formed from the sub-periosteal blastema which is in part carried away attached to this membrane when it is dissected from the bone.

Our experiments demonstrate that bone will be obtained wherever one succeeds in engrafting the periosteum; they prove that a membrane may preserve its essential properties in spite of its removal and transplantation in the midst of foreign tissues; and, as a practical consequence, it extends the field of anaplasty.—*Gazette Médicale de Paris, of Dec. 18, 1858. Nashville Monthly Record.*

iii. Dr. A Mercer Adam, in his recent sketches of the Medical men and institutions of the continent (*Ed. Med. Jour.*), says: The Würzburg museum is large, and contains many rare and unique preparations—both wet and dry—in human and comparative anatomy. It is particularly rich in pathological specimens—thanks to the contributions of Heusinger, Hesselbach, Brunninghausen, and others, but especially to the zeal and industry of Virchow. In it I saw the beautiful series of preparations made by the late Professor Bernard Heine, illustrative of his celebrated experiments on the formation of new bone from periosteum. Here we see the femur of a dog, formed entirely of new bone, which was developed from periosteum eleven months after the extirpation of the original bone; and there is a rib similarly developed seven months after the operation. Heine cut away all the bones of the right fore leg of a dog, leaving the periosteum behind. Fourteen months afterwards a new scapula, with a perfect articular surface, was formed, and similar development of new bone took place, a few months after the resection, at the site of all the other bones. The beautiful dried preparations of these curious results are preserved with great care, and are shown with a justifiable pride.—*Am. Med. Monthly.*

ART. XVII.—*The Enteric Juice. Vena Porta. Chloroform, etc.*

THE following are the results arrived at, after numerous experiments, by Professor Busch, of Bonn: 1. Hunger is constituted by two sensations: the first is represented by the nervous system in general, and derived from the impoverished condition of the tissues; the

second originates with the nerves of the digestive organs, indicating their emptiness. The former is removed only by the required assimilation of nutritive elements, and not by merely filling the first passages. 2. The peristaltic motion of the intestines takes place with the same power within the abdominal cavity as when exposed to the atmospheric air. Its propelling power equals a column of water twenty-four inches high. 3. The alimentary canal has its periods of rest and action. 4. The quantity of enteric juice secreted is invariably small, and of alkaline reaction. Its percentage of solids averages 54.7. 5. Enteric juice is capable of digesting amylaceous and protein substances. 6. Enteric juice converts starch into grape sugar. 7. Enteric juice prepares protein substances for assimilation, under the phenomena of putrescence. 8. Enteric juice leaves cane sugar unchanged. 9. Cane sugar, absorbed as such, is not discharged in the urine. 10. Fat, unless exposed to the action of bile or pancreatic juice, is absorbed, either not at all, or in insignificant quantity. 11. Food appears, between fifteen or thirty minutes after being taken, in the superior third of the thin intestine. Solution of cane sugar disappears in part before entering the small intestine; all that enters the latter is converted into grape sugar. 12. Raw albumen, taken from hen's eggs, is directly absorbed in the stomach and the adjoining portion of the small intestine. All that descends to the lower portion of the latter is unchanged. 13. Gum is not converted into sugar, but remains unchanged. 14. Gelatine is dissolved, and loses thereby its coagulability. 15. Casein remains partly dissolved in the digestive fluids. 16. Fat is entirely emulsified by the digestive fluid when alkaline or neutral, but partially when acid. 17. The digestive liquids of the small intestines possess digestive powers over floating substances. 18. The minimum of all digestive fluids entering the small intestine in the course of twenty-four hours, amounts to more than the seventeenth part of the weight of the body.—*Dublin Hospital Gazette.*

*Circulation of the vena porta in hepatic obstructions.*—M. Sappey endeavors to show the road by which the blood of the vena porta returns to the inferior vena cava, when its circulation through the liver is arrested, as happens in some cases of cirrhosis. According to his researches, the blood is conducted through the small vein which is comprised in the suspensory ligament of the liver, the vein being abnormally enlarged for the purpose. It is this vein which has been mistaken for a persistent umbilical vein. The blood is thus conducted downwards, and by anastomoses passes into the principal vein of the leg—the crural vein. The venous current thus passing downwards towards the leg, is perceptible to the hand by a *frémissement*, and to the ear by a murmur. This symptom, therefore, becomes a sign of cirrhosis, and a favorable sign, because ascites is prevented by the stream of blood which it indicates.—*Gaz. Hebdomadaire.*

*A New Muscle.*—Prof. Budge, in the dissection of the muscles of the human eye, noticed a muscular fasciculus, which, separating from the *M. levator palpebræ*, proceeded inwards, dividing into two portions,

each of which ended in a tendon, and were inserted into the trochlea. Further investigation was made upon thirteen eyes, with the following result: In one case the muscle was absent; in the remaining twelve it was invariably present, although in some cases very thin and attenuated. Of the thirteen eyes, three were from new-born children, and in each the muscle was very evident; six from females, in three of which the muscle was strongly developed, in two was merely perceptible, and in one was absent; four from males, in two very marked, and in the other two consisted of a tendinous cord. To this muscle B. gives the name *tensor trochleæ*, from the necessary action thereof. It is possible that it may have some effect upon the direction of action of the *M. obliquus superior*.—*Med. and Surg. Reporter*.

*Modus Operandi of Chloroform*.—Dr. F. Piossek read before the Physiological Society of Greisswald, an account of experiments with chloroform, made under the direction of Prof. Hunefeld, which seem to establish the following conclusions as to the modus operandi of chloroform, beyond a doubt:

*Chloroform produces anæsthesia by abstracting from the blood some of the oxygen necessary to the continuance of the organic processes, thus causing impaired nutrition of the central organs and nerves; hence the insensibility of the sensory, and the relaxation of the motory nerves.*

The oxygen of the blood probably combines with the carbon (liberated by the decomposition of the chloroform) to form carbonic acid, while the chlorine and water of the chloroform probably form hydrochloric acid, etc. Into what combinations this hydrochloric acid may then enter with the ingredients of the blood, is as yet unknown.

The other anæsthetics, ether, amylene, etc., act similarly, and their modus operandi may be compared to the narcotizing or asphyxiating action of carbonic acid on nitrous oxide.—*Ibid.*

#### ART. XVIII.—*The Blood.*

- i. *On the modifications undergone by Blood-globules of certain Animals injected into the circulation of other Animals, etc.*—By Dr. BROWN-SÉQUARD.

This physiologist has observed, on examining by the microscope the blood of the dog, cat, or rabbit, even a single hour after injection therein of the blood of the bird, that none of the oval large globules are to be met with; and if examined some days, or even several hours, after, none whatever are to be seen in any organ of the body where it might be supposed that they had been arrested. The blood,

however, of such an animal, if examined a quarter of an hour after the injection of the bird's blood will be found to contain oval globules in the veins of all the organs. Thus it appears that all the capillaries of the body are permeable by and do not arrest these large oval globules. But, even for the space of a month after injecting the blood of the dog, rabbit, guinea-pig, etc., into the circulation of the bird, some of the circular discs are to be seen within it when examined by the microscope. In the case of the cock, into whose veins dog's blood has been transfused, on the following day as many round as oval globules are to be seen; but in a few days the circular globules have become much fewer, and three weeks after only two or three are to be seen in proportion to a thousand of the oval ones.—*Br. and For. Med. Chir. Rev.*

- ii. *Researches into the possibility of Recalling to Life Temporarily, in Persons Dying of Disease.* By Dr. BROWN-SÉQUARD. (*Journal de la Physiologie, Oct., 1858.*)

A considerable number of cases of transfusion have satisfied Dr. Brown-Séquard that in mammiferæ dying of various diseases, and especially of peritonitis, life may be restored for some hours by the process, when the agony has all but ended in death; he concludes that in human beings who have already become unconscious, and in whom agony has commenced, the intellectual faculties, the senses, and speech, may be restored for some hours by the combined effect of transfusion, artificial respiration, and bleeding at the jugular. Dr. Brown-Séquard does not give us the result of his entire experience, but he finds that of eleven experiments made upon dogs, cats, and full grown rabbits, four animals came to life entirely for two, three, and four hours, that three others recovered the circulation, respiration, and reflex action for one or two hours without restoration of the voluntary movements or sensibility, while in the four remaining animals there was no result, except a slight increase of the movements of the heart. The following is a brief summary of one experiment, which the author gives in detail:

In October, 1851, a dog in whom the abdominal sympathetic had been divided was attacked with peritonitis and death was at hand. Voluntary and reflex movements had entirely ceased: there was no respiratory action, and the convulsions of death were limited to a few muscular tremors; the fæces and urine were expelled, the pupil was dilated, and no movement of the heart was perceptible. A silver T-shaped tube was now inserted into the left carotid of the dying dog, and connected with the carotid of another dog, which was attached firmly to a table. The healthy arterial blood at once passed to the head and heart of the dying animal. At the same time, the left jugular vein and one of the femoral veins of the dying dog were opened. The jugular almost immediately, and the femoral after twenty or thirty seconds, yielded some blood. The period of transfusion lasted two minutes, and ligatures were placed on the carotids of both animals. The jugular vein was left open four or five minutes, during which the heart was felt beating. Gradually the pulse was

restored as the blood flowed from the jugular. Artificial respiration was then had recourse to, and continued for half an hour. Eight minutes from the commencement of the insufflation, the cornea became sensitive, and soon after, respiratory movements were perceptible. After twenty minutes, the animal executed voluntary movements, and on leaving off insufflation, respiration continued regularly. The return to life was complete as regards all the chief functions of animal and organic life. Though feeble, the animal raised himself on his fore-feet and wagged his tail when caressed. The dog lived eleven hours and a half after the transfusion.—*Ibid.*

iii. *On certain peculiarities in form and size exhibited by the Red Globules of Blood in Embryos.* By Dr. C. ROBIN. (Dr. Brown-Séquard's *Journal de la Physiologie*, Jan., 1858.)

According to this author, the red globules in the embryo of man, from the time of their appearance to the period when the body has attained the size of twenty-five millimetres or more, possess a diameter for the most part of eleven thousandths of a millimetre, their thickness being from three to four thousandths, unless when rounded by contact with water. In the embryo of three millimetres in size, a few are only eight thousandths of a millimetre, the most being from eleven to thirteen thousandths. In an embryo of twenty-five millimetres long, some of the globules were from seven to eight thousandths, and some from fourteen to sixteen thousandths long and three thousandths in thickness. In an embryo of nineteen millimetres long, in addition to red globules of the above size, there were a number sixteen or seventeen thousandths in size, with a thickness of five thousandths. In embryos from three to twenty-five or thirty millimetres long, most of the globules are discoid and biconcave, and often thicker at one point than another; but some are spherical, and some are ovoid but not flattened, measuring seven, eight, or ten thousandths of a millimetre long by from twelve to fifteen thousandths. Some have a wallet shape, with a contraction towards the centre, and at this age the red globules are remarkable for the facility with which they may be put out of shape by reciprocal pressure, or when compressed in small capillaries, etc.; but they are very elastic, speedily assuming their natural shape. The above particulars hold good in all domestic mammalia and rats.

In very young embryos the red globules change very quickly after death, from being discs, becoming in many cases hemispherical, or spherical, then softening, becoming diffuent, and running one into the other; some also become indented and plicated irregularly, the circumference of the globule becoming generally irregular.

The author proceeds to speak of the nuclei of embryonic blood-globules. In almost but not quite all embryos, from the time that the globules are first found to that when the embryo attains twenty-five or thirty millimetres in size, these globules contain nuclei; but about that period nearly half of the globules are deficient in any nucleus, and the number of these goes on increasing. At the fourth month of intra-uterine life occasional globules contain nuclei, but

after this time none are to be seen containing them. The globules which towards the fourth month retain a nucleus are from one to four millimetres longer than surrounding globules. As to the nucleus itself, it is spherical, of three or four thousandths of a millimetre in size, greyish, insoluble in water, as also in acetic acid, which contracts it slightly, and renders its edges clear and its centre brighter. It is finely granular, and without any nucleoli, and pretty often situated at a point slightly removed from the centre. When placed centrally, the nucleus masks the central depression of the discs, which, when the nucleus is on one side, is well seen.

The embryonic blood-globules which possess nuclei contain, especially in the herbivora, one, or rarely two, very small fatty granulations, often disappearing after distension with water.—*Ibid.*

#### iv. *Comparison of Menstrual and other Blood Spots.*

M. Robin terminates a memoir upon this subject with the following conclusions: 1. Menstrual blood differs under the microscope from blood drawn from the vessels by the mixture with the sanguineous globules of epithelial cells and leucocytes termed mucous globules; the former proceeding from the epithelium of the utero-vaginal mucous membranes, and the latter from the surface of these mucous membranes. 2. The spots produced by menstrual blood contain elements not found in those caused by ordinary blood, and which are held in suspension by the mucus of the genital passages carried away by the blood, consisting principally in epithelial cells and in the leucocytes of mucus. 3. By a microscopical comparison these two descriptions of blood can therefore be distinguished.—*Annales d'Hygiène.*

#### v. *Blood Poison.*

Professor Kölliker, of Würzburg, maintains that we have no true blood poison—meaning thereby one which would so change the mutual physiological relations of the elements of the vital fluid as to render its circulation positively injurious; but he holds that all poisons act on the parts which they affect through the medium of the blood. Some poisons he regards as purely muscular, and others as purely nerveine, in their elective affinities for specific tissues. The upas-tree poison, and perhaps veratria, are the only true muscular poisons. Nerveine poisons, however, are divided by Kölliker into three classes, according to the mode in which they operate on the nerves: 1st, those which act on the gray matter, as strychnia, veratria, and opium; 2d, those which change the nerve-tubes, as curari and conein; and 3d, those which probably act in both ways, as prussic acid, nicotin, and æther. He adds the corollary that there are likely excitant and paralyzing agents in all the three groups. Such are a few of the results of Kölliker's important physiological experiments in toxicology; and believing them to be comparatively unknown in England, I have stated them at some length. On Kölliker's other recent researches—as, for example, on the absorption of



fat by the glands of the alimentary canal, on the existence of a physiological fat-liver in the young of mammalia, and on the functions of the spleen—I have no space to enlarge. They seem to confirm satisfactorily the opinion now generally entertained of the use of the spleen, viz.: that it is a blood-forming organ, which develops colorless corpuscles, and that these blood-cells (at least in young animals) became transformed into red globules, partly in the spleen, partly in the liver, and probably also in the current of the general circulation.—*Am. Med. Monthly.*

ART. XIX.—*Abscess of the Brain.* (New York Pathological Society.)

DR. GEORGE F. SHRADY presented a specimen of extensive abscess of the brain. It was removed from the body of one of the victims of the Thirtieth-street tragedy, who died at the New York Hospital on the 14th of November. The history of the case was as follows: Elizabeth C——, æt. 23, native of Ireland, was admitted to the New York Hospital October 26th, during the attendance of Dr. Markoe, with scalp wounds, which were inflicted by a hatchet. Two of these wounds were situated just to the left of the median line, near the vertex, longitudinal in their direction, about half an inch from each other. The third was situated posteriorly on the right side, over the occipital bone, semicircular in form, and caused by a glancing blow of the weapon. They all extended to the bone. The skull itself did not seem to be injured materially. Several small pieces of the external table were chipped off in the posterior wound, and at the bottom of the wound nearest to the vertex there was a small groove in the bone about  $1\frac{1}{2}$  inch in length, evidently caused by a direct blow upon the part. When first seen, she was considerably prostrated, both from the shock of the injuries received, and the loss of a considerable amount of blood; though there was no cerebral disturbance present. Reaction soon came on, and everything progressed favorably. She continued to do well, the wounds granulating nicely, until the 3d of November, eight days after the injury, when she complained of severe pain in the side of the head, in the region of the wound, attended with a great deal of febrile excitement, dilatation of the pupils, and the occurrence of hemiplegia of the right side. From that time she began to be stupid; her bowels were freely opened, leeches were applied round the wound, a blister to the back of the neck, but without any good effect. Coma supervened on the third day after, when an operation was attempted for her relief by Dr. Parker, who was at that time attending surgeon. During the night previous to the day of the operation the patient had four general convulsions, which were quite severe.

Taking these symptoms into consideration, it was thought possible

that a portion of the internal table of the skull was depressed at the situation of the groove on the outside. Dr. Parker trephined over this spot, removed a button of bone, and found a small spicula of the vitreous table splintered off at a point corresponding with the external injury. The dura mater was diseased at that point, thickened, softened, and was unavoidably wounded by the trephine. A portion of this membrane, about as large as a twenty-five cent piece, was cut away. The brain substance immediately underneath did not seem to be injured, and presented no marks of any abnormal action except a considerable discoloration of the part.

The wound was left open, and dressed with cold water. The next day after the operation her mental functions were sufficiently restored to enable her to answer some questions intelligibly, but the appearance of her pupils was unaltered. On the 8th, three days after the operation, a small fungus, about the size of the end of the finger, made its appearance, projecting from the centre of the wound. Still, she kept on improving, complaining only of an occasional pain in the head, of which she was always alleviated by the application of leeches. The pupils still kept moderately dilated. Her pulse, during all the time, ranged between 90 and 100, was moderately full, but compressible.

On the morning of the 11th, six days after the operation, she was seized with another general convulsion, making the fifth. It was by no means so severe as the former ones, and lasted but five minutes. She continued in this state with a good deal of intelligence, able to answer questions promptly, pulse quiet and full, until the night of the 12th, seven days after the operation, when rather suddenly coma again came on; her pupils became again widely dilated, pulse 92, and thready. Leeches were again applied, and blisters placed on the back of the neck, but no abatement of the symptoms followed their use. During the day the fungus, which had increased none since its formation, was noticed to shrink away into the cavity of the skull, leaving nothing but the thickened scab which formerly covered it.

The coma grew gradually more profound during the following night, the pulse growing more and more feeble until 11, A. M., of the 14th, when she died. Early on the morning of her death, on removing the wet cloth from the wound, it was found to be smeared over with brain-matter; this discharge continued until death took place, and amounted in all to a small teacupfull.

The *post mortem* examination was made four hours after death. The whole superior surface of the dura mater was thickened, and at the situation of the hole in the skull there was an opening into it nearly as large as a half dollar, nearly circular in shape, with ragged edges. This opening discharged through it a considerable quantity of broken down brain-substance, mixed with pus of a pinkish hue, which formed the contents of an immense abscess of the brain, occupying fully two-thirds of the whole substance of the left hemisphere. The cortical portion of the organ was destroyed for a considerable space around the wound in the dura mater. The cavity of the left ventricle was found to be entirely obliterated; the *corpus striatum* and *optic*

*thalamus* were almost entirely destroyed by the diseased action. The septum lucidum was partially destroyed. The corpus callosum was very much softened. The right hemisphere was very slightly affected, the softening being very superficial, and limited to the superior margin of the longitudinal fissure. The walls of the right ventricle were somewhat broken down, and the cavity was to a certain extent filled with the same material. Besides this, there was a small deposit of flaky pus around the optic chiasm, in the fold of the longitudinal fissure.

I omitted to state that, at the time of operation, there was a very small quantity of pus seen to flow from the wound in the skull.

Dr. Detmold, in this connection, referred to a case of an extensive abscess of the brain-substance, about half an inch below its surface, evacuated by a free incision. When he first saw the case the patient was comatose, the pulse 40, breathing stertorous, etc. He removed a portion of the bone, and found the dura mater perfectly healthy, but from the nature of the case, being under the firm conviction that an abscess did exist in the brain-substance, boldly cut through, and evacuated it. While the pus was flowing the patient recovered consciousness. The quantity of pus discharged amounted to five or six ounces. After that time the patient got about for a couple of months, and with the exception of his memory, which he entirely lost, he did very well. His reasoning powers seemed to be unchanged. He lived sufficiently long to allow the wound to heal up, with the exception of a small opening, through which a probe could be introduced for its whole length into the lateral ventricle. At the end of a couple of months trouble in the brain again made its appearance, pus formed in the ventricle, and was evacuated; but he died soon after. It was found that the septum lucidum had ruptured, and discharged some of the contents of the diseased ventricle into the one on the opposite side.

Dr. Van Buren, in this connection, referred to an analogous case of another one of the victims of that tragedy. The wound was about an inch and a half to the left of the mesial line on the top of the head. It presented a solitary fissure, which penetrated the skull, and resembled very much the appearance of a chop in a log by a single blow with a broad-axe. It was impossible to bring the edges into any correct apposition. A probe being introduced into the wound, went into the substance of the brain. I was satisfied that the dura mater was injured, and went no farther. The patient had no concussion, and no paralysis whatever. Under the circumstances I thought it was best to close up the wound, in hopes that the opening in the dura mater might also be obliterated. This was accordingly done by sutures. The patient did well for thirteen days; then he was seized with convulsions, after which he was thoroughly paralyzed upon the opposite side. With a pair of cutting forceps the cranial bones at the seat of the injury were carefully gnawed away. It was then discovered that some of the fragments of the internal table were detached by the force of the blow. These spiculæ were found to be adherent to the dura mater, and when detached pus was seen to flow. The child looks very much at present as if he was going to

get well. It is now fourteen days since the operation. The convulsions have never recurred, the paralysis is going off, and the child is improving in every way. There could be noticed no obvious perforation of the dura mater at the time of the operation. After the operation everything went on very slowly, and in the meantime he began to be excessively pallid. This state of things had been noticed by Rokitansky, who supposed it to be due to a lack of power to elaborate the blood corpuscles.

He stated that four out of six of these victims were paralyzed on the opposite side of the injury—the child, the father, and the young woman taken to the hospital. The other two were not paralyzed; one of them was injured but very slightly, the other very severely. The mother had only two superficial scalp wounds, and one deeper one, injuring the squamous portion of the temporal bone. He hoped that the internal table was not injured. The eldest son was very severely injured. It was the most extensive and formidable one of the six, and yet not a bad symptom has shown itself since the injury, and is now on a fair way to recovery.

Dr. W. Bibbins cited a case of a little child who had fracture of the skull, caused by a spade falling a considerable distance, and sinking upon the top of the head. The wound was quite an extensive one, though the dura mater was found to be uninjured. The wound was left open, and the child went on perfectly well, and recovered without a bad symptom.

Dr. W. Parker thought that surgeons strove altogether too much for union by first intention in these cases. He could never succeed to his satisfaction. Upon the whole, he preferred nature's second intention to her first—it always seemed to turn out better.

Dr. Batchelder stated that if the dura mater was injured, and the scalp could be induced to close up by first intention, the hole in the dura mater would close itself up, and fungus by that means would be prevented.

Dr. Krakowitzer stated that Dr. Isaacs examined a case of fungus cerebri, where the matter protruded was essentially brain-matter; that is to say, he found under the microscope the peculiar granules, cells, and nerve-fibres. Dr. Krakowitzer thought if the dura mater was extensively lacerated, so that there was a large opening into it, fungus cerebri would not occur. He had trephined a few months previous, where he removed a square inch of the dura mater, yet no fungus made its appearance. In another case, where the dura mater was wounded, he closed it over with the flaps, and at the end of five days after he found that a considerable portion of the external table was necrosed. He thought if the wound had been left open this would not have taken place.

Dr. Parker referred to a very interesting paper upon this subject by Dr. Gurdon Buck. In all those cases the fungus was found to be brain-matter. He stated that Abernethy's idea was that these growths were of a morbid character, and sprung from some portion of the brain. Dr. Parker believed that a portion of the brain became softened by inflammatory action, and as a consequence was protruded through the opening in the dura mater by the *vis a tergo* of the heart.

Dr. Krakowitzer thought that the fungus was a new growth, and that Dr. Isaacs's case was a very rare exception.

Dr. Peaslee thought that those cases where the brain-substance itself is protruded must be very rare. He thought that they were made up of granulations, and were a new formation.

Dr. Clark stated that he had examined, in two of Dr. Buck's cases, portions of the fungus under the microscope, and was positive that there was not a particle of brain-matter in them. He had examined in all about five cases, and in neither did he find the least trace of any brain-matter. He thought that the production of new brain-matter was an impossibility. He did not believe that any microscopical observer had ever found it. Instances, he said, had occurred where a considerable amount of this material had been found, and where the cavity had been filled with serous fluid, the patient recovering. He stated that there was not the slightest shadow of evidence that went to show that brain-matter once destroyed could ever be reproduced.—*Am. Med. Monthly.*

#### ART. XX.—*Treatment of Fractures and Bed Sores.*

##### i. Chassaignac's Mode of Treating Fractures. (Middlesex Hospital.)

There have been several cases of fracture lately in this hospital, which have been treated according to the plan recommended by Chassaignac of Paris, which consists in applying plaster-of-Paris with a bandage around the limb, immediately after the occurrence of the injury. This becomes quite hard, and forms a thick case or envelope, thus keeping the part perfectly at rest. The next day, chloroform having been given to the patient, a portion of this thick coating is cut away, by means of proper scissors, at a situation completely surrounding the seat of the fracture. The limb is then carefully adjusted by means of proper traction and suitable extension, and a fresh piece of bandage is applied with plaster-of-Paris, as before, between the two cut ends, which keeps the limb quiet and firm. If the fracture is in the middle of the thigh, as we noticed in a female on the 3d instant, a Liston's splint is likewise applied in the usual way. Whether Chassaignac's plan has any advantage over the ordinary starched or gummed bandage in use at some of our hospitals we are not prepared to say. The readjustment of the fracture, however, under chloroform, on the second day, is a point which might be considered as of importance by some surgeons. In the same ward in which is the case of fractured thigh, is another female patient, thirty-two years of age, who fractured the neck of the right femur, external to the capsule, six weeks ago. It was put up in the manner just described, and was examined a fortnight back, but there was no union of the broken bone. It was moved about a good deal under chloro-

form, and readjusted, and ends of the bone now become knit. Splints are not dispensed with in any case in which the plaster-of-Paris bandaging is employed.—*Lancet*.

ii. *Treatment of Bed Sores.*

M. Leclerc, physician to the Hôtel Dieu, of Laon (France), recommends tannate of lead to prevent bed-sores. He prepares it in the following manner: Oak bark, one ounce; water, eight ounces; boil down to four ounces, strain, and add liquor of the diacetate of lead in sufficient quantity until no more precipitate is thrown down. Collect the latter and spread a thick coating of it with the finger on the parts threatened with gangrene, the whole to be covered with a fine piece of linen. No eschar forms with this application, except in rare cases, when the wound, on the falling off of the eschar, is to be dressed with the same tannate of lead, to which turpentine may be added.—*Ib.*

ART. XXI.—*Presentations of the Fœtus.—Use of the Speculum.—Ergot.*

i. *The Relative Frequency of the Various Presentations of the Fœtus.* By WILLIAM C. ROGERS, M. D.

MESSRS. EDITORS—Having been engaged for a number of months past in collecting Midwifery Statistics for a special purpose, I send you one of the results of my labors, in the following Table of the Relative Frequency of the Various Presentations of the Fœtus:

Whole number of presentations.....	88,342
Head, (including "face to pubis," etc.).....	85,210
Breech.....	1,754
Feet and knees.....	445
Funis, (the accompanying presentation not given).....	219
Arm.....	83
"    and head.....	38
"    "    and funis.....	9
Head and funis.....	57
Placenta.....	25
Face.....	299
Shoulder.....	69
Transverse.....	120
Sacrum.....	1
Back.....	6
Belly.....	6
Forehead.....	1

I have gathered these figures from the standard and periodical

literature of the profession, and from my professional friends and correspondents. I regret that I did not note the number of twin and triplet cases, and other facts worthy of notice.—*Am. Med. Monthly.*

GREEN ISLAND, Albany Co., N. Y., March 17, 1859.

ii. *On the Use of the Speculum in Uterine Diseases* By Dr. ROBERT LEE, Obstetric Physician to St. George's Hospital.

[In the last volume of 'Medico-Chirurgical Transactions' Dr. Lee published the details of eighty cases, in which the speculum and caustic had been employed by other practitioners, which, with 220 cases previously published, make up 300. Upon the use of this instrument he remarks:] Of these 300 patients, 47 were unmarried, one had barely completed her eighteenth year; there were several under twenty, and the greater number under thirty years of age, and were suffering from hysteria, leucorrhœa, dysmenorrhœa, or some nervous affection of the uterus, without inflammation, ulceration, or any structural disease or displacement of the organ. In one case the patient had been informed that the womb was prolapsed and much ulcerated, and some instrument had been introduced daily for six weeks, by a physician extensively engaged in the treatment of uterine diseases, and great expense incurred, with an aggravation of all the symptoms. In this case I found the hymen so perfect that it was impossible to reach the os uteri without employing an unjustifiable degree of violence. On the ground of morality, and on every other ground, the employment of the speculum in these 47 cases could admit of no defence.

Of these 300 patients 70 were barren, and the sterility was not removed, nor the hysteria, leucorrhœa, or disordered menstruation, under which the greater number were laboring, in a single instance relieved, or any benefit obtained. The injurious effects of a long course of speculum and caustic treatment upon the moral feelings and character of several of these individuals were not attempted to be concealed, the treatment being spoken of with horror and shame.

Of these 300 patients there were a considerable number suffering from cancerous diseases of the uterus, in all of which the symptoms were increased by the introduction of the speculum, and the application of caustic or the actual cautery to the ulcerated vagina, and os and cervix uteri. In one case, though the carcinomatous ulceration was in an advanced stage, and the nature of the disease obvious to the most inexperienced, after an examination with the speculum, a false prognosis was given, and iron, heated to a white heat in fires of coke, was for months passed through the tube, and delusive hopes of recovery held out to the last, and the pecuniary concerns of the husband involved in irretrievable ruin by the charges, medical and surgical, incurred by such unscientific, unprofessional, and unprincipled proceedings.

Neither in the living nor the dead body have I ever seen a case of simple ulceration from chronic inflammation of the os or cervix uteri, and to apply the term ulceration to states of the os uteri in which the mucous membrane, or, as it is termed by some, the basement membrane, is not destroyed by ulceration, is an abuse of language calcu-

lated only to deceive and mislead the members of the medical profession, from whom the truth has been carefully concealed. The speculum emanated from the syphilitic wards of the hospitals of Paris, and it would have been better for the women of England had its use been confined to those institutions.—*Med. Chir. Trans.*, Vol. XL., p. 201.—*Braithwaite*.

iii. *Detection of Pregnancy by Ergot.*

MESSRS. EDITORS—I do not recollect of ever having seen the use of ergot recommended for the purpose of detecting pregnancy in its earlier stages. For many years I have been in the habit of administering small doses of this drug for this purpose, and in my hands it has seldom failed of furnishing the evidence sought. The specific action of the medicine is not felt by an unimpregnated womb, while the gravid uterus, I believe, almost invariably responds to its action by some uneasiness in the back, but more particularly by pain in the upper part of the thighs, sufficiently to enable you to diagnosticate the case with great certainty. I have in many doubtful cases trusted to this test, and have very seldom been disappointed in my diagnosis. I will only add that the ergot can be given with entire safety in sufficient quantity to accomplish the object sought. If the use of the secale for this purpose is new to the profession, as I believe it is, and you consider these remarks worthy a place in your Journal, you will please insert them, and oblige  
Your ob't serv't, w. w. c.

MIDDLEBORO', March 29, 1859.

[*Boston Med. and Surg. Jour.*

ART. XXII.—*Physiology and Treatment of Placenta Prævia.*

At a meeting of the Obstetrical Society of London (April 6, 1859), a paper was read on this subject by ROBERT BARNES, M. D., which elicited the discussion which follows, as reported in the *Med. Times and Gazette*:

*Some recent cases (fourteen) Illustrating the Physiology and Treatment of Placenta Prævia.*—The author sought to avail himself of the foundation of the Obstetrical Society of London to elicit the experience of its members as to the different principles of treatment of placenta prævia. He submitted fourteen cases which had come under his own care since the publication of his work on the subject; and appended two series of propositions—the one physiological, the other therapeutical—which appeared to be either proved or illustrated by those cases.



Among the physiological propositions were the following: That in many cases of placental presentation, there arrives a stage when the hæmorrhage is spontaneously arrested. That this physiological arrest is not owing to pressure upon the bared surface of the uterus by the bag of liquor amnii, or the child; nor to death of the child; nor to syncope; nor to total detachment of the placenta. That this physiological arrest of the hæmorrhage is observed when that part of the placenta which had been implanted within the cervical or lower zone of the uterus has been all detached, contraction of the uterus attending. That, this stage reached, there is no physiological or pathological reason why further detachment of placenta seated within the middle and fundal zones should occur until after the expulsion of the child, when—and not till then—the remainder of the placenta is cast off as in normal labor. That the position of the greater portion of the placenta to the posterior wall of the uterus in these cases, where it forms, by resting on the projecting promontory of the sacrum, a solid inclined plane, directed forwards, is a frequent cause of the transverse presentations which are apt to complicate placenta prævia. That in the great majority of cases where an edge of the placenta comes down to the os internum uteri, the umbilical cord springs from this edge, and thus is ready to fall through into the vagina, should the os not be occluded by the child's head.

Amongst the therapeutical propositions were the following: That owing to the high vascularity and development of the lower segment of the uterus, resulting from this part being the seat of the placenta, uterine inflammation and puerperal fever are exceedingly likely to ensue from the pressure and contusion attending the passage of the child. That this danger is much increased by the forcible introduction of the hand for the purpose of turning and extracting the child before the os uteri has expanded. That in some cases, where it is observed that the placenta has been separated spontaneously from the lower segment of the uterus, the os being expanded to the size of a crown-piece, and the hæmorrhage having ceased, it is not necessary to interfere with a labor now become natural *quoad* placental attachment. That since the os internum uteri must expand to the diameter of the child's head, and since, during the dilatation, placenta adhering to the lower segment is liable to successive detachment, causing hæmorrhages, it is an indication to expedite this stage of the labor as much as possible. That in some cases the ordinary means of inducing contraction—such as rupturing the membranes, plugging the cervix, ergot or galvanism—will suffice to cause the rapid and safe expansion of the os. That the adhesion of placenta to the lower zone of the uterus impedes the regular progress of labor, and delays the equable expansion of the os uteri. That in those critical cases where forced delivery or the artificial total detachment of the placenta are dangerous or impracticable operations, the introduction of the index finger through the os, and the separation of the part of the placenta adhering to the cervical zone, is a safe and feasible operation.

Dr. Rigby would ask, was Levret's opinion as to the connexion between partial implantation of the placenta over the cervix and

prolapsus of the funis borne out by Dr. Barnes's experience? He would make one remark on a point which had, he believed, escaped notice. In cases of placenta prævia there now and then occurred danger from slight but continuous hæmorrhage after delivery, in consequence of a small rent in the os, which at any other time would be of no consequence, but was of importance in these cases, owing to the extreme vascularity of the cervix.

Mr. Borham had found preternatural position of the child in five out of seven or eight cases of placenta prævia. He was at a loss to explain this. Was it connected with undue shortness of the cord?

Dr. Elkington believed no universal law could be laid down for treatment of these cases. When the placenta was partially implanted over the cervix, and the position of the child natural, he believed that rupture of the membranes and the use of ergot were sufficient. If the implantation was complete, the patient exhausted from loss of blood, etc., the proper treatment was to turn. His success had been greatest when turning had been performed early. He had always found the os easily dilatable when much hæmorrhage had occurred, even though only as large as half a crown. He could not consider that the patient was secure from further hæmorrhage after the placenta had been separated to a certain degree. He would be disposed to deliver as soon as possible.

Dr. Barnes, in reply, stated that in two of the cases of marginal presentation the cord was prolapsed. The cause of the preternatural position of the child in cases of placenta prævia was, as Levret long ago pointed out, the placenta prævia itself. He believed that in cross presentations generally, also, it would be found that the placenta was attached low down, and that this gave rise to the malposition of the child. He had found the os not dilatable in cases where much blood had been lost, although undoubtedly the opposite was the rule. He would submit that the "insecurity" to which Dr. Elkington had referred as hanging over a woman when the hæmorrhage had spontaneously ceased, and the consequent necessity for further proceedings was a feeling having its seat only in the mind of the accoucheur. He hoped to convert Dr. Elkington to a belief in the physiological views enunciated in the paper.

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## MISCELLANEA.

### ART. I.—*Suicide by Chloroform Suspected.*

[It is probable that at no distant day suicides, infanticides, etc., will be frequently effected by anæsthetics, instead of the pistol, the rope, strangling, throat-cutting, stabbing, poisoning and the like. A pain-

less death (euthanasia), as well as painless surgery, is, it may be assumed, often a desideratum in suicide, in child murder, etc. Should this mode of death be practised, the coroner and jury of inquest will, at the very onset, frequently need a large stock of knowledge in anatomy, pathology, and analytic chemistry to determine the cause of death or the *corpus delicti*. In courts of criminal jurisprudence upon the final trial, for example, in anæsthetic homicide, infanticide, etc., the scientific or other evidence must be altogether conclusive to produce conviction. A new chapter awaits the Forensic Medicine in toxicology and pathological anatomy.

The following history, copied from the *Medical Times and Gazette* (April 16, 1859), possesses a melancholy interest independently of the probable suicide of an eminent medical gentleman.—B. D.]

It is singular that one of the first instances of suicide—or suspected suicide—by chloroform, should have to be recorded in giving some account of the death of a member of our profession who was one of the first to investigate the properties of this anæsthetic, and to analyze cases of death attributed to its influence. Dr. R. M. Glover, late of Newcastle, more recently attached to the Civil Medical Staff in the East, and a Physician to the Royal Free Hospital, has just died, and the evidence brought forward at the inquest shows that his death was caused by the agent whose effects he had studied so closely. Dr. Glover was a fellow of the Royal Society of Edinburgh, a Doctor of Medicine of the University of that city of 1840; and was well known as a Chemist at Newcastle-on-Tyne, before he came to London. He was the author of a “Manual of Chemistry,” and numerous scientific papers, which proved him to be a man of remarkable ability. He is said to have suffered from chronic dysentery, which led him to take opium and stimulants in considerable quantities. About a month ago he married, and very soon afterwards his wife became insane, and was removed to the asylum at Hanwell, where she still remains. Mental distress, bodily suffering, and pecuniary difficulties seem to have led to a renewed recourse to stimulants and narcotics, and chloroform or chloric ether was taken. Several successive doses of the latter, taken at short intervals, lead to coma and death; and the jury came to the conclusion that it was not taken with the intention of committing suicide, but of obtaining a temporary oblivion of suffering. It would not be difficult to moralise upon this sad tale of the life and death of a man of genius; but it would be too painful to follow the old maxim, *De mortuis nil nisi verum*, and we must recur to the more common one, *De mortuis nil nisi bonum*.

ART. II.—*Æsthetics of a Suicide.*

[THE *British Med. Jour.* (April 23, 1859), gives the following statement concerning a recent suicide.]

The anatomy of suicide is truly an interesting subject. The motives that tempt men to shuffle off the mortal coil, are as diverse as are the motives and passions of men. The manner of making the final exit partakes also of the same diversity. But it is not often that we find the poor mortal, thus hurrying himself into the presence of his Maker, is anxious about picturesque effect with regard to the scene of his desired quietus. The mind of man is, however, wondrously fashioned, as we see by the inquest held on the body of a Spanish gentleman, at the Bear Hotel, Woodstock, on Friday the 15th inst. The unfortunate man arrived at the above hotel on the 12th inst.; he employed himself upon his arrival in viewing the grounds and palace of Blenheim, and especially in admiring the pictures, one of which, in a letter to the Duke of Marlborough, while apologising for the act of desecrating his park, he has still time and taste to compliment him upon, as being the finest Rubens that can be seen. Fresh from this act of connoisseurship, and after carefully inquiring as to "the most forest-like looking part of the park," he went out into that noble domain, and shot himself. The letter he left behind him is perhaps the most extraordinary epistle ever written upon the verge of eternity; it is addressed to the Duke, and begins as follows:

"WOODSTOCK, April 14th.

"*My Lord*: I humbly ask your Lordship's pardon and forgiveness for the great liberty I have taken in coming to put an end to my dreary and miserable existence in your park. It may be a childish feeling; but one cannot blow his brains out in a common road, or one of those cultivated fields full of cottages, and life, and civilization, and railways, and establishments of all kinds, of which your blessed country of England abound. So I have not found another proper place to die decently than your handsome park, and you must bear the inconvenience of a dead man in your grounds. I mean no offence."

There is something so comical in this apologetic pleading for having inconvenienced the lordly owner by the presence of his "unhandsome corpse" in his grounds, that, but for the grimness of the subject, it almost moves one to mirth. Nevertheless, with all the ultra romance of the act, there is a touch of nature in it worthy of notice, especially to the artistic mind. Such a mind he evidently possessed. Some men will deliberately lay their head on the rail on which a train is advancing—these are of the heedless race who throw their lives away without more ado; others, such as our poor Spanish gentleman, even lay nature under contributions to adorn their final exit. Such is life.

ART. III.—*Nicotiana*.

DR. JOHN LE CONTE, who contributed a paper, entitled *Observations on the Species of Nicotiana* (published in the *Proceedings of the Academy of Natural Sciences of Philadelphia*, for March 29, 1859), gives a preliminary history, derived from the ancient classics, concerning plants more or less analogous to tobacco. Passing over the history of the latter, both ancient and modern, it is proper to say that Mr. Le Conte advocates its use as both "harmless and pleasant," and denounces "its detractors and persecutors," including fanatical kings, theologians, doctors, etc.

Mr. Le Conte says : The fanatics who first colonized New England, and who wished to make mere animated statues of their fellow-creatures, could not resist the opportunity of putting a restraint upon this innocent enjoyment. They therefore ordered 'that no man shall take any tobacco publicly in the street, highway, or in any barn-yards, or upon training days, or in any open places,' under the penalty of six-pence for each offence. I might fill pages with similar relations, and with accounts of attacks made upon this favorite weed, but I forbear.

I have used tobacco for more than sixty years without perceiving any ill effects produced by it. I was once induced to abandon it for about six months, but this disuse brought on numerous and painful ulcers of the tongue, which promptly vanished on resuming its use.

I have never observed it to have any exciting effects on the body or mind, but, on the contrary, its action appears to be entirely soothing and sedative. Let a person, overwhelmed with fatigue of body and mind, set himself down in an easy posture, light his cigar, or pipe, and cease to think ; by the time his *fumate* is burnt out, he will find himself entirely relieved from his fatigue, with mind refreshed and body strengthened. Drs. Pereira and Christison say they have never known any well ascertained ill effects having been produced by the habitual practice of smoking. \* \* \* \*

NICOTIANA TABACUM. Annual, viscid, branching. Leaves oblong lanceolate, broad, acuminate, most entire, for the most part strictly sessile, at the base more or less decurrent, subamplexicaul. Flowers paniculately corymbose, terminal, with linear lanceolate bractes. Calyx oblong, five-cleft, the divisions lanceolate acute. Corolla infundibuliform, much longer than the calyx, the tube viscid, greenish, the limb pale rosy, spreading, the lobes ovate acute, capsule a little longer than the calyx, stigma transversely sulcate on the top.

This is the common tobacco of commerce, called by different names, Virginian, Kentucky, Nagadoches, etc. It is not agreeable to smoke, unless weakened by washing in water. It is the only kind fit for chewing. Too much care cannot be taken in the operation of curing it, and much of its goodness depends upon the manner in which it has been dried and fermented. The Indians in this country are in the habit of mixing it with the leaves of *Rhus glabrum* and *Laurus Bor-*

*bonia*, or the scraped bark of *Cornus sanguinea*, all of which improve its taste in a remarkable degree.

*N. FRUTICOSA*. Perennial, pubescent, viscid, branching. Leaves, lanceolate, acuminate, most entire, sometimes very shortly petiolate, most generally sessile, the lower ones amplexical. The inflorescence the same as of the preceding, stigma subbilobate.

This is the far-famed tobacco of the Island of Cuba and of all the tropical parts of America. I have been told that it is the species cultivated in the interior of Africa. It is probably indigenous to that portion of the globe. An African, from a region in that country far distant from the sea, who was well acquainted with the cultivation of the plant in his own country, as well as in this, told me when he first saw this species growing in Georgia, that it was the kind which grew in his country. He could not well be mistaken, for the leaves of *N. tabacum* are very wide, whilst, on the contrary, of the *N. fruticosa*, they are rather narrow. It is from this species that the so-called Havana cigars ought to be made. But it seems to me that very little of it enters into the composition of what we now receive from Cuba.

*N. RUSTICA*. Annual, villous, viscid, branching. Leaves petiolate, ovate or roundish obtuse most entire, sometimes more or less cordate, divisions of the calyx short, ovate or roundish. Corolla greenish yellow. Stigma entire.

From this species, which is nearly as agreeable for smoking as the last, are produced the varieties called Turkish, Chinese, East Indian, Shirazian and Latakia tobacco. It is said to have been imported from America into England in the year 1578, and yet has never been seen here except cultivated as a curiosity in gardens, the seed being always brought from China. It must be considered as a species confined originally to the older continents and not known on this side of the Atlantic until after the discoveries of Columbus and others. I think that I have seen it growing in a quasi indigenous state on the road sides in Europe. No tobacco has ever been seen growing without cultivation in the United States. This circumstance taking place with most of our cultivated vegetables is a certain proof of their having been imported from some other country.

As has been observed before, these three species will mix together in every possible degree, hence the great number of species which appear in our books. If we take the trouble to analyze these, it can easily be perceived that they have been produced by hybrid intermixtures, unless fertile hybrids are to be considered as valid species. All those which resemble the *N. tabacum*, but with narrower leaves than common, or in any degree possessed of the peculiar characteristics of the *N. fruticosa*, have been produced by the mixture with this species, and all those with leaves more or less petiolate, whether lanceolate or ovate, as formed by a combination of *N. tabacum*, *N. fruticosa* and *N. rustica*. It is remarkable what strange appearances these will put on; every possible variation of the principal forms and every gradation of position will be found, all, however, easily reducible to the three original types.

ART. IV.—*American Medical Association.*

THE twelfth annual meeting of the American Medical Association (305 members), convened at Louisville, May 3d, 1859, and after a session of three days, adjourned to meet again at New Haven on the first Tuesday of June, 1860. The following extracts from the *Semi-Monthly Medical News*, of Louisville, comprehend the most important transactions of the late meeting:

Dr. D. Meredith Reese, of New York, chairman of the Committee on Nominations, reported the following officers for the ensuing year:

President—Henry Miller, of Kentucky.

Vice Presidents—H. F. Askew, Delaware; Chas. F. Tripler, U. S. Army; L. A. Smith, New Jersey; Calvin West, Indiana.

Treasurer—Caspar Wister, Pennsylvania.

Secretary—S. M. Bemiss, Kentucky.

Dr. Sayre moved the adoption of the report, which was unanimously agreed to.

Dr. Reese also called attention to the necessity of some radical change in the mode of appointing committees to prepare treatises on scientific subjects to be reported at the annual meetings. It had been seen, that on yesterday, a large majority of the committees made no reports, and did not even see proper to send in any communication explanatory of delay. The difficulty heretofore has originated in the mode of selection adopted by the nominating committee. It has been customary for gentlemen to hand in their names and the proposed subjects, on slips of paper, and the committee without further investigation, have so published in the annual reports. Thus it has happened that appointments have been most injudiciously made, and gentlemen to whom a special duty has been assigned, have been found to know less of that than any other subject. We therefore hoped that no committee of last year would be reappointed or continued, from which no report had been had and no communication received.

On motion, the Nominating Committee was unanimously instructed to act upon the suggestions of the chairman, who also stated, that there should be some definite expression of disapprobation as to the course of these gentlemen who had volunteered essays, and had their names reported in the newspapers and spread over the land, and then paid no attention to the matter.

Dr. Flint, from the Committee on Prize Essays, begged leave to report that they had received four dissertations in time for a careful and thorough examination, and two others, quite voluminous, only two days before the meeting of the Association. The latter we have felt constrained to exclude altogether from the competition of the present year on account of the absolute impossibility of reading them with a critical purpose and effect. The others have been carefully examined by all the surviving members of the committee—one estimable associate, Dr. Evans, having been called from all his earthly labors before the active duties of the committee began.

More than one of the four essays we examined exhibited much labor, and a commendable scholarship in their preparation—are voluminous, and in some respects very meritorious papers; but in the unanimous judgment of the committee neither of them possesses the degree and species of merit which should entitle its author to the Association prize.

The committee beg leave furthermore to report, that in their opinion, and as the suggestion of their own recent experience, the Association should determine in more precise and formal manner than has yet been done, the terms and conditions of competition and of success in the contest for prizes, for the government alike of contestants and the committee of adjudication, and that a committee be now appointed to consider and report on the subject.

Dr. J. B. Lindsly, chairman of the committee appointed to inquire into the propriety of dividing the Association into sections, for the better performance of its work in considering the various branches of medicine and surgery, recommended the adoption of such a plan as being indispensably necessary to making this body a working scientific association. They do not deem it necessary to enter into any argument in favor of this plan, it being the one already universally adopted by similar bodies. They would simply recommend, for the present, a division into the following sections, as being most suitable to facilitate the transaction of business, viz:

1. Anatomy and Physiology; 2. Chemistry and Materia Medica; 3. Practical Medicine and Obstetrics; 4. Surgery.

The committee do not propose that this subdivision of labor shall in any manner interfere with the regular business of the Association as now conducted; but only that after having assembled each day in general session, each section shall meet separately for the purpose of hearing and discussing papers on such subjects as properly belong to them, and they therefore recommend that the Committee of Arrangements for the ensuing year, be requested to provide suitable accommodations for the services of these sections, and that each of said sections shall be authorized to make such arrangements as may be required for the proper transaction of its business.

This report was considered and adopted after a very able speech in its support by Dr. Davis.

*Medical Education.*—At the convention of Medical Teachers held the day previous to the regular session of the Medical Association, the following preamble and resolutions, offered by Dr. Joynes, were adopted:

WHEREAS, It appears that a large portion of the Medical Colleges of the United States are unrepresented in this convention, that no changes in the present system of education can be effected unless adopted by the schools generally.

*Resolved,* That it is inexpedient at this time to take any action upon the propositions contained in the report presented by the special Committee on Medical Education at the last meeting of the American Medical Association.



*Resolved*, That with the view of obtaining a more general union in counsel and action upon this important subject, this convention do now adjourn to meet again on the day preceding the next annual meeting of the Medical Association, and at the place which may be agreed upon for such meeting, and that the several Medical Colleges in the United States be requested to appoint each a delegate to such adjourned meeting of this convention.

An amendment was offered by Dr. Wright, by adding another resolution to the effect that a committee be appointed to examine the different propositions offered. The vote of the colleges being called on this resolution, the vote stood ten for the substitute and nine against.

The following gentlemen were appointed by the chair to serve on that committee: Dr. L. P. Yandell, of Louisville; Dr. G. Shattuck, of Massachusetts; Dr. G. C. Blackman, of Ohio; Dr. H. F. Campbell, of Georgia, Dr. M. Gunn, of Michigan.

The meeting was then adjourned.

Dr. Crosby subsequently offered these resolutions for the consideration of the Association :

He strongly recommended a committee from this body to confer with the Teachers' Committee, and felt great confidence that something beneficial to medical education would be the effect of such conference.

Dr. Comegys moved the appointment of a committee of five to confer with the Committee of Medical Teachers and report at the next annual meeting, provided that no Medical Teacher be selected on the part of this Association.

Dr. T. M. Blatchford, of New York, offered as a substitute the following preamble and resolution :

WHEREAS, of all the subjects which can engage our attention in our associate capacity, that of Medical Education is paramount ; and WHEREAS, harmony of action is essential to success in establishing definite qualifications entitling to admission in our ranks ; and WHEREAS, nothing can be gained by hasty action in a matter so vital to our very existence, as a permanent Medical Institution ; Therefore,

*Resolved*, That further action be suspended for the present upon the subject of the resolutions offered at the last meeting of the Association, by the Chairman of the Special Committee on Medical Education, and that a committee, consisting of S. W. Butler, of Pennsylvania, L. A. Smith, of New Jersey, Dixi Crosby, of New Hampshire, C. A. Pope, of Mo., and T. Buckler, of Maryland, shall be appointed to confer with the Committee appointed at the meeting of Medical Teachers, to report some plan for action at the next meeting of the Association.

This amendment was lost and the original resolution adopted.

The following comments upon the proceedings of the Association, by the editors of the *Semi-Monthly Medical News*, one of whom, Dr. Bemiss, was one of the two Secretaries of that body, are subjoined :

In its social aspect, we hope that the delegates to the annual

meeting of the Association just past, were fully satisfied with its results—in a scientific point of view it fell short of some of the preceding sessions. We do not mean that there has been a greater amount of scientific discussion or investigation during former periods of convocation, for we believe all of them have been quite barren in this particular ; but we mean to say, that there was a decided falling off in the number of essays and reports presented, so that the volume of Transactions will, in all probability, fall short of the dimensions of any of its predecessors, though we do not doubt, from all we know of the character of the committees who have reported, that so far as they go, the papers will possess the usual scientific interest.

The failure on the part of committees to discharge the duties assigned them was a natural sequence upon the conduct of the Association towards its committees during the session of 1858. At that meeting, time which should have been occupied in hearing abstracts, at least, of the various reports, was taken up with the discussion of ethical questions, and some of the reports were not even called by title. So unusual does this course of procedure seem to some readers of the minutes of 1858, that a reviewer in the Buffalo Medical Journal charges the omission to the fault of the Secretaries, and supposes it to be by a neglect of their duties, that this hiatus in the record occurs. It should not be attributed to neglect of these officers, but to the hasty manner of dispatching a portion of their scientific duties on the part of the Association, which scarcely permitted notice of some of the most elaborate and valuable essays. Such cavalier treatment does not encourage the production of reports. The laborer who serves a master without other hope of reward than a smile of approval, will not enter with enthusiasm upon new toils, if, after having performed an allotted task, he does not receive even a nod of recognition. The division of the Association into sections will almost entirely remove this source of mortification to those who bring reports ; they will now have ample time for the consideration and even discussion of the various papers that are brought before the Association.

We are indebted to the Nominating Committee, how much to its energetic chairman we are unable to say, yet we attribute a moiety of this good work to his zeal and acuteness, for an important reform in the exercise of some of the duties entrusted to this committee ; they will, in future, not dwindle into mere channels of publicity to any and all seeking notoriety, by being paraded through the public press as committees chosen by the American Medical Association, to report upon subjects of which, as the chairman has intimated, they may be as ignorant as darkness itself. But the committee will hereafter take this matter into their own hands, and select as committees, men of acknowledged abilities, and those who will serve. The Nominating Committee should be held exclusively responsible for the character of the appointments, and should exhibit as much interest in the propriety and wisdom of their selections as the Senators of the United States in the endorsement of appointments referred to them.

ART. V.—*Medical Literature in New Orleans.*

WE have recently received, from their authors, "Yellow Fever, the Epidemic of 1858," a pamphlet of 150 pages, by Chas. Delery, D. M. P.; also, a pamphlet of 112 pages, entitled "Medical Study of some questions important to Louisiana, with a succinct exposition of a paludian endemic of a catarrhal form, which ravaged New Orleans, expending its force particularly upon children, during the yellow fever epidemic of 1858," by J. C. Faget, D. M. P., etc.

Dr. E. J. Coxe has also published a treatise of 107 pages, entitled "Practical Remarks on Yellow Fever." Two articles, by the same author, appeared in our January and March numbers, and those desirous of profiting by his experience, will find it detailed at length in the work before us.

Drs. Delery and Faget have each treated his subject in his mother tongue—the French. The former is well known in this city, not only for his familiarity with yellow fever from his earliest years, for he is a native to the manor born, but also for his ability to profit by his experience. And none among us stand higher than Dr. Faget, for scientific acquirement and medical erudition. A perusal of these books will amply repay the student of yellow fever, and, if we do not illustrate by quotation or criticism their merit, it is not because they are unworthy of such notice at our hands. We regret to say that we have been unable to bestow upon them the close examination which they deserve, and which we contemplate for the future; farther, our pages have been already so filled with articles upon the subject which is of such intense interest to us, that we have trembled lest we might exhaust the patience of the large number of our readers, to whom this disease has no immediately practical interest. To these this topic is somewhat like the Egyptian plague to us—a subject whose study is more curious than profitable.

But, though many of our readers may take no great interest in this theme, it is yet a duty which the medical men of New Orleans owe to themselves and this community, to labor zealously to elucidate this fatal mystery, and to popularize rational views of what is known, and of the much more which is unknown. It is a subject of congratulation, then, to recall the number of able articles and treatises which have succeeded our last epidemic, and more especially when we remember that those of but a few years back had neither a

historian nor a commentator, but bare apologies for a statistical record, and of some epidemics even the date of the years when they occurred has been suffered to become a matter of doubt and discussion.

We, therefore, cordially welcome these contributions to the medical literature of New Orleans, not only for their merit, but also for the pledges which they give of increased interest, and renewed investigations in the future. And, without mutilating their proportions, by excised quotations, we would simply inform any of our readers, disposed to complain of this brief notice, that these works can be easily procured, and at little expense, from any of our book-stores.

CHAILLÉ.

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ART. VI.—*Yellow Fever of Charleston in 1858.\**

A CAREFUL examination of the report of deaths, from fevers, by the City Registrar, for a period of thirty-seven years, will show that they amount to four thousand seven hundred and two (4,702). Of this number, one thousand three hundred and forty-five were of bilious and catarrhal fevers. Three hundred and eighty-three (383) of typhus and typhoid. Thirty-one (31) of dengue or break-bone, and two thousand nine hundred and forty-three (2,943) of yellow fever. Of this number, three thousand four hundred and eighty-one (3,481) were adult whites—four hundred and sixty-seven (467) white children. Four hundred and forty-five (445) adults colored, and three hundred and nine (309) colored children. Of this number, six hundred and three (603) were natives of Charleston. Eight hundred and eighty-two (882) natives of the United States. Two thousand four hundred and sixty-five (2,465) foreigners. Of colored, six hundred and forty (640) natives of Charleston; one hundred and eleven (111) of United States, and one foreigner.

The fatality of the past year, 1858, will long be remembered in the history of this city, not only on account of the fearful mortality which it records, but the unprecedented panic amongst the foreign and native population.

From the 1st to 12th August, the fever became generally diffused over the city, attacking the "unacclimated foreigner and the adolescent native;" an epidemic of fearful character, and long to be remembered for its mortality. Almost coëval with the city of Charleston, the yellow fever made its appearance at Moultrieville, Sullivan's Island.

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\* Report of the Committee of the City Council of Charleston, on the Origin and Diffusion of the Yellow Fever in Charleston, in the summer of 1858. Printed by order of Council. Pp. 68. Charleston: 1859.

Table exhibiting the Deaths in each year, from 1817 to 1858, by Yellow Fever.

YEAR.	June.	July.	August.	Sept'r.	October.	Novem'r.	Decem'r.	Total.	WHITES.		COLORED.		Natives under 18 years.	Non-Natives under 18 years.
									Adults, Males.	Adults, Females.	Adults, Males.	Adults, Females.		
1817		3	80	149	33	7		272	164	56	14	5	33	
1818				97	25			177	180	24	3	2	18	
1822	1		1					2	2					
1824			36	185	55	9		235	150	30	2		36	17
1825			1	1				2	2					
1827			21	27	14	2		64	42	6			10	6
1828			13	13				26	22	4				
1830			15	15	14	1		30	25	1			1	3
1834			1	28	20			49	35	12			2	
1835			3	22				25	15	5			1	1
1838			54	214	78	5		351	292	35		1	16	7
1839	3	25	65	36	5			134	111	13		1	7	2
1840			1	15	6			22	20	2				
1843						1		1	1					
1849			3	88	79	5		125	97	26			1	1
1852			6	121	162	21		310	221	65	1		5	18
1854			46	407	152	22		627	405	134	7	2	39	40
1856			17	95	86	13		211	135	48	2	1	8	17
1857				1	9	3		13	9	2				
1858		11	111	417	161	16	*1	717	380	177	12	3	88	75

\* A Child on 22d December.

The mortality for the year 1858, from all diseases, amounted to nineteen hundred and twenty-two (1922), and in proportion to population as 1 to 26.14—and the per centum for ten years from 1849 to 1859, including five epidemics, 1 in every 35.82. Five epidemic years, 1 in 29.72. Non-epidemic years, 1 in 41.92.

*Mortality from Fevers in 1858.*

	April.	May.	June.	July.	Aug't.	Sept'r.	Oct'r.	Nov'r.	Total.
Fever, Congestive .....	1	2	0	1	1	1	3	0	9
“ Remittent .....	0	0	0	2	4	0	2	0	8
“ Intermittent .....	0	0	0	0	0	0	1	0	1
“ Bilious .....	0	0	0	1	0	4	1	1	7
“ Catarrhal .....	0	0	0	0	0	0	1	0	1
“ Typhoid .....	2	2	6	6	4	2	7	2	31
“ Yellow .....	0	0	0	11	111	417	161	16	716
Totals .....	3	4	6	21	120	424	176	19	773

Nativity.	Whites.	Colored.		Whites.	Colored.
Charleston .....	99	25	Adult Males .....	403	25
United States.....	127	37	Adult Females.....	192	17
Foreigners .....	485	00	Children, Males.....	74	12
			Children, Females...	42	8

Alderman Robert Leiby, M. D., Chairman of Committee, of Council on Quarantine, addressed a circular to a number of physicians who had been engaged in practice for at least twenty-five years, proposing three questions, the first of which related to the contagiousness of yellow fever. The voluminous answers which were returned to this interrogatory, *seriatim*, be thus summed up:

Dr. Bellinger answers: I had not formed an opinion; Dr. Johnson attributes the fever to local causes; imported cases serving to aggravate the disease; Dr. Jarvey says the disease is importable, but is not certain as to its contagiousness; Dr. Waring denies contagion; Dr. Horlbeck disallows both contagion and importability; Dr. Campbell, ditto; Dr. Porcher rejects contagion; Dr. Ogier affirms it, as to young and unacclimated persons; Dr. Robertson repudiates contagion and importability; Dr. Wragg does the same with some qualifications. There does not appear to be one decided or unqualified contagionist among the respondents.

It will be seen that the opinion adverse to the contagiousness of yellow fever is almost unanimous among these witnesses, so far as this document extends. Doubtlessly the learned Professor Dickson

and his contagionists could, from the same data, adduce documentary evidence and certificates flatly contradictory to the letters in the official report. From the same data, a third party, the contingent contagionists, could probably make quite a plausible argument in favor of their ætiological theory. These contradictory statements are from time to time proclaimed officially by city governments and legislative bodies, as well as by individuals. Norfolk affirms, and the sanitary and quarantine convention of New York denies the personal contagiousness of yellow fever. The yeas and nays of the last century are repeated in the present, and will probably be in the next, *ad infinitum*.

Such contradictions do not exist in the constitution of things, but in the imperfection of observers and writers. In order to produce unanimity of opinion, a complete definition or explanation of contagion with known examples should be given and be accepted. These signs or standards, whether direct or analogical, being fixed and known, all the phenomena or data should be tried by them in the most rigid manner, and be classified and generalized accordingly. There can be no permanent limits assigned to contagion as some expound it. They advocate contagion, but it is according to them inoperative unless there be filth, some undefined meteorological condition, or something else throughout the category of possibilities. Thus the Quarantine Convention at Philadelphia (May, 1857) declared that yellow fever can be introduced by persons and things; that its action upon individuals coming within its influence is limited, and *cannot produce* an epidemic in the community, unless there exist in the latter, circumstances which are calculated to produce such disease *independent of importation*.

Now such expositions of contagion are not only vague and indefinite, but so far as they are comprehensible, are contradictory. If individuals contract yellow fever by approaching patients suffering from it, they might communicate it to others, and the latter to others, and so on indefinitely. If local circumstances are positively known to exist altogether sufficient to produce an epidemic independent of importation, it is unphilosophical to assign other causes, either domestic or imported to account for the same. Dogmatism in assuming the place of positive knowledge, should be at least consistent with itself. Scientific medicine should not be humiliated by charlatanic pretensions to a knowledge which can be achieved neither by flat contradictions, nor by equivocal verbiage.

A small minority of the medical faculty, either from personal interest, or from a better motive, continue to influence several of the municipal and State legislatures to enforce oppressive quarantine laws, chiefly with regard to the prevention of yellow fever. Thus, one portion of the community becomes hostile to the other. One parish points its cannon charged with grape ready to destroy visitors from the next. This cannon logic—*ultima ratio regum* has been recently replaced in New York, by extensive conflagrations, worse than the logic of the dark ages. In the eleventh century a great question agitated a large portion of Europe, as to which of two liturgies was to be received as orthodox. "A great fire was kindled; a copy of each liturgy was cast into the flames. It was agreed that the book which stood this proof, and remained untouched should be received." Nevertheless, the parties disputed as to the facts, and the controversy was not settled any better than that of the existing quarantine against yellow fever.

B. DOWLER.

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ART. VII.—*Pharmacopœia of the United States.*

It is now nearly half a century since the learned Prof. John Redman Coxe (who still lives), suggested the expediency of forming a National Pharmacopœia. The medical colleges and societies accordingly appointed delegates, who for this laudable purpose met at Washington, January 1st, 1820. This meeting formed the first National Pharmacopœia, which, though not universally satisfactory, has been at every decennial meeting extended, improved and revised. The fourth decennium will have elapsed by the beginning of the next year, when, as the following letter from the gifted Professor Wood, President of the Convention, shows, a further revision will take place, so as to bring the work up to the level of the existing state of pharmaceutical knowledge and improvement.

An authoritative work devoted to the revision of the past—the addition of whatsoever is new, true and useful—the rejection and exclusion of the arbitrary and useless—the classification of medicinal agents and their combinations into their primary and secondary



values, and the giving to the whole a rigid simplicity characteristic of truth, unbiassed by the prejudices and hoary errors of centuries, are high aims, involving great professional responsibilities and must exercise a great influence on the well-being of society. Every article of the Pharmacopœia should be judged in reference to the questions—does it alleviate? does it cure? Antiquity, tradition, and mere authority should stand aside whenever experience, which is cumulative in its tendency, speaks adversely or not at all.

Much of the *Materia Medica*, Pharmaceutical preparations, and the *Formulae* now more or less current require retrenchment. Many inert, ill understood, uncertain and dangerous articles should be consigned to the "*Index expurgatorius*." The *theriaca* of Andromachus nearly two thousand years ago contained sixty-one articles, to which eleven have been added in modern times, including vipers' flesh. An apothecary informed me, not long since, that he was obliged to import this monstrous farrago in order to fill some prescriptions sent to him.

Dispensatories, Pharmacopœias and Formularies abound with articles and preparations which the physician abandons as the horizon of his experience extends, until his list is, perhaps, reduced to six or eight *per centum* of the whole.

B. DOWLER.

*Medical Convention for Revising the Pharmacopœia of the United States.* The Medical Convention for revising the Pharmacopœia, which met in Washington, in May, 1850, provided for assembling a Convention for the same purpose, in the year 1860, by the following resolutions :

"1. The President of the Convention shall, on the first day of May, 1859, issue a notice requesting the several incorporated State Medical Societies, the incorporated Medical Colleges, the incorporated Colleges of Physicians and Surgeons, and the incorporated Colleges of Pharmacy throughout the United States, to elect a number of delegates, not exceeding three, to attend a general Convention to be held at Washington, on the first Wednesday in May, 1860.

"2. The several incorporated bodies, thus addressed, shall also be requested by the President to submit the Pharmacopœia to a careful revision, and to transmit the result of their labors, through their delegates, or through any other channel, to the next Convention.

"3. The several medical and pharmaceutical bodies shall be further requested to transmit to the President of the Convention the names and residences of their respective delegates, as soon as they shall have been appointed, a list of whom shall be published, under his authority, for the information of the medical public, in the newspapers and medical journals, in the month of March, 1860."

In accordance with the above resolutions, the undersigned hereby requests the several bodies mentioned to appoint delegates, not exceeding three in number, to represent them in a Convention for revising the Pharmacopœia of the United States, to meet at Washington, on the first Wednesday in May, 1860; and would also call the attention of these bodies to the second and third resolutions, and request compliance with the suggestions therein contained.

GEORGE B. WOOD,  
President of the Convention of 1850.

PHILADELPHIA, May 1, 1859.

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ART. VIII.—*Official Report of the Yellow Fever in the Hospitals of Lisbon.*

FROM the official report of the special provisional hospitals for the treatment of yellow fever, at Lisbon, in 1857, by M. Sequeira Pinto, peer of the realm, the following statistical history has been summed up: Up to the 24th of December, when the epidemic ceased, the admissions amounted to 5,161, of which number 4,043 were men, and 1,118 women. *Mortality*—1,932 men; 388 women; a proportion of 37.43 per hundred of the attacked. *Ages*—Infancy was generally exempt from the epidemic; only 19 boys and 12 girls, under 11 years of age, were attacked, 7 of whom died. On the other hand, from 11 to 20 years, the sufferers amounted to 1,269; from 20 to 30 years, 1,734, etc. Constitutions the most vigorous and robust suffered most. The death-rate of the natives of Lisbon, who were attacked, was 46.39 per hundred; that of others only 31.06. Of the foreigners attacked, 1,762 were Spanish and 15 French; deaths among the former 734, and among the latter 8.

These hospital statistics (quoted from the *Gaz. Heb. de Méd.*, of April 29, 1859), constitute but one feature of the tableau of the yellow fever of Lisbon and its environs, in 1857. A more extended and complete report, comprehending the whole population, the number attacked, the medical topography of the city, its hygienic history, vital statistics, mixed races, etc., is desirable.

B. DOWLER.

ART. IX.—*Hygiene.—Mortality.*

i. *Life and Labor.*—The “Sanitary Review” for January has an article on the influence of various common occupations on health and life. The effects of sand-paper making are illustrated as giving rise in the young to a modified phthisis, which is sometimes rapidly fatal. Walking-stick making and hemp and flax-dressing are described as exciting bronchitis and bronchorrœa. The Neapolitan hemp has also the peculiar property of producing a spasmodic paroxysmal attack like that produced by drying hay and by ipecacuan. Trimming manufacturers’ work leads to bronchial mischief. Fur dyeing, by exposing the artisans to the fumes of nitrous acid and to the dust arising from dried sulphate of copper, specially leads to a number of serious evils, affecting the teeth, the digestion, and worst of all, the organs of respiration. Cigar and snuff-making, in the first instance, give rise to the peculiar toxical effects of tobacco, and afterwards to chest disorder, ending in chronic bronchial flux and inanition. The writer of this paper suggests that a Parliamentary committee of inquiry should be organized to inquire into the subject of occupations and health. He concludes as follows:

“When we look at diseases as a whole, we stand amazed at the varieties of type which they assume. When we classify them into groups according to their causes, we stand equally amazed at finding to how very few groups all diseases may be reduced. We find all the disease causes out of the body and clothed in externals. Resolving the causes, there stand out some dozen poisons of communicable and reproductive power, improper dietary, variations of atmosphere, and occupations. Of all these, the last stands most invitingly for inquiry. The inquiry is of a kind to which the most rigid rules are applicable. Its results might be demonstrations, its suggested remedies simple certainties.”—*Sanitary Review, January, 1859. Br. and For. Med. Chir. Rev.*

ii. *Mortality of England in 1856.*—Of 399,506 deaths in the year, the causes were not specified at all in 4666 cases, and 3474 are simply tabulated as sudden deaths, all inquiries having failed to elicit further definite information; 94,407 of the whole number of deaths were those of infants under one year old.

The causes of death are arranged in a few classes with numerous subdivisions. 78,047 of the deaths are classified as zymotic diseases: typhus (15,398), scarlatina (14,160), and diarrhœa (13,815), proving fatal in 43,373 instances, or considerably more than one-half of the whole number due to this class. Hooping cough, measles, and croup stand next in order: Small pox, formerly so fatal, was the cause of death in 2277 cases.

Of the class of diseases denominated “constitutional,” 82,856 persons died. Phthisis (consumption) stands sadly preëminent in this list, its victims numbering 48,950 persons, by far the greater proportion of whom were young women. “How many of the thousands of deaths are to be ascribed severally to the fatal stays and to the in-door life of women, etc., it is not easy to calculate. Air is the

pabulum of life, and the effects of a tight cord round the neck, and of tight lacing round the waist, differ only in degree in the time of their manifestation and in some of their symptoms; for the strangulations are both fatal. To wear tight-laced stays is, in many cases, to wither, to waste, and to die, and is perhaps the natural chastisement of the folly which inflicts this Chinese deformity, natural only to wasps and other insects, on the human figure." The tubercular diseases carried off in all 63,832 persons.

The "local diseases," as inflammations, the allied pathological phenomena or their results, and functional diseases of particular organs, proved fatal to 149,911 persons. 50,535 of these died of diseases of the brain and nervous system, including also 23,946 deaths by convulsions. 13,672 deaths were referred to diseases of the organs of circulation. Diseases of the respiratory organs proved fatal to 52,908 persons, 21,528 of whom died from bronchitis, and 22,653 by pneumonia. These diseases, and all others of the class, were less fatal than in the previous year. 22,620 persons died from diseases of the digestive organs. The other causes of death under this heading are numerous, but the cases are few in each.—*Condensed from the Registrar-General's Annual Report. Ibid.*

#### ART. X.—*Gratuitous Medical Services.*

[*The Medical Times and Gazette* (April 23, 1859), has an article on this subject, of which the following extract is the initial portion: ]

We have already, and on a late occasion, referred to the suicidal folly adopted by so many of our Profession—we may say, so universally adopted by our Profession—in their gifts of gratuitous advice to the sick. We have shown how it has demoralized the whole system of our Medical ethics, how it tends to ruin us as a body, to produce the severest competition between man and man, and to lower us all in the estimation of the public. The matter is of such grave importance that we return to it again, hoping and believing that some light will at length shine forth to guide and lead us out of this position of folly and darkness. We have stigmatized, as one of the most fruitful sources of the evil, the system of out-door Medical aid, as it is so generally administered at our public Medical charities. Any one who can obtain letters of recommendation to a hospital or dispensary, receives relief; and it is notorious that numbers do obtain such letters who are not entitled to relief; so that to this extent the Profession is actually robbed of their actual rights by such charities. The Medical officers of these public institutions not only receive no pay for their duties, but in the performance of those duties they actually abstract indirectly from the pockets of the Medical

practitioners, who live in the neighborhood of the charity attended by them, their due earnings; and this they do by administering relief to those who are not deserving objects of the charity. No one but those who are intimately acquainted with the working of our large Medical charities, can have any idea of the extent to which these charities are abused in this way. The patient, it is true, must obtain a letter of recommendation; but who ever troubles his head as to whether the recipient of it is or is not a fit object of charity? And how is the doctor to judge? and how can he be expected to be continually battling against such abuses? Besides which, it is evidently not to his interest to be too particular in this respect. And then again, there is another sort of charity which has lately sprung up, and which parades its excellencies to the world, as being different from other charities in this particular, viz: that its portals are free and open to all, that every one may enter freely without let or hindrance, and freely obtain relief. A new one of this species, lately established, we observed a few weeks ago trumpeting itself in the corners of the papers, as having cured its 60,000 out-patients during the year! Herein lay its merits, that no one was questioned, but all instantly administered to! Can *charitable* demoralization go beyond this?

We cannot, in one sense, but be glad that the abuse has thus, as it would seem, reached its culmination; for it is from the exaltation of abuses that good reforms do frequently arise. We lately had an opportunity of seeing demonstrated the mischief inflicted on our Profession by this indiscriminating charity, as it is falsely called. In one of our metropolitan hospitals the out-door patients had been allowed, through neglectful management, to receive relief in a most indiscriminate manner. This system was suddenly arrested, and at once the number of the members of an adjoining Provident Society increased most rapidly. The patients, so long as the doors of the hospital were open without impediment to their entrance, flocked there; but when the ordinary check was established, then numbers of them found out that they could afford to pay so much per week for Medical aid!

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*Remuneration of Medical Attendants in France.*—M. AMÉDÉE LATOUR, editor of *L'Union Médicale*, has lately published a very instructive article on the above subject. From this contribution we find that the majority of medical men, especially in the country, are lamentably paid, the principal cause of this sad state of things being the unfortunate practice of underselling one another. There are surgeons in the country who will pay a visit for five pence, and even two pence half-penny; attend a midwifery case for twenty pence or half a crown; set a limb for tenpence, etc., etc. In Paris the evil is likewise very great, it being well known that some unscrupulous characters offer their services underhand at a lower rate than the usual attendant has fixed, that rate being indeed low enough, as it ranges from twenty pence to half a crown a visit, even in Paris.

At Toulouse a club was formed some time ago, composed of a thousand individuals, who offered a surgeon one thousand francs (£40)

a year to attend all the members. He, being a beginner, accepted these miserable terms; seeing which, a professional BROTHER went and offered his services for half the sum. But the latter was soon outwitted, as a third sprang up and offered to do the work for £10 a year!! M. Latour throws out the idea that a minimum should be fixed and agreed upon in each locality; but we fear that he did not consider human frailty sufficiently when he thought of that expedient.—*Lancet*, 1851.

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ART. XI.—*The Ophthalmoscope and its Triumphs.*

NEITHER the stethoscope nor the ophthalmoscope afford exceptions to the Socratic maxim, "Good things are hard." To the successful employment of either instrument a special and extended training is needful. The committee of the Moorfields Hospital have most zealously coöperated with their surgical staff in providing the means for the successful study and teaching of the ophthalmoscope. At first a single small room was fitted up for the purpose; but this being found insufficient, a large room provided with partitions was built. In this there is now every convenience for seven persons to work simultaneously. Let us briefly enumerate some of the gains to the practice of our art which have already accrued from this instrument. *a.* It will enable the merest tyro to pronounce positively as to existence or non-existence of cataract—a feat which, in certain peculiar cases, used formerly to baffle the best-trained observers. *b.* It makes easily evident the very first beginnings of cataract at a period long before the opacity is discoverable by the unassisted eye. *c.* It reveals in cases of true glaucoma the existence of a cup-like depression of the optic entrance, a sign of the greatest importance as to showing a morbid degree of intra-ocular pressure, and indicating the necessity for iridectomy. *d.* It affords a clear demonstration of the physical cause of muscæ, and sweeps aside the old notions about varicose vessels in the choroid, etc., etc. *e.* In affording indisputable proof of the dependence of many cases of what used to be classed as amaurosis on retinal apoplexy, or punctate extravasations of blood into the retina (a sort of purpura of the retina), it saves the surgeon from the risk of the serious error of prescribing mercurials and antiphlogistics to patients already suffering severely from humoral dyscrasia. *f.* In another class of cases it exhibits to the observer, in the clearest manner, the existence of limited effusions of lymph in the deep structures of the eye, such effusions requiring for their removal immediate resort to mercurial treatment. *g.* In yet other cases in which with amaurotic symptoms of the most serious kind, the surgeon might have felt bound to attempt something even without a certain knowl-

edge of their pathology, it will show the retina already detached and disorganized, and demonstrate the uselessness of treatment. We might very lengthily extend this list of the triumphs of this instrument; but enough has been said to prove that it must henceforth take equal rank in its own department with the stethoscope in diseases of the chest; and that before long a medical education which has not included its use will be held to be very imperfect.—*Med. Times and Gaz.*

### TO OUR SUBSCRIBERS.

Enclosed in the present number of the Journal our subscribers will find their accounts, stating the amount due by each for past years, and in advance to the year 1860. We trust these bills may receive the prompt attention they deserve, and thereby enable us to pass through the present summer free from pecuniary embarrassment.

Any bank bills current in the subscriber's State will be accepted in payment, and if enclosed in a letter, duly registered as "valuable," will be at our risk. When convenient, it is preferable to forward an order on some commercial firm in New Orleans or Mobile, but even the risk of *money* by mail subjects us to far less loss than is expended in the payment of commissions to our collectors.

Those only are authorized to collect for this Journal who have in their possession the written authority of one of the proprietors. Our general agents are: Jas. Deering, and his associate, E. W. Wiley, for the States of Louisiana, Mississippi, Alabama, Arkansas and Tennessee; D. Richardson, of the Galveston News, and his associate, J. K. Preston, for the State of Texas. These gentlemen are our accredited agents until January, 1860. To postmasters and others the bills of those subscribers who may reside in their towns or counties are sometimes intrusted, but in all such cases they have our written authority to collect, which should be demanded by the subscriber before payment.

In future the business department of our Journal will be conducted by, and all business letters should be addressed to—

Drs. CHAILLÉ & NICHOLS,  
Proprietors of the N. O. Med. and Surg. Journal,  
New Orleans, La.

*Mortality Statistics of New Orleans, from April 10, to June —, 1859, compiled from the Weekly Reports politely furnished by Dr. Baldwin, Secretary of the Board of Health. Population of New Orleans by the last census, 138,000.*

Time.	Total Deaths.	Children under 2 yrs.	Under 20.	U. States
April (5 weeks).....	585.....	165.....	286.....	401.....
May (4 weeks).....	505.....	151.....	151.....	315.....

Principal Diseases.	April (5 weeks.)	May.	Principal Diseases.	April (5 weeks.)	May.
Still born.....	25.....	24.....	Tetanus.....	10.....	3.....
Trismus Nascentium.....	22.....	7.....	Diarrhœa and Dysentery.....	23.....	40.....
Teething.....	4.....	22.....	Gastro-Enteritis.....	4.....	8.....
Cholera Infantum.....	7.....	12.....	Inflammation of Liver.....	10.....	3.....
Infantile Convulsions.....	41.....	23.....	Inflammation of Lungs.....	24.....	15.....
Infant. Marasmus.....	12.....	14.....	Phthisis.....	89.....	89.....
Croup.....	8.....	5.....	Apoplexy.....	12.....	11.....
Inflammation of Throat.....	21.....	26.....	Congestion of Brain.....	9.....	18.....
Scarlatina.....	21.....	13.....	Fever, Typhoid.....	12.....	13.....
Rubeola.....	1.....	1.....	“ Miasmatic.....	10.....	12.....
Variola.....	13.....	1.....	“ Yellow.....	0.....	0.....

No cases of Yellow Fever have been reported by the Board of Health up to the present time, June 10th. One or two suspicious cases may have occurred, but none, it is believed, have been stamped with an impress so unequivocal as to be unhesitatingly and unanimously pronounced this disease.

CHAILLÉ.

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.—*From the Medical Purveying Office, U. S. Army, N. O. New Orleans, La., Lat. 29 deg. 57 min. 30 sec. N.; Long. 90 deg. W. Altitude of Barometer above the level of the sea, 35 feet.*

1859.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
	Max.	Min'm	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
April.....	7 A. M. 23d.	2 P. M. 3d.	30.188	2 P. M. 13th.	7 A. M. 17 & 18.	71.85	2 P. M. 13th.	7 A. M. 17th.	70.43
	30.400	29.964		84	62		86	54	
May.....	7 A. M. 28th.	7 A. M. 9th.	30.172	Several, 85	7 A. M. 9 & 10	78.60	2 P. M. 31st.	7 A. M. 8th.	79.10
	30.362	29.790		85	71		89	68	

1859.	HYGROMETER.			PREVAILING WINDS.	RAIN.	
	Max.	Min'm	Mean.		Days.	Quantity.
April.....	2 P. M. 12 & 13.	7 A. M. 17th.	67.42	N.; S. E.; AND S.	9	3.99 inch.
	80	51				
May.....	2 P. M. 21st.	7 A. M. 8 & 9.	75.48	E.; S. E.; AND N.	7	1.94 inch.
	84	67				

T. HARRISON, Clerk.



THE  
NEW ORLEANS  
MEDICAL AND SURGICAL JOURNAL.

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SEPTEMBER, 1859.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Researches into Purpura, Scurvy, Hæmorrhage, and Petechiæ.*  
By BENNET DOWLER, M. D.

i. PURPURA HÆMORRHAGICA.

THE term purpura has oscillated to and fro in the nosological systems of book builders whose classificatory and pathological biases were often antagonistic or variant according to the special point of departure which each had taken, whether in humoralism or solidism, sthenia or asthenia, phlegmasia or cachexia. Among the exanthemata, and other diseases, purpura is still running the gauntlet. It is febris miliaris, it is scorbutus, it is roseola, it is morbis maculosus, it is scarlatina, it is urticaria, it is hæmorrhagica universalis, it is hæmorrhœa petechialis, that is to say, it is a malady ill understood in its diagnostics, pathology and treatment. It is a malady, therefore, which requires further investigation, and, with this view I contribute a brief sketch of my limited stock of information and research, indulging the hope that others of larger experience and abler powers of generalization, will do likewise, and the more so because little has appeared in this Journal, on this disease—a disease which is apparently allied to the last or hæmorrhagic stage of yellow fever. In the same spirit of humble inquiry, I propose to examine certain pathological conditions of a kindred character with that of purpura

hæmorrhagica, as will appear in the sequel of this paper or upon some other occasion. It is desirable to multiply reported cases of purpura, particularly with reference to treatment and pathological anatomy, because very little of a satisfactory character is known of either.

By reporting, comparing and analyzing numerous cases whether of purpura or any other obscure disease, it may reasonably be supposed that a better understanding of the same may be eventually obtained.

The existence of purpura appears to have been little, if at all noticed, except in times comparatively modern. It is true that Hippocrates, in his Epidemics gives the following brief histories, some of which may allude to this malady. He says: "Euphranor's son had eruptions like bites of gnats for a while, and the next day he grew feverish."—"The fuller in Syrus was marked upon the skin like the bites of gnats."—"Timonax's little boy, about two months old, had small eruptions on his legs, hips, loins, and lower part of his belly. The swellings were very red, and upon their subsiding, or going in again, convulsions and epileptic fits attacked him, without a fever, for many days before his death." (Coxe's Hippocrates; 445, 439, 446.)

That purpura is always essentially either a disease of debility or a merely secondary affection, is by no means well established, although it occasionally appears as an accompaniment of febrile, cardiac, pulmonary hepatic, intestinal dropsical, scorbutic and syphilitic diseases, as may be more fully remarked in the sequel. It may to a great extent, be regarded as the type of the so-called passive or asthenic hæmorrhages.

The term passive hæmorrhage may be of doubtful import, being relatively rather than positively correct, yet in a practical point of view the distinction is useful. Purpura, which thus underlies hæmorrhagic morbidity, is often an idiopathic affection, or at least as much as many diseases so named in systems of nosology. It does not, like scurvy, appear to have any necessary connection with personal filth, bad lodging, imperfect ventilation, unwholesome or salt animal diet, concerning which more will be said hereafter.

#### CASES OF PURPURA.

F., aged 5, daughter of a bank officer, enjoying apparently the best hygienic conditions; delicate constitution; had suffered an attack of diarrhœa, which after the interval of one month returned with the

additional symptom of a slight fever. She was treated for four days for diarrhœa, during which, quinine, morphia, and eight grains of calomel and blue mass were used. A blister was applied to the epigastrium. The fœces in four days became natural, with the exception of a discharge of blood from the bowels, on the night of the fourth.

The next morning (December 2d), numerous purpurous spots appeared upon the body and limbs, being well defined but darkest in their centres; one vesicular extravasation on the nose had broken through the epidermis and bled for a time. The bleeding from this spot was renewed whenever the child was excited by crying or passion. Hæmorrhages took place from nose and mouth. There was no indication of salivation: no swelling of the gums, or mercurial odor. The fever ceased; the pulse weak, and occasionally during sleep, uncommonly slow.

December 3d. The hæmorrhage has augmented. The petechial efflorescence has extended without any apparent increase in the momentum of the circulation.

December 14th. This patient, now restored to health, has used little medicine other than tonics, lemonade, a suitable diet, and occasionally short excursions for a change of air.

K., a negress slave, aged 25, intelligent; never bore a child; catamenia copious, but at irregular periods. In her twenty-first year she suffered from syphilization, for which she had been long under treatment, and still longer for the severe secondary symptoms which ensued, as ulcerations in the throat, nose, and skin, periosteal pains, cutaneous eruptions, etc.

The case while under my care (from July 4th to the 28th) presented, with some variations, the following general group of symptoms: Hæmorrhages to an enormous extent, from the roof of the mouth, near to but not from the gums; bloody oozings from an old sore on the leg; extensive purpurous efflorescence upon various parts of the body; eyelids œdematous; slight vaginal hæmorrhages. During her whole illness, her senses, sleep, appetite, digestion, thirst, temperature and urine were but little disturbed; her alvine evacuations were normal except during the last three days of her life, when they became bloody. The pain which she suffered the most from originated in the fungous vegetations in her mouth. Her gums in both jaws were pale, free from swelling or sponginess; the teeth firm; respira-

tion generally easy; sweats freely; breath constantly highly foetid. Petechial discolorations of variable size, from a mustard seed to that of a water melon; skin never hot; pulse generally small, quick and soft, even under the use of stimulants. From the roof of the mouth within the gums the dark red or purplish strawberry-shaped pullulations shot forth from time to time, but sometimes falling off, were renewed again. These vegetations next appeared on both lips near their commissures, and ranging backward upon the mucous membrane of the cheeks and under jaw formed dark bunches or clusters from which constant leakage of blood took place in defiance of astringents, etc. She had, some months ago, been using the preparations of iodine, etc., and for the last month Swain's panacea, and probably other medicines.

The treatment, during my attendance, was varied, or intermitted for a time and then resumed. Astringent washes of galls, lead, alum, chlor. sod., also solid nit. argent., tinct. fer. mur., etc., appeared neither to arrest the hæmorrhages nor destroy their fœtor.

The internal treatment was tonic; quinine, wine, astringents, tinct. mur. fer., acids vegetable and mineral, porter, wine, brandy, morphia, Dover's powder, chlorin., sod., sarsaparilla, acid. sulph. arom., good and varied diet; an occasional aperient of ol. ric., or an enema.

Astringents, such as gallnuts in powder and infusion, muriated tincture of iron, acetate of lead, applications of the solid nitrate of silver to the bleeding parts, nitro-muriated spongings of the skin, together with other similar means having been tried without benefit, were abandoned.

The persisting debility was so great, that quinine, iron, brandy, porter, and good diet seemed to be called for. Nevertheless, hæmorrhage continued, the anæmia increased, the fungous vegetations grew larger, bluer, blacker, and more foetid, until finally they became gangrenous before death. The gums immediately surrounding the teeth, and the teeth themselves, were firm and fit for masticating solid food had not the spongy vegetations interfered.

#### POST MORTEM EXAMINATION TWO HOURS AFTER DEATH.

Body of medium size, symmetrical, supple, warm; mammary glands well developed; abdomen concave; several cicatrices in the groins, knees, legs and chest; excavated ulcer on the left leg, above the internal malleolus, as large as a dollar, with raised indurated margins;

skin discolored, with isolated petechiæ or blue blood spots; muscles pale; blood diffluent, forming soft, claret-colored coagula.

*Chest.* Lungs fixed by innumerable firm, bloodless bands, to all the surrounding tissues of the costal pleuræ and diaphragm; parenchyma pale, anæmic crepitant; large clusters of raspberry-like vegetations, one nearly as large as the palm, another large one a quarter of an inch thick on the posterior surface of the right lung, together with minute petechiæ, visible through the pleura pulmonalis, etc.

*Heart.* Large, tumid, pale, flabby; portions of the right auricle half an inch thick, being solid, elastic, and of a mulberry color; auricle dilated, being of double size; disseminated subserous petechiæ of the heart; the spots on the heart being of a bright red color. The walls of the cavæ near the heart, hypertrophied, resembling the right auricle in color, consistence, etc. Bronchial glands somewhat enlarged.

*Abdomen.* Oesophagus, spleen, pancreas, kidneys, natural; liver universally adherent upon its convex surface by firm, bloodless tissue; its parenchyma cork-colored, enclosing several indurated, disseminated masses; the gall-bladder contained a spoonful of yellow bile.

*Stomach.* About half of its submucous tissue infiltrated with disseminated petechiæ of a strawberry color.

*Intestines.* The duodenum and jejunum empty, bloodless, blanched, contracted, brittle; a portion of the submucous tissue of the ileum was for a few inches red and congested, below which, for several feet, a little blood was found effused, which appeared to dye the mucous membrane. Except a few fragments of desiccated fæces in the cæcum, the large intestine was nearly empty, and was dotted at intervals with distinct petechiæ in the mucous membrane; these petechiæ were red, the residue of the tissue anæmic and blanched. Mesenteric glands red and enlarged. The omenta well developed; the larger had strong bloodless adhesions within the pelvis.

*Uterus.* Small, contained an irregular shaped clot, being in its longest diameter half an inch.

*Ovaries.* Each nearly as large as the uterus, consisting of a dark, hepatized and nearly uniformly solid mass, having some dense sacks filled with black blood.

As it was desirable in this case not to disfigure the head and face, these parts were not dissected. The examination was a private one,

and occupied two or three hours, the appearances having been carefully noted at the same time.

In the above case the nitrate of silver was found sufficient to disorganize the vegetations of the mouth and cheeks; but they were speedily reproduced, and became larger and more vascular in a few days.

With regard to the nitrate of silver, I will here remark that a negro slave, pregnant, mother of fourteen children, whom I attended in 1858, in yellow fever, accompanied with slight hæmorrhages from the gums and vagina (and who was subsequently delivered of living twins), had a highly vascular bleeding polypus in one nostril, which appeared a few weeks after recovery from the fever. As she was pregnant, timid and unwilling to submit to the usual operation, palliative astringents, as tannic acid, sulphates of zinc and copper, corrosive sublimate and repeated applications of the solid nitrate were tried without any permanent relief, until, at length, a severe and prolonged application of this caustic, at one sitting, was effected, which was soon followed by the sloughing away of the tumor and by an unexpected cure. The disease has not returned as yet—a period of eight months.

The case of *purpura hæmorrhagica* above mentioned in the negroes, occurred, as the history shows, upon a syphilitic diathesis—a complication, which, it may be assumed, gave rise to the fungous shoots in the interior of the jaws and between the latter and the cheeks. These growths, if due to secondary syphilis, appear to differ considerably from the latter, being softer, like fungus hæmatodes, blue or black, and externally more vascular. Syphilitic excrescences when chronic, so far as I have seen, are pale externally, solid, and not apt to give rise to hæmorrhage, although upon excising these masses from the female when large, I have encountered alarming hæmorrhage. It is said that after removal of these masses, there will be no return of them, whereas in the above case the contrary took place.

In the above described case of purpura, some points of resemblance with scurvy may be traced, but, upon the whole, the differential diagnosis is not only possible but easy. Some able writers have, however, confounded these diseases. Willan, whose work *on cutaneous diseases* appeared about the close of last century, considered purpura as a variety of scurvy. Others have regarded land scurvy and

purpura as identical. Purpura with fever, and without fever, maculated, senile and cachectic purpura, are distinctions rather accidental than fundamental in the pathology of this malady. If purpura attack the aged, why should it be called *purpura senilis*, while no distinctive name is given for it among the young, who are most liable to the disease?

Purpurous hæmorrhages and petechiæ take place in the interior as well as upon the exterior of the body. This, with other still more significant phenomena, shows that purpura is not essentially a cutaneous disease.

Purpura, though a hæmorrhagic disease, is not necessarily an essential hæmorrhage, that is, a hæmorrhage having no symptoms or phenomenon but hæmorrhage itself. It is often a sequence of a known antecedent morbidity. Whether it be considered as an idiopathic or symptomatic malady, there is probably an alteration in the blood, or in the capillaries, or in both at the same time. The stasis of the blood may, hypothetically speaking, be due to previous capillary congestion, diminished tonicity or paralysis, or to the elementary corpuscles and plasma of the blood itself; such as a diminished tonicity, disintegration or solution of the envelopes of the corpuscles; a diminution of febrin, hydræmia, necræmia.

In referring hereafter to the blood in scurvy, which has been more thoroughly investigated by physiological chemists, it will be seen that nothing satisfactory is known of the chemical constitution of this fluid even in this malady.

The red, violaceous or blue spots, so far as I have seen, are not as some writers affirm, sub-cutaneous, but sub-epidermic externally, and sub-serous and sub-mucous internally. In yellow fever sub-epidermic extravasations of blood are generally traumatic, as from cupping, blisters, sinapisms, bites of insects, etc. Sub-cutaneous extravasations often infiltrate the areolar or cellular, the sub-serous and the sub-mucous tissues; also, the sub-conjunctival epithelium, the arachnoidiän sack, etc.

I might, did space permit, enumerate cases of *purpura hæmorrhagia*, evidently the sequelæ of severe organic diseases in their advanced stages. A well known apothecary of New Orleans, whom I treated for a complication of cardial, pulmonary, and hepatic diseases, had for some months before death (in Cuba), anasarca, slight albuminuria and well marked purpura.

## ii. SCURVY.

The above case of K., is suggestive of scurvy, and a few observations on that allied disease will be subjoined on this occasion, as well for comparison as for the purpose of illustrating the general doctrine of the *molimen hæmorrhagicum*, etc. The hygienic antecedents of this patient as to diet and the physical comforts were, however, the antitheses of those which originate scurvy.

My experience, however, in scurvy is, I confess, very limited. Some years ago I treated a negro slave, who, long confined in jail, had lived, as I was informed, chiefly upon salt fish and the like. His gums were swollen, offensive, bleeding and of a leaden hue; his legs were painful and tumid; skin hot; breath fœtid; pulse hard;—in a word, his symptoms were sthenic; his scurvy acute or inflammatory. He was bled from the arm (x to xii oz.); the blood had the appearance usually considered inflammatory—it was fibrinous, capped, and had a dense buffy coat, and had little serum. Had he suffered much and long from hæmorrhages and debility, the blood would probably have been very different in appearance, particularly in reference to the relative proportions of fibrin, serosity, clot, etc., all of which, however, is but conjecture.

The subsequent treatment, consisting of a purgative, suitable diet, and improved hygienic conditions, was, in a short time, followed by a restoration to health.

Vogel says: "The blood in scurvy readily induces liquefaction of the tissues (ulcerative destruction) *without any trace of reëction or inflammation.*" (Path. Anat. 86.) That this view does not invariably hold good, the above case, both in its symptoms and treatment, shows clearly enough. On consulting a high authority, after having written the above remarks, I find my opinion confirmed at the conclusion of the following quotation, which is otherwise so important that the whole passage will be reproduced.

Professor Lehmann, in his recent work on Physiological Chemistry (1855), says: "The blood has not been examined with accuracy in scurvy, and its composition has therefore been deduced principally from physical relations; thus, for instance, its imperfect coagulation led to the conclusion that it exhibited a diminution of fibrin, while other causes led in the same manner to the supposition that there was an augmentation of the salts, the few investigations of scorbutic blood which we possess, give but little idea of the true constitution



of this fluid in the condition which we term scurvy. Becquerel and Rodier have been led by their most recent analyses, which, however, are not very conclusive to adopt the opinion that 'the essential anatomical character of scurvy must be sought in an original modification of the fibrin,' while they show that there is an *increase of the fibrin in the acute form of the idiopathic disease*, 'depending upon an excess of the soda salts in the blood.'" (i. 636.) Another highly respectable authority, Dr. Simon, in summing the analyses of the scorbutic blood, says, "These analyses are sufficient to disprove the general notion that in this disease the corpuscles are dissolved in the serum. In the blood taken from these scorbutic patients, the separation into serum and clot was as perfect and took place as rapidly as in healthy blood. In two cases [out of three] the clot was buffed and cupped." (*Chemistry of Man*, 258; anno 1846.)

The diagnostic differentiations between purpura and scurvy, if not in every particular salient, clear, and completely satisfactory are nevertheless, sufficiently so at present to prevent the confounding of them, as was the case generally during former times.

The almost exclusive but improper use of salted animal food during long voyages and in camps, is considered to be the chief cause of scurvy. Nevertheless, land scurvy, when suitable diet has been attainable, cannot be explained in this manner, examples of which, however, appear to be very uncommon. Purpura, on the other hand, occurs among individuals who enjoy the luxuries of life. Although purpura is comparatively rare, it is probably diffused over all varieties of climate. Scurvy has usually prevailed most in cold latitudes, and presents a high ratio of mortality, which, however, has been greatly reduced—indeed, almost extinguished within the current century, owing to the progress of hygiene, dietetics and therapeutics.

Without attempting to analyze the mortality statistics of scurvy, it may not be amiss to note from the Mortality Statistics of the United States, the deaths from scurvy in this republic for the year ending June 1, 1850: Missouri, 18; Alabama, 2; Arkansas, 2; California, 1; Delaware, 1; Louisiana, 4; Maryland, 1; New Jersey, 1; New York, 4; Ohio, 5; Pennsylvania, 1; South Carolina, 3; Virginia, 4; Territory of New Mexico, 5; Iowa, 3. Whence it appears that Missouri furnished a mortality from this cause of 18, while that from the ten Southern States reached only 14. New Mexico, consid-

ering the sparseness of its population, gives the highest relative mortality, its population being 60,000 upon an area of 207,000 square miles—a larger territory than that of France. In the official report of the City Inspector of the city of New York, for the year ending December 1, 1854, scurvy and purpura are classed together as giving a mortality of 30 for the year; the report of the preceding year assigned 11 deaths to scurvy.

According to the medical statistics of the U. S. Army, 40 deaths occurred among the troops from scurvy, from the years 1840 to 1854, exclusive of the years of the Mexican war (1847, 1848) “it having been found impossible to collate correct statistics of those years.”

It appears from this work, that in a command of 4,450 troops upon the southern frontier of Texas, in 1849, no less than 510 cases of scorbutus occurred; three of which proved fatal. (369.) “On the Rio Grande the troops have suffered from scurvy to a very great extent.” (401.) Assistant Surgeon McParlin, says, “the troops in various portions of New Mexico have been afflicted with scurvy.” (429.)

If mankind are not grateful for the benefits bestowed upon them by medicine, they should, and perhaps would be after a thorough retrospection of the recorded accounts of the dreadful ravages by scurvy which desolated the world, during the four centuries anterior to the present, but which now, thanks to science, desolate no more.

M. Ozanam says that the first information we have concerning scurvy is found in the history of the expedition of Thorstein, who, as the chief of the Normands, embarked for Greenland in 1002. (*Hist. Méd.*, T. iv.) In the crusade of 1248—in Scandinavia in 1431—in Germany in 1481—in Misnie in 1486—in Calcutta in 1498—in the fleet of Cartier on his voyage of discovery to Canada in 1535—in Silesia in 1591—in Ulm in 1632—at the siege of Breda in 1636-7—at Derbyshire, England, in 1679—at Belgium 1678, in the first settlements in America, not to name many other places, including fleets and camps in which it prevailed most disastrously during the seventeenth century. The history of the ravages of this disease during the last century is comparatively well known. The simple yet effectual hygienic preventive and remedial measures of the present day have saved multitudes from this miserable and fatal malady.

“Even as recently as the time of Lord Anson, scurvy was so fatal that during the first two years of his voyage he lost more than four-

fifths of his original crew. The improvements which, at the suggestion principally of the late Sir G. Blane, were introduced into the victualling of the navy at the end of the last century, especially the free employment of lemon-juice, have banished this disease from our navy, though it is still by no means infrequent in the merchant service." (*Nat Cyc.*; x, 746; London.)

Anson's voyage took place in 1740. The mortality from scurvy which took place in his squadron has been generally referred to as the greatest ever known. But Sir Gilbert Blane, the eminent physician above referred to, mentions an example which occurred in 1720, in which, owing to improper treatment, every one of 400 affected with sea-scurvy died. (*Med. Logic*, 246.)

The mortality from *land-scurvy* in the neighborhood of New Orleans, in 1809, has probably never been paralleled in the annals of epidemics. Dr. Jabez W. Heustis, of the United States Army, in his work on the Topography and Diseases of Louisiana, himself an eye witness of what he has recorded, says, that the division of the army, a force of two thousand, encamped at Terre-aux-Bœufs, fifteen miles from the city, in the months of June, July, and August, 1809, lost by scurvy one thousand, that is half of its number. (89 *et seq.*) Although only one hundred and fifty men died at the camp of Terre-aux-Bœufs, two hundred and fifty died on the passage up the river to Washington (Mississippi), and six hundred after having reached their destination. "They were *forty days* in ascending the river, before they arrived at Washington, which, at that time afforded a healthy retreat"—(97,) a speed of less than ten miles per day.

The consternation produced by this terrible mortality gave rise to a special Committee of Inquiry in the House of Representatives which reported at great length upon its reputed causes. Dr. Heustis says: "One of the principal causes of this pestilential disorder was, undoubtedly, the use of unwholesome provision. The pork, which constituted the principal part of the animal food with which the soldiers were supplied, was old and stinking; the little beef which they occasionally had, was lean and meagre in the extreme, and so saponaceous, glutinous and adhesive, that it would stick to any substance with which it came in contact. Their flour was also in a spoiled condition; having been wet, then becoming mouldy, black, and full of worms, and so firmly agglutinated together, that when the barrel was separated from its contents, the cemented mass retained

the shape given to it by the eask, and stood firmly erect like a block of wood; and such was its solidity, that an axe and other instruments were necessary to cut it up and pound it, so as to reduce it to a state of flour."

A striking contrast might be drawn between the past and present hygienic conditions, medical treatment, and ratios of army mortality in regard to scurvy; this will be sufficiently illustrated by a few data taken from the recent report of the army statistics, which are reproduced here principally to show the treatment of scorbutus which has been adopted by the medical officers, particularly in the Southern division.

Assistant Surgeon Israel Moses (1854), of the Ringgold Barracks, (lat. 26° 23' 17" N.) Texas, reports that "from the entire absence of vegetable food, *scorbutus* manifested itself in many cases, although only fifteen are reported, of whom one died. The remedy made use of was the bicarbonate of potassa, in twenty grain doses, three times a day; opium grs. ij, every four hours, and a wash of tinct. cinchonæ et myrrhæ. Two cases were treated with the juice of maguey with benefit; but in others I was obliged to discontinue its use, on account of its cathartic action on the bowels. Vegetables, oranges and limes (whenever they could be procured), were given freely." 360.

Assistant Surgeon Glover Perin (1852), of Fort McIntosh (27° 31' N., Texas), says of that Post: "Owing to the great drought which prevails here, succulent vegetables cannot be procured for the command, and *scorbutus* is of frequent occurrence. In its treatment I now use exclusively the expressed juice of the agave Americana, and it is not usual for the soldiers to go upon the sick report. [The use of the expressed juice of the agave Americana was first suggested by Assistant Surgeon Perin. The preparation, by the direction of the Surgeon General, has been tried at several stations in Texas, and the reports are such as to establish the fact of its being a valuable remedy in *scorbutus*. The recently prepared juice is preferable to the extract, which seems to lose strength by time.]" (*Med. Stat., U. S. A.*, p. 361. Washington: 1856.)

In 1851, Dr. Perin, in a report to Surgeon General Lawson on the treatment of scurvy by the juice of the maguey (a plant, by the way, which flourishes in New Orleans), says: "The manner in which it is used is as follows, viz: the leaves are cut off close to the

root; they are placed in hot ashes until thoroughly cooked, when they are removed, and the juice expressed from them. The expressed juice is then strained, and may be used thus, or may be sweetened. It may be given in doses of from two to eight ounces, three times daily. It is not disagreeable to take, and in every instance it has proved to agree with the stomach and bowels. After the leaves have been cooked, the cortical portion may be removed, and the white internal portion may be eaten. It appears to be wholesome and nutritious food." (See Dr. Perin's Communication, pp. 361-2-3.)

Assistant Surgeon Crawford, in reporting upon scurvy at Fort Croghan, Texas, says (1852) "that having been disappointed in the effects of citric acid, he came to the conclusion that as the maguëy plant had been highly spoken of, the same virtues might be found in a greater or less degree in the whole family of *cacti*; and he was therefore induced to try the common prickly pear (*cactus opuntia*), which was abundant near the fort. The result was highly satisfactory. It was prepared 'by removing the thin epidermis from the thick succulent leaves, and, after cutting them into slices, macerating them in water. This furnished a thick solution, which was given as a drink to the patients.'" (401.) Assistant Surgeon Anderson gave with good effect, nitrate of potash, in this disease, at Fort Terrett. The reticence of the army doctors in regard to the pathology of this malady is remarkable.

As already stated, a bad diet, particularly one consisting chiefly of salt animal substance is either the direct or predisposing cause of this malady, according to the testimony of the best observers. Dr. Ferrier, in his Medical Histories (vol. iv), records a number of cases in which an exclusively animal diet prescribed for the cure of diabetes caused, as he affirms, scurvy, in the course of five or six weeks.

The following curious case from the second volume of Percival's Essays, published nearly a century ago, is significant and sufficiently allied with the scope of this paper to justify its reproduction: "A young lady aged sixteen, tall, thin, and of a delicate constitution, though in tolerable health, was advised to use sea-water on account of a strumous swelling and inflammation of her upper lip. She drank a pint of it every morning for ten days successively: which did not pass off by the usual evacuations. At the end of this period she was suddenly seized with a profuse discharge of the catamenia, was perpetually spitting blood from the gums, and had innumerable petechial

spots on different parts of her body. Her pulse was quick, though full; her face pale and somewhat bloated, and her flesh soft and tender. She was often faint, but soon recovered her spirits. The flux from the uterus at length abated; but that from the gums increased to such a degree, that her apothecary took a little blood from her arm. From the orifice blood continually oozed for several days. At last a hæmorrhage from the nose came on, attended with frequent faintings, in which at length she expired, choked as it were with her own blood. Before she died, her right arm was mortified from the elbow to the wrist. And it is further to be remarked, that though blood let from her some weeks before she began the use of the seawater, was sufficiently dense; yet that drawn in her last sickness was mere putrid and dissolved gore." (118-19.)

The pathological chemistry of the blood in purpura and scurvy is too little known or too equivocal to be available in diagnosing either of these maladies, much less their pathognomonic differentia. The same uncertainty has hitherto veiled their microscopic history.

Professor Vogel, a little more than a decennium ago, in his work on Pathological Anatomy, says: "All our knowledge respecting the chemical changes of the blood has been acquired during the last few years, and although it seems to explain several important points, it is still far from being in a complete and satisfactory state. (77.) It is very difficult, indeed impossible, to draw any certain conclusions respecting the changes of the separate constituents of the blood, or the causes of such changes. In fact, our whole knowledge respecting the physical and chemical changes of the blood is in the highest degree unsatisfactory, and the statements of different observers vary so widely, that it is impossible to deduce any general laws from them." (86.)

Taking as a fundamental point of departure for biology, the blood corpuscle or cell, a presumption arises favorable to the theory that the same structure may be the seat of morbidity, and that its pathological changes may be of equal potency with the more obvious coarser lesions recognized in pathological anatomy, thereby filling a hiatus of frequent occurrence, namely, the absence of such structural changes as may readily be ascertained by unaided vision—changes which explain the nature of a disease and account for its fatal termination. A kind of correlation exists, or is suggested upon analogical principles, between cell-life and cell-death. The term cell therapeutics is

already planted in the vocabulary of several writers upon pathology, though scarcely warranted by positive discovery in that behalf. There is apparently an inherent obscurity in the nature of life, disease, and therapy, not yet dissipated. It is, however, humiliating to the high claims of science, to force into its vocabulary, terms, however clear and positive in themselves, when their objective or concrete realities are not yet known as absolutely valid. Many diseases and many remedies, judging from the precise language in which they are set forth, would seem to be thoroughly explained, while, in fact, little is known concerning them, the *word* being exact and luminous in proportion to the dubitation and darkness of the *thing*. This is demonstration reversed—a philological usurpation, not a philosophical explanation.

No histological, physical, microscopic, chemical, physiological, or pathological analysis or natural history of the blood is sufficiently known to serve as the basis for the classification, pathology, or cure of diseases. In a very few instances the normal ratios of the elements of the blood are found to vary, but these diversities of red and white corpuscles, of serum and fibrin, of albumen and salts, concerning which the highest authorities often disagree, the *clinicien* seldom finds sufficient to guide him in writing a prescription. Lime-juice is almost a specific in scurvy. But neither the micrologist nor chemist introduced it into practice, nor has either given its rationale to this day.

### iii. REMARKS ON HÆMORRHAGES.

In atonic or feeble condition of the vital forces, hæmorrhage may possibly occur from exosmotic exudation independently of active hyperæmia or sthenic morbidity. This effusion, it may reasonably be assumed, will take place from tissues or organs where either vital tonicity is minimized or where the physical, anatomical, and physiological structures and functions are most favorable to leakage, and therefore oppose the smallest resistance to exosmotic transmission.

In yellow fever the developement of the hæmorrhagic diathesis is sudden, and secondary, boding no good. The individuals, however, the most susceptible to yellow fever, being the robust, present in general the very antithesis of that which is usually considered the hæmorrhagic diathesis, which is characterized by debility, atony, relaxation, cachexy, dyscrasia, hydræmia, etc. But here as in many other uncertain pathological reasonings, exceptional facts present

themselves which cannot be referred to any known general rule. Thus, it sometimes happens that violent hæmorrhages occur and subside without appreciable antecedent or subsequent disease. Such cases are sometimes witnessed in epistaxis.

Active hyperæmia, doubtlessly is the antecedent of hæmorrhage in many cases. In the primary stage of yellow fever, from twenty-four to thirty-six hours from the invasion there is injection of the skin, eyes, mucous tissue of the mouth, nose, etc., which probably paves the way for the subsequent sanguine effusions that so often occur towards the closing stage. It is difficult to account for the impulse or *molimen hæmorrhagicum* which in this atonic stage of the general circulation, strongly injects the deep seated cellular tissue of the intermuscular spaces, as the popliteal, pectoral, etc. Sometimes effusion under the great pectoral muscle amounts to pounds. Probably the capillary engorgement, and the great attendant heat of the early stage, weaken the tone or integrity of the capillaries and other tissues, and thereby favor these effusions, though the vis à tergo may be feeble.

In the typhoid, and particularly in the typhus fevers which I encountered a quarter of a century ago, in Western Virginia, hæmorrhages apparently the most alarming, occurring from the first to the third week very generally had a favorable termination. These hæmorrhages sometimes occurred in patients who had not used mercurial medicines. Judging from a limited number of cases it appears probable that these hæmorrhages, which usually proceed from the nose, gums, mouth and throat are either critical, that is, salutary, or at least they are apt to be followed by recovery, although the patient may be too weak, without aid to clear his organs from accumulating blood and clots. A reliable clergyman (Mr. L. S.), who lived remote from medical aid, informed me that his child, a young woman, suffered from this fever for three weeks without having had a physician, when a copious hæmorrhage from the mouth took place. Her family considering the hæmorrhage a certain sign of speedy death, began to prepare her burying clothes, but were soon agreeably surprised to find evidences of recovery, which progressed in a satisfactory manner from that time.

The term *crisis* of disease as used by Hippocrates denotes a termination or change and generally a favorable one, supposed to take place on certain days or definite periods—a doctrine almost entirely



or at least too much ignored in modern times. This crisis was attended with hæmorrhages, perspirations, and so forth. Some of these cases will best serve to show his pathological doctrines.

In the third book of Epidemics, Hippocrates says: "In Lariffa, a maid was seized with an acute burning fever, attended with a want of sleep, thirst, a fuliginous (or sooty) dry tongue, and urine that was well colored, but thin. The second day she was uneasy, and got no sleep. The third had several watery stools, and the following days the like, without fatigue. The fourth the urine was thin, a little in quantity, with an elevated cloud that subsided not. A delirium at night. The sixth, *she bled very freely at the nose*; shivered a little; sweated plentifully and hot all over; and the *fever came to its crisis*. But in the course of the fever, and upon the *crisis* happening, her menses came down then for the first time, she being a young virgin. She was all along qualmish, subject to horrors, red in the face, and had a pain in her eyes, with a heaviness in her head. The crisis happened without a relapse." Cox's Epitome, 368.

In the first book of Epidemics (year III) the Father of Medicine has generalized his doctrine of crisis as illustrated by the beneficial effects of spontaneous hæmorrhage, which, however, is more applicable to Western Virginia than to New Orleans. He says: "In the beginning of the spring burning fevers come on, and continued to the equinox, and even to the summer. Most of those escaped who were seized after the beginning of the spring and summer, and some few died; but when the autumn and wet weather set in, they proved mortal to many. These fevers were of such a nature, that when any one *bled freely and plentifully* at the nose, *he was saved by it* more than by anything else; and not one of those who were taken thus died this season, so far as I know. For Philiscus, and Epaminon, and Silenus, bled but a few drops at the nose the fourth and fifth day, and died. Most of them were seized with shiverings about the crisis, especially where there had been no hæmorrhage, and with the shivering came on a sweat about the head and shoulders. Others again were attacked with a jaundice the sixth day, and these were relieved either by a discharge by urine or stool, a plentiful hæmorrhage, as Heraclides was, who lived with Aristocydes. Not but he bled at the nose, and had the benefit of other evacuations too; and so was freed the twentieth. It fared otherwise with the

servant of Phanagoras ; for, as none of these things happened to him, he died. Hæmorrhages were very frequent, especially among young persons and adults ; and, where nothing of this kind happened, it very often proved fatal." (Coxe's Epitome, 332.)

Active hyperæmia, increased momentum of the blood, and a lost or diminished tonicity of the capillary system, would seem adequate to account for most hæmorrhages and sub-epidermic efflorescences called petechiæ. Some of these supposed precursors, obtain, more or less in yellow fever, particularly in the first stage—a stage in which there are, as already mentioned, active congestions of the skin, great heat, and often excitement of the general circulation. In this stage, however, hæmorrhage is rare ; but these antecedent conditions may have so debilitated the capillaries, that notwithstanding the simultaneously diminished force of the circulation, these vessels may be relatively much weaker. And hence, the effusion may take place from the capillaries, whose tonicity is impaired, not only positively but in a ratio much greater than that of the general, though less depressed condition of the cardiac circulation and vital forces.

Necræmia, "death beginning in the blood ; death of the blood," is naturally suggested by the intolerable fœtor, purple or dark hue of the blood in purpura hæmorrhagica and scurvy, as well as by the coincident loss of vital energy in the general system. This bold term (attributed to Dr. Williams) might very naturally have been suggested by Hunter's firm reëffirmance of the vitality of the blood ; for Moses had affirmed the same physiological doctrine more fully and without any reservations. Mr. Hunter says: "This living principle in the blood, similar in its effects to the living principle in the solids, is the *materiæ vitæ diffusa*, giving to both the power of preservation, the susceptibility to impression, etc.; the blood has as much the *materiæ vitæ* as the solids ; it is not clear to me whether the *blood dies* sooner without the body, or the body without the blood," etc. *Hunter on the Blood*.

If pathological alterations of the blood, such as leucocythæmia, hæmitis, etc., be allowable, necræmia may, upon analogical principles, be provisionally accepted, although its ætiology and diagnosis remain undetermined.

## IV. PETECHLÆ IN YELLOW FEVER.

In a paper entitled *Contributions to Morbid Anatomy* published in *The Western Journal of Medicine and Surgery*, for October, 1843, I made the following statement: "I think it by no means ridiculous or unimportant to mention certain appearances, called *petechiæ*, which, if not strictly lesions of the skin in the living, and in the dead subject, are in my opinion, generally neither more nor less than *Mosquito petechiæ* or *echymoses*. These are found only on such parts as are exposed to these insects. These are of no small value as indicants of a hæmorrhagic diathesis or tendency. The same may be said of the dark purplish discoloration which is apt to appear to a considerable extent around the nearest orifice in the arm in venesection.

"Mosquito-petechiæ I have met with in several intense forms of remittent and congestive fevers during the present month (July 13, 1843), every exposed part of the body being thickly dotted with circumscribed, red spots, seldom as large as a grain of flaxseed. These dottings occur sometimes in bowel affections, as diarrhœa, dysentery, and cholera morbus. In healthy infants, recently born, the mosquito-bite causes spots difficult to be distinguished from those above described.

"Pringle, in his work *on Diseases of the Army* (262), says that *petechiæ* appear thickest upon the breast and the back, less on the legs and arms, and adds, 'I do not remember to have observed any upon the face.' In mosquito-petechiæ the contrary happens. If the patient's shirt be of very thick linen (as in the Charity Hospital of this city), no spots will be found on the trunk, except at the opening of the bosom, etc., unless the body be exposed as in delirium.

"De Haen nearly a century since characterized typhus as a petechial fever (*febris petechialis*) and it is now considered by Louis and Chomel as possessing no other pathognomonic character. Since pathologists attach so much importance to petechial eruption, it is desirable to discriminate its varieties as accurately as possible. It will be found in the South at least, as I incline to believe, that those spots called *petechiæ* are owing to the bites of insects in almost all instances. The advanced stage of fever, the prostration of the vital forces, and hæmorrhagic tendency, are circumstances generally connected with these appearances. These bites, however, seem to produce no [very] uneasy sensations as in healthy persons: upon

the latter, mosquitos sometimes produce irritating effects not a little formidable," etc.\*

The above description applies chiefly to the so-called petechiæ of yellow fever patients. Since this account was published (now sixteen years), exceptional cases of petechiæ, somewhat similar, yet apparently not caused by mosquito-bites or other traumatic lesions have been occasionally seen in yellow fever, and still oftener in the typhus or ship fever of immigrants from Europe. But even among the latter petechiæ are too rare to be considered characteristic of typhus in the almost tropical climate of New Orleans. Typhus, petechial, or spotted fevers, though common in Europe, as well as in the Northern States of this Republic during and also since their early settlement, have rarely if ever appeared in New Orleans, except in ships from northern latitudes during the last twelve years.

#### V. BLACK VOMIT PETECHIÆ OF THE MUCOUS MEMBRANE IN YELLOW FEVER.

The great length to which this paper has already attained admonishes me that I must not trespass much longer upon the reader's patience. I will, therefore, only allude to a MS. paper of forty pages written many years ago, on black vomit, giving a few paragraphs concerning the above-named lesion, although the whole paper would be necessary to show the facts and reasonings which have led me to the conclusion that black vomit is neither blood nor bile effused into the alimentary canal, and subsequently changed by acids. The lesion itself, as described in the sequel, isolated from all collateral or corroborative evidence, may appear insufficient to warrant this conclusion so contrary to that of many great authorities. These writers, who generally have no personal experience in yellow fever, speak of black vomit as the resultant of effused blood and an assumed hydrochloric acid, which two, meeting, mixing and combining, form this black pigment. The theory is hypothetical, the hydrochloric acid a myth as to black vomit in New Orleans, though any painter can make from a great variety of substances, black liquids or pigments resembling all the varieties of color witnessed in the melanic excretion, the *vomito negro*. Those who make yellow fever black vomit artificially (accessories

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\* On referring to my MS. vol. IX, I find the following note made during the yellow fever epidemic of 1841. "*Petechiæ from mosquito bites.*—These have a redish purple circle around them like small ecchymoses, the blood infiltrating the spots; but these spots will not be found on those parts of the body not exposed to these insects. The patients' declarations that these spots were mosquito-bites, led me to examine, and conclude that such was the case. These spots are common in bad cases.

after the fact) prove too much seeing that healthy men, civil or savage, also animals, that eat blood-puddings, raw meat, or suffer from internal hæmorrhages, ought to have this black vomit matter three times a day, provided blood and hydrochloric acid are its essential elements, as these experimenters assume that this acid exists in all stomachs.

In Virginia, as well as in New Orleans, I have had patients who discharged matter resembling black vomit, particularly in fatal cases of puerperal peritonitis, in fevers, in gastritis, in dysentery, etc. In these cases, which are extremely rare, I have never made any experiments with this pigment (which often like black vomit, resembles the pigmentum nigrum of the eye), nor have I made in these cases post-mortem examinations, and can, therefore, say nothing of their identity or differentiæ.

The internal black vomit petechiæ (to be mentioned hereafter) of the mucous membrane, as well as the pigment ejected from the stomach or bowels, vary in color. But whether the former and the latter in each individual case, coincide, I cannot say with certainty. The ejected fluid is often mixed with blood, and almost always with other accidental matters, as mucus, saliva, food, drinks, acids, or medicine. The pure liquid itself is generally bland and inodorous, particularly when excreted and ejected rapidly and in great quantity without any tinge of blood or other accidental admixture, and the retained fluid is also found after death, not to present the physical appearance of blood in not a few cases. These accidental substances often change the characteristics of the black vomit liquid before death, and give rise afterwards to the most offensive putrefactive condition, although its color and general appearance remain as indestructible, perhaps, as charcoal; at least such seems to be the case after the lapse of nearly twenty years. The flakes, concretions, and aggregations of ejected black vomit, take place in the stomach and bowels after it reaches their cavities and mingles with their contents.

If micrologists, instead of conflicting in their accounts of black vomit, agreed that this fluid is blood, or its broken down corpuscles, etc., they might possibly, with equal pains, find the same in *melanosis*, whether idiopathic or symptomatic, not to mention many diseases, and hæmorrhagic ones too, in which melanosis does not appear at all. These explanations fail to show, either during life or afterwards, anything in the blood of yellow fever patients peculiar to that disease in which alone black vomit is characteristic or common.

As melanotic products, liquid and solid, have been found in various tissues where no hydrochloric acid has ever been suspected, an analogical argument arises that such liquid when found in yellow fever, might originate in like manner, independently of this acid. Kolliker says, "sulphuric acid renders the blood black-brown. Hydrochloric acid which colors the blood brown, produces a white precipitate. Nitric acid renders the blood olive-brown. A solution of potassa containing ten per cent. makes the blood black. Caustic soda and caustic ammonia act like the corresponding potassa-solution." (*Microscopic Anatomy*. Dr. DaCosta's edition.) "A white precipitate" is very unlike black vomit sediment which is the darkest portion of the liquid.

The chemical character of melanotic matter, according to Thenard and later chemists, is that of carbon. The normal coloring matter of the negro's skin—that found in the sub-serous tissue of the lungs and in the bronchial glands—in the choroid coat of the eyes may or may not originate in the blood, and so of certain morbid pigments, but in neither case does such reference afford any satisfactory explanation of their nature.

The œsophagus and the lower portion of the ileum may be, as well as the stomach, the seat of the black petechial dottings under consideration, though these organs are more or less remote from the hydrochloric acid supposed to be in the stomach.

Many years ago, in order to ascertain the proportional frequency of this black petechial infiltration of the mucous tissue, I examined the first one hundred and fifty-eight yellow fever autopsies, whereupon the following result was arrived at, and will now be given, as I have not leisure to search further at present among the more numerous autopsies of later years: In these one hundred and fifty-eight yellow fever dissections, a number was made before I noticed this very delicate appearance, and, after noticing it, I was for sometime in doubt about its character, erroneously supposing, that these black dottings extended no deeper than the mucosity which adheres to the inner surface of the intestines; finally, not attaching importance to this lesion, I did not in every instance carefully wash and examine the intestines with such minute attention as to enable me now to say positively that nothing was overlooked. Omitting descriptions in which the mucous tissue was designated as being simply dark, but not otherwise materially altered in the site of this lesion, I find it fully noticed in sixteen cases. Of these, seven were more or less circumscribed,

without, however, resembling the elliptical patches of Peyer, which are much developed, occasionally, in almost all diseases, as well as in typhoid fever ; at least such is the case in this climate. In the nine remaining autopsies it occupied much of the mucous membrane, particularly of the small intestine.

The mucosity of the intestines, the epithelium and inner stratum of the mucous tissue may be removed, as already stated, without removing these petechiæ which are planted in the submucous cellular, and, therefore, deeper stratum. Hence it may be assumed that these apparent dottings are really cylinders having their longest diameter perpendicular to the plane of this tissue.

The small intestine, with the duodenum, is perhaps, oftener the seat of this lesion than the stomach. The œsophagus is more rarely affected. The mucous membrane in which these punctiform dottings prevail, are not otherwise necessarily changed or different from other cases as regards vascularity, consistence, etc., excepting color.

Hæmorrhagic punctuations, arborizations, ecchymoses, and button-shaped extravasations are seated also in the deep cellular stratum, but differ from black vomit petechiæ not only in color, but in configuration. The latter are regular and very minute; the former of various sizes and shapes, some being dense clots of blood of considerable magnitude, having broad bases towards the muscular coat.

The black points of infiltration constituting this lesion, are distinctly isolated, yet closely crowded together, being extremely minute, and from this cause likely to be neglected or unnoticed. At first view they seem to be seated *on* the mucous tissue, but are really deeply embedded *in* it, and cannot be washed away or scraped off without disorganizing the membrane. This imbedded position of these countless millions of minute punctiform infiltrations, in no wise like sanguineous punctiform injection, is a significant fact, notwithstanding its comparative infrequency. Whether black vomit matter be a melanoid fluid *sui generis*, carbonized blood, or something else, its position indicates that it is arrested *in transitu* towards the hollow organs, whence it is ejected or retained towards the close of life. Its infrequency in post-mortem examination may be owing to its complete passage into these cavities before death, thereby leaving the mucous tissue in not a few cases blanched to an unnatural degree, forming a strong contrast, for example, between the color of the stomach and its black contents. Melanosis is a species of morbidity concerning which

the most eminent histological pathologists agree in one particular, namely, that its nature is altogether unknown, and to this unwelcome category, may be referred the melanotic fluid resulting from yellow fever. It is no explanation to say it is altered blood, seeing that the nature of this alteration is unexplained by any of the artificial mixtures by which black pigments may be manufactured.

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ART. II.—*A View of the Pathology of Diabetes, founded chiefly upon an Inquiry into the Physical, Physiological and Chemical Nature of Diabetic Sugar*: By JOHN GOODMAN, M. D.

[An Inaugural Dissertation submitted to the Faculty of the Medical Department of the University of Louisiana, for the Degree of Doctor of Medicine. Session of 1858-9. To my esteemed friend and preceptor, DR. L. ROGERS, of Louisville, Ky., this Thesis is respectfully inscribed.]

THE term diabetes is derived from a Greek word, signifying to pass through, and was formerly applied to all disorders in which the most prominent symptom was an increased flow of urine. But, as medical science advanced and our knowledge of the nature of diseases became more exact, it was ascertained that the urine not only underwent a quantitative, but also a qualitative change from its normal condition, and in certain cases (especially the most virulent), an entirely foreign substance, viz: sugar, was present. This led to a distinction into two kinds of diabetes—diabetes *insipidus*, in which the aqueous principle of the urine was augmented, with or without a variation in the absolute amount of its normal ingredients,\* and diabetes *mellitus* or saccharine diabetes. But the pathology of the two diseases was so widely different, that it was thought inexpedient to retain names suggestive of a similarity in their nature; accordingly the former term has been almost entirely abolished (the disorder being now designated chronic diuresis) and the saccharine variety alone is recognized as diabetes. Even thus restricted, the term has been sub-divided to keep pacc with the march of discovery,

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\* Bright's disease in certain stages constitutes a form of diabetes, in which albumen is present.



into idiopathic and symptomatic diabetes ; and that we are warranted in doing so there is every reason to believe. For while idiopathic diabetes, or diabetes properly so-called, has an origin, progress and symptoms peculiar to itself ; symptomatic diabetes occasionally makes its appearance during the course of various affections, such as diseases of the brain and spinal cord, of the digestive apparatus and liver, but is not attended with any of those violent symptoms which mark the other variety. An evanescent form of the disease, which may be classed under the last species, is sometimes induced by certain medicinal agents—quinine, arsenic, etc.; these probably act by deranging the nervous system.

It may easily be inferred, from what has been said, that the pathognomonic symptom of idiopathic diabetes (of which alone we intend to speak) is the persistent appearance of sugar in the urine. This fact led Dupuytren, Thenard and others to consider the kidneys as the seat of the morbid action which gave rise to the disease. But this cannot be ; for it has been conclusively shown that the sugar exists pre-formed in the blood, and the kidneys are only *one* of the emunctories by which it is eliminated. It was discovered by McGregor in the stomach, saliva and fæces—by Simon in the perspiration—by Dr. Francis in expectorated matter—by Dr. Gibb in the lachrymal secretion—by Dr. Londerer in the fluid of ascites, and in the pus of abscesses by Frick. This general distribution throughout the system could not take place unless it was carried thither by the circulation. As to the organic changes noticed in the kidneys after death, such as hypertrophy, congestion, infiltration, etc., we would only say that none of them are constant, and may all be accounted for as arising from determination of blood to them, caused by the diuretic action of the sugar or as secondary affections from debility. Additional evidence may be deduced from the fact that no sugar can be detected in their substance.

The question, then, in regard to the pathology of this disease resolves itself into this—What is the morbid action which gives rise to an abnormal quantity or quality of sugar in the *blood*? Hence the term glucœmia is occasionally applied to it. But before proceeding to attempt its solution, it will be advisable to take a brief survey of the ground to be gone over from a physiological point of view and consider the origin and destiny of the saccharine principles in the human organism during health.

Sugar may be said to arise in the animal economy from three distinct sources : 1stly. That which is taken in as such with the food; 2dly. That which is formed by the action of the saliva upon amyloseous substances; and 3rdly. That which is generated by the liver.\* The first of these is so palpable that a mere mention of the fact is all that is necessary. In regard to the second, however, it may be well for us to enter further into detail ; but even this has been so often discussed and so thoroughly substantiated of late, as to make a minute examination of it superfluous. That sugar is thus created may be shown by a very simple experiment. If, for instance, we add a small portion of starch to an ounce of saliva and keep it for twenty-four hours at a moderately elevated temperature, the starch will have entirely disappeared, and in its place we shall find glucose mixed with dextrine, which may be rendered evident by the ordinary tests. The substance exerting this catalytic influence upon fecula was extracted from the saliva by Mialhe, and from its resemblance to the diastase of malt he styled it "animal diastase."† It has its maximum of action in the presence of water at from 70° to 80° cent, and is capable of converting two thousand times its weight of starch into glucose. It is now looked upon as the debris resulting from the different glandular structures concerned in the secretion of saliva.

This process of transformation of starch into sugar probably begins while the food is undergoing mastication, and is continued to some extent after it reaches the stomach ; here, however, it is soon arrested by the acid it meets with in that organ,‡ but is, no doubt, allowed to resume its course, and also receives assistance from the pancreatic fluid, after the chyme has passed the pyloric orifice and become neutralized by the alkaline secretions of the intestine.

The agencies to which saccharine matter is subjected and the changes it undergoes, during its passage through the alimentary canal, have not been definitely ascertained. Some physiologists hold that it is partly converted into fat by the pepsin of the gastric juice—others that this metamorphosis is accomplished by the bile. But, be this as it may, the point which most concerns us and which is

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\* There are others, such as *lactose* in the mammary secretion, and *inosite* in the muscular tissue. But the former of these does not concern us in the present inquiry ; and of the latter nothing definite is known.

† Also called Ptyaline.

‡ Since hearing Dr. Hunt's lectures upon saccharine digestion, my opinion on this point has been materially altered. Instead of the gastric juice arresting the transformation it seems probable that it is capable of accomplishing it.

undoubtedly a fact, is, that the greater portion of it is absorbed unchanged by the venous radicles of the portal system, where it is found most abundantly about five hours after meals. That the lacteals do not take part in this absorption has been clearly pointed out by Bernard ; for, although he found sugar in the thoracic duct it was not until the lymphatics coming from the liver had emptied into it. The lacteals directly from the intestines contained none. Thus we see the sugar from the two first sources is ultimately subjected to the influence of the liver, and what this influence effects, we will proceed to inquire.

When an animal has been fed upon any particular variety of sugar—cane-sugar, for example—the presence of the same variety may easily be demonstrated in the portal blood. But if we examine the blood again as it emerges from the hepatic veins, after having traversed the liver, the cane sugar will have entirely disappeared and in its place be substituted a saccharine substance differing from it in certain of its properties, to which the name “Hepatic Sugar” has been given. The question then arises, is this the modified cane sugar or is it a newly created product? To a certain extent both these queries may be answered in the affirmative. Bernard has not only shown the liver to possess this modifying influence over cane and grape sugar, but that it is capable of generating the hepatic sugar from other principles besides the saccharine and amylaceous, most probably from the protein compounds. We have a precedent for assuming the possibility of the formation of sugar from nitrogenized bodies, by physiological forces, in the secretion of lactose by the mammary glands of carnivorous animals. All the sugar from the alimentary canal, however, is not converted into hepatic sugar by the action of the liver, but a portion of it is converted into fat ; the two transforming processes varying according to the nature of the ingesta and the demands of the system. In herbivorous animals, whose food contains a large quantity of amylaceous and saccharine matter, the transformation into fat predominates ; while in the carnivorous, which have already a supply of fat in their food, little or none is generated by the liver.

We will not pretend to detail the steps by which Bernard was led to discover this glucogenic function of the liver, as our limits compel us to be as concise as possible, but simply to note the manner in which it may be verified.

As has been already mentioned, when sugar is taken into the stomach the same species may be detected in the portal vein. The converse of this has also been found to be true. If, for example, an animal be confined for a while strictly to a meat diet, and pains be taken to obtain the portal blood pure, no saccharine matter can be found in it, but at the same time there will be an abundance of the hepatic variety in the hepatic veins and from thence upwards in the ascending cava. When Bernard first published these discoveries to the world, his experiments were made upon dogs from whose diet amylaceous substances had been excluded only for a few days, and it was objected by some opposed to such innovations, that the liver might have retained the sugar in its tissues as it is known to retain arsenic and other metallic poisons. Whereupon he kept them for six weeks upon flesh with equally satisfactory results. In addition to this, sugar has been discovered in the blood of hibernating animals after their winter sleep had continued for months, and even in that of the fœtus; showing beyond a doubt that it is secreted *de novo* in the system. Other investigations were made to ascertain if the oleaginous constituents of the blood were subject to this transformation, which led to the conclusion that they were not; hence he inferred the azotized, either directly or indirectly, alone were.

But this was not all the same physiologist brought to light upon the subject; for he showed the glueogeny of the liver to be as distinctly under the influence of nervous force as the secretion of tears is, and, that when the afferent fibres of the pneumogastric nerve were stimulated, either by puncturing the nerve itself or by applying chemical irritants, the impression was conveyed to the medulla oblongata, whence it was reflected to the liver along the great sympathetic and excited the organ to secrete sugar to such an extent that it appeared in the urine, constituting artificial diabetes. The normal stimulus of this function he supposed to be the oxygen of the inspired air, acting upon the branches of the pneumogastric distributed to the lungs; but that he was correct in this has not been clearly proved. I think Mr. Harley is most probably right in attributing it to the influence of the portal blood upon the nerves of the liver.

Very little is positively known in regard to the destiny of the saccharine principles in the human body, and as an exact knowledge is not of vital importance to the present inquiry, we will speak of it quite briefly. According to Liebig they are first converted into lactic

acid, which, by its combustion, contributes to support animal heat. Others contend that they furnish to different tissues and organs the pabulum necessary for their nutrition. Neither is the place of their decomposition settled upon, for while Bernard and Pavy assert this to be the capillaries of the lungs, Chaveau and Hailey go to the other extreme—the capillaries of the systemic circulation. One thing which it is necessary for us to know and which it is admitted by all, is that it loses its identity as sugar in *some* part of the body and is appropriated in some way to maintain its vitality or integrity. In speaking of its capability of undergoing this change, we shall use the word *destructible*, which, although somewhat ambiguous, is not suggestive of any particular theory, and at the same time, is definite enough for all purposes to which we will apply it.

So far we have not said anything in respect to the distinctive characteristics of hepatic sugar, as we were desirous of speaking of it in connection with diabetic sugar, with which Bernard supposed it to be identical. In its chemical and physical nature it seems to be similar to glucose, having the same composition—rotating the plane of polarization to the right and reducing the salts of copper. It differs only in certain physiological properties, its most remarkable feature being the ease with which it undergoes the process of destruction, just spoken of in the circulation. According to Magendie, the following are the relative proportions in which the leading saccharine compounds must be injected into the blood, before they appear in the urine: cane sugar, 1; glucose, 50; liver sugar, 240. From this we would judge liver sugar to be 240 times as destructible as cane sugar, and five times more so than glucose.

This facility of decomposition is not possessed by diabetic sugar to so marked a degree and thus serves to establish a distinction between the two. Dr. Pavy's attention was first called to this fact by observing that sugar disappeared from the blood of diabetic patients more slowly than it did from healthy blood, and in order to satisfy himself upon the subject devised the following experiments: He took three vessels; the first containing a solution of *artificial* diabetic sugar (which is acknowledged to be genuine liver sugar), of a specific gravity of 1045; the second and third, respectively, solutions of grape and true diabetic sugar of spe. grav., 1040. Into each of these he placed an equal quantity of pounded liver (any other animal matter would have answered as well), and after they

had remained quiescent for nine days, submitted them to examination by Bareswill's test ; not a trace of sugar could be detected in the first vessel, but it was still manifest in the second and third ; thus showing that diabetic sugar and glucose were more capable of resisting disintegrating forces than hepatic sugar.

Upon instituting a comparison between diabetic sugar and glucose it will be found, that notwithstanding they resemble each other in chemical properties there is one circumstance which goes to point out a difference. It has long been noticed that during the course of diabetes, the urine occasionally lost its sweet taste, becoming perfectly insipid, while all the symptoms of the disease remained unmitigated. At first it was supposed the sugar had disappeared from the secretion ; but at length it was discovered that instead of disappearing it had only lost its characteristic taste. Now it is hardly presumable that there are two kinds of sugar from separate sources which can thus alternate with each other—it must be of the same variety and of the same origin with this slight change impressed upon it by a modification of the chemical or nervous forces concerned in its generation, and if so, it is evident that all reasoning which may be applied to the one is equally applicable to the other. So far as I am aware, there is no method by which we can render starch sugar tasteless without destroying its identity, or generate a tasteless sugar possessing all the other properties of glucose from amylaceous substances. Hence I think we may safely conclude that diabetic sugar possesses properties peculiar to itself, which distinguish it from grape as well as hepatic sugar. In this view I fancy I can draw support from the opinions of others. Thenard says, "I have had occasion to extract from the urine of a diabetic patient thirty-three pounds of tasteless sugar, so little sweet that it might be taken for a kind of gum. It dissolved in water ; and when in contact with yeast, fermented as well as any other kind of sugar : Whence it is evidently a kind of sugar though a distinct species." On the grounds above stated, although Thenard does not do it, *we* claim a right to extend this conclusion to the ordinary diabetic sugar.

These preliminary remarks will enable us to be more concise and to speak more to the point in treating of the pathology of diabetes. Many and conflicting, indeed, have been the theories devised to account for the cause of this strange malady ; and it would be utterly impossible for us, within the utmost limits to which we could think of

extending our thesis, to review even the most prominent of them individually. We shall, therefore, endeavor to classify those we think worthy of notice, and speak in as general terms as will be compatible with perspicuity.

First, let us direct our attention to those which refer the disease to the *primæ viæ*. There are various modifications of these, in fact almost every author who has written upon the subject has insisted upon making some innovations of his own. But all those advocated at the present day agree in ascribing it to the formation of inordinate quantities of sugar in the stomach, either owing to the presence of an abnormal ferment acting upon the feculent portions of the food, or to a lowering of the vital force, which allows the natural ferments to run riot among the starch, and prevents the conversion of the sugar thus formed into proximate principles. This last—the theory of *dialthesis*, is that advocated by Dr. Prout; but as the physiological views upon which it is founded conflict with those set forth by us in the outset we will not honor it with special attention.

Our objections to these as a class may be briefly stated. In the first place, the experiment of McGregor, by which he showed the existence of a larger amount of sugar in the stomachs of diabetic patients than in those of healthy subjects, and to which the advocates of this theory point with triumph, in my opinion bears no weight in their favor; in fact, if rightly interpreted, it belies the conclusion they deduce from it. This gentleman obtained by an emetic the contents of the stomachs of two persons, the one in health, the other diabetic, and both fermented, especially the latter. Again, “in order to avoid every possible source of fallacy,” as he said, he excluded all vegetable substances from their food, and confined them to roast beef and water for three days. Several hours after the last meal, he administered an emetic to each, and found that the matter ejected by the first contained no trace of sugar, while it was still present in that of the second. Now, if we will reflect that whatever exists in excess in the blood has a tendency to be thrown off by the various secretions, we may easily account for sugar being present in the stomachs of diabetics, as an excretion from the mucous membrane of that viscus along with the gastric juice. But, as I have intimated, the experiment of McGregor is absolute proof of this, for we know of no ferment which can convert nitrogenized compounds into sugar, and surely the diastase which Bouchardat and others profess to have discovered, if as

active as they say, or even the saliva would have had ample time to metamorphose all the amylaceous articles taken three days previously into sugar, and this in its turn would have had time to be absorbed. So we see it could have no other source than from the blood through the secretory glands of the stomach.

But again : the *abnormal* ferment which Bouchardat and Blondelot supposed they had detected in the alimentary canal of those suffering under this disease, has been shown by Bernard and Bareswill not to be abnormal, but a constant constituent of the gastric juice. For by simply rendering this fluid alkaline with carbonate of soda, it loses its power to dissolve flesh and becomes as active as saliva in the conversion of starch into glucose.

But suppose, for the sake of argument, from some cause or other, amylaceous substances are converted to too great an extent in the stomach into sugar. Let us follow it up and see what would become of it. It is first absorbed by the portal vein and carried to the liver ; here it must be permitted to pass unaltered (as Dr. Gibb has suggested), or be transformed into the hepatic variety and fat. If the former be the case, we have as much right to think cane sugar would escape that sentinel as grape (and in truth more, for it is of a higher order and more stable), and as it is less tolerated in the circulation, we would look for a medley of the different saccharine principles in the urine ; but this is not the case. And even allowing that the cane sugar is destroyed and the grape alone passes (or it would answer as well to admit that it is absorbed by the lacteals and does not reach the liver, as still is maintained by some), we have good reason to believe this does not constitute true diabetic sugar.\* But taking the other horn of the dilemma—suppose it to be converted into hepatic sugar, but in too large quantities to be consumed, then it must appear, in the urine, slightly modified into artificial diabetic sugar, which also we have shown not to be the case.

So much for the sake of argument ; now to make a long matter short, be it said, there is one objection to this view which never has been and never can be met satisfactorily, viz : That if the sugar be formed from amylaceous substances by the action of a ferment, why does it not *entirely* disappear when these are *entirely* prohibited

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\* We would have to assume two varieties of diabetes, in one of which sweet sugar was excreted by the kidneys, and in the other tasteless, and then go to work and construct a separate theory for the latter, for none but the sweet could come from this source.



as food? There are well authenticated cases on record, where the patients were placed in solitary confinement and kept rigidly upon an animal diet; without a total cessation of the excretion of sugar by the kidneys. I am aware that it has been evaded, by saying the patients might have escaped the vigilance of the nurses and obtained bread; but this is absolutely begging the question. As to there being any ferment in the economy either normal or abnormal, which can change flesh into sugar, I will not admit it until I have demonstrative proof.

The second class of theories to which we would refer, are those, which, without assigning any positive source to the sugar, that if proved incorrect would invalidate them, attribute the disease solely to an inefficiency on the part of the system to appropriate it after it enters the circulation, where it accumulates and acting as a foreign substance is thrown off by the various emunctories. Of this class, we may take the beautiful, but to purely chemical theory of Mialhe as the type. This is grounded upon the assumption that in diabetes there is a diminution of the alkalinity of the blood, and consequently an inability on the part of its saccharine ingredients to undergo those changes necessary for their final elimination. But this, his fundamental principle, cannot be substantiated experimentally; it having even been asserted by some that a contrary condition exists. The mistake probably arose from the fact that diabetic blood soon loses its alkalinity after being drawn from a vein, owing to the conversion of the sugar into lactic acid, which combines with its soda and potash bases forming neutral salts. As our time is limited we will not be able to notice others of this class individually; but since all of them, as stated in the definition admit the sugar to be fully formed and only excreted because it cannot be destroyed, it would necessarily appear as hepatic or rather artificial diabetic sugar in the urine; which, as we have said, *is not the case*.

Consequently we are disposed to dissent from Mialhe and Jones as well as Bouchardat and Prout. Then to whose doctrine will we give our adherence? To Bernard's? No, for some of the objections advanced above apply equally well against his. This physiologist, after having made the discoveries already cited, in reference to the glucogenic function of the liver, proposed to attribute the disease in question to a derangement of this organ, in which, owing to irritability of some part of the nervous system, it was excited to a

hypersecretion of sugar. This necessarily supposed diabetic sugar was the same as hepatic, and it was to test this point that led Dr. Pavy to the investigations which decided in the negative.

The only partial disappearance of sugar from the urine upon an entire abstinence from vegetable food, points clearly to the liver as its source, while the peculiar character of diabetic sugar goes to show, that owing to some abnormal stimulus either applied through the mediation of the nerves or contained in the portal blood, or an anatomical lesion too minute to be detected by any means at present in our hands, it is led to secrete a saccharine principle of a higher order than the normal hepatic sugar, which, owing to its less destructible nature cannot be assimilated by the system, and accumulating in the circulation is thrown off by the kidneys. If it be granted, as I think it must, that the two varieties of sugar—the sweet and tasteless—have the same origin and are essentially the same substance, sufficient evidence can be deduced from a consideration of it alone, to render this view probable, and taken in connection with what has been advanced, will amount to *almost* positive proof. In the first place, the tasteless variety is evidently a secretion, bearing some resemblance to the sugar of milk in its incapability of being formed by any but physiological forces and its insipidity, and as the liver is the only sugar secreting organ we know of (except the mammary glands), it must be referred to it. Secondly, the simple fact that the sugar undergoes this change, while it indicates the pathological action to be at its place of origin (for we can conceive of nothing which could effect such a change upon any known species of sugar after it enters the circulation), also goes to show that the generating organ is capable of taking on an altered action and originating different products, all of which cannot be perfectly normal; hence we are licensed to assume the possibility of its being perverted throughout the disease.

As to the ultimate cause of this morbid function of the liver, I do not think we have as yet, sufficient data to form even a plausible theory, and until this is ascertained few therapeutical indications can be deduced. At present I would only suggest two points, both of which accord with the results of experience. The one is that amylaceous substances be withheld as much as possible from the food for they no doubt furnish a pabulum for the genesis of the abnormal sugar. This is in direct antagonism to the plan of saccharine

treatment ; to which I object for the following reasons : Those who practice this method, argue that by far the greater portion of the sugar taken into the stomach is converted by the liver into oleagenous compounds, and its prohibition as food cuts off the supply of fat from the system. Now it has always been noticed that one of the most prominent symptoms of diabetes, no matter what may be the nature of the food, is emaciation, which consists chiefly in absorption of the adipose tissue, and when the blood is examined analytically its fatty constituents were found to be deficient ; thus resembling the blood of tuberculosis in which glucosuria often terminates. From this I think we may infer the generation of fat as well as that of healthy hepatic sugar to be more or less subverted, and in their place is substituted the genesis of diabetic sugar. The other is, that opium by restraining the secretory power of the liver would probably reduce the quantity of sugar generated ; or by allaying nervous irritability, if any exist, would tend to correct the pathological process.

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ART. III.—*Delirium Ebriosorum ; with Report of a Case and Remarks :*

By HUMPHREY PEAKE, M. D., of Yazoo City, Mississippi.

ON May 26, 1859, there came a man to my office and desired a prescription. He appeared to be a man of some intelligence, and told me that he had been drinking very hard during the preceding ten days, and that during the three nights preceding, he had been unable to obtain any sleep. Although not intoxicated at the time, he seemed considerably under the influence of liquor, the use of which I advised him to leave off. He wished something, he said, to make him sleep. He had a strong, full pulse, and his head was very hot, and he said, ached. I gave him the following prescription, with directions to take a tablespoonful every hour until bed time, or until sleep followed:

R	Antimonii tartarizati .....	grs. iv ;	
	Liquoris opii sedativi.....	ʒi ;	
	Syrup simplicis ;		
	Mucilaginis accaciæ.....	aa ʒi ;	
	Aquæ.....	ʒvi.	M.

On the following morning he again called upon me, and said that he had not yet had any sleep. He had taken some drams after having seen me on the day previous, and before calling, on the morning alluded to. I again urged the necessity of his leaving off drinking, which he had only done in a measure, and directed him to continue the mixture. I saw no more of him that day.

On the following morning, about eight o'clock, he called again. He was smoking a cigar, and had a wild look. Instead of appearing talkative, he seemed in a meditative mood. He sat assiduously working his handkerchief into a kind of knot, which was no sooner formed than undone. While his hands were busily engaged in this way, his mind seemed to be engaged with something else. I asked him a few questions, and from his answers, I soon saw that his mind was not clear. I said no more to him, but left him to his reflections and his handkerchief, while I observed him closely. In a short time, without raising his eyes from what his hands were engaged in, he told me that he had just been to the Masonic Lodge, and, that having become disconcerted in his examination, was refused admittance. He seemed very much hurt for this, and cried most touchingly. He repeated the story and cried again. After much persuasion, I prevailed upon him to go with me to his hotel, and lie down. I remained to see him undressed and put to bed, and during this time he became quite delirious. His delirium, however, was of a somewhat peculiar character. At one time, for a few moments, he prayed to be saved from the fangs of serpents which he imagined he saw. With this exception, he neither feared nor sought to do any harm, but prayed continually and in a loud voice. At times, he fairly screamed. His pulse was hard, though not very frequent; his skin hot, and his head literally steamed with heat. I now gave him one drachm of liquor opii sedativus. In two hours I returned and found him worse, when I gave him a drop of croton oil. This I ought to have done in the first place, *i. e.* before giving the dose of Battley's sedative. Within an hour I saw him again, and found him still worse. He had escaped from his room and came into the hall, where he was with difficulty secured and carried back. At this juncture I sought the advice of two medical gentlemen with the view of getting their concurrence in the propriety of bleeding him. While one of these consented to, but did not urge its propriety, the other objected. He now got an enema of tepid salt water, which in a short time brought away a large amount of fluid feces,

containing scybala. I then poured several buckets of cold water upon his head, and suffered it to run over his body, This had the effect of bringing him partially to his senses. He ceased screaming for a short time. He was wiped dry and put to bed, and given another drachm of the sedative liquor of opium. Before giving the drachm, however, I sought to quiet him with chloroform, but a few inhalations of the drug threw him into the most violent convulsion, wherefore, I desisted from pressing it to the point of producing insensibility or sleep. After having him wiped dry and put to bed, I held ice to his head for nearly two hours. For a while this promised well, but he soon became as bad as ever. I sought the advice of still another medical brother of excellent judgment and long practice. He objected to the bleeding and urged the further exhibition of opium. Trusting to his judgment, I gave another drachm of the sedative liquor. So within six hours he had taken about the value of eighteen grains of opium. His raving had so disturbed the neighborhood and persons of the hotel, that arrangements had been made to place him in the county jail, in care of the jailor. When the carriage came to convey him away, he was raving as bad as ever. I gave him a little chloroform, which produced a convulsion, but he ceased to scream when it passed off. When taken to the jail, however, he continued to pray for a while, in a subdued tone. He was restless too, and instead of lying down would grope about over the room. He could not steady himself erect, and acted much like a man too drunk to walk. The opium seemed to greatly stupefy him, for he seemed like a man half asleep, though he continued trying to go about. He remained much in the same condition until eleven o'clock, when the jailor left him for a while. At two o'clock he visited him and found him lying upon the floor, as he thought, dying. He immediately came for me, and by half-past two o'clock I saw him myself. He was a horrid sight to behold, for he was literally covered with blood, as was also a good part of the jail floor. My first impression was that he had fractured his skull by jumping or falling against the wall, and that his quietude was dependant on concussion of the brain. On examination, I found that he had fallen against the end of one of the bars of the door, and had received a wound three inches long in the forehead which extended in depth quite to the skull. The hæmorrhage had been very free. I found that he was actually in a deep sleep. I ordered that he should not be disturbed, but left as he was until he should awake.

At seven the following morning the jailor went up to see him. The noise of opening the door awoke him, and he raised up and conversed rationally. I soon called, and was delighted at the change. I ordered him a good dose of calomel, gamboge, and jalap, which moved his bowels several times during the day. Except when disturbed by this or otherwise, he slept all the while. In the evening he took some soup. He slept the whole of the following night. On the next day he was still farther improved. I ordered him diet and a few weak toddies during the day. He went on steadily improving and I discharged him yesterday.

*Remarks.*—When I visited the patient at half-past two o'clock, as stated above, the jailor asked me if I thought there was any chance of his recovery. He seemed much surprised when I informed him, that in my opinion the accident was the very best thing that could have happened him. Had I just such another case I should most certainly take blood from the arm, and even at the risk of being accused of concluding by the rule *post hoc ergo propter hoc*. I must remain of the opinion that the blood this man lost was of great advantage to him. Nay more, that in all probability it saved his life. I wished in the first place to bleed him because I thought it would quiet his excitement, and that during this calm the great desideratum, sleep, would supervene. He was a stout muscular man, and, with many of the symptoms of inflammation of the brain, could, I thought, spare the blood. Accident at least confirmed that opinion. The pain caused by this accident would, doubtless, have been sufficient to have prevented a well man from sleeping for hours. What shall we say, then, of the almost immediate supervention of sleep on such an accident? Is it not a legitimate conclusion that the sleep which almost immediately followed it, was caused by the amount of blood lost? And does it not follow, *à fortiori*, that the same effect would have followed the simple abstraction of a like quantity?

The strongest objection urged against the bleeding of this man was, that he had delirium tremens. True, this case was one of what generally passes for this disease. I have purposely named it a case of *delirium ebriosorum*, because I think it better to have a separate name for distinct affections, than to call two by the same name. I consider the two affections just about as different as day and night. The term *delirium ebriosorum* is not new. I have seen it somewhere but forget where. *Delirium ebriosorum* may be excited in any in-

dividual by the continued use of stimulating drinks. I have no doubt that in it the brain is characterized by a condition of hyperæmia. Delirium tremens is produced by the very opposite cause. I quote the first paragraph from Dr. G. B. Wood's article on delirium tremens: "This is the delirious affection which follows the suspension of the habitual use of alcoholic drinks or other stimulants." Now this man had not suspended the use of stimulants, but on the contrary was seized during their continued and excessive use. Delirium tremens, moreover, is no doubt characterized by an anæmic condition of the brain. It is a disease characterized by exhaustion and may supervene in persons not accustomed to drinking, during the latter stages of fevers, bronchitis, pneumonia, hepatitis, and other affections, while delirium ebriosorum, as the very name implies, never does. There is another fact worthy of a thought. These persons who get delirium tremens by being deprived of their accustomed stimulus, are generally the poor and low order of people—at least those who are not able to gratify their desire for want of means. Hence we frequently see them in Hospitals. The victims of delirium ebriosorum, on the other hand, are generally found in the middle or better ranks of society, where the means of procuring drink are not wanting. The indications of treatment are as opposite as the causes of the two affections. To treat antiphlogistically in delirium tremens is to kill as certainly as to treat by active stimulation is to destroy life in delirium ebriosorum, but these modes being reversed the true indications will be fulfilled. In delirium tremens restore the stimulant in moderate quantity, and give opiates, and in the generality of cases your patient will recover, while in delirium ebriosorum you may give antimony and even take blood from the arm or head. There is one grand object to be kept in view in both cases, i. e. the restoration of sleep. If you do not get your patient to sleep he will die. Now it is a well known physiological fact that the same set of symptoms may depend on opposite conditions of the brain. For instance, convulsions may depend on a hyperæmia of the brain, or on the lack of blood in the vessels of that organ, as in the convulsions with which animals die when bled to death. A certain degree of torpidity of the blood vessels of the brain favors sleep. Hence, it sometimes happens that by a long and arduous day's labor, either mentally or physically, we have so far expended our nerve force as to become "too tired to sleep." A glass of brandy, which

is a stimulant, puts us to sleep at once. At other times men by indulging too freely in drink, find themselves unable to sleep, and with, perhaps, some headache to boot. A cup of strong coffee, which is a sedative in this case, will soon procure sleep. The man who has delirium tremens is, in an exaggerated degree, in the condition of the man who is "too tired to sleep," while he who has delirium ebriosorum is, in a corresponding degree, like the man who, from having taken too much liquor, finds himself with a headache and unable to sleep. In delirium tremens we might kill by giving opium in very large doses, from its sedative effect, for it is to be remembered that the stimulant effect of opium is, within certain bounds, in an inverse degree to the size of the dose. Just so, on the other hand, we might do great harm by giving opium in small doses in delirium ebriosorum, where we wish a sedative, and not a stimulant effect.

It seems to be a physiological law that the omission of an accustomed stimulant is in effect equal to giving a sedative, and vice versa. In the former case the nerve force falls below the artificially established vital standard, and in the latter it rises above it. Thus we find persons who are in the habit of drinking spirits before going to bed, unable to go to sleep when they omit taking their usual drams, without which they would be somewhat in the condition of a person "too tired to sleep." And again, when we remember that coffee is a sedative, and not a stimulant, as many consider, we are enabled to explain why many persons get headache by omitting to drink their accustomed coffee for breakfast. We can also understand why a dram makes this headache worse, as many have learned from experience. By reflecting further on this subject we may also see why, when one has drank so much coffee at night as to prevent him from sleeping, a good drink of some stimulant spirit will soon produce sleep. By generalizing these facts we may at once understand the great difference between delirium tremens and delirium ebriosorum.

In answer to those who may ask why I did not bleed this man if I believed so strongly in its propriety, I answer, that I sought the advice of medical brethren who are, perhaps, better informed than myself, and, that they opposed it.

YAZOO CITY, MISS., June 3, 1859.



## NOTES.

1. Celsus maintains that delirium is relieved by profuse hæmorrhage from a varix, or from opened veins, or from the bowels: "*At varix ortus, vel per ora venarum subita profusio sanguinis, vel tormina, insaniam tollunt.*" (l. ii.)

In another book he quotes the opinion of the Asclepiades who consider bloodletting as murderous in delirium; but he adds as his own opinion, that robust patients ought to be bled: "*Quod si vires ægri patiuntur, sanguis quoque mitti debet.*"

The difficulty and the necessity of sleep, he states in the most absolute manner: "*Omnibus vero sic affectis somnus et difficilis, et præcipue necessarius est: sub hoc enim plerique sanescunt.*"

2. The Med. Stat. U. S. A., contains the following classifications: *Delirium Tremens—Ebrietas.*—The deaths from 1840 to 1854 (1847–8 not returned)—a period of thirteen years, are given thus: Delirium tremens, 98; ebrietas, 53. B. D.

ART. IV.—*Parotiditis — Suppuration — Recovery:* By J. THOMAS RUSSELL, M. D.

DR. BENNET DOWLER: In the May Number of the *N. O. Med. and Surg. Journal*, I notice a letter from Paris, by Dr. S. J. Bigelow, in which, after relating some "queer" and "rare cases," he concludes, "I cannot but prognosticate a fatal result under the most favorable circumstances, from a complication of acute parotiditis occurring during or at the close of any acute affection, like pleurisy, pneumonia, typhoid fever, peritonitis," etc.

During a practice of five years I have met with two cases only, in point. One, a complication of pneumonia, which I merely saw while under the treatment of a physician, the patient dying after a painful lingering of two months. The quantity of pus that escaped was wonderful, and the cavity a disgusting sight. The latter was a case to which I was called in November, 1858. The patient was a young man of twenty-two years, just off from a long fatiguing trip from

Alabama, suffering with all the dread symptoms of typhoid fever. He was of a nervo-bilious temperament, and previous to the attack was a stout strong man. The characteristic symptoms were—great vital depression; nervous prostration; persistent muscular weakness; mind flighty; delirious paroxysms; the pathognomonic eruption black; crusted and cracked tongue, etc.

The following remedies were invoked, which proved happily successful, namely: castor oil, two drachms; spirits of turpentine, fifteen drops, given *pro re nata*, to keep the bowels open; valerian, xanthoxylum, ginger, in equal parts made in infusion—dose, a half or a wine-glassful every two hours; also spirits of turpentine, five drops; olive oil, eight drops on lump loaf sugar—take three times per day. These, with the usual common-place agents, made up the treatment.

In two weeks he was conscious that that length of time had been blocked out, as a mere segment, from his memory. It was with difficulty that he could be persuaded to a knowledge of the fact. At this stage, both of his parotid glands began to swell and harden. The right directly subsided, but the left increased prodigiously and soon formed an enormous bulk. It progressed on slowly to suppuration. The pus finally made its exit through the ear. I waited a few days, during which time purulent matter still escaped. Finding the fluctuation yet deep-seated, and apprehending some danger from delay, I made three incisions through the thick, indurated integuments, which made ample passage for the contents of the abscess—it being large, incredibly so to the look, and its contents extravagantly great. For this reason I deemed it all-important to make large openings.

The patient's condition was much improved after this free discharge. The metastatic swelling of the testes, the glands themselves, together with the sacral and trochantral sores were all immediately ameliorated. The rapid improvement with which he met, soon restored him to his usual health.

Besides the treatment instituted for the fever, quinine, tinct. hydrastis can. were given. The abscess was washed with alkaline baths, followed by burnt alum lotions, ointments and flannels applied to the parotids to prevent metastasis. He is now in fine health, and weighs about one hundred and seventy-five pounds.

The above treatment for typhoid fever, I must acknowledge, and I do it with pleasure, has *universally* been successful with me. And my practice has been pretty fair for a village doctor. Of course I do not

treat all particularly alike, reserving the right to treat symptoms as they arise ; but the same in the general.

But to the subject : Were the *bolstering* of *strength*, the *economizing treatment*, the *timely opening* of the abscess, or some climatic, meteorological conditions, the causes which effected the good result ? I *think* the two first undoubtedly did *good*.

ALMA, TEXAS, June 20, 1859.

ART. V.—*Typhoid Pneumonia.*

MONTICELLO, ARK., May 27, 1859.

DR. DOWLER—*Dear Sir*: I take this liberty of addressing you, and giving a short account of a very malignant and fatal form of typhoid pneumonia, which has prevailed in this section of our southern country.

About the latter part of last November, after a mild form of intermittent and remittent fever had subsided to a considerable extent, which had been prevailing through the summer and fall seasons, typhoid pneumonia set in in a most alarming form, and which carried off many of our most valuable citizens, despite of all the skill that could be brought to bear by the many eminent and skillful physicians of our country.

We have had a very warm winter, comparatively speaking, but very changeable in temperature; one day as warm as summer, the thermometer as high as 80°; and the next day as cold as winter, when compared by the sensible effect produced by the change in the weather the thermometer as low as 40°; the system never being prepared for the change. This kind of weather set in about the middle of November last, and about the time above stated, the pneumonia commenced its ravages among our people. In the majority of the cases the premonitory symptoms would be a slight chill, followed by a slow fever, quick and corded pulse, and a partial derangement of the mind, producing at times almost a constant comatose condition; when the patient was aroused, and then only, he was partially rational; a slight cough, without any pain or apparent soreness about

the chest, and the cough did not in all cases present itself at the beginning of the disease. In this condition the disease would run for a few days, more or less. When all the aggravated symptoms of peripneumonia or pleuro-pneumonia, as the case might be, and frequently combined, carrying on, as it were, a constant warfare of extermination on the muscular, nervous and mental systems, killing at least one half of all who were attacked. In some cases, all the aggravated forms of typhoid pneumonia were set up with the premonitions of the disease.

It is needless for me, in a letter, to try to enumerate the various modes of treatment, for it would take a volume of no ordinary size to do so, and it would be of no practical use; for I am well satisfied, that the doctors of this country, with myself, have tried all the remedies that we have ever read of, and many that has never been put in practice, and all to little purpose, and we are not prepared to say what has been the best treatment, if there was any difference; and what is more strange is, that it has proved most fatal among the strong and robust subjects which is not in accordance with the past history of typhoid diseases.

Omens, preludes, or forerunners are things little believed in, in our day; but there is one thing we do know, that a certain cause will produce a certain effect, and there was last year, and has been presented this year, one effect (the cause is not for me to tell) that was never known to occur before so far as I have been able to trace back. The appearance of rust in the oat was never known before last year, and it is well known that the crop was destroyed throughout the South, and it has made its appearance again this year, entirely destroying the crop. Wheat is coming off too early to be damaged by the rust.

It is a well settled fact, that there was more sickness last year in the South, carrying with it more fatality, than any year we have any knowledge of. Now, if a certain cause will produce a disease in the vegetable kingdom, is it not plausible and probable that the same cause will produce disease in the animal kingdom?

This detail, and the few suggestions that I have made, I submit to your inspection, and if you think them worthy you can publish them. Hoping to hear from some scientific man on the subject,

I remain yours, etc.,

DR. J. N. SLEMONS.

ART. VI.—*Case of Suppression of the Menses, Treated by Cauterization:*

By S. E. WINNEMORE, M. D., Benton, Lowndes Co., Ala.

JUNE 10th. Called to see a negress, æt. 30, mother of four children, age of youngest about thirteen. Found that for several days she had complained of headache, and had done several things that led her mistress to believe she was laboring under temporary insanity.

When I saw her the following symptoms presented themselves: Coldness of the extremities, hands and arms; pulse very quick, 180; nervous, with violent fluttering or palpitation at heart; tongue very foul, with a clean spot in the middle; bowels constipated; mind much disturbed—sometimes delirious, at other times would relapse into a sullen condition from which no threats could rouse her to talk, or take medicine or drinks of any kind; respiration at times hurried, then again scarcely perceptible.

On inquiring into her uterine function, I found that she had been irregular for the last three months. I had her purged freely, and put upon quinine with anti-spasmodics by mouth and per ano. After watching the case closely for several days and finding but little amelioration of nervous symptoms, I made an examination per vaginam. I there discovered a chronic enlargement of the neck of the womb, with a yellowish white discharge, together with a slight ulceration of the os uteri.

I concluded at once that this was a case of hysteria, complicated with derangement of the liver and bowels, with every other day fever. The hysteria in my opinion being caused by the suppression of the menses. As she was within eight days of her *expected* returns I determined to cauterize the os uteri. I did so, using a solution of 30 grs. of nitrate silver to  $\bar{3}$ i of water, applied by means of a sponge through a speculum. I made two free applications, and on proceeding to make the third, I discovered the normal discharge instead of the leucorrhœa, which discharge continued for three days, relieving her of all the nervous symptoms. Her mind became clear; pulse came down to the normal beat, and she had no more returns of the every other day fever, but continued to improve until the 28th of June, when I ceased visiting her.

My idea in cauterizing her was to endeavor to bring on the discharge, believing that the ulceration was the effect of the engorgement, and that the engorgement was the result of the suppression. I thought that the *modus operandi* of the nitrate of silver would be

that of a stimulant to the vital properties of the tissues of the womb. My opinion I found to be correct, and I firmly believe (and it is only for this reason I publish this case) that, if in these cases of chronic suppression of the menses, an examination of the womb by the *speculum* would be made, much *less medication* and more cauterization would be resorted to. I do not wish to be understood to recommend cauterization in all cases of suppression of the menses, but I do honestly believe that in cases of chronic enlargement with or without ulceration, cauterization would be the main dependence.

It is not my intention to report this case *in toto*; my object is mainly to call the attention of the profession to the use of the *speculum* in these cases.

Before closing this article I must mention that the day after I first saw the patient, the circumstances of the case were such as to have the advice of another member of the profession. I called in Dr. Sprott, a physician in whose judgment I have the utmost confidence. He believed with me, that from the condition of her symptoms, the insanity depended upon a deranged condition of her liver, stomach and bowels. He saw the case only once; as I remained with her and watched the symptoms more closely, I was induced on the 4th day of her attack to change my opinion and attribute to the condition of her womb (which until then had not been examined), the main cause of all her nervous and head symptoms. And the result of the treatment has shown my opinion to be correct.

As her bowels seemed to be constipated, I prescribed the following mixture:

R Decot. aloës comp. f..... $\bar{3}$ iv;  
 Mist. ferri. co..... $\bar{3}$ vi;  
 Fiat. mistura, sumat cochl.....ii; bis die.

ART. VII.—*Notes of Cases from the Dublin Lying-in Hospital*: By W.

A. McPHEETERS, M. D., of Natchez, Mississippi.

CASE 1ST.—*Craniotomy*.—J. W., æt. 38, delicate frame, entered the Hospital July 18th, 1858, in labor. The pains commenced about

three, A. M. She had had seven children, four of whom were born dead. At half-past eleven, A. M., the os uteri was fully dilated, and the membranes ruptured. The pains became violent and frequent, but at half-past two, P. M. the head (which presented in the first position) had made no progress, still being high up at the superior strait. Dr. McC., the resident accoucheur, decided to apply the forceps. The patient was accordingly chloroformed, and the hips brought to the edge of the couch, she lying on her left side with the knees drawn up towards the abdomen. The forceps were applied over the ears of the fœtus and traction made in the axis of the pelvis. The forceps slipped a little, were withdrawn and reäpplied, but without success. The forceps were then withdrawn and the vectis tried, but with the same result. It was then determined that craniotomy was the only resource, which was resorted to with little reluctance, as the foetal heart could not now be heard. The cranium was accordingly perforated with Smellie's scissors, and its contents evacuated. The crotchet was then introduced, the point being guarded with one hand and traction made with the other, and in a few minutes the head descended and the fœtus expelled without difficulty. The placenta soon followed and the uterus contracted firmly, a dose of ergot having been given, just as the fœtus was expelled. The labor was terminated about half-past three, P. M.

This patient recovered without a bad symptom and was discharged on the ninth day.

CASE 2.—*Breech Presentation—Scarlatina.*—C. G., æt. 26, primipara, entered the Hospital at eleven, A. M., August 6th, 1858. On examining *per vaginam*, the os was found to be fully dilated and the membranes apparently unbroken; a foot could be felt through the membranes. A short time afterwards a quantity of meconium passed, which showed that the membranes must have been ruptured, though a pocket could still be felt over the presenting part.

At twelve o'clock a foot protruded through the vulva and the toes of the other could be seen during uterine contraction, also, the scrotum which became very much congested. At one, P. M., the uterine contractions being feeble, ergot ʒss, was administered. At this stage titillating the foot of the fœtus would cause it to move it, thus giving unmistakable signs of life; but at about two, P. M., this sign having ceased, it was thought prudent not to wait for any further dilatation of the soft parts, so Dr. B., the assistant, drew down the

feet, and proceeded slowly to extract the body and head. The occiput was to the pubes and the face toward the sacrum; after the body was delivered and the arms brought down, a finger was introduced into the mouth of the child and the back carried up towards the abdomen of the mother; the face was delivered first while the occiput turned under the arch of the pubes. There was a slight laceration of the perinæum as the head passed out. The child was alive, though its vitality was very low, and it required a warm bath and friction with spirits, before it breathed freely. The placenta was expelled in a few minutes and the uterus contracted firmly.

*The cuticle peeled off the legs and feet of the child during the delivery, and, afterwards off the whole body; a very unusual occurrence when the fœtus is alive.* This patient did well till the fourth day, when she was attacked with fever and became very nervous. On the next day, August 11th, her pulse was 96, tongue and fauces red, and a rash appeared on her body, showing that she had an attack of scarlatina. Solution of citrate of ammonia, a tablespoonful every two hours, was prescribed as a diaphoretic.

August 12th. Sixth day after delivery. Lochial discharge ceased; pulse 104; tongue very red, and the eruption more extended; very nervous. The citrate of ammonia appeared to affect the bowels, and was discontinued to-day.

R Confect. aromat.....gr. v;  
Pulv. cretæ comp.....gr. x. M.

To be given if bowels are moved again. Milk and lime water, port wine, 4 oz., during the day.

Seven, P. M. Pulse 104; tongue red and slightly dry; fauces very red; bowels moved twice since morning; complains of violent pain in the abdomen, and flinches on slight pressure, but not on firm pressure, showing that the pain is not owing to inflammation. Port wine four ounces, and negus to be taken during the night; and the following anodyne draught to be taken at once:

R Battley's sedative.....gt. xx;  
Sulphuric ether.....gt. xxv;  
Aromat. confect.....grs. xx;  
Aq. cinnam..... $\bar{3}$ ss; M  
Ft. haust.

Warm poultice over the abdomen. The pain subsided after taking the draught.

August 13th, ten, A. M. Tongue red but moist; skin hot; pulse



ninety-four and full ; no pain on pressure over the abdomen. A dose of sol. morphia was given early this morning. Port wine nauseated her : ordered Sherry wine four ounces, to be taken during the day ; arrow root. This is the third day of the eruption and seventh after delivery.

August 14. Tongue less red and moist; no sore throat; eruption disappearing. She took, last night, two ounces of wine and the following draught:

R Sol. morph.....(gr. j to f. ʒi) gtt. xxv ;  
 Vin. ipecac .....gtt. xv ;  
 Mixt. camph.....ʒij. M.

For to-day ordered wine, three ounces; arrowroot.

August 15. Doing well. Tongue moist and less red; pulse 86; redness of skin nearly disappeared. Ordered port wine, three ounces, to be taken during the day; chicken broth.

The child had an abscess of the mamma a few days since, but is now quite well.

This patient was discharged from the hospital on the 21st of August, the fifteenth day after delivery, perfectly well.

This case presents several points of interest : 1st. The child was born alive, which is contrary to the general rule in breech presentations of primiparæ. However, it would most probably have perished in this case if timely assistance had not been given to aid the efforts of nature. 2d. Scarlatina is usually considered a very grave complication of the puerperal state, but this case recovered in a short time under the use of the stimulating plan of treatment. 3d. It is probable that the child had an attack of scarlatina *in utero*, from which it was convalescing when born, as evinced by the scaling off of the cuticle, etc.

CASE 3.—*Complex Labor—Presentation of Head, Hand, and Funis.* M. M., æt. 32, robust, entered the hospital on the morning of the 15th of August, 1858, to be delivered of her first child. The pains began at twelve o'clock the previous night, and at nine, A.M., the os uteri was almost fully dilated. On examination *per vaginam*, the head, left hand, and funis could all be felt. The pulsations of the funis were strong and normal. It was evident that the child must inevitably perish unless artificial aid was rendered. Hence, it was resolved by Drs. McC. and B., that an effort should be made to push up the hand and funis, and if that was impracticable, to proceed at once to bring down

a foot and make version. Accordingly, the patient having been put under the influence of chloroform, and placed in the usual position on the left side, Dr. B. introduced his right hand into the vagina, and failing in his attempt to push up the hand and funis, he went in search of the feet, and, after some little difficulty, succeeded in grasping and bringing down the right foot according to Simpson's rule of bringing down the foot on the side opposite to the arm presenting. After the foot was brought to the vulva, it was determined to desist from further tractions, and let the uterine contractions perform the rest, so as to give the soft parts time to dilate. The pulsations of the funis were still strong, and there was no occasion to hasten the delivery. After waiting about an hour, the foot was found to be receding, instead of advancing, and the pulsations of the funis were becoming weak, so that efforts were then made to accomplish the delivery by making traction on the foot, and at the same time endeavoring to push up the head, but after making repeated efforts, and using all legitimate force without any effect, and the pulsations of the funis having ceased, it was determined to desist for a while, to allow the patient to recover from the exhaustion, and take some wine and arrowroot, and then to perforate the cranium of the child which was still within reach. Accordingly, after waiting about half an hour, Dr. McC. introduced the perforator and evacuated a portion of the contents of the cranium, then again making violent tractions on the legs, the head slipped up and the breech came down. A blunt hook was then passed up into the mouth of the child, and the delivery readily accomplished. The placenta was expelled in a few minutes, and there was no hæmorrhage.

This was a case of extreme difficulty. The left leg must have caught behind the head and prevented its rotating, which could not have occurred, had both legs been brought down at first instead of only one. This patient recovered without an unfavorable symptom, and was discharged on the ninth day.

CASE 4.—*Forceps.—Puerperal Mania.*—M. L., æt. 35. Primipara; vertex presentation; first position; first stage of labor 26 hours' duration; second stage sixteen hours. The pains being feeble, ergot ʒss. was given at two, P. M. At a quarter past three, P. M., it was deemed advisable to apply the forceps, as frequent experience has proved that the fœtus will be stillborn if not extracted within two hours after ergot has been administered. The forceps were accordingly

applied by Dr. B., and the head extracted with considerable difficulty. The difficulty was at the outlet, probably owing to ankylosis of the sacro-coccygeal articulation. The funis was very short, and so brittle that it broke while removing the child a few inches from the vulva. The perinæum was lacerated by the expulsion of the shoulders. The child, a girl, was asphyxiated, but was resuscitated by the usual means, rolling it from back to side, *à la* Marshal Hall, frictions, warm bath and spanking the buttocks, etc. The blades of the forceps produced considerable contusion with abrasion of the cuticle on the side of the child's head.

Second day. Pulse 104 per minute; slight tenderness on pressure over hypogastrium. Bowels moved twice during the night, from medicine taken before delivery; tongue clean.

R Pulv. Doveri.....gr. xx;  
 Pulv. Jacobi.....gr. ix. M.

Ft. pulv. No. iij ; one every four hours. Cloths dipped in hot water to be applied to the abdomen.

Evening.

R Pulv. opii.....gr. j;  
 Pulv. Jacobi.....gr. vj;  
 Hydrarg c. cretâ.....gr. iv. M.

Ft. pulv. No. iij ; one to be given every four hours; turpentine stupe to the abdomen.

Third day. Slept well; pulse 84.

R Pulv. camphor ; pulv. opii comp., aa ʒss. M.

Ft. pulv. No. x ; one every three hours. Sherry wine, two ounces.

Fourth day. Pulse 104 ; Bowels moved. Continue powders as yesterday; wine two ounces; turpentine stupe to abdomen ; vagina swollen and sloughing ; to be bathed with warm water several times a day.

Fifth day. She was seized with puerperal mania at eleven, A. M., to-day, and became very violent, jumping out of bed, and running out of the ward. After getting her to bed in a private ward, she refused to take any medicine, but was forced to inhale chloroform, which quieted her, and as she was recovering from its effects, she was made to swallow a draught of antimonial wine, and solution of morphia.

Evening. Appears to be quite rational, and answers intelligibly, but in an excited manner.

R Vini. antimonialis.....gtt. xl ;  
 Battley's sedative .....gtt. xxx. M.

To be taken at once.

6th day. Pulse 96 ; slept several hours during the night, but while awake was restless and incoherent ; appears quite rational ; says she has no pain except in the right shoulder, of which she has complained for several days. Has had it on previous occasions. There is no redness of skin ; shoulders to be stupefied and covered with flannel. Diet : arrowroot with two ounces of wine.

Evening. Pulse 104 ; tongue dryish and coated ; abdomen tympanitic, but not tender on pressure.

R Sol. morph. acetat.....(gr. i to f. ʒi) gtt. xxxv ;  
Vini antimonial.....gtt. xxxv ; M.

Take at once. Warm water stupe to vulva.

7th day. Quite rational ; pulse 104 ; skin hot, but moist ; tongue less coated.

R James's Powder.....gr. ij ;  
Camphor.....gr. iij. M.

Take at once. To-night repeat draught of sol. morphicæ and vini antimonial.

8th day. Much improved ; allowed to sit up a little to-day ; wine two ounces ; arrowroot and beef tea. To take to-night the following :

R Vini antimonial ;  
Battley's sedative, aa gtt. xl.

9th day. She was anxious to get home to-day, and was accordingly discharged this morning. The sloughing of the vagina had ceased, and the wound nearly cicatrized. The child was sent off when she was attacked with mania.

CASE 5.—*Forceps Case—Puerperal Fever.*—B. D., æt. 27, primipara, was admitted into the hospital, September 9th, 1858. The vertex presented in the second position. The labor began at seven, A. M., and at two, P. M., the os uteri was fully dilated. The contractions of the uterus being feeble, enemata of salt and water were given. These increased the force of the pains, but the head did not advance, but remained stationary at the inferior strait. At about half-past ten o'clock, P. M., ergot ʒss. was administered. This did not produce the desired effect, so at eleven, P. M., Dr. McC. proceeded to apply the forceps. Chloroform was first administered and the urine drawn off. The patient being on her left side, the first blade was applied over the ear nearest the pubes. It was used as a vectis but without success. The other blade was then applied and the head slowly extracted, allowing time for the perinæum to relax. The child, an

unusually large boy, was easily resuscitated and bore no marks of the forceps. The placenta was expelled in a few minutes and the uterus contracted firmly. There was no hæmorrhage.

September 10th. Pulse 124 ; tenderness over the hypogastric region on pressure. Twenty-four leeches to seat of pain and the following draught to be given every two hours :

R Tinct. ferri chloridî.....gtt. x ;  
 Acidi hydrochlor.....gtt. ij ;  
 Mist. camphoræ.....ʒiij. M.  
 Ft. Haust.

Evening. Pulse 130. Sol. morphia (gr. j. to f. ʒj.) ʒss ; mercurial ointment spread over a linseed meal poultice to be applied to the abdomen.

Sept. 11. Pulse 98 ; skin warm and dry ; abdomen tympanitic and tender ; tongue coated in the centre and red at the end ; lochial discharge free ; bowels moved once. Continue draughts of yesterday every three hours. Apply twelve leeches over the hypogastrium.

7, P.M. Pulse 100 ; skin moist ; abdomen less tympanitic and less tender on pressure ; apply a linseed meal poultice with mercurial ointment over the abdomen. Sol. morph. ʒss.

Sept. 12. Pulse 104, and strong ; tongue moist and blackened by the iron ; abdomen tense, but less tender ; skin warm ; bowels moved twice during the night. There is a superficial sloughing of the vagina ; lochial discharge diminished and pale. Continue draughts of tinct. ferri chlorid. every four hours ; mercurial ointment on a poultice to be applied over the abdomen. (Owing to sickness, I did not see this patient for several days.)

Sept. 16. Appears better. Pulse 108 ; has a miliary eruption over the chest ; the calf of the right leg is red and swollen. Gave her, this morning, ol. ricini and ol. terebinth, aa ʒiij.

R Quinæ sulph.....gr. xij ;  
 Pulv. opii.....gr. ij ;  
 Pulv. camphor.....gr. xxiv. M.

Ft. pil. No. xij. One to be taken every three hours.

September 18th. Continued pills yesterday ; same to-day ; pulse 104. Leg and foot œdematous. An incision was made in the calf of the leg last night, but no pus escaped.

September 19th. Pulse 116 ; bowels moved this morning. She

had a rigor at 9 o'clock last night. Tongue red and dry ; skin warm ; leg still much swollen and red ; foot also œdematous and pits on pressure. Continue pills every three hours. Porter, arrowroot and milk.

September 20th. Pulse 116 ; tongue red. Has diarrhœa. The leg was lanced this morning, and a quantity of thin bloody pus escaped.

R Quinæ sulph.....gr. iij ;  
 Pulv. opii.....gr. i ;  
 Zinci sulph.....gr. iss. M.

Ft. pil. No. iij ; one three times daily ; lime water ; rice milk ; Port wine four ounces ; at night to take an anodyne draught.

September 21st. Tongue red ; pulse 112 ; continue pills of yesterday ; charcoal poultice to leg ; wine three ounces.

September 22d. Pulse 108 ; tongue less red ; countenance improved ; bowels moved three times since yesterday ; leg less swollen ; purulent discharge slight.

R Pulv. opii comp.....gr. iv ;  
 Quinæ sulph.....gr. i. M.

Ft. pulv. No. i ; repeat every three hours. Wine.

September 23. Looks better ; complains of pain in the other leg ; a tumor can be felt in the left iliac region. Continue prescription of yesterday ; wine four ounces ; arrowroot and milk ; leg lanced again, and a small quantity of pus escaped. At night to have an anodyne draught.

September 24th. Pulse 120 ; tongue moist and red ; respiration hurried ; still has a miliary eruption on the chest ; has pain in the left leg caused by tumor in iliac region. Right leg discharges fœtid pus freely. Continue charcoal poultice ; repeat powders, wine, arrowroot and milk. Anodyne at night.

September 25th. Tongue moist and red ; pulse 118 ; right leg discharges freely ; left foot œdematous.

R Quinæ sulph.....gr. ix ;  
 Ferri sulph. sic.....gr. vj ;  
 Pulv. opii comp.....ʒss. M.

Ft. pil. No. xij ; one three times daily ; anodyne at night.

September 26th. Pulse 120 ; tongue red and dry ; respiration hurried ; leg discharges freely. Continue pills.

October 10th. This patient began to sink about a week ago, and was only kept alive by stimulants until last night when she expired.

In the beginning of the attack she appeared to be benefited by the tincture ferri chloride and would doubtless have recovered, had not the phlegmonous attack of the leg supervened.

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ART. VIII.—*A Case of Anomalous Tumor of the Uterus during Pregnancy and Expelled after the Birth of the Child*: By GILBERT T. DEASON, M. D., Elyton, Jefferson Co., Ala.

Mrs. GUNTER was delivered of a child, born dead on the 4th of May, 1859. The midwife, who was her attendant, making a vaginal examination after the birth of the child, found, as she thought, another child in utero. With the hope that labor again would soon return, she patiently sat and waited for its action. After watching for several hours and finding no symptom of renewed action on the part of the uterus, she again examined and found the placenta in the vagina, which she easily extracted, and found that there had been no advancement in the descent of the remaining fœtus, which she said was very high up having been entirely on the top of the one that was born. In this condition every thing remained until the end of twenty-four hours when a messenger was dispatched for assistance. I arrived at the house at the end of some forty hours or more from the time of the birth of the child. I was soon informed by the midwife that the other child had descended to the os uteri, and as yet, there had been no manifest symptoms of labor. I at once made a vaginal examination and found presented at the mouth of the womb a hard, but somewhat elastic substance, and with one hand placed upon the umbilical region, I found it to be a solid mass, movable either up or down, but so attached to the uterus as not to be turned around. I at once was satisfied that the existing difficulty presented a case of no ordinary occurrence. I suspended the examination for a while, resolving to ascertain something in relation to her history. On inquiry from two midwives who had been her attendants for the last three years, I was informed that she had never realised a well

hour during that time and for some time previous ; that she had been married three years, and that the present was her first pregnancy ; that previous to her marriage she had a very large tumor on or about the cervix uteri, which increased in size until she presented the appearance of a woman at the full period of gestation ; that its pressure upon the neck of the bladder was so great that it caused an entire suppression of the urine, the latter escaping only when the catheter was introduced ; that she remained in this condition only a few days, when nature commenced the process of expelling the abnormal growth and for two or three days there was a constant discharge of what I supposed to be a bloody mucus, until the tumor was entirely gone and the woman reduced to her natural form ; I said entirely gone, but it appears that from subsequent examinations, there yet remained in the cervix uteri a hard inelastic substance the size of a hen's egg, which was there at the time of her marriage and continued so until two months previous to her delivery, when it commenced growing very rapidly, and in the short space of ten days was of such an enormous size, that the old midwife's expression signifies more than I can say, when she said, " I believe upon my soul if she had gone one day longer she would have bursted wide open." But it appears that the interposition of nature again rescued the woman by finding an outlet through the vagina, through which that portion of the tumor lying below the child was discharged. About six weeks from this period she was delivered of her child ; then occurred what I have already mentioned.

I now resolved to satisfy myself of the nature and position of the tumor. On making my second vaginal examination, I found the os uteri about the size of a silver dollar, and that its walls were very thin and much relaxed ; that the relation of the tumor with the uterus was of such a nature as to completely fill up the lower segment of the uterus, presenting the appearance of the apex of a cone. On passing my finger up between the walls of the uterus and the tumor, about one inch and a half, I found the inner walls of the uterus reflected upon the tumor. My first impression was, that the tumor was external to the uterus, and that there was a retroversion of the uterus, but with my finger nail I could easily penetrate the tumor, showing that it was only the mucus coat reflected, and that the tumor was between the mucus and fibrous coats.

On passing my finger around, I found that this reflection occupied



about two-thirds of the space around the tumor. The upper portion of the tumor from what I could ascertain by an examination through the walls of the abdomen, presented a flat or rather concave surface, with an oval shaped edge protruding somewhat at the umbilicus, and extending diagonally towards the antero-superior crest of the ilium. This tumor during the period of gestation occupied, as I was informed, the highest portion and on the right side of the umbilical region, and was supposed to be a portion of the child until one was born, and then it was thought to be a second child. The woman herself acknowledged that she had never felt any movements in that part, while she could frequently feel distinct movements below it. I here gave it as my opinion that the flattened upper portion of the tumor was caused by its pressure and growth against the liver, and that the tumor that was discharged six weeks previous to the birth of the child was entirely disconnected from the one above, and when this was discharged and the child was born the gravid state of the tumor in question, brought it to the position in which I found it, and much to the astonishment and fright of her attendants, there was no labor pains to expel it.

After giving my opinion that the supposed child was a large tumor, I was urgently solicited to take it away.

Her present feeble condition indicated anything but the loss of blood, and from this cause I reluctantly consented. I first penetrated the tumor with a pair of Smellie's scissors, from which issued a large quantity of coagulated blood and mucus, which came in ropes from two to four inches long, and these ropes were composed of strings of dark venous and of bright red arterial blood. On introducing my finger in the cavity I had made, it appeared to be a spongy mass, composed of cells, similar to that of honey-comb, and each cell containing the matter mentioned above. There was also a loss of a good deal of vivifying blood which prostrated my patient so that I had to wait some time before proceeding; and at each succeeding attempt I was forced to stop and give her time to recover; and before I had taken one half of the mass away, her fast failing pulse warned me of the approaching danger, and I was forced to relinquish further operation entirely. In this extreme case of debility, my only hope of success was the resort to some therapeutical agent, that would arouse the now somewhat contracted uterus to reaction that

might assist in the expulsion of the offending mass. In this hope I was soon encouraged by the frequent exclamations from her "that she felt other lumps passing."

I remained with her some three hours subsequent to the operation, during which time she passed several of those same rope-like forms of coagulated blood and mucus. While making out a prescription to leave, my attention was called for the first time to her leg which was much swollen and painful. I at once recognized a fully developed form of phlegmatia dolens; not only this but the mammary glands were very much swollen and were hard. I made a liniment composed of the tincture of myr. et capsic,  $\zeta$ ii, chloroform,  $\zeta$ ii; with which I ordered her leg to be rubbed three times a day. I then made a circular plaster of belladonna about three inches in diameter, and applied to her breast, having cut a hole in the centre for the nipple. I then placed her on the use of aloes et canella,  $\zeta$ iv; saponis,  $\zeta$ i; oleum caryophylli, gts., xxiv; made into pills, one to be taken twice a day.

By this I hoped to act upon the lower portion of the bowels and stimulate the functions of the uterus to action. I now wanted an anodyne and diuretic, for which I united, spts. æther. nitrici.  $\zeta$ ii, tinct. camphoræopii  $\zeta$ ii. Dose: a teaspoonful three times a day.

The patient living thirty-six miles from town, I heard nothing more from her until the 13th (having operated on the 4th), and then I heard nothing definite, only that she was alive, and had some prospect of recovery. My next information was by letter from Jasper, Walker County, dated the 20th, to my friend B. F. Stovall, from which I make the following extract: "In relation to the case of Mrs. Gunter, there is no doubt but that she will recover. This morning her husband is in town; left her yesterday, and says she was able to go about and seemed to have no fears but that she will get well." Although I could learn nothing definite concerning the tumor, yet I am satisfied that the discharge [of blood] never ceased until it had entirely disappeared. The enormity of its growth, peculiarity of form, viscosity of contents, presented to my mind a case worthy the perusal of every obstetrician, to whom I dedicate these lines, hoping that such contributions may induce adepts to unravel all such mystical threads of our science.

ART. IX.—*Molar Pregnancy.*

DR. B. DOWLER—*Dear Sir*: Below, I send you a sketch of a case of pregnancy which, from its novelty, may be of interest to some of your readers. For the want of a better term, I shall have to call it a case of *Molar Pregnancy.*

The subject of this sketch is a negro woman (slave) belonging to James Bennet, who lives about five miles from our village. She is about thirty-five years old, has had five children, the last one about three years ago. Up to that time she had enjoyed good health; but since then she has been in delicate health, and her menstruation irregular. She menstruated the last time about the end of last January.

As she was able to attend to her duties no physician was called in to see her, until about the 20th of April, at which time I desired Mr. Bennet to send her into my office, which he did the next day. I found her suffering alarmingly with dropsy. She was œdematous from the crown of her head to the sole of her foot. Every part of her was full of water. The womb was as large as it ought to have been at the full term of gestation, and she contended that its enlargement was from pregnancy. I directed the free use of cremor tartar, and a dose of tinct. columba three times a day, under which she improved very rapidly. Her appetite, which had before been very bad, was now good, and she had gained strength when I saw her ten days afterwards. I then gave her a tablespoonful of infusion of chamomile and three grains of iodide of potassium, three times a day, and an occasional dose of cremor tartar, which I continued at irregular intervals until the 9th of June. I then saw Mr. Bennet, and desired him to send her in the next day, in order that my partner, Dr. Whelan, might see her with me. I was absent and did not see her. She returned home, and soon after getting there, was taken with labor pains. Mr. Bennet, immediately, sent into town and I went out. (I omitted to mention that the uterus had continued to increase in size, and at this time, it was very much distended.) When I got there (11 o'clock, P. M.), I found the bed and everything about her completely saturated with bloody water, and a large amount of clotted blood had escaped; also a lump of partially organized matter, tough, fibrous, and circular, and about three inches in its longest diameter, and an inch and a half thick, and another about the size and shape of a hen's egg, similar in texture.

As soon as the pains came on they gave her a dose of laudanum,

and when I got there, I found her having slight convulsions ; but the labor pains had ceased, and the uterus still extended above the umbilicus. I first used the usual means for checking the convulsions and allaying the nervous excitement, and in a short time she became quiet and went to sleep, and slept until five o'clock. When she awoke, I commenced giving her ergot, and after continuing it for sometime without success, I gave her a copious enema of soap suds and spirits of turpentine which brought on pains in about fifteen minutes, which, during the next hour, expelled a mass of heterogeneous matter, about three feet long. It consisted of lumps of fatty matter of irregular size inclosed in a delicate pellicle or fibrous membrane, and in the fat was a number of lumps of various shapes and sizes, the largest as large as a walnut, some of which looked like clotted blood, and some like muscle. The lumps of fat were attached to each other by the membrane spoken of above, and at intervals of from one to four inches. The entire mass filled a gallon measure. She has been doing well since then, and I now hope that she will soon be in good health.

If you find anything in the above which will be of interest to your readers, you can publish it. It was not written to gratify any ambitious aspirations.

Respectfully yours,

R. QUINNEY, M. D.

ANDERSON, GRIMES COUNTY, TEXAS, June 29th, 1859.

[Moles may be classified as hydatidiginous (vesicular), fleshy (at first fibrinous concretion), and embryonic degenerations (false conceptions). In this case, the moles appear to have been a compound of fleshy and fatty matters, the lardaceous predominating. The origin of these products is obscure. It is not improbable, however, that the ovum may sometimes perish immediately after conception, whilst its shaggy chorion not being devitalized, and its cells being progressively and irregularly developed may send forth vegetations, cysts, and so forth, from it as the point of departure for these anomalous formations.

The 7th volume of the French translation\* of Morgagni's great work, *De Sedibus et Causis Morborum per Anatomen Indagatis*, is devoted chiefly to the investigation of false pregnancy, uterine hydatids,

moles, and their allied abnormalities. This volume upon these ill-understood physiologico-pathological growths is enriched by the author's autopsies and contains copious references to the examinations and opinions of his predecessors and contemporaries. Morgagni considers that both real and false moles have generally as their antecedent, conception, the abnormal products or degeneration of which, whether in the fœtal placentæ, cords, or other secundines, gives rise to these anomalous formations. He thinks, nevertheless, that independently of conception, a simple concretion of blood within the uterine cavity, may become organized excrescences which grow so as to resemble flesh and even become fleshy and vascular, having also vesicular portions, cells, septa, or multilocular cavities containing different liquids or solids, some of which contain *large quantities of fat*, as in one of the cases above mentioned.

It is believed that the student who refers to this work written a century ago, will find a fuller account from actual observation of these abnormalities, than he meets with in the modern text books.—(See from p. 340 to 521.)

In adding these paragraphs to the two preceding articles, conformably to the expressed or implied request of their writers, I am sensible that nothing satisfactory has been adduced explanatory of these important cases.

Independently of the physiological and pathological import of these intra-uterine formations simulating pregnancy, their moral and medico-legal relations are of extraordinary importance. Here an error of diagnosis may embitter the life of the practitioner, and still more that of others.

It will be difficult in either tragic poetry or prose to find a more pathetic example of the moral sublime than that given by Professor Bedford in his Clinical Lectures in the following words :

“ I was requested to visit a lady who was residing in the State of New Jersey, about thirty miles distant from New York. I immediately repaired to her residence, and on my arrival was received by her father, a venerable and accomplished gentleman. He seemed broken in spirit, and it was evident that grief had taken a deep hold of his frame. On being introduced to his daughter's room, my sympathies were at once awakened on beholding the wreck of beauty which was presented to my view. She was evidently laboring under pthisis, and it was manifest from her wasted frame, that death had claimed

his victim. My presence did not seem to occasion the slightest disturbance, and, with the smile of an angel playing on her countenance, she greeted me with these words: 'Well, doctor, I am glad to see you on my beloved father's account, for he will not believe that I cannot yet be restored to health. Life, however, has lost all its charms for me, and I long for the repose of the grave.' These words were spoken with extraordinary gentleness, but yet with an emphasis that at once gave me an insight into the character of this lovely woman.

"Her father was a clergyman of high standing in the English Church, and had a pastoral charge in England, in which he continued until circumstances rendered it necessary for him to leave that country, and seek a residence in America. At a very early age, this young lady had lost her mother, and had been almost entirely educated by her father, whose talents and attainments admirably fitted him for this duty. When she had attained her eighteenth year, an attachment was formed between her and a young barrister of great promise and respectability. This attachment resulted in a matrimonial engagement. Soon after the engagement, she begun unaccountably to decline in health. There was considerable irregularity in her menstrual periods, with more or less constant nausea, loss of appetite, inability to sleep, feverishness, and an uncontrollable dislike to society. In addition to these symptoms, there was a marked change in her personal appearance; her abdomen became enlarged, with increased size of the breasts, etc. These changes attracted the attention of some of her female acquaintance, and the rumor soon spread that they were the result of pregnancy.

"The barrister to whom she was affianced heard of these reports, and, instead of being the first to stand forth as her protector, and draw near to his heart this lovely and injured girl, thus assuaging the intensity of grief with which she was overwhelmed, addressed a letter to her father, requesting to be released from his engagement. This was of course assented to without hesitation. The young lady conscious of her own innocence, knowing better than any one else her own immaculate character, and relying on Heaven to guide her in this her hour of trial, requested that a physician should be sent for, in order that the nature of her case might be fully ascertained. A medical man accordingly visited her, and, after an investigation of her symptoms, informed the father that she was undoubtedly pregnant, and that means should be instantly taken to keep the unpleasant

matter secret. The father, indignant at this cruel imputation against the honor of his child, spotless as he knew her to be, spurned the proposition, and immediately requested an additional consultation. This resulted in a confirmation of the opinion previously expressed, and the feelings of that parent can be better appreciated than portrayed.

“ Without delay, that good man determined to resign his living, gather up his little property, and proceed with his daughter to America. On her passage to this country, she became extremely ill, and there being a physician on board the vessel, his advice was requested. After seeing the patient (she was affected at the time with excessive vomiting from sea-sickness), he told the father there was danger of premature delivery. Such, therefore, was the general appearance of this lady, that a medical man, merely taking appearances as his guide, at once concluded she was pregnant. This was about the substance of what I learned respecting the previous history of this interesting and extraordinary woman, and my opinion was then requested as to the character of her malady. My feelings were very naturally much enlisted in her behalf, and I proceeded with great caution in the investigation of her case. Without entering at this time into details as to the manner in which I conducted the examination, suffice it to say, that, after a faithful and critical survey, most minutely made in reference to every point, I stated in broad and unequivocal language, that she *was not pregnant*. The only reply this gentle creature made on hearing my opinion, was, ‘ Doctor, you are right.’ These words were full of meaning, and their import I could not but appreciate. They were uttered neither with an air of triumph, nor with a feeling of unkindness towards those who had so cruelly abused her. The father was soon made acquainted with the result of my examination, but he indicated not the slightest emotion. His bearing was quiet and dignified. It was evident that he had never faltered for one moment in the belief of his daughter’s virtue, and required no assurance from me or any other living being, that his child had been shamefully wronged. He asked me with great solicitude whether something could not be done to restore her to health, and I thought the old man’s heart would break when I told him that his daughter was in the last stage of consumption. I left him with the pledge that he would inform me of her dissolution, and afford an opportunity, by a *post-mortem examination*, of testing the truth of my opinion.

“About four weeks from this time, I received a note announcing the death of his daughter, and requesting that I would immediately hasten to the house, for the purpose of making the examination. Dr. Ostrum, now practicing at Goshen, at my request accompanied me, and assisted in the autopsy. It may surprise you, gentlemen, but yet it is an interesting fact to communicate, for it exhibits the true character of the man, that, during the *post-mortem* examination, the father stood by, and witnessed every stage of the operation; his form was erect, his face pale and thoughtful, and one tear would have broken the agony of his grief. As he stood before me, he was not unlike the stricken oak in the forest, which, though blasted and stripped of its branches, was yet upright and majestic. As I removed the tumor from the womb, he seized it convulsively, and exclaimed: ‘This is my trophy, and I will return with it to England, and it shall confound the traducers of my child.’”

The case of Lady Flora Hastings, who was exiled from Court, covered with calumny, broken hearted, and prematurely hurried to the grave, was of a similar character, as proved by a *post-mortem* examination.

The part which a then youthful Queen took in this affair, doubtlessly from the best motives, and, perhaps supported by the advice of professional or non-professional persons, touched that chord which makes the whole world akin, and is, perhaps, the most regretful souvenir of a sovereign whose benificent reign and personal virtues have endeared her to a great nation—B. D.]

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ART. X.—*Ligation of the Common Carotid Artery for Aneurism by Anastomosis; with other Surgical Cases*: By E. D. CHERRY, M. D., of Columbus, Mississippi.

1. THE patient, Marietta G., a white female, aged about twelve years. Her disease was congenital, and had been gradually increasing since birth, involving the entire right side of the face and extending from the temporal region to the inferior border of the maxillary, and on the upper lip, a little beyond the median line.



Internally it extended as far posteriorly and involved the right tonsil. The extensive congeries of diseased vessels formed a large vascular tumor, which endangered life by repeated hæmorrhages from the slightest abrasions.

The common carotid was ligated at the point of election, and also the superior coronary artery on the opposite side. Several ligatures were passed through the upper lip, for the purpose of facilitating the obliteration of this portion of the tumor.

The ligature from the carotid came away spontaneously on the fifteenth day, and the tumor subsided entirely, except that portion on the upper lip, which seemed to be sustained by free anastomosis with the branches of the temporal and ophthalmic arteries.

I am induced to report this case from the fact that the ligation of large arteries for external aneurisms of this character is seldom attended with such encouraging results.

2. *Division of the Tendons of the Ham for Contraction of the Leg on the Thigh.*—The leg was flexed on the thigh at a right angle, by a firm contraction of the flexor tendons and ligaments, caused by an attack of rheumatism. The limb had been in this position for six years, and as there was slight motion of the joint, and the patient a young and perfectly healthy man, twenty-five years of age, it was deemed practicable to attempt the removal of the deformity by a division of the tendons of the semitendinosus, semimembranosus, and biceps flexor cruris muscles, by sub-cutaneous sections. This procedure causing the limb to yield but little, it became necessary to use great force to break up the firm ligamentous union about the joint. The limb was immediately placed in nearly a straight position, and then returned to an angle of about 45 degrees. A double inclined plane extension apparatus was applied on the third day, and the leg gradually extended to nearly a straight position.

In four weeks after the operation he had recovered without any unfavorable symptoms, and although there was but slight motion in the joint, the limb, in two months, was restored to usefulness, and he walked with facility without the aid of crutches.

3. *Operations for Impermeable Stricture of the Urethra.*—My first operation was on a man thirty-five years of age, with stricture of ten years' standing. When I first visited him, he was occasionally delirious with profuse perspiration from almost an entire retention

of urine. It was impossible to introduce a catheter of any size through the stricture, which was situated near the bulb. He was placed in the position for lithotomy. A catheter introduced as far as the stricture, and an incision made in the median line, on the perineum upon the point of the instrument. A grooved probe was then introduced through the wound into the stricture which was divided with a narrow bladed scalpel. The stricture involved an inch of the urethra, and was of a dense fibro-cartilaginous structure. The relief after the introduction of the catheter was prompt, and the patient gradually recovered in five weeks without any unfavorable symptoms. During convalescence it was found necessary to introduce the catheter occasionally, to prevent undue contraction, and restore the normal size of the urethra.

The second case was a stricture of three years' duration; the patient a negro about twenty-three years of age. It was anterior to the bulb, but also involved the latter and membranous portion of the urethra. The prostate gland was slightly enlarged, but not indurated. A similar operation was resorted to and the patient did well for thirty-six hours, when rigors and collapse supervened, and he died of cystitis on the fifth day after the operation.

I regard this operation more tedious and dangerous than that of lithotomy, on account of the difficulty in dividing a very narrow stricture near the prostate, as it is sometimes impossible to introduce the smallest probe to guide the scalpel in the division of the diseased structure. The thickening of the coats of the bladder and enlarged or diseased prostate and other and perhaps more dangerous complications are found in strictures of long standing.

4. *Operations upon the Veins.*—In operating for varicose veins of the lower extremities by introducing long needles behind the veins and applying the sutura circumvoluta, and allowing them to remain for eight days, I have found the most effectual means to prevent the return of the circulation in the veins, and secure their permanent obliteration, to be, after the removal of the needles, the application of adhesive strips firmly across the enlarged veins, and the roller bandage daily until the ulcerations have entirely healed. The points where the needles are applied will generally, but not invariably slough and form ulcers, which more effectually destroys the veins. If there is no sloughing the circulation is often restored and the operation fails, without the application of the above means.

ART. XI.—*Accidental Poisoning with the Perchloride of Mercury.*

MONROE, MICHIGAN, JUNE 4, 1859.

DR. B. DOWLER—*Dear Sir*: I have to contribute to your Journal the following case of accidental poisoning by the unwary inhalation of the perchlo. hyd. in alcoholic solution, which I judge will be of some interest to *all regular practitioners* of medicine, as it so perfectly illustrates the peculiar susceptibility of some subjects to the overpowering effects of some of our remedies very frequently used in practice, to the great detriment of the patient in many cases; and also the extreme caution to be observed in its use and administration.

On the 30th ult., Mrs. S., a widow lady of this city, of a full plethoric habit, sixty-three years of age, and, at the time of this occurrence, in the enjoyment of good health, who, believing that a bedstead in the house *was, or would be* infected with *bugs*, anointed it with the alcoholic solution of perchloridi hydrargyri, of the strength of  $\text{ʒi.}$  of the hyd. to the  $\text{o. j.}$  of alcohol. In the operation she says she probably was occupied fifteen minutes; using a swab for the purpose.

Within half an hour afterwards, she was found by me, laboring under the following symptoms, viz: violent burning pain in the mouth, throat, œsophagus and stomach; difficulty of deglutition; violent vomiting of mucous and bilious matters; violent purging; inexpressible anxiety; flushed countenance; pulse quick, small, and contracted; cold sweats; much thirst; short and laborious respiration—which symptoms continued for the space of one hour, more or less, before they began to subside, leaving the patient in a state of extreme exhaustion. Ordered an anodyne, with mucilaginous drinks.

The following day she was comparatively well. On the 2d instant, I was again consulted, and on examination, I found her most thoroughly mercurialized; the ptyalism and swelling extending over the mucous membrane of the mouth, the inner surface of the cheeks, and the tongue, palate, and throat. I ordered her saline draughts and astringent gargles, together with tonics, and she is fast recovering her usual state of health.

I am, sir, your obedient servant,

CHAS. T. SOUTHWORTH, M. D.

ART. XII.—*A Remarkable Case with remarkable deficiency in the expected Symptoms in a Severe Injury*: By GEORGE WHITFIELD, M. D., Spring Hill, Marengo County, Alabama.

MARCH 9, 1859, Richard Lay, aged eight years, rather thin, but very healthy, active and sprightly, was run over on hard smooth ground by both wheels of a heavy ox-wagon, heavily laden with manure, the front wheel passing across the abdomen about the region of the stomach and liver, the patient lying on his back; the hind wheel passing obliquely across from the small of the back over the left shoulder, patient lying on his face. He was taken up senseless, black in the face, cold and pulseless; had involuntary fæcal evacuation at the time of accident; fæces of natural color and consistence. When first seen, six hours after the accident, had vomited a few clots of venous blood; was cold, pale; tongue almost white, pulse 160, very feeble, thready, scarcely perceptible; rational, calling constantly for water; breathing rapid, but unembarrassed; *right lung* doing most of the work; *left side* of chest *almost inactive*; a few small scratches and spots of ecchymosis across the epigastrium, right and left hypochondria and left side of the front of the chest; attempts to vomit if raised into sitting posture. Decubitus on the back *inclined to the right side*; screams if turned on left side, and turns back immediately; abdomen slightly swollen, tympanitic and tender to pressure; complains of pain in shoulders, particularly the right, and back of the neck; *no bones broken*; spine apparently uninjured; can move the legs without difficulty; disposed to sleep. *Percussion*: right side of chest normal; left side tympanitic, particularly in clavicular, axillary and scapular regions, and loudest between the left scapula and spine. *Auscultation*: right side in all respects normal, excepting very loud respiratory murmur; left, vesicular murmur rather feeble; action of heart very feeble, but tumultuous; *no cough*. R, whisky toddy, and sinapisms to the extremities and epigastrium; soon began to react; pulse coming down to 150, and gaining more volume. At 6, P. M., seven hours after the accident, had natural alvine evacuation, after which: R, calomel, gr. vj.; tinct. opii, gtt. x.

By 12, P. M., thirteen hours after the accident, he had fully reacted; pulse 150, moderately full; skin hot and dry; great thirst, and restlessness; pain in the shoulders and back of neck very severe; respiration very frequent, panting; no delirium; *no cough*. R, tinct. verat. virid. syrup scillæ, aa gtt. ij, every three hours.

March 10th, 10, A. M. There being no reduction of pulse, the dose of sedative mixture had been gradually increased to four drops of each ingredient, every three hours. The pulse had now come down to seventy-two, with great nausea and vomiting ; *no cough or difficulty in respiration* ; tympanites changing position alternately from axillary to scapular and dorsal regions of chest, has had several evacuations of bilious character, presenting no traces of blood. Arterial excitement kept in abeyance all day, by tinct. verat. virid.

March 11. Spent a miserable night, screaming, tossing, and unable to sleep ; finally was quieted by large doses of tinct. opii ; tympanites of left side still distinct, and shifting from place to place, but respiratory movement of that side becoming more active ; pulse kept at 100 by verat. virid. ; considerable enlargement and tenderness of the liver. R, calomel, gr. viij ; and blister over the liver, eight inches by four. *No cough yet.*

March 12. Blister has drawn admirably ; pulse 100, soft ; skin warm and relaxed ; slept several hours last night ; cries for food ; several bilious evacuations to-day ; gums touched ; *no cough.* Urine scanty, high-colored, but otherwise natural. Tympanites less distinct, loudest between the shoulders ; stript the cuticle from the blistered surface and dressed it with unguent. resinæ. Kept patient on verat. virid. all day, and allowed gum water *ad libitum.*

March 13. Better in all respects ; swelling of liver decreasing ; appetite returning ; complains of nothing but weakness, tympanic sound becoming fainter, audible only between the scapulæ ; vesicular murmur almost as loud as on right side. Bowels open of themselves, with bilious evacuations.

From this time forward, the patient gradually improved, until March 31, when I find my note book saying : " patient well, with no deformity, and both sides of chest alike, and normal in all respects."

*Remarks.*—Whatever degree of importance the profession may be disposed to give this case, I consider it, in some respects, a very remarkable one ; first, as illustrating the vast amount of violence to which the human frame may be sometimes subjected without permanent injury ; and, secondly, on account of the absence of those very symptoms we might most naturally have expected to find as a consequence of the existing lesions. That there was air in the cavity of the left pleura there can be no doubt, and that the air made an en-

trance through a rent in the left lung is equally as probable. But that a rent in the lung, communicating with the cavity of the pleura, should have existed without cough, pleurisy, or pneumonia, or any of their symptoms, almost exceeds belief.

Upon first examination the idea occurred to me that the stomach and intestines might have entered the chest by rupture of the diaphragm ; but a subsequent and more careful investigation convinced me that this could not be so. I am sure there can be no mistake about the tympanitic sound, as being desirous of avoiding all obscurity and uncertainty I caused to be brought to the bed-side a healthy boy of the same age and physical conformation, and there compared the sounds emitted by their chests respectively. The difference was so marked as to be noticed by all the by-standers, and to call forth exclamations of surprise. The question now arises, how could such a lesion exist without a single symptom or sign to indicate it except the tympanitic sound ? I am compelled to confess I do not know. *My theory* is, that the lungs were distended when the pressure of the second wheel was applied; that the force of that pressure caused the left lung to burst, making a small rent communicating with the pleural cavity; that a portion of the air in the lung entered the pleura through the rent at the time of the accident; that hæmorrhage was prevented by syncope from the shock of the accident, and that subsequent inflammation was kept in abeyance by the tinct. verat. virid. and the *vis medicatrix natureæ*; the patient having one of those temperaments and constitutions we occasionally meet with, admirably adapted to resist inflammatory attacks, being lithe, light and active, disposed to neither plethora nor anæmia, and having a nervous system little susceptible of external impressions. Cases of ruptures of the air cells and pleura by direct pressure, are recorded, as also recoveries from violent injuries to the thoracic organs; but I am not aware of having met with any case analogous to this, either in medical literature or my intercourse with medical men. The gradual disappearance of the air from the pleura, I can account for in no other way than by absorption. Discussion of this subject by the profession is solicited, and if a public notice of this case should be of service or even interest, I shall feel more than compensated for any trouble it has cost me.

ART. XIII.—*Notes of Surgical Cases in the Charity Hospital of New Orleans*: By T. G. RICHARDSON, M. D., one of the attending Surgeons.

*Gangrene of the Penis, supervening upon an attack of Typhoid Fever, in a Patient suffering at the time with Gonorrhœa.*—L. C., æt. 30, a stout, hearty-looking Irishman, entered the Hospital, April 5, 1859, to be treated for a clap which he had contracted a week or ten days before. As is my invariable rule under such circumstances, I made an examination of the organ, and found the prepuce contracted, and a profuse discharge of laudable pus flowing from the opening. Desirous of also ascertaining whether chancres existed beneath the foreskin, I made strict inquiry of the patient concerning the condition of the concealed surfaces before the narrowing took place, and, in addition, made as thorough an inspection as the nature of the parts would permit. The result of the inquiries and examination satisfied me that the case was a pure gonorrhœa, with no other complication than the aforesaid phimosis. I directed accordingly an emulsion of balsam copaiba—the ordinary Chapman's mixture, and the patient to lie upon his back with the organ elevated and swathed in cloths wrung out of tepid water.

On the third day, I discovered that the man had an open, well developed fever, and learned that he had been taken with a chill a few hours before, and that for the preceding twenty-four hours he had complained a great deal of the medicine making him sick. Supposing the attack most likely to prove an intermittent or a remittent fever, which was exceedingly prevalent in the house at the time, I requested Mr. Elliott, the active and intelligent student attached to the ward, to watch the case, and if an intermission or a decided remission should occur, to administer full doses of quinine. The next morning I found the fever still in existence, but was told that a remission had taken place the evening before, and that fifteen grains of quinine had been given. A farther examination revealed nothing of a more definite character than had been ascertained the day before, except that the mental faculties were more obtuse, and the countenance more expressive of suffering than is usually the case in malarious fevers. Under these circumstances, I ordered a repetition of the anti-periodic to the extent of twenty grains, in case of another remission.

On the fifth day, the symptoms were decidedly typhoid; the skin

hot but moist; the tongue dry, and covered with a brownish coat; the abdomen tympanitic; the intellect dull and heavy, and a small bleeding at the nose. And now, for the first time since the occurrence of the fever, the patient asked me to look at his penis, which, he said, pained him very much. Complying with his request, I discovered upon the outer and upper surface of the prepuce, a gangrenous spot, about the size of a five cent piece, surrounded by an angry looking areöla, and the adjacent tissues somewhat œdematous and acutely sensitive. The great tenderness of the parts, and the contracted condition of the prepuce, rendered it impossible to ascertain whether the morbid action had begun upon the glans penis or in the foreskin, but my impression was, judging from the superficial inspection to which I was limited, that it had its origin in the latter. I ordered the organ to be enveloped in a poultice of flaxseed and charcoal, and the patient to take a grain of opium, with half an ounce of brandy, and a liberal supply of beef-tea, every four hours; the quantity of opium to be increased, if necessary to allay pain.

The gangrene, however, spread rapidly, involving the whole thickness of the organ, which in less than four days' time was in complete ruins up to within half an inch of the pubes. Here a line of demarcation formed, and the charred and blackened mass soon sloughed off without any accident. In the meantime the fever, although somewhat modified, continued its course and did not entirely disappear for three weeks after the detachment of the slough. The treatment was not materially altered from the time that the disease pronounced itself.

*Intercranial Suppuration following a Lacerated Wound of the Scalp; Application of the Trephine; Death; Autopsy.*—The subject of this accident was an intelligent-looking Irishman, æt. 26, who, in a fit of temporary insanity, attempted to destroy himself in the following singular manner: Taking a sail-needle in his left hand, he held the point of it against the scalp, about an inch and a half above the forehead directly in the middle line of the head, and then, with a large caulking hammer—a wooden mallet with an iron band surrounding each of its extremities—he endeavored to drive it into his brain; but the point of the needle broke off at the first blow, having barely entered the bone. Irritated by his failure, he then seized the hammer with both hands and battered himself with great force over the same spot.



He was admitted into the hospital the day after the injury, and upon examination, I found the scalp bruised and torn into transverse strips over a space as large as the palm of the hand, and the skull completely denuded of its periosteum to the extent of half a dollar, though not broken or even dented. The point of the needle was also discovered sticking in the bone, but broken short off, and from his own account, did not probably exceed an eighth of an inch in length. The poor fellow had now recovered his mind and was calm, but melancholy and indisposed to answer questions. I succeeded, however, in getting from him that he was not addicted to intemperance, and had never before suffered from any cerebral difficulty.

As there were no symptoms of injury to the brain, not even the slightest headache, I did not deem it necessary to do more than direct him to lie with his head and shoulders elevated, and apply cloths wrung out of cool water to the wound, warning him, however, at the same time, against leaving his bed, or indulging in any indiscretions in eating or drinking.

The next day I found him in the same condition, but was told by the nurse that in the early part of the previous night he had suddenly become very much excited, attacking the patients lying near him and rushing up and down the ward in a state of violent frenzy, and that it therefore became necessary to confine him with stocks and handcuffs. As he appeared entirely composed, I directed him to be released, and advised a dose of cathartic medicine and a continuance of the wet cloths to his head. Under the use of these simple means, together with a restricted diet, he had no return of the mania, but remained quiet in bed for six or seven days, when he persisted in getting up and walking about the hospital grounds in spite of my repeated injunctions to the contrary, and my positive assurance that it was at the risk of his life. In the mean time, the wound not having suppurated, and the exposed bone continuing dry and bloodless, the apprehension which I expressed at the first, of the likelihood of the formation of abscess within, or of softening of the brain, was much increased.

Matters continued thus, the patient moving about every day in a melancholy and reserved state of mind, and the wound manifesting no disposition to granulate, for nearly three weeks longer, when he was obliged to take to his bed in consequence of general debility. This was speedily followed by fever of an asthenic character and con-

fusion of intellect. Having up to this time kept his bowels in a soluble condition by the occasional use of comp. cathartic pills, I put him now immediately upon calomel and opium in small doses every three hours, and directed cloths dipped in cold water to be applied constantly to his head. No good resulted from this course, but, on the contrary, the fever continued unabated, and the mental faculties became more and more obtuse, thus plainly indicating that mischief of a serious character was going on within the cranium, a fact which was still farther substantiated on the third day after the accession of the febrile symptoms by loss of muscular power in the right lower extremity. He was now unable to answer questions and lay in a state of semi-stupor, occasionally throwing his head from side to side and kicking the bed clothes from off his body.

Having for the two previous days suspected the formation of pus within the skull, I requested the advice of Professor Stone in regard to the propriety of trephining. We both agreed as to the legitimacy of the procedure, but from the well known ill success of the operation in such cases, concluded to continue the treatment then in force until the next day. No improvement, however, occurred, and the paralysis had in the meantime extended to the right arm and corresponding side of the face, with involuntary discharges from the bowels.

I proceeded therefore at once to open the cranium, uncertain, of course, whether the pus existed between the dura mater and the bone, in the arachnoid sac, upon the surface of the brain or in the substance of the latter, although the absence of convulsive movements previous to the occurrence of the paralysis led me to suspect that the integrity of the cerebral tissue was not much, if at all disturbed. I employed a small trephine and placed it upon the already denuded bone, directly in the middle line of the head, so as to include the point of the needle. Upon penetrating the external table a small quantity of offensive pus oozed out by the side of the instrument and from the opening made by the stylet, and when the internal table was divided fluid of a similar character flowed from the groove. When the disc of bone was detached, which was readily accomplished without injury to the superior longitudinal sinus or the dura mater on either side, pus was observed to come from beneath the upper edge of the opening where the fibrous membrane was evidently detached for a short distance. The total amount of pus which escaped during and immediately subsequent to the operation was estimated at about

a drachm and a half. The inner surface of the bony plate was smooth and unbroken, the needle having penetrated only as far as the diploë: the exposed dura mater presented a gray granular appearance owing to the deposit of little masses of semi-fluid lymph.

The operation was not followed by any relief to the symptoms, and on the following day, I made an exploratory opening through the dura mater upon each side of the superior longitudinal sinus for the purpose of ascertaining if pus existed beneath the membrane; but only a little serum escaped and I did not deem it proper, therefore, to enlarge the openings. The patient lived until the evening of the ensuing day, and, in the mean time, had several severe convulsions.

*Autopsy, fourteen hours after death.*—Upon removing the calvarium the vessels of the dura mater were found crowded with blood and presented a most beautiful arborescent appearance. Opposite the site of the injury the membrane was of a dark brown color, produced, doubtless, by exposure and contact of free blood through the opening made by the trephine. Within the arachnoid sac there was a quantity of consistent lymphic pus spread out over the whole of the upper and lateral surfaces of the cerebral hemispheres and extending as far forward as the bases of the anterior lobes and as low down behind as the bases of the posterior. It was impossible to ascertain with accuracy the amount of this fluid but it could not have been less than an ounce or an ounce and a half. It was a little more abundant upon the left than upon the right side, but the difference was very slight. Upon washing off the pus, the visceral layer of the serous membrane presented its usual healthy appearance; not the slightest opacity or thickening indicative of the severe morbid action of which it had been the seat could be detected.

The vessels of the pia mater were deeply congested and the same was true of the brain substance, both of which were otherwise perfectly normal in their structure so far as could be ascertained. Not the least degree of softening had taken place in either of the hemispheres.

The viscera of the chest and abdomen were healthy.

*Inguinal Entero-epiplocele; the Bowel reducible and the Omentum adherent: Excision of the Hernial Sac and Omentum: Recovery.*—A large robust German, æt. 40, was admitted into the Hospital in the latter part of March to have something done for a hernia which he was afraid was

about to become strangulated. Upon examination I found a scrotal tumor about the size of the fœtal head, proceeding from the left inguinal region, tense but not tender upon pressure, and the seat of no great amount of suffering. I learned upon questioning the patient, that the swelling had commenced in the groin twelve years before, could usually be easily pushed back into the belly, and had steadily increased in bulk notwithstanding the use of a truss. The tension and irreducibility had existed only twenty-four or thirty-six hours.

I made an attempt to reduce the parts in the usual way, but failing after considerable effort, and there being no great urgency in the case, I had the tumor firmly drawn up by means of a handkerchief tied to a band passed around the loins, ordered a dose of castor oil, and left the house with the understanding that I should be called in case more decided symptoms of strangulation should supervene. Not being sent for, I did not see the patient until the next day, and, upon examination, discovered that no change whatever had occurred during the interval. I endeavored again to put the protrusion back, but was not more successful than before. The suspensory bandage was replaced, and cloths wrung out of cold water directed to be constantly applied. On returning the following morning, the patient informed me that the parts becoming relaxed during the night, under the influence of the cathartic and the cold cloths, he had succeeded in getting them back as usual. I now found on manipulating the scrotum, that notwithstanding the greater portion of the contents of the sac had undoubtedly disappeared, there was still remaining an inelastic, doughy mass, which it was impossible to return, although the finger could be readily inserted into the abdominal opening. On making closer inquiry of the man, I ascertained that such had been the condition for six or eight months, and that since then he had been obliged, in a great measure, to give up the use of the truss, on account of the pain that it produced. Making him force the bowel out by coughing, and then reducing it myself, I became satisfied that the remaining tumor was omentum, which had probably become irreducible by fatty deposition.

Although the patient was thus relieved of the immediate symptoms for which he entered the hospital, he insisted upon having the parts put in a condition that would enable him to return to work, without being liable to a recurrence of his recent trouble. I explained to him as well as I could, the nature of the difficulty, and told him that

the only effectual remedy consisted in laying the sac open, and cutting off the protruded omentum. He begged that I would perform the operation, whatever the risk might be, as it would otherwise be impossible for him to follow his occupation successfully or comfortably.

Having resolved the matter in my mind for a few days, and becoming convinced that the operation was not only justifiable, but, under the circumstances, positively demanded, I consented to perform it.

*Operation, May 5th.* The patient's bowels having been thoroughly emptied, he was placed upon the table of the amphitheatre, chloroform administered, and the intestinal portion of the tumor returned into the abdomen. Dr. Foster, the accomplished Resident Surgeon, Dr. Smith, his assistant, Dr. Dirmeyer, and Dr. Hart, of Carroll County, Mississippi, kindly lending me their valuable advice and aid, I proceeded to operate in the presence of numerous students, who, from the first, had taken a deep interest in the case.

The first steps of the procedure did not differ materially from those usually followed in cutting for strangulated hernia, *i. e.*, the skin was divided by a long incision, commencing immediately above the neck of the tumor, and terminating just below the most prominent part of the body of the same, and the subjacent layers of fascia carefully raised upon a grooved director, and incised with a probe-pointed bistoury. The sac, which was much thickened, having been reached and cautiously slit up, but to a somewhat more limited extent than the skin and fascia, what was my disappointment on finding the omentum extensively and closely adherent to the inner surface of the latter. Such indeed was the extent and firmness of this adhesion, that it was almost impossible to distinguish between the two structures, and it was not until the omentum was divided, that the cavity in which the bowel rested was discovered. I attempted to effect a separation, but soon finding this impracticable, it became a most serious matter to decide what should be done. Canvassing all the difficulties as rapidly as possible, for the patient having already lost an unusual amount of blood, there was no time to be lost, I resolved upon ligating the neck of the sac, then dividing it below the point of ligature, and finally exsecting the entire sac and its included omentum. For the first of these steps I employed a silver ligature and cut off its ends close to the knot, and then divided the neck of the sac about half an inch below. I next proceeded to dissect out the thickened sac from its attachments in the

scrotum, taking care not to interfere with the component structures of the cord which was spread out and closely connected to the posterior surface of the tumor.

Two or three small arteries having been secured, the edges of the large gaping wound were brought together by three or four points of interrupted suture, the patient put to bed, a grain of morphia administered, and cloths wrung out of cold water directed to be incessantly applied to the parts. I further ordered half a grain of morphia to be given every two hours, until there should be a tendency to sleep, and to be continued thereafter every six or eight hours, my object being to prevent, in this way, if possible, the development of peritoneal inflammation.

Within six or eight hours after the operation, the abdomen became tense and resonant, but not at all tender upon pressure or percussion, and the patient was indisposed to sleep although he had taken three grains of morphia. I therefore ordered the medicine to be continued as before, and it was not until five grains had been taken, that he began to feel drowsy. The intervals between the doses were then increased to three hours, and this was continued for the two succeeding days without producing any narcotism. In the meantime, the wound which had been kept constantly under the influence of cold wet cloths looked as fresh as when it was first made. On the fourth day, however, it began to assume an angry appearance, and the surrounding parts became red and tumid. Erysipelas, that much and justly dreaded enemy of the surgeon, especially in hospital practice, had evidently begun its work, and soon spread throughout the scrotum and upon the abdomen, although the tension of the latter had somewhat subsided. Sloughing and deep suppuration about the wound, and in the sub-cutaneous areolar tissue of the scrotum was shortly the result, and in a few days the lower half of the left testicle was exposed. About the same time also, inflammation of a similar character appeared upon the face, and extended over the greater part of the scalp; and the patient's strength began now to decline quite rapidly. Under these circumstances, I directed brandy and egg and beef tea to be given in liberal quantities, the morphia to be continued, and in addition, twenty drops of muriated tincture of iron every six hours. Locally, cloths wrung out of tepid water were substituted for the cold applications from the onset of the inflammation, and when sloughing occurred the addition of a little chloride of soda.

Under this treatment, modified slightly as circumstances seemed to demand, the patient remained without perceptible improvement for nearly a week, when he began to rally; and the sloughs, which were large, soon became detached, leaving the parts in a tolerably healthy condition and without the loss of any very important structures. In the course of four weeks more—six weeks from the time of the operation—cicatrization was complete and the tissue over the abdominal ring firmly consolidated. Nothing was seen of the silver ligature after its application, and it is now doubtless lying harmlessly imbedded in the mass of organized lymph that blocks up the external inguinal ring.

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## PROGRESS OF MEDICINE.

### ART. 1.—*Statistical Researches upon the Development of Infants.*

In a report of the Children's Hospital, at Manchester, Drs. Schoepf and Whitehead have published, among other things, the results of their long and laborious researches upon those points which concern the more or less normal development of infants. From these we extract the following data :

*Solidification of the Anterior Fontanelle.*—This fontanelle possesses the largest dimensions, not at birth, but at from five to seven months of age; it then measures from one to two inches between its edges.

In well developed infants, the fontanelle was

At 6-7 months of age, closed in 3, and open in the balance.

" 8	"	"	"	8,	"	"	"	"
" 9	"	"	"	2,	"	"	"	"
" 10	"	"	"	2,	"	"	"	"
" 11	"	"	"	4,	"	"	"	11
" 12	"	"	"	11,	"	"	"	3
" 13	"	"	"	13,	"	"	"	3
" 14	"	"	"	13,	"	"	"	2
" 15	"	"	"	9,	"	"	"	0
" 15-18	"	"	all solid, with two exceptions.					

After 18 months of age, no well developed child had the fontanelle still open. In badly developed children the fontanelle was,

At the age of 7 months,					1,				
"	"	11	"	"	1,	"	"	"	"
"	"	12	"	"	3,	"	"	14	"
"	"	13	"	"	1,	"	"	12	"
"	"	14	"	"	5,	"	"	11	"
"	"	15	"	"	4,	"	"	12	"
16 mos. to 3 years,					13,	"	"	14	"

In a very few instances, of children from three to four years, the fontanelle was still open; all of such cases were of bad constitution and rachitic. These tables show that in well developed children the anterior fontanelle is generally closed at 13 months, which is not the case in badly developed children of this age.

Relation of the circumference of the cranium to that of the thorax: the first measured by a line passing over the upper part of the brow and the most prominent part of the occiput; the second from one-fourth to an inch or more according to the age, below the nipple.

Under twelve months, the circumference of the cranium was almost always greater than that of the thorax, rarely more than two inches greater. This difference was more marked between one and two years, amounting often to three and three and a half inches. Nevertheless, some extraordinarily robust infants were found, in whom the circumference of the thorax exceeded that of the head by two and a half to three inches. Between two and three years the relation is the same as between one and two years; only that the preceding exception became more frequent. From three to four years of age, almost all well developed children have a greater thoracic than cranial circumference. However, it is only at the seventh year that this predominance becomes constant, and annually augments.

From three months to four years of age, the circumference of the cranium enlarges six inches, whilst from four to twelve years of age, the increase amounts to only two inches. But during the latter period, the circumference of the thorax enlarges five and a half inches, and continues to increase whilst the cranium enlarges much less.

Precocity in walking is the most positive sign of the quality of development, without, however, having an absolute value; for delay in walking may result from either local or general causes, transient or durable.



Out of 164 well developed children, 9 commenced to walk at 9 mos.						
“ “ “ “	16	“	“	10	“	“
“ “ “ “	29	“	“	11	“	“
“ “ “ “	44	“	“	12	“	“
“ “ “ “	26	“	“	13	“	“
“ “ “ “	15	“	“	14	“	“
“ “ “ “	16	“	“	16	“	“
“ “ “ “	5	“	“	16	“	“
“ “ “ “	3	“	“	18	“	“
“ “ “ “	1	“	“	20	“	“

The proportions in 137 badly developed infants were as follows :

0 commenced to walk at 9 months.

1	“	“	“	10	“
2	“	“	“	11	“
8	“	“	“	12	“
4	“	“	“	13	“
10	“	“	“	14	“
14	“	“	“	15	“
20	“	“	“	16	“
22	“	“	“	17	“
48	“	“	“	“from 18 months to 3 years.	

8 (7 of whom were rachitic) commenced to walk after 3 years. Every well developed child has begun to walk by the 20th month.

In general, it may be admitted that a diminution in, or a loss of walking, is one of the first evident signs of an arrest of development.

*Influence of Diet on the Development of Infants.*—The author divides his observations on this point into five classes: 1st. Infants who had had from their birth, or from the second or third month abundance of mother's milk, and an artificial diet consisting of milk porridge, white bread in *eau sucrée*, and arrowroot. Total of this class, 105 ; of whom 55 were well developed, 29 moderately so, and 21 badly developed. 2d. Infants who had taken the breast until the sixth or ninth month, were then weaned, or nourished for some months longer with mother's milk and other aliments. Total number of this class, 45 ; of whom 30 were well developed, 9 moderately so, and 6 badly developed. 3d. Infants who were suckled (without other nourishment) until the ninth month or later, even to the twenty-fourth month. Total, 30 ; of whom 23 were well developed ; 4 moderately so, and three were badly developed. 4th. Infants who had received an insufficient

quantity of mother's milk for some months and as late as the ninth to to the eighteenth month, with other nourishment at the same time: total, 129; of whom 29 were well developed; 34 moderately so, and 66 badly developed. 5th. Infants who had not been suckled: total, 10; of these 1 was well developed; 3 moderately so, and 6 badly developed.

We would, however, observe that the disastrous influence of artificial nourishment upon the development of infants, as illustrated by figures taken from hospitals, is not perceived in like manner among the better classes. The hygienic conditions in which these children are found who are received in hospitals, cannot be compared to those which are enjoyed by the better classes. Has artificial nourishment the same effect, in these different conditions? Yet, in the last aspect, the higher classes approximate the inferior classes; for the children of the former often take an artificial nourishment which is too rich, and above all, too large in quantity; hence gastro-intestinal affections are very frequent. For those infants not nourished with the breast, the diet most to be preferred is cow's milk, pure, or with the addition of barley water, sugar, etc., according to the quality of the milk, and given in sufficient quantity and at regular hours, as the mother's milk is or should be.—(*Translated from L'Union Médicale*, by S. E. C.)

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ART. II.—*On Spontaneous Generation.*

- i. *Remarks of Prof. MILNE EDWARDS, on the value of certain facts as evidence of the spontaneous generation of animals, made before the Academy of Sciences at Paris,\* at the session of January 3, 1859.*

Physiologists have long been divided on the subject of the origin of life in organized beings. The larger part believe that this force exists only where it has been transmitted; that from the creation of the species till the present time, an uninterrupted chain of possessors of this power has communicated it successively; and that dead

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\* Comptes Rendus, 1859, p. 23. The Zoölogists and other members of the Academy who have here expressed their views on spontaneous generation, are at the head of their respective sciences in France.

matter has no power of organizing a plant or an animal unless it be submitted to the action of a living being or a germ that has proceeded from an individual of some species.

Others, on the contrary, have held that inert matter, under certain chemical and physical conditions, could take on life without the agency of a generating being ; that plants and animals may produce themselves in all their parts without deriving the principle of existence from another living body ; and that consequently life itself must be considered, not as a force which has been imparted peculiarly to organized beings, but as a general property of organizable matter manifesting itself under certain favorable conditions.

In my lectures and writings, I have often combatted this last doctrine ; and the hypothesis of *spontaneous generation* has to-day so few supporters among zoölogists, that I should have feared to abuse the patience of the Academy in discussing it at this time, had I not seen in the Report of a recent session of this body, that one of our correspondents, Mr. Pouchet, had made it the object of new researches and had arrived at conclusions, which, if right, sustain the idea that living beings may be made by the same general forces on which chemical combinations in inorganic nature depend. Since reading this memoir, I have thought it might be useful to submit to the judgment of my colleagues my reasons for rejecting its conclusions ; and it appears to me desirable also to know the opinions of other physiologists on a point of so much importance : besides, the question reaches beyond the domain of the natural sciences, and we may look for additional light from our chemists.

Long before the invention of the microscope had enabled zoölogists to discover the animalcules which are produced in myriads in waters containing an infusion of organic matters, it had been observed that dead bodies when left to putrefy often became populated with swarms of life ; and as the intervention of no living being was manifest in their production, the old naturalists supposed them a product of the putrefaction which was in progress, believing that the material, after ceasing to pertain to a living being, could reorganize itself under a new form and so constitute animals which had no parent ; accordingly, that life is not the cause, but the consequence of a certain mode of arrangement of the molecules composing these substances, and that this kind of molecular grouping could be determined by inorganic forces in nature.

The occurrence of maggots in carrion was one of the cases. But since the study of the origin of these animals by the Florentine Academy, happily named "del Cimento," and the exact investigations of Redi, one of its members, it has been well understood that these worms about dead bodies, far from being a result of spontaneous generation, are the brood of well known insects, species which find in such bodies the conditions requisite for development, and hence, through a marvellous instinct, deposit there their eggs.

The experiments of Redi, which date from the middle of the 17th century, left no uncertainty respecting these larvae. But while very easy to establish the fact respecting animals as large as flies, it was far less so with regard to infusory animalcules, which are discernible

only by means of the microscope, and whose germs are so excessively minute that they have escaped all the methods of observation which the science of optics has supplied. When, therefore, Lewenhoek and his successors made known the existence of these animalcules, the hypothesis of spontaneous generation regained favor. While some physiologists regarded them as derived from germs of extreme minuteness which were spread every where in nature, and floating as fine dust in the atmosphere, settled on all bodies to develop only where the conditions of air, water and organic decomposition favored; others denied the existence of germs, and supposed that under the dissolving action of the water, the dead organic substance took on life and so came out as new beings.

Analogy afforded a strong argument for the first of these opinions. The second has often been sustained by appeals to researches claiming that animalcules were produced under circumstances in which all germs from external sources were excluded, and all present in the waters used had been destroyed. Frey and several other observers have thought that they had succeeded in securing these conditions and still had found their infusions populated with microscopic plants and animals; whence the conclusion that these organisms were a result of spontaneous generation.

It does not pertain to me to pronounce on the origin of microscopic plants, for this difficult subject must be left to botanists. But as regards animals, I do not hesitate to say that the experimental conditions required to prove the truth of spontaneous generation have not been realized by any of the predecessors of Mr. Pouchet. And are the researches of this naturalist, that have recently been communicated to the Academy, free from the objections which are made against earlier experiments? I believe not: and before mentioning some observations I have had occasion to make on this subject, I will briefly state the reasons that lead me to this conclusion.

I do not question the facts stated by Mr. Pouchet. The point is, have these facts the significance attributed to them? I believe not. His experiment is briefly as follows. After having boiled some water and kept the liquid from contact with the air, he puts it into contact with pure oxygen, and introduces a certain quantity of hay, which had been previously enclosed in a flask and heated for half an hour in a stove whose heat was carried up to  $100^{\circ}$  C. or to the boiling point of water. The infusion thus prepared was hermetically sealed, and after some days Mr. Pouchet found infusoria developed in it.

To make these facts sure proof that the animalcules obtained were not derived from the hay put into the infusion, it must be shown that the heat of the stove had destroyed all the germs. Mr. Pouchet presumes that this is true, because on boiling in water the spores of a *Penicillium* he has seen that they were decomposed. But this reason does not satisfy me.

In the first place, was the hay, although enclosed in a flask and kept at thirty minutes in a stove at  $100^{\circ}$  C. ( $212^{\circ}$  F.), really carried up to the temperature of boiling water? Mr. Pouchet believes it; but I think to the contrary, and I think that physicists and chemists will judge so too. The equilibrium of temperature under such conditions

is not established so promptly as this ; it appears to me probable that the hay, enclosed in a glass vessel and surrounded by air in repose, both substances bad conductors of heat, was in reality heated but little by the heat of the stove during the short time it was exposed to it.

But supposing that the hay was heated up to 100° C., can we then conclude that the germs had lost their vitality and were incapable of development? No, for there is an important distinction here to be recognized between the action of heat on organized bodies which contain water and on those which are in the dry state. This follows directly from the researches, already old, of our learned colleague, Mr. Chevreul. Although in ordinary circumstances death takes place when animals are exposed to a temperature sufficient to determine the coagulation of the hydrated albumen in their tissues, we know that this is not always so in the case of those which have been previously dried. In fact, fifteen years since, Mr. Doyère made known that certain animalcules, such as the *tardigrades*,\* after being sufficiently dried would preserve their vitality for several hours while exposed in a stove whose temperature is much higher than that used by Mr. Pouchet for his flask of hay. I have seen these animalcules resist thus the very prolonged action of a stove whose temperature stood at 120° Centigrade (248° F.); and in the researches of Mr. Doyère, the heat of the ambient medium was carried to 140° C. (284° F.) without death ensuing from the heat.

What is true for the *tardigrades*, animals of a very complex structure, may also be true for the germs of infusoria in general ; and I conclude that nothing in the trials of Mr. Pouchet authorizes us to infer that the germs of the animalcules obtained by this naturalist were not in the hay that was used in his experiment. I will even say that the experiments of our correspondent do not seem to me to add any new probability in favor of the hypothesis of spontaneous generation.

I have often made analogous experiments ; and I have always found that the living animalcules which appeared in water containing dead organic matters, were increasingly rare the more complete the precautions employed for protecting the liquids from the introduction of germs. In more than one trial, I should have believed that spontaneous generation had taken place under my own eye, had I not, on reflecting on the conditions under which I was operating, perceived sources of error, and on setting these aside, observed negative results to multiply.

I will not occupy the Academy with the general recital of these trials, but will ask permission to recount briefly a single series of experiments in which some infusions, that if exposed to the air would in all probability have given birth to animalcules, afforded none when the imprisoned matters in the hermetically sealed vessel had

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\* The tardigrade animalcules are minute worm-shape animals about a fortieth of an inch in length, belonging to the rotatorio of Ehrenberg, and therefore much higher in structure than the ordinary infusoria.

been subjected to a temperature high enough to cause the coagulation of the contained albuminoid substances.

I placed in two tubes, having the form of test-tubes, the water and the organic matters for the trial. One of these tubes, which was two-thirds filled with air, was then closed by means of a lamp, and both this and the other tube were then plunged into a bath of boiling water. The bath was kept in ebullition long enough to establish an equilibrium between the water outside and the liquid of the two infusions; and then the tubes were allowed to cool and left to themselves, care being taken to examine the contents from time to time. After some days, I found animalcules in the tube which remained open to the atmosphere, but *not a single one in that which had been hermetically sealed.*

I have been accustomed to cite these experiments in my lectures, but had not thought of bringing them before the Academy, because negative results acquire importance only when they have been obtained constantly in a large number of trials, and also because the spontaneous generation of animals appears to me so little probable that I would not devote time to the repetition of researches on a subject which seems to be already settled. Only in view of the communication of our correspondent, and the interest that experimenting in this direction may excite in our young physiologists, have I been induced to bring out these facts among the reasons for still rejecting the hypothesis of spontaneous generation as an explanation of facts connected with the multiplication of animalcules.

An hypothesis which is not necessary in order to understand the phenomena made known by observations, and which is in flagrant discordance with all that analogy teaches us, seems to have no right to a place in science. It may be that chemistry will be able to make all the kinds of substances which occur in the constitution of living bodies; but as to the genesis of living organisms without the concurrence of vital force, I see no reason for believing it. Until more amply instructed, I shall therefore continue to think that in the animal kingdom there is no such thing as spontaneous generation, and that all animals, large and small, are subject to the same law, and can exist only when they have been generated by living beings.

ii. *Remarks on the same occasion*, by Mr. PAYEN, Professor in the Conservatoire Imperial des Arts et Metiers.

Some time in 1843, there occurred an alteration of the bread at Paris by a rapid growth of cryptogamic vegetation; and after having determined in connection with Mr. Mirbel the cause of the phenomenon, which had produced some excited dissatisfaction among the people, I endeavored to determine the temperature at which the sporules of the *Oidium aurantiacum* lost their germinative power. These sporules were heated at first for an hour to 100° C. (212° F.) in a tube inserted in an oil-bath. A part were then withdrawn and exposed in the proper circumstances for growth; and germination took place. The remainder of the sporules were then heated to 120° C.; and they neither underwent change of color nor lost their property of germination. Finally, they were heated to 140° C., when

their appearance was altered, the color became reddish-orange in place of brownish-yellow, and the germinative power was destroyed.

The results sustain, as regards the lower orders of vegetation, the opinion expressed by Prof. Milne Edwards respecting animalcules.

iii. *Remarks on the same occasion*, by A. DE QUATREFAGES.

I have often expressed on the subject of spontaneous generation similar opinions to those of Milne Edwards ; and I now give my full adhesion to the conclusions of my learned associate. I take the floor only to communicate to the Academy an observation, which, although incomplete, confirms ideas now generally admitted. [De Quatrefages adds some facts sustaining his opinion.]

iv. *Remarks on the same occasion*, by Dr. CLAUDE BERNRAD.

Among a large number of experiments which I have made to ascertain the influence of saccharine substances in liquids where microscopic vegetation was developed, I will cite one, as it bears directly on this subject of spontaneous generation now under discussion.

On the 1st of September, 1857, I put into two glass flasks, each half a litre in capacity, about fifty cubic centimeters of a same dilute solution of gelatine in water to which some thousandths of cane-sugar had been added. The liquid was then kept boiling in the two flasks for a quarter of an hour, the tubular neck of each having been previously drawn out so that it could easily be sealed. Up to this point there was no difference between the flasks. Now, when the flasks were still boiling and filled with steam, a difference was begun by allowing ordinary air to enter one, and highly heated air the other. To accomplish this, while ebullition was going on, the neck of one of the flasks was connected with one of the extremities of a porcelain tube filled with fragments of porcelain and brought up to a red heat by a furnace ; at the other extremity the porcelain tube was terminated in a glass tube of fine bore, so that the air should enter gradually and pass very slowly over the red-hot porcelain. Thus situated, the vapor of the liquid in ebullition rose into and filled the porcelain tube, and even passed out at the end of the fine tube. The lamp was then removed to arrest the ebullition ; and by degrees the steam was condensed and the outside air (air of the laboratory) entered to take its place, passing through the red-hot porcelain tube above described. After the liquid had cooled, the flask was hermetically sealed at the neck.

The other flask was allowed to cool without any connection with the porcelain tube, and the atmospheric air entered freely. When the flask was cooled it was sealed like the other.

The two flasks were then placed in the same conditions, exposed to the light and to the ordinary temperature. After ten or twelve days, at the surface of the flask containing the ordinary air, vegetation was visible, a well-characterized mould, whilst in that which had received the heated air the liquid remained perfectly limpid, and without any thing on its surface. After a month the mould had much increased in

the former, while nothing had appeared, except that the water had slightly lost its clearness. After six months (March 4, 1858) the mould remained stationary in the former, while in the other the liquid continued the same, without any trace of mould.

The extremities of the two flasks were now broken under mercury. In the case of the one with heated air, considerable mercury was absorbed, but none in the other. The air of the two flasks being analyzed, no oxygen was found in either. The air from the flask with ordinary air contained 13.48 per cent. of carbonic acid, that of the other, in which no mould had formed, 12.43 per cent.

The liquid of the flask with ordinary air had a putrid and very disagreeable odor, while the other had none. These liquids were examined by Mr. Montagne; and our associate ascertained that the mould developed in the flask with ordinary air was the *penicillum glaucum*, which was in full fructification; in the other he found no trace of any vegetable or animal organism.

It is plain that this experiment, like those which have been before cited, is not favorable to the hypothesis of spontaneous generation.

v. *Remarks on the same occasion and subject by the chemist DUMAS.*

Dumas stated that he was in full agreement with his honorable associates. For thirty years he had had under careful examination the question on which Prof. Milne Edwards had instructed the Academy with so high authority, and he had arrived at precisely the same conclusions.

He was incited to experiment on the subject by the publication of Mr. Frey, who had announced results analogous to those communicated to the Academy by Mr. Pouchet.

In his experiments he has assured himself that organized matters heated to 120° C. or 130° C. with water artificially made by means of hydrogen and oxyd of copper, and with artificial air in closed tubes, the glass of which had been recently heated to a red heat, produced neither vegetation nor animalcules. On opening these tubes and allowing ordinary air to enter, there was soon an appearance of vegetation and animalcules. These results had surprised him, as he was disposed to think that the germs of these plants and animalcules might be distributed in the organized matter as well as in the air itself, and that certain of these germs might well be of a nature to resist a temperature of 100° C. or even a higher temperature.

As the tardigrades when absolutely dry resist 140° C., and the sporules of *oidium aurantiacum* 100° C. in a moist medium, it will not suffice in order to establish the hypothesis of spontaneous generation, that living beings should sometimes appear in boiling water in contact with artificial air and with the presence of organic matters that had before been heated, especially if these matters were heated when dry. When among these inferior animals and plants, life is suspended by absolute desiccation to return to action again on a return of humidity, the being so treated is in that state of latent animation which belongs to germs. It is hence a matter of astonishment that on putting heated organic matters into connection with oxygen and artificial



water, we do not sometimes find living beings to appear. Even such an observation as this, would not therefore suffice to establish the theory of spontaneous generation, or prove that the germs of these beings were not previously deposited in the organic matters employed. But, in fact, whilst animalcules appear when the ordinary air has access, without this access under the precautions mentioned they do not appear.

vi. *Note on Spontaneous Generation*, by JAMES D. DANA.

1. There is a well-known principle in the system of nature that deserves to be considered in this connection. The principle is so fully sustained by all research both in chemistry and zoology, including the important experiments above mentioned, that it may well carry with it great weight, and quiet both apprehension and expectation on this subject. It is this: The forces in life and inorganic nature act in opposite directions, the former *upward*, the latter *downward*.

The vital force, in the organic substances it forms, *ascends* through vegetable and animal life to an exalted height in the scale of compounds at an extreme remove from saturation with oxygen; inorganic force *descends* towards the saturated oxyd. The former reaches a point which from its very elevation is one of great *instability*; the latter tends towards one of perfect *stability*. There is hence a counterpart or cyclical relation between the two great lines of action in nature.

As some readers of these remarks may not be familiar with chemistry, a further word of explanation is added.

When an element unites with its full allowance of oxygen, as determined by its affinities, it is in a sense saturated with it. Since the attraction of the elements for oxygen is the most universal and, in general, the strongest in nature, the oxyds as a class are the most stable of compounds; the rocks, the earth's foundations are made of them. But evanescence and unceasing change are in the fundamental idea of the living structure; and, consequently, the material of the plant or animal contains only oxygen enough to give increased instability to the combination. Moreover, the compounds augment in instability, through this and other ways, with the rise in the grade of organic life, and reach probably their farthest extreme in this respect in the brain. Here then is the summit of the series of compounds which arise under the agency of life. The stable oxyd is at the lower end of the series in nature, the material of the brain at the upper. Passing from the latter condition towards the former is therefore a real descent; and it is the natural downward course of inorganic forces; while passing towards the latter is as truly an ascent;—it is the counter-movement of life.

The plant through its vital functions may take carbonic acid, and from it continue to elaborate the organic products constituting vegetable fibre, until a whole tree of such material is made, and then produce the higher material of the flower and seed. The animal may then go to the plants and use them in making a still higher

class of products, muscular fibre and nerve. After all this is done, now turn over the material to the action of chemical and physical forces—and the work of years of life is soon pulled down from its height, and one part after another descends towards that state of comparative inactivity, the condition of an oxyd. Chemistry makes organic products by commencing with those of a higher grade than the kind to be made, but not otherwise. Albumen is a prominent material of the egg; and chemistry has not succeeded in making dead albumen, much less living.

The very relation of life to chemistry is therefore evidence that chemistry cannot make life; it works in just the reverse direction. And in this reciprocal relation one of the profoundest laws of nature is exhibited. It leads the mind to recognize one author for both, and not to imagine that one side in the cycle has generated the other.

2. There is another consideration, which, if it has not the force of demonstration, may help the mind to understand the extent of the transition from dead matter to living.

(a) In ordinary *inorganic* composition, there is the simple formation of inorganic particles, and, on consolidation, their aggregation into crystals, the perfect individuals of inorganic nature. With the enlargement of the crystal there is no gain of new power or qualities: it simply exists. In fact, in entering this state of perfection, there is a *loss of latent force*; for the gas is the highest condition of stored or magazined force in inorganic nature, the liquid the next, and the solid the lowest, this condition of power being related directly to the amount of heat.

(b) The *plant* grows from its germ, enlarges, accumulates force storing it away in vegetable fibre, and accomplishes its highest functions in its blossoms and fruit. But there is here only *latent or stored force* generated, besides that which is used up in growth, and *no mechanical force*. The minute spore or reproductive cellule of some seaweeds has locomotive power, but it is lost at the commencement of germination; and the plant is ever after as incapable of self-locomotion as a rock.

(c) In the *animal*, there is not only a storing of force in animal products (the fifth and highest grade of stored force in nature), but there is also increasing *mechanical force* from the first beginning of development. It is almost or quite zero in the germ; but from this, it goes on increasing until in the horse, it gets to be a one-horse power; or in the ant, a one-ant power; and so for each species. And in addition to mechanical force, there is, in the higher group, the more exalted *mental force*; for the mind, while not itself material, is yet so dependent on the material, that its action draws deeply upon the energies of the body. To make an animal germ is then to make a particle of albuminoid substance that will grow and spontaneously develop a powerful piece of machinery, and continue a system of such generations through ages of reproduction.

The creation of any such animal germ out of dead carbon, nitrogen, hydrogen and oxygen, or any of their dead compounds, is therefore opposed to all known action or law of chemical forces; and as much so, the creation of a vegetable germ from inorganic elements.

Moreover, it is seen that the two kingdoms, the vegetable and animal, have their specific limits and comprehensive reciprocal relations, and are obviously embraced as parts of one idea in a single primal plan—not a plan involving the generation of one out of the other, or of either out of inorganic nature, but of the three, through some Creating Power higher than all.—*American Journal of Science and Arts*, May, 1859.

vii. *Spontaneous Generation.*

In a recent number of the *Lancet*, the following allusion to this subject occurs :

French physiologists, and especially those who are members of the Academy of Science, of Paris, have been lately bringing forward arguments to support the belief that spontaneous generation has no existence. Many of our readers will, perhaps, be surprised that any trouble should have been taken to defend so universally-received an opinion; but such exertions have become necessary, as the experiments of M. Pouchet, lately brought before the Academy, have been apparently almost conclusive in proving the existence of spontaneous generation. We shall enter into a few particulars respecting this controversy when it has been brought to a close; but we may now mention that M. Pouchet maintains that what many physiologists look upon as ova of infusoria, are particles of fecula. Nor does M. Pouchet believe that rotifera can bear a very high temperature after having been previously dried, and be resuscitated by the action of moisture. The phenomenon is, according to him, deceptive, and due to simple edosmose.

*The Medical Times and Gazette* (April 30, 1859) has the following paragraph upon the topics discussed *ut supra* :

*Omne vivum ex ovo.* This dictum of Harvey is now submitted to by the French Academy of Sciences. M. Flourens considers that a word or two may yet be said in favor of spontaneous generation. The Academy has, therefore, determined to let loose the vigor of young science on this subject, and thus to endeavor out of stones and gases to make living entities. It offers a gold medal, of the value of 2500 francs, for a satisfactory exposition of the subject. "Competitors must endeavor, by properly made experiments, to throw new light into the question of generation called spontaneous." The essay must be sent, post paid, to the secretary of the Institute, before the 1st October, 1862. We shall expect to see the quadrature of the circle or perpetual motion looking up again soon in the scientific market. At all events, if M. Pouchet can manufacture an infusorium in 1862, we see not why his great grandchild need despair of making cats or monkeys in 1962 by some "happy combination of elements."

viii. *Dust floating in the Air.*

M. Pouchet finds that the dust floating in the air contains the detritus of the mineral constituents of the globe, atoms of animals and

plants, and the finest débris of all the matters we make use of. But one item he especially points out, viz: *wheat-starch*, which is invariably found in the dust, whether old or recent. Surprised at the quantity of it present among the aërial corpuscles, M. Pouchet investigated the dust of all ages and of every locality; and everywhere he found this wheat-starch presented. "I have found the starch in the most inaccessible corners of old Gothic churches, mixed with dust blackened by six or eight centuries of existence; I have found it in the palaces and caves of the Thebiad, where it may have dated from the time of the Pharaohs! I have found it in the tympanic cavity of the tympanum of a mummified dog, which I had found in a subterranean temple of Upper Egypt. In all countries, in a word, where wheat forms the staple of food, starch always penetrates into the dust, and is met with in greater or less quantities. Hence, therefore, the *corpuscles* of which we have heard so much are granules of starch and silica." Twice only in a thousand experiments has M. Pouchet observed the large ova of infusoria in the atmospheric dust.—*Med. Times and Gaz.*, May 14, 1859.

ix. *On the origin of Vibrio*: By H. JAMES CLARK, of Cambridge, Mass. (From the proceedings of the American Academy, Boston, April 12th, 1859.)

[The following papers from that learned periodical, *The American Journal of Science and Art*, for July, 1859, will be read by the physiologist with interest, independently of their bearings on the doctrine of spontaneous generation.]

A few months ago a French physiologist, Pouchet, revived the long-exploded doctrine of equivocal or spontanecus generation, and asserted that he had been able to obtain certain living beings from substances which were entirely shut off from the outer world, and in which, after having undergone certain preparations, there could not possibly be any germs of these animals. A discovery, which I made on the 20th of March, may not be uninteresting, as it has more or less relations in its nature to the theory so earnestly advocated by Pouchet. There are certain well known bodies described as animals by Ehrenberg, under the name of *vibrio*; their peculiarity consists in that they are composed of a single row of globular bodies, resembling a string of beads, more or less curved, and move in a spiral path with great velocity, even faster than the eye can follow in many cases. They exhibit, by their activity, more plausible signs of animality than any of the *desmidiæ* [or *diatomacææ*, and fully as convincing indications of life as the spores of *algæ*, to which they were first referred by the late lamented Dr. W. I. Burnet, and after him by Rudolph Wagner and Leuckart. They have always been spoken of as developing around decaying animal and vegetable matter. I was very much surprised to discover the manner in which they originate from such substances. I was studying the decomposing muscle of a *sagitta*, a little crustacean, as I consider it—which, in passing, I would observe was found by me a year ago last March,

for the first time in this country, at Lynn Harbor—when I noticed large numbers of vibrio darting hither and thither, but most frequently swarming about the muscular fibres. I was struck with the similarity of these bead-like strings to the fibrillæ of the muscle, and upon close comparison I found that the former were exactly of the same size, and had the same optical properties as the latter. Some of these appeared to be attached to the ends of the flat, ribbon-like fibres, and others at times loosened themselves and swam away. I was immediately impressed with the daring thought, that these vibrios were the fibrillæ set loose from the fibres; but as this was a thing unheard of, and so startling, I for the time persuaded myself that they must have accidentally attached and subsequently loosened. However, I continued my observation until I found some fibres in which the fibrillæ were in all stages of decomposition. At one end of the fibre the ultimate cellules of the fibrillæ were so closely united, that only the longitudinal and transverse striæ were visible; further along the cellules were singly visible, and still further they had assumed a globular shape; next, the transverse rows were loosened from each other excepting at one end; and finally, those at the extreme of the fibre were agitated and waved to and fro as if to get loose, which they did from time to time, and, assuming a curved form, revolving each upon its axis and swam away with amazing velocity. There was no doubting, after this, the identity of the vibrios and the muscular fibrillæ; but I thought such a strange phenomenon ought to have a second witness to vouch for it, and therefore went for the best that could be wished for, Professor Agassiz. I simply placed the preparations before him, and, without giving him the least hint of the origin of the muscle, I was pleased to have him rediscover what I had seen but fifteen minutes before.

The number of ultimate cellules in a moving string varied from two to fifty; the greatest number of strings were composed of only three or four, often six to eight, and rarely as high as fifty. Very rarely the fibres split longitudinally, and in such instances the fibrillæ were most frequently long, and moved about with undulations rather than a wriggling motion. A single ultimate cellule when set loose, danced about in a zigzag manner; but whenever two were combined, the motion had a definite direction, which corresponded to the longer diameter of the duplicate combination; and if only three were combined, the spiral motion was the result of their united action. What it is that causes these cellules to move I do not profess to know, but certainly it is not because they possess life as independent beings. This much is settled, however, that we may have presented to us all the phenomena of life, as exhibited by the activity of the lowest forms of animals and plants, by the ultimate cellules of the decomposed and fetid striated muscle of a sagitta. I do not pretend to say that everything that comes under the name of vibrio or spirillum is a decomposed muscle or other tissue, although I believe such will turn out to be the fact; but this much I will vouch for, and will call on Professor Agassiz to witness, that what would be declared, by competent authority, to be a living being, and accounted a certain species of vibrio, is nothing but absolutely dead muscle.

x. *On Apparent Equivocal Generation*: by H. JAMES CLARK, of Cambridge, Mass. (From the proceedings of the American Academy, Boston, May 10, 1859.)

At the close of our last social meeting I was asked if I had seen any trace of organization in the globules of the vibrio-like fibrillæ of the muscle of sagitta. My answer was in the negative. No longer ago than yesterday I was fortunate in discovering the origin of another, or rather of several forms of these pseudo-animate bodies called infusoria. Whilst watching the decomposition of the inner wall of the proboscis of a young *aurelia flavidula*, our common jelly-fish, I observed that the whole component mass of cells was in violent agitation, each cell dancing zigzag about within the plane of the wall. If any one will shake about a single layer of shot in a flat pan he can obtain an approximate idea of the appearance of this moving mass. In a perfectly healthy condition these cells lie closely side by side, and do not move individually from place to place, but yet are active on one side, which constitutes the surface of the stomach, where they are covered by vibratile cilia. As the young aurelia grows, this wall becomes separated from the outer one, but not completely, for the cells of the two adhere to each other by elongated processes varying in number from one to six or seven. Each cell of the inner wall contains numerous red or brown granules, a few transparent globules, and a single large clear mesoblast. When decomposition ensued, these cells became still farther separated from each other and danced about in the manner which I have thus described. The vibratile cilia were not observed to share in this movement; in fact I could not detect their presence, because, no doubt, they had become decomposed and fallen away; but the elongated processes, which heretofore had remained immovable and stiff, lashed about with very marked effect upon the cells to which they belonged, and caused them to change place constantly. At last the inner wall fell to pieces and every cell moved independently and in any direction. If at this time they were placed before the eyes of Ehrenberg or any one of his adherents, he would at once pronounce every cell with a single process a *monas*; the red or brown granules would be recognized as the stomachs filled with food, the transparent globules as the empty stomachs, and the large mesoblast as the genital organ or propagative apparatus. Those with two processes would be to him a *chilomonas* or some other genus closely related to it; those with three or four on one side would be the *oxyrrhis* of Dujardin; and those with six or seven processes the *hexamita* of the same author. To complete the apparently truthful determinations of these microscopists I would only have to place before them some of these cells which I have found in a state of self-division, each half possessing its genital-like mesoblast. In all their various shapes and actions, and in the mode of self-division there is a remarkable and undistinguishable resemblance to numerous moving bodies which go under the name of infusoria, and which may be found, unconnected with any living organism, in various kinds of infusions.

ART. III.—*On the Tactile Sensibility, and the Means to Measure the same in Anesthesia and Hyperesthesia*: By E. BROWN-SÉQUARD, M. D.

THE curious results obtained by Ernest Henry Weber, when investigating what differences existed in the diverse parts of the skin, in relation to the sensation it gives, when touched simultaneously by the two points of a compass, are generally known. We are aware also, that when those points are brought near to each other, and on this side of a given limit, the individual upon whom the experiment is made, only feels one of them; whereas, when they are at a greater distance from each other, and beyond a certain limit, both points are felt. The limit within which one point only is felt, although two are applied, and that beyond which the two points are felt, varies greatly for the diverse parts of the skin, as Weber and twenty other experimentalists have stated; but it varies only very little, for a similar part of the skin, with different individuals in a healthy state, and moreover, it is the same or nearly so, for the homologous parts of the right and left sides. In 1849, it occurred to me to make use of this valuable expedient, to discover the existence, and degree, of the anesthesia and hyperesthesia.

The results of the researches I made at that time (especially under the service of M. Rayer), are recorded in the *Comptes rendus des Séances de la Société de Biologie*, 1849, p. 162. (See also *Gaz. Médic. de Paris*, 1849, p. 1012, and the article *Touch*, in Todd's *Cyclopædia of Anatomy and Physiology*.)

From the facts I observed in 1849, and have seen since, this process has enabled us:

1st. To discover the existence of a decrease or increase of the tactile sensibility in these cases, where, by other means, it would be impossible to ascertain it.

2d. To discover exactly the degree, of diminution or augmentation of tactile sensibility.

3d. And lastly, to assure ourselves in a precise manner, of the changes which supervene, in the cases of anesthesia and hyperesthesia, under the influence of the progress of disease, or the action of medicines.

Among the cases, where the application of the points of the compass, has given me very interesting results, I will mention the three following:

1st. Upon a paraplegic, the two points of the instrument, applied simultaneously upon one of the legs, gave but the sensation of one single point, whatsoever the distance between the two extremities was—10, 15, and even 20 centimetres—and whatever the parts of the leg, where the compass was applied.

I have seen two similar cases, in one of which it was the thigh, in the other the arm, where the phenomena could be observed. In two of the three cases, there was a loss of the knowledge of the place whence the excitation arose.

The patients felt they were touched, but did not know where. In the other case, the patient indicated, that the impression came from

the neighborhood of the place of application of one of the points—sometimes the one, sometimes the other.

2d. In a case of paralysis of the movement of the inferior extremity, the patient perceived the sensation of the two points on the back of the foot, even at a distance of five millimetres, which in its normal state, at the same place, the sensation of the two points is only felt when at least at a distance of from twenty-four to twenty-eight millimetres. The hyperesthesia in this case existed in an excessive degree, not only for what concerns the touch, but even in regard to the sensibility to pain, to cold, and to heat.

3d. In a case of fracture of the fibula, with laceration of the perineal nerve, the sensibility became, at first, nearly completely lost, and I have been enabled to state the rapidity of its return, and its almost daily change, by means of the compass. The theory of Weber, by which the sensation of one single point, when two are applied, should be occasioned by only one single nervous filament which might be impressed, cannot be sustained; and it would be easy to show, by means of figures, that by keeping the two points of the compass very near to each other, and by applying them alternately to parts very near to each other (the points being always within the zone of the preceding experiment), we could run over all the surface of the body, and give every where only the sensation of one point; from which we would be led to the absurd conclusion, that there is but one simple tactile nervous fibre for all the skin.

Meissner, by supposing that the sensation of one or of two points, depends upon the corpuscles of touch, has set forth an opinion altogether as extravagant.

In the case of hyperesthesia, which I have mentioned above, we will have to admit, if Weber, or if Meissner, were right, that new nervous filaments had formed themselves, or that the corpuscles of touch had augmented.

Since we have made known the utility of the compass, in the diagnosis of the morbid modifications of the tactile sensibilities, several physicians have made use of this process. We will mention among them specially Dr. R. B. Todd, one of the most eminent practitioners of London, and Dr. E. H. Seiveking, the capable director of the excellent English review, *The British and Foreign Medico-Chirurgical Review*. In that Journal is found a note on an *asthesiometre* by Dr. Seiveking. This instrument is nothing more than a compass for measuring thicknesses, slightly modified. This compass being in fact superior to the compass of mathematicians, we had one made which differs, in certain respects, from the one which is represented in the English review. We find in all the recent treatises on physiology, tables indicating for the diverse parts of the body, in its normal state, the limits where we begin, and where we cease to feel the two points of the compass (according as they are brought near or widened).—*Oglethorpe Medical Journal*.



ART. IV.—*Syphilization*: By M. W. BOECK, of the University of Christiana, Norway.

IN the eight number of the *Gazette Hebdomadaire*, of the present year, I find a succinct review of the pamphlet of M. Damélszen, on syphilization, considered as a curative method of syphilis and *spedalskhed*.

In this review is mentioned my opinions concerning the effect of syphilization, and I am made to say, that, by saturating the system with the poison of a chancre, the successive inoculations produce the cure of, and immunity from syphilis. I would, under all circumstances, regret such statements; and I will explain my views in reference to the effects from protective influence of syphilization.

We have seen in Christiana a great number of persons cured of syphilis by syphilization. The fact cannot be denied; but the explanation of this question is as interesting as difficult. I observed my first cases with great exactness; and I supposed that syphilization produced in that organism, relatively to syphilis, a disposition analogous to that caused by vaccination as related to variola and cow-pox. This does not aid us much, since we do not understand the action of vaccination. What we prove, and what concerns us above all things, is the great immunity which vaccination confers for a long time against variola and cow-pox. This is the opinion that is contended for, and we shall see whether it possesses any truth. Briefly, does this immunity arise from any impregnation of the organism by vaccine virus, or by the overthrow of a disposition which the system possessed for contracting variola or cow-pox? One thought which has led me to believe that it does not act by the destruction of an original disposition is, that children born of parents recently sick of small-pox, may come into the world with a great susceptibility to this disease. But if vaccination does not take anything from the system, does it necessarily follow that it adds something thereto, and that this something is an impregnation, a saturation of the humors of the virus? I have no knowledge of this matter, but, I confess, I find difficulty in believing it; for a saturation of the organism by a virus would not bestow much benefit upon the individual. This is the reason why I tend to admit, that the disposition to contract the disease has been repelled or lies dormant. It is no longer manifested in the individual, but it still exists in the body, since the child possesses it at birth.

Does the same view apply to syphilis? Would the malady be

simply repelled, and not destroyed, by syphilization? I do not possess sufficient knowledge to decide in a categorical manner. These mothers who were syphilized gave birth to children in whom the symptoms of syphilis appeared in a short time after birth. This would seem to indicate that the syphilis was not extinct in the mothers. But this is only a presumption; for the syphilitic virus slowly pervades the organism, and the effect of the last inoculations still continues after the ulcerations have dried up and the sick have left the hospital. It is, therefore, necessary to take in consideration the time that has elapsed between the completion of syphilization and the moment of conception. Let us then suspend, for some time, a definitive judgment on this point, and let us confine ourselves, for the present, in all things concerning syphilization, to observe nature tranquilly and without prejudice. But let us suppose a most unfavorable case, namely, that we did not obtain, in reference to the birth of children, a better result than by the mercurial treatment. That ought still to satisfy us, expecting that the parents enjoyed as perfect health as before constitutional syphilis existed among them, and that children, in their turn, can be treated by syphilization with as much benefit as by mercurial preparations. To speak of all that can be said against syphilization, I shall report that I have recently had a relapse in a man treated for two years and four months. But that man had constitutional symptoms for a year before being treated. There was an obstacle in this case that I shall explain at another time.

I shall now notice the motives that induce me to believe that syphilization acts in a similar manner to vaccination, and, not, as many have recently supposed, by a simple derivation, that is, by the production of ulcers that procure the depuration of the morbid material. I shall not devote a long time to theoretic considerations. I shall not examine whether there are other dyscrasies which afford a like depuration, nor whether there is another malady, which having gradually penetrated, that is, molecule by molecule, into the blood, can be drawn through the skin by derivatives. We should be very happy in possessing the powers of extracting from the body by cantharides, tubercle, elephantiasis, and cancer. But, unfortunately, there is no truth in this, and such a fact may be affirmed as certainly of syphilis as of other maladies. Therefore, let us make a truce with all theory, and confine ourselves to facts. I would state that I have learned from a thousand observations, and now it shall be decided whether

I am correct in supposing that syphilization acts in a manner analogous to vaccination.

1. When a person attacked with constitutional syphilis, is inoculated with the virus of a primitive chancre, either *simple*, or *indurated*, and when the inoculation of the pus from the ulcer produced by the last inoculation is repeated every third day in this person, that matter furnishes no result. Is it because it has lost all power? Certainly not; for if it is introduced into a subject not syphilized, it causes in the latter as much effect as it produced on the first in the beginning of the treatment.

2. If instead of taking the last pustule, which gave no positive result, we take the pus of an inoculation earlier in the series, we shall then obtain a result; and it will be possible, in dating from that moment, to obtain a succession of events almost as if we had commenced a new series.

3. Then inoculate at the same time two persons with the same matter, and they will acquire nearly at corresponding periods protection against this matter. But if matter be taken from one in order to inoculate the other, the matter of the last pustules, which gave only negative results in one of the subjects, will give positive in the other, *et vice versâ*, and that during two or three generations.

4. The protective influence of the matter takes effect in the same time, whatever may be the number of inoculations made each time, even if one only was made at a sitting.

5. The first matter not producing any result, another may be taken which shall give a positive result, but for a series of generations less considerable than for the first matter, and thus also, in regard to the third and fourth matter, etc. And then we arrive by pustules and a series of generations gradually decreasing to negative results in the sequel of inoculation with every kind of matter. I therefore repeat what I have said elsewhere. *The syphilitic virus is at last not more active than a drop of water.*

Are the facts which I have related reconcilable with the idea of a peculiar, and, as it were, anæmic condition of the skin? It might be affirmed, without doubt, that the new matter was more intense, since it had been effectual even when the old had failed. But can as much be said when recourse is had to the inoculation of a matter more distant in the series, or rather when (as I have related above, in two persons inoculated at the same time with the same matter),

the matter is inoculated in both in such a manner as to produce a result? Why then does not the thigh in which three inoculations are made, attain greater immunity from disease than that in which only one is made?

I shall now speak of local immunity, which has been a subject of great doubt. I observed this effect a long time before daring to mention it. I found it so strange that I considered myself deceived in making observations, or more accurately, I believed it to be produced by accidental circumstances. Therefore, since it really exists, we must conform to the fact and study it. The following is the result of my observations :

1. When one part of the body is inoculated so as to afford protection, the matter, which no longer caused any effect on that part, will give a positive result if applied to another part of the body.

2. But the effect produced on this last part of the body is less forcible than if the matter had been at first applied to this region.

3. In regard to influence which the inoculate matter has on the entire body, it is not a point of indifference in inoculating one part rather than another. When repeated inoculations are made on the thighs alone even to complete protection, the result which is obtained at a later date in inoculating the arms and trunks will be insignificant. If, on the contrary, we commence on the arm and trunk, a considerable result will finally be obtained on the thighs, without which we ought still to disregard the influence on the entire body of inoculations previously practiced on the trunk and arms.

From such results, local protection far from being invoked as an objection against the effect of syphilization on the system, serves on the contrary to confirm it. "But it has been objected, that it does not comply with what is understood by the word *immunity*, nor with what is considered as forming true principles in physiology. Then of course it cannot be advisable." There are still some facts beyond gainsaying, that I shall relate. Nature does not wish to lose any of her rights, and justice must be awarded her in whatever concerns syphilization as well as in all other things; and then it remains to know whether the fact is entirely novel, or whether, on the contrary, it does not exist by analogy. I do not think it necessary to go far in order to explain the observation. We find a parallel case in vaccination. Among children that I have vaccinated in late years, I have inserted eight days after the first inoculation, the matter

furnished by this inoculation in the arms and thighs, and I have sometimes obtained on the thighs vaccine pustules more or less developed. I reported some facts on this subject in 1857 to our medical society.

We have here the same local immunity, and I have seen with satisfaction that this phenomenon has been recently observed at Paris, by M. Coste, who having touched by accident, the end of his nose with the point of a lancet charged with vaccine, had on that part a vaccine pustule, which the inoculation made on the arm with the same matter and at the same time gave no result. It is very possible that we shall arrive by syphilization to greater precision in our knowledge of vaccination, each virus certainly not presenting many differences. It behooves us to examine with care their analogies and differences.

There is still an important point concerning immunity. I refer to its continuance, which has been confounded with its existence. As I have explained it, the protective influence can be produced in each individual; but after some weeks or months, the same person becomes again susceptible to the contagiousness of syphilis. Still it is necessary in such persons to produce some pustules and ulcers by a primary inoculation. But they will not be developed as well as in the primary inoculation, and it will be possible to produce only a limited succession of generations. Thus we perceive that the organism does not possess the same aptitude in sustaining the syphilitic influence as before syphilization. Some time after vaccination, an individual may be attacked by varioloid, and at an indefinite period even by variola. We do not know whether after syphilization the system returns to its former condition, and whether the inoculated virus possesses sufficient activity to produce a succession of generations similar to the first; yet analogy renders such a conjecture rational.

My observations induce me to admit that repeated inoculations of syphilitic virus impress the organism with an universal modification perfectly analogous to that which is produced by vaccination. Such is my reason for hoping that we have found in this method an efficient aid against syphilis, and the great number of fortunate results obtained by syphilization prove to me that such a hope will not be deceived.—*Trans. from Gaz. Hebdom. de Méd. et de Chir.*, by W. C. N.

ART. V.—*Mercury in Syphilis: pro and con.*

- i. *On the Evils Resulting from the Non-mercurial Plan of Treatment in Syphilis:* By Dr. CAHILL. (Read before the Western Medical and Surgical Society.)

Dr. Cahill commenced by narrating a number of cases in which the constitutional effect of syphilis had been very severe, and has extended, in spite of treatment, through many years, in all of which the early stage of the disease had not been attacked by mercury. After detailing these very carefully, he entered into the history of this plan of treatment, remarking that most of the Irish surgeons who have adopted it have since changed their opinions respecting it. He maintained that the severest and worst forms of constitutional syphilis occur when no mercury whatever has been given for the primary disease, and that the notion that the severe cases of secondary disease are mainly due to the mercury administered is not founded in fact. Mercury, to be of use, must be persevered in for at least six weeks, and to produce its good effects need not be given lavishly or recklessly. If attention be not paid to this fact, no permanent effect is produced, and other forms of the disease, as secondary or tertiary, are apt to follow immediately after. He recommended the use of the bichloride in doses of one twelfth to a quarter of a grain, together with the inunction of the strong mercurial ointment, until the gums showed that the system was affected by the mercury. The bichloride is borne better than any other form of mercury, and can be continued with safety longer than any other mercurial preparation, not only in those cases, but even scrofulous diseases, in which its use is advisable; hence its peculiar value. Iodide of potassium, given with iodine, is the next remedy to be relied upon, though it is chiefly valuable in secondary and tertiary affections of the periosteum. In tertiary symptoms, the iodide is our sheet-anchor, and mercury should never be given in such cases to the extent of producing its specific effects; should a mercurial be required, the hyd. c. cretâ is to be recommended in conjunction with the iodide of potassium. Iron, arsenic, and mineral acids, under certain circumstances, are useful, especially after mercury has been fairly tried; these, with the ordinary means of restoring health, as good food, pure air, and rest, will generally promote a cure. He then alluded to the difficulty of distinguishing primary chancre, and the means employed by inoculation, and the peculiar hardness of the true Hunterian chancre, and to the chance there was of a chancre in the urethra being overlooked, and to other sources of error as to the curability of syphilis without mercury. He concluded with the following observations: 1st. That the severest and most prolonged forms of the disease have arisen where no mercury has been given for the primary or early secondary affections. 2d. That as no symptoms identical with those of constitutional syphilis are produced by mercury, the notion that mercury is a cause of constitutional syphilis is founded in error. 3d. That the administration of mercury may be so regulated as to preclude any of its severe effects; and supposing any are produced, it is manifest that they would be less severe than those caused by constitutional syphilis. 4th. That

the constitutional effects of mercury should be avoided in tertiary syphilis. 5th. That the supposed successful treatment of syphilis without mercury, is founded on erroneous diagnosis, or the cases have occurred in individuals already protected, or the subjects have been of that class who seem to enjoy immunity from the worst part of the complaint.—*Lancet, Braithwaite.*

ii. *Mercurial Inunction in Secondary Syphilis.*

Professor Hebra, in a late discussion at the Vienna Medical Faculty meetings, observed that he generally treated secondary and tertiary syphilis with the iodide of potass; but found the cure far less rapid and complete, and relapse much more frequent than when mercurial ointment was employed. During the last seventeen years he has kept an exact account of 2125 cases of secondary syphilis treated by inunction—25 drachms, and often much more (in one case 288 drachms) of the ointment being employed for each case. Out of this large number he has never met with a single case of chronic hydrargyrosis. *Wien Wochens*, 1859, No. 11.—*Med. Times and Gaz.*

iii. *Diseases caused by the action of Mercury.*

*To the Editor of the Gazette Hebdomadaire*—A late number of the *Gazette Hebdomadaire* contains a very interesting analysis of an article by M. Hermann, on the maladies produced by the action of mercury. Recurring to an old theory, the author maintains that “there does not exist a syphilitic cachexia;” that “the accidents generally placed to the account of secondary and tertiary syphilis should be attributed to mercury employed in its cure.”

M. Hermann supports this opinion by what is often observed among subjects saturated with mercury—“divers eruptions, ulcers, anginas with or without ulceration, engorgement and suppuration of the lymphatic ganglions, periostitis, otitis, exostosis, and osteocopic pains. It is of importance, it appears to me, not to conceal under the observation of facts, an hypothesis eminently erroneous and equally dangerous.

That, by the effects of mercurial poisoning, may be developed any accident resembling those of syphilis, is something I shall not decide, never having had occasion to observe that agent administered in such a manner to individuals except from syphilis.

That the constitutional accidents manifested among syphilitics, are due to mercury and not to syphilis, are a paradox that I regard with astonishment when sustained by authority boasting of some merit.

What is necessary in order to dispel this error? Having treated without mercury persons infected with syphilis, and having seen the

constitutional accidents immediately manifested in the ordinary form, my practice permits me to furnish the proof. Since 1855 (from motives I shall finally announce), I have treated without mercury a fixed number, nearly two-thirds, of my syphilitic patients. Then among two hundred and fifty cases (of which I have one hundred and thirty detailed cases), the chancre developed, independent of any mercurial influence, its general and local consequences. Then what follows? Among all syphilitic patients not subjected to mercurial treatment, I have observed the specific eruptions, the ulcerated throats, inflammation of the glands, occasionally nodes, and every symptoms that supervened in those persons treated with mercury.

Let M. Hermann and all the German anti-mercurialists draw the inference. P. DUPUY.—*Translated from the Gaz. Hebdom.*, by W. C. N.

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#### ART. VI.—*Communicability of Secondary Syphilis.*

[DR. SILAS DURKEE, of Boston, whose elaborate work on Gonorrhœa and Syphilis has just appeared in a splendid octavo of 442 pages, maintains, as reported in the Boston Medical and Surgical Journal, that secondary syphilis is communicable, examples of which I have reason to believe have occurred in my own practice, both the mother and child having been infected. B. D.]

Dr. Durkee considered that the preponderance of medical opinion is in favor of the communicability of secondary symptoms. The singular doctrine, advanced some years since, by Ricord—that of all venereal affections, indurated chancre, alone, furnished inoculable matter, had been the occasion of awakening other distinguished observers to the general subject of contagion arising from syphilitic manifestations. The facts furnished by such men as Vidal, Velpeau, Trousseau and Cazenave, of France; by Wilson, and Langston Parker, of London; by Professor Porter, of the Royal College of Surgeons of Ireland; by Sigmund and Hebra, of Vienna, and many other high authorities, had now become quite numerous, and they were in direct opposition to the views of Ricord. These facts are constantly accumulating, and the most respectable medical journals teem with well-authenticated instances of infection derived from secondary lesions. Two cases of this kind, related by Professor Bennett, of Edinburgh, are



entitled to our remembrance, whatever theory we may entertain on the subject. They are as follows: A child of respectable parents was brought to Bennett for his opinion, whether an eruption, which it had, was, or was not, venereal. Bennett decided that it was. The family physician maintained that it was not. The child was put under the care of two wet nurses, both of whom, soon after having had the care of it, became affected with venereal ulcers about the nipples, and other well-marked syphilitic symptoms. Bennett had given his opinion that it was unsafe for the child to be "wet-nursed." Both nurses brought an action for damages against the physician who had caused the diseased infant to be under their care, and both recovered damages.

In view of such facts, Dr. D. asked if any member of this society would feel justified in giving his professional opinion that a syphilitic infant might safely be committed to the custody of a healthy wet nurse; or, to reverse the circumstances, and suppose that a healthy nursling was in want of a wet nurse, and a woman with a large breast of milk should desire the situation, but upon examination should be found to have secondary syphilis; would any gentleman present be inclined to encourage the friends of the child to commit it to the fostering care of such a woman?

Dr. D. remarked that in his own practice he had seen several cases of secondary syphilis in both sexes, where no evidence, whatever, of primary lesion existed. He considered that the proof, as to the communicability of secondary lesions, was as reliable as that relating to the contagiousness of chancre. Once, he was a believer in Ricord's theory. But facts observed in his own practice, as well as those adduced by the most trustworthy men from all quarters of the medical world, had fully convinced him that Ricord was not sound on this point. He is preëminently entitled to praise for the great scientific advancement he has made in regard to the pathology and treatment of venereal affections; but he is not infallible.

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ART. VII.—*Albuminuria in Croup—Leucocythæmia.*

i. *Presence of Albuminuria in Croup and its Prognostic Value.*

Dr. Sée, physician of the Children's Hospital, has recently demonstrated that the urine of all of his little patients who were attacked with diphtheritis (*angine couenneuse*), or with croup, contained albumen. This fact is an additional proof of the infectious nature of diphtheritis. The most important instruction derived from these researches is that the disappearance of the albumen always coincides with the amelioration of the disease; as soon as the cure is effected

the albumen is no longer to be found. This is then a most important prognostic sign, and well worthy of interest to the practitioner. Messrs. Bouchut and Empis, who have tested this point, state that they found albumen in the urine only eleven times out of fifteen experiments. Even this figure proves the importance of M. Sée's statement.—*Translated from the Bulletin de Thérapeutique*, by S. E. C.

ii. *Leucocythæmia*. (Letter from Paris.) Communicated for the Boston Medical and Surgical Journal.

*Messrs. Editors*—Not very long ago, there was in the service of M. Nélaton, at the "Hôpital de la Clinique," a pale and sickly-looking man, aged 27, afflicted with a disease which, from its rare occurrence, was examined and watched with unusual interest. It is said M. Nélaton meets with only one or two examples of it in his service annually. Our attention was called to this affection, some five years ago, while in Berlin. Since then, we do not think we have met with more than half a dozen cases.

The patient, about a year before entering the Hospital, perceived first that the glands of the right axilla enlarged. Then followed swelling of the cheek and under the chin. When first seen at the "Clinic," the parotidian, mastoidian, supra-maxillary and supra-hyoidian regions were swollen on both sides, so as to encircle completely the face, and to embarrass considerably the respiration. The lymphatic glands in the axilla, under the clavicle, in the supra-clavicular region and beneath the pectoral muscles, were also enlarged. The cellular tissue surrounding the glands was thickened, constituting a kind of œdematous induration. The deformity thus produced in the patient's appearance was very striking. The neck was almost entirely obliterated, appearing as if it had settled down within the thorax. Contrasted with this enormously hypertrophied condition of the upper part, the lower half of the body appeared much emaciated.

This disease, in the majority of cases, develops itself externally, in the manner observed in this patient. It commences by invading, first, the cervical and submaxillary glands, then the other regions possessed of lymphatics, progressively from above downward, the glands in the inguinal and popliteal regions being the last to become affected. In these cases there is really great emaciation, notwithstanding the apparently large size the patient often presents. The disease, in the generality of cases, progresses slowly, destroying the patient, according to M. Nélaton's observations, in from three to six years. Thus far no means that have been resorted to have exerted any influence on the course of the affection. In this case, the disease progressed very gradually, and terminated with the life of the patient, not many months after he had been transferred from M. Nélaton's service to the "Hôpital de la Pitié."

Within a fortnight, a man of middle age, and similarly affected, entered the service of M. Nélaton. This case, moreover, was one of unusual surgical interest. There was, in this instance, a general diseased condition, with hypertrophy of the lymphatic system. The

lymphatic glands in the left axilla were enormously hypertrophied, forming a tumor so large that the arm was elevated, thereby, nearly at a right angle with the body. The constant suffering which the tumor occasioned, by pressure upon the large nerves in that region, was so great, it was evident the patient could not survive long, unless relief was obtained. After taking into consideration all the circumstances having a direct bearing upon the case, M. Nélaton considered it his duty to advise an operation. The dissection had to be performed with the greatest care, as some of the larger nerves and vessels were embraced by the tumor. Within a few days after the operation, however, the patient was seized with a purulent infection, so common after surgical operations in the hospitals of Paris, and died within a week. *Post-mortem* examination revealed an extensive disease of the lymphatic glands, especially within the abdomen. Some of these were larger than the two fists. The spleen and bronchial glands were also in a hypertrophied condition.

I have selected these two cases, as presenting a very fair type of a disease of the lymphatics, which the researches of Prof. Bennett and Virchow have shown to be dependent upon, or connected with, a morbid change in the blood called (by Prof. B.) leucocythæmia. This consists in a considerable augmentation of the white globules, or colorless corpuscles. It does not appear, however, notwithstanding the diseases of the blood have strongly engaged the attention of modern pathologists, that the real pathology of this affection is clearly defined to the satisfaction of all. The term leukæmia, or white blood, given to it by Prof. Virchow, of Berlin, who, according to some, was the first to discover (1845), if not to describe, this abnormal state of blood, has not generally been adopted in Europe. It appears from the observations of this distinguished pathologist, the anatomical lesion more frequently observed existing along with this malady, is a hypertrophy of the spleen, and not unfrequently that of the liver. Following the pathological anatomy, he makes two varieties of leucocythæmia, the splenic and lymphatic. In both varieties we have this abnormal condition of the blood, but in the lymphatic variety there is not only a hypertrophy of the spleen and liver, but also tumors of the axillary lymphatic glands, of the cervical, and in the abdomen, of Peyer's patches, and not unfrequently of the solitary glands of the intestines.

Prof. Virchow, whose pathological researches and labors have gained for him the foremost position as an exact and reliable authority, has examined more searchingly into the pathology of this disease than any one else. But we have not set out with the intention of producing a paper on this subject, and therefore shall not bring together all that could be said upon it. The microscopical anatomy and appearances of the blood in this affection, to be brief, we shall be obliged to pass over. Our object is merely to call attention to this novel affection, while describing some of the more striking pathological changes and symptoms as observed in the cases described above. Following the course or march of the symptoms in the two varieties of leucocythæmia already described, Virchow considers them under two heads—1st, the febrile; 2d, the hæmorrhagic. The patient in the

first instance becomes more and more weakened, finally sinking under increased dyspnœa and hectic fever, etc. In the second form, the patient sometimes sinks, from external hæmorrhage or from repeated epistaxis, and sometimes by hæmorrhage from the intestinal organs. These cachectic phenomena supervene, occasioned by general failure in the vital forces, chlorotic symptoms, "bruit de souffle" of the arteries, hæmorrhage from the mucous membranes, dyspnœa more or less grave, and finally death.

With regard to the treatment, as we have stated, nothing appears to be of the slightest service in well-marked cases, with distinct glandular enlargement. Iron, quinine, hydriodate of potass. and a variety of medicines administered internally, with tincture of iodine applied externally, have effected absolutely nothing. The principal indications in advanced cases will be to restrain or check the diarrhœa and epistaxis, and to support the vital powers.

J. F. NOYES.

PARIS, April 10, 1859.

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ART. VIII.—*Croup.*

i. *Statistics of Tracheotomy in Croup.*

M. E. Bouchut, who has taken a very active part in the recent discussion in Paris, on the surgery and statistics of croup, maintains that this disease, and its proportional mortality, as shown from official records of the last two years, have been increasing, and that 89 in every 100 cases of tracheotomy, die, how skillful soever the operators may be! M. Bouchut announces, in a letter to the President of the Academy of Medicine, that he had received orally from the following distinguished gentlemen, whose names are given below, an account of 351 operations, which were followed by 312 deaths and but 39 recoveries! These operations appear to have been made, not in the public hospitals, but in the family residences.

MM. Gosselin.....	23	operations.	23	deaths.	0	Recoveries.
Brochin.....	3	"	3	"	0	"
Follin.....	15	"	13	"	2	"
Broca.....	10	"	4	"	6	"
Depaul.....	7	"	6	"	1	"
Ad. Richard.....	12	"	10	"	2	"

MM.	Alph. Guérin	12 operations.	11 deaths.	1 Recoveries.
	Michon	20	18	2
	Deguisse	12	12	0
	Laugier	8	7	1
	Velpeau	13	9	4
	Huguier	8	8	4
	Jarjavay	12	12	0
	Faure	6	6	0
	Auzias	2	1	1
	Robert	21	18	3
	Nélaton	36	33	3
	Jobert de Lamballe	60	50	10
	Lenoir	20	19	1
	Desormeaux	11	9	2
	Monod	40	40	0

M. Bouchut mentions three of these surgeons who wholly abjure tracheotomy for croup, namely, MM. Jarjavay, Monod and Lenoir. M. Bouchut not only lets 'tracheotomy "alone severely," but says even more than this: "Vous le voyez, Monsieur le Président, je n'ai rien exagéré en disant que cette opération faisait périr de 80 à 90 malades sur 100 opérés." Well does M. Bouchut characterize these candid admissions as "a tribute to truth." A little knowledge of surgical human nature suffices to show that recoveries, rather than deaths, obtain the precedence in giving the reports of cases. It may be humiliating to professional pride and worldly ambition to confess even inevitable failures. Yet failures are great teachers. Often the suppression of truth is virtual falsehood. For science, the beautiful and the good is Truth.

Although it does not appear that the surgeons above named have denied Mr. Bouchut's statements, yet he seems to put the efficacy of tracheotomy in the very worst light possible, for which he has no warrant in the doctrine of probability. Eighty or ninety in the hundred, according to him, die *from the operation*; he makes no reckoning whatever for the *croup itself*! This he calls no exaggeration. The statistics are horrible enough, but his inference is not only a *non sequitur*, but murderous.

B. D.

ii. *Improvement in the Operation of Tracheotomy.*

[The following important suggestions in regard to tracheotomy, emanate from the pen of Dr. Brainard, Editor Chicago Med. Journal.]

The suggestions which I have to offer in regard to tracheotomy, relate to the means of preventing hæmorrhage; to keeping the opening made by the operation previous without resorting to a tube; and at the same time, controlling the entrance of cold air abruptly into the lungs.

1. *Hæmorrhage.*—No experienced surgeon will deny that this is a serious difficulty in this operation. M. Guersent, surgeon of the children's hospital, at Paris, than whom there is no better authority on this subject, relates several cases where death occurred from opening large vessels, and one where it happened from the division of the thyroid veins.\* Syncope is a frequent effect of this loss of blood, and even suffocation may occur. M. Guersent advises sucking the blood out of the trachea with a catheter, when this is threatened.

In order to prevent hæmorrhage I proceed in the following manner: Having incised the skin and fascia by successive and careful incisions, I press the sterno-hyoid and sterno-thyroid muscles to each side with the fingers, and thus expose the thyroid body. This effected, I pass under the isthmus a director curved, or an aneurismal needle. This is followed by a common suture needle, which may be passed with the blunt end foremost, armed with two very strong ligatures. A ligature is then tied very firmly on each side, and the isthmus of the thyroid body divided between them. A little dissection with a blunt instrument denudes the trachea to the required extent, and an opening can be made without danger of a drop of blood being drawn into it.

The ligatures which have been thus secured, serve the purpose of fixing the trachea, if desirable, and they may be tied behind the neck so as to raise it forward, and keep the wound open. I never open the trachea until the hæmorrhage is stopped, and a large surface of it has been quite denuded.

2. *Keeping the opening in the trachea pervious without resorting to a tube.* The objections to a tube are twofold: 1st, When the operation is performed for the extraction of a foreign body, it prevents its exit; and it is desirable to leave this opening in such a state that the foreign substance may escape whenever it becomes loosened from its situation in the bronchia. 2d, In tracheotomy for croup, the prolonged sojourn of the tube has been considered, by the most eminent surgeons, as a cause of the pneumonias which so frequently are the cause of death.

The necessity for using the tube I avoid by the following means: Having denuded the trachea, insert a small suture needle, armed with a ligature, beneath two of its rings. Withdraw the needle, and, drawing gently upon the thread, make a semi-circular incision on one side, so as to form a valve, readily opened by drawing upon the thread.

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\* *Gazette des Hôpitaux*, for 1854, p. 59.

The opening thus formed can be kept patent or be allowed to close at will.

This is a matter, perhaps, of much greater consequence than might be supposed without reflection. Most surgeons have found their operations for tracheotomy less successful than they had reason *a priori* to expect, and this has been attributed to the direct entrance of cold air into the lungs. Trousseau and Guersent have both advised that the air inhaled at that time should be quite warm without being too hot.

I have ascertained, by direct experiment, that tracheotomy on dogs is followed by a diminution of temperature. I made a small opening in the skin of a strong dog, upon the side of the thorax, and introducing a thermometer, I found the temperature 98° Fahrenheit. I then performed tracheotomy, and at the end of forty-five minutes the temperature had fallen three degrees. This experiment was repeated twice. In one case the result was the same; in the other, when the temperature before the operation was only 95°, it remained the same after.

Every experienced physiologist knows the facility with which young animals die from reduction of temperature, and when we consider the tender age and depressed state of most of the subjects of tracheotomy, it is reasonable to believe that a reduction such as we have noticed, is capable of producing, directly or indirectly, the most injurious consequences. Impressed with this belief, I some years since contrived a cover for the opening into the trachea, composed of wire gauze and sponge, to retain the warmth and moisture of the expired air, and to communicate them to that which is inspired. The difficulty of procuring and using this cover, prevented me from publishing an account of it. It afterward appeared to me that the same object might be in a great degree attained by making a valvular opening, so that the air could be directed through the larynx whenever its issue below might not be necessary.

In croup this may be impossible to any great degree, but in the case of foreign bodies, my experience leads me to expect that it will be found both useful and of easy execution.

### iii. *New Treatment of Croup—Artificial Dilatation of the Larynx.*

The intensity of this affection, so dangerous to children, it is supposed will soon be completely subdued by the progress of modern science. Some time since, Dr. Loiseau published his bold and ingenious method of introducing caustic solution into the windpipe; we have now to register another process, recently communicated to the Academy of Sciences, by Dr. Bouchut. Some time ago the same gentleman announced, that when asphyxia is imminent in croup, the patient is in an anæsthetic state—in other words, insensible to pain. This state Dr. Bouchut takes advantage of, in order to introduce a tube into the glottis. To effect this operation, a probe of a peculiar construction is used; the tube is from six to fifteen millimetres in diameter, and from eighteen to twenty-four in length; it is fastened to two silken cords, by which it is moored, so to say, to a collar outside, to keep it in its place. It might be supposed that the larynx,

which is so irritable that a single drop of water or a crumb, going the wrong way, according to the familiar expression, will cause considerable coughing, could hardly bear the permanent contact of such a tube; but Dr. Bouchut has found by experience that the mucous membrane of the larynx easily gets accustomed to external irritations, and that patients tolerate the tube very well. It is kept in for a day or two, until the phenomena of asphyxia have disappeared. The false membranes in the trachea are expelled through the tube by expectoration, and the seat of the disease may be directly acted upon, through it, by the insufflation of bicarbonate of soda, or by introducing proper instruments with which to clear the windpipe. In a subsequent paper, read at the last sitting of the Academy of Sciences, Dr. Bouchut establishes curious statistical facts concerning this disease. From the tables he has compiled it appears: 1. That croup is much more frequent in males than females. 2. That it commits its greatest ravages at the ages of two and three. 3. That from 1837 to 1853, the mortality by croup in France, for every 1,000 inhabitants, had nearly doubled. 4. That from 1847 to 1858, it was five times larger than in 1838. 5. That the number of deaths by this affection, throughout all France, during the last thirty-two years, is 10,044. Now, as it cannot be proved that the cases of croup have increased during that period in a double or fivefold ratio, Dr. Bouchut attributes this increase of mortality to the practice of tracheotomy, which has become more frequent since 1840, in consequence of Dr. Trousseau's learned lectures on that operation. Dr. Bouchut is by no means led to infer from this circumstance, that tracheotomy is useless or hurtful; but he contends that it is had recourse to long before the patient's case is hopeless by any other means. If it were practised at the last stage, when the patient had become insensible, it might save a child exposed to the risk of all but certain death; but when performed while there are fits of suffocation with anæsthesia, it will cause the death of patients who might still have a chance of a spontaneous cure.—*Dublin Hospital Gazette.*

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#### ART. IX.—*Antiphlogistic Treatment.*

##### i. *Treatment of Pneumonia*: By Professor FORGET, of Strasbourg.

Professor Forget in his medical clinic recounts ten cases of pneumonia, four men and six women, the character, nature, and results of which, show, as he maintains, the superiority of the long established and legitimate treatment (*traitment classique*), to which he still adheres, notwithstanding the numerous and novel remedies daily proposed.



In two of the ten cases which he treated, death ensued ; one being a woman aged sixty-eight, who was admitted in the advanced stage of the malady. The post-mortem examination in this case, showed the characteristics of a pneumonia in a state of suppuration, complicated with tubercular caverns. The other case was that of a young woman of twenty-three, in whom we had felicitated ourselves with having conducted happily through the most critical stage of an intense pneumonia, when, suddenly a syncope, brought on by a disturbance in her neighborhood, proved fatal. The anatomical examination did not fully account for this fatal result. The posterior part of the right lung was engorged, a portion at the base having undergone red hepatization ; the other organs essential to life were healthy. Antiphlogistics had been in her case used moderately ; antimonials did not cause her fatal prostration, for that drug had not been given for three days before death. Her state was altogether favorable until just before death. This, then, was a misfortune, the cause of which was not foreseen, but to which, nevertheless, the fatal result may be in all fairness attributed.

The mean duration of the disease in those who recovered, was seventeen days ; the minimum ten, the maximum thirty.

Blood-letting, antimony and blisters, constituted the fundamental therapeutic method ; and these, according to the picturesque expression of M. Forget, are the three sacraments in the treatment of pneumonia. Very rarely did the symptoms of this malady require any deviation from the course mentioned.—*Translated from the Gaz. Méd. de Strasbourg*, by B. D.

ii. *The use of Bleeding in Inflammation and other Diseases* : By J. H. BENNETT, M. D., Professor of the Institutes of Medicine, and of Clinical Medicine in the University of Edinburgh, etc.

Twenty years ago, it was supposed that the primary cause of inflammations was an increased action of the vessels, but more especially of the arteries, which sent forwards or determined to the parts a greater amount of blood than the veins could carry away. As a result the smaller vessels became blocked up and distended, while the fluid part or lymph was squeezed through their walls, and infiltrated into the surrounding tissues. The remedy for this was the removal of blood from the part, either by local or general bleeding, by which it was thought this condition could be at once removed. But it is now understood that not only is this theory wrong, but facts have shown that the practice which flows from it is not effective. Instead of the morbid action beginning in the vessels, it truly begins in the tissues outside the vessels. These, in health, are continually

attracting and selecting from the blood such of its constituents as are necessary for their own nourishment. Thus the cells of the liver select what is necessary to form bile ; the salivary glands what is necessary to form saliva ; the muscles what is required to form muscle ; and bone what is wanted to form bone. If these parts are injured, healthy nutrition does not take place ; but instead of it there is attracted from the blood that lymph, or fluid part of the blood, which is so essential a feature in all inflammation. Instead of being pushed out by a *vis a tergo*, it is drawn out by a *vis a fronte*—the active force causing the disturbance is not in the vessel or in the blood, but in the cells and tissues to which these are distributed.

Again, when the living fluid part of the blood has in this way got outside the vessel, a wonderful new growth of cells takes place in it, whereby what is unnecessary is removed by absorption and excretion, and what is required is developed into new texture. This stage of the process is accompanied by more or less excitement ; an increased flow of blood is drawn to the part ; more nourishment is required, and nature supplies it. In the same manner that when the leaves bud in spring, more sap exists in and is drawn from the vessels in plants ; that during the annual growth of the stag's horn, all the vessels in the neighborhood are turgid with blood ; that when new teeth appear, in the infant, the gums are red and tender ; that when, in short, all active processes of local growth are proceeding, the pulse is strong, the blood flows with increased velocity, and the neighboring tissues are congested ; so, for the wisest and best purposes, does nature set up a similar action in inflammation, and turns to use that exudation which has been poured out, by transforming it into cells.

According to this theory, we must look upon such excitement in the neighborhood of an inflamed part as salutary, whereas formerly it was regarded with apprehension. The increased force of the pulse was thought dangerous, and the rapid flow of blood which used to be looked upon as injurious to the individual we now regard as necessary for a cure. Instead, therefore, of diminishing or lessening these useful changes by blood-letting and antiphlogistics, it is clear that theoretically they ought to be left undisturbed, and even supported when necessary. The correctness of this theory has been proved by actual experiment and experience on a large scale, a marked improvement in the recovery from severe inflammation having everywhere resulted from abandoning a lowering treatment in that class of diseases. Inflammations of the lungs, of the pericardium, of the pleura, and so on, have been proved not only to be much less fatal, but to get well in a much shorter time, since bleeding and antiphlogistics were replaced by nutriment and stimulants.

But this is not saying that blood-letting can never be useful in diseases, and may not occasionally be beneficial even in inflammations. Though it has been industriously circulated that I am opposed to bleeding under all circumstances ; so far is this from being the case, that I have carefully maintained the contrary. Thus, in my original paper in the *Edinburgh Monthly Journal*, for February 1857, pp. 782-3, I observe : "But whilst large and repeated bleedings, practised with

a view of arresting the disease, appear to me opposed to a correct pathology, small and moderate bleedings, directed to palliate certain symptoms, and especially excessive pain and dyspnoea, may reasonably be had recourse to, and unless there be great weakness, without any fear of doing injury. I have often been struck, especially in cases where large thoracic aneurisms cause these symptoms, with the small loss of blood which will occasion marked relief. The same result may be hoped for in other cases where the congestion is passive, even when that is associated with the active repletion of blood, followed by exudation. But I need scarcely remark, that this mere palliative object of blood-letting is not the ground on which the practice has hitherto been based, and that in this point of view it requires to be very differently explained."—*British Medical Journal*, May 21, 1859.

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ART. X.—*Influence of Diet.*

[A quarter of a century has not yet elapsed since the current of medical opinion began to change in regard to diet as a means of preventing, curing or modifying diseased action. The extreme doctrine of Broussais, in generalizing nearly all morbid phenomena into inflammation, and, consequently nearly all remedial measures into antiphlogistics, with a restriction as to diet little short of actual starvation, was very naturally calculated to produce reäction, and with this reäction a more careful revision of the influences of diet. In fact, diet seems to be destined to displace drugs to an extent never before paralleled in the history of pathology and therapeutics. Already fats and oils are inaugurated in the prevention and treatment of consumption and some other diseases, while a sustaining diet is seldom withheld either during the progress of, or recovery from, most diseases.

B. D.]

*Royal Medical and Chirurgical Society. Tuesday, May 10th, 1859. F. C. SKEY, ESQ., F. R. S., President, in the Chair. Practical Deductions from an Experimental Inquiry into the Influence of Food: By E. SMITH, M.D., L.L.B., etc.*

The author, in some preliminary remarks, referred to the large amount of vital action which is necessary to maintain life, and mentioned the various circumstances which he had noted during the con-

tinuance of a prolonged fast. He stated that the practice of administering arrow-root, or other fashionable foods, consisting of starch, with water, under the impression that it was more nutritious and easier of assimilation than wheat-flour, was indefensible, since it did not sustain the vital action to a degree capable of maintaining life; and that Nature has not provided starch, as food, altogether apart from nitrogenous substances. He contrasted the action (or rather want of action) of starch with that of the cereals, and showed that the latter is nearly as great as that of any substances with which we are acquainted. He drew the distinction between an action which increases the existing amount of vital force, and that which only tends to prevent loss of vital power—two circumstances which, in practice, are commonly confounded; and showed that beef-tea, wines and brandy, can act only in the latter mode, whilst the cereals act in the first named manner. Hence, in cases of prolonged exhaustion, where there has long been more waste than supply, the former is not sufficient, and it is essential that the cereals be added or substituted.

The action of milk is exceedingly analogous to that of the cereals, both in extent and duration; and the combination of the two appeared to be the most perfect kind of food. The casein is to the milk what gluten is to bread; and the oil in milk is associated with substances (respiratory excitants) which call it into action in a manner quite analogous to the common combination of bread and butter, or of a mixture of fat and lean flesh. He showed that milk and flesh are the best and most natural modes of exhibiting fat, and altogether preferable to the administration of separated oils. He referred to the frequent use of skimmed milk in Germany as a medicinal agent, and of sour milk in Greece and America as a part of food; and explained the action of the former by its casein and sugar as respiratory excitants, and that of the latter by the advantage of administering lactic and other acids in that combination in the summer season, and at other times when the blood, by tending to undue alkalinity, is less capable of carrying on the oxygenating process. He showed that, in fevers, skimmed milk is preferable to new milk.

As fats lessen the respiratory changes, they ought to be, and are, combined with other articles of food which increase them. He referred to the importance of determining the reasons for the administration of both fat and starch, and showed that there is less difference in the relative amount of these two substances used in different climates, than has been commonly believed. He attached importance to the physical properties of fat, and explained the beneficial action of that substance when applied to the skin by its power to retard the conversion of the fluid perspiration into vapor, and thereby to lessen the waste of heat by the skin. He thought this latter mode of employment of fat to be especially fitted for cases of debility with lessened appetite and perspiring soft skin, in whom the waste is always greater than the supply.

The beneficial action of sugar was insisted upon; and the love of the French for sugar and water was explained by the refreshing coolness, the innocuousness and the agreeable flavor of the fresh made beverage, and the great freedom and lightness of respiration which

attends its action. He thought the ill effects of sugar in the healthy system had been exaggerated.

The action of animal substances in increasing the respiratory process, in addition to the supply of plastic materials, was dwelt upon, and shown to be of great value to the system. These are allied to gluten, and some of them probably act as ferments; and, in illustration, he especially cited cheese, which promotes digestion if taken in small quantity, but is apt to disturb it if much is eaten.

Tea was shown to cause increased waste, and to excite every function of the body; and hence was well fitted to cases where there was a superfluity of material in the system, or where we otherwise desire to induce a temporary increase in the vital action; but is injurious to those who are under-fed, or in any case where there is greater waste than supply; and, in illustration, cited the increase in the loss of weight of the prisoners at Wakefield, when tea was added to their food. The action of tea had been hitherto misunderstood; but the sagacious observation of Liebig, as to its analogy with the active principle of the bile, was much commended. He recommended its use instead of spirituous liquors, by soldiers on march, or otherwise exposed for a lengthened period to great heat; since, by its powerful influence in increasing respiration and the action of the skin, without increasing pulsation, it was particularly fitted to counteract the influence of heat in its tendency to induce "heat-apoplexy," or, as more suitably termed by Mr. Longmore, "heat-asphyxia." Twenty-five grains of tea, in a concentrated cold infusion, taken every hour or half hour, during exposure, would suffice. For similar reasons he urgently recommended it as an adjunct in the treatment of suspended animation, as from immersion. It has a rapid and accumulative action so that small and repeated doses have much greater effect than large and isolated ones. It differs from coffee chiefly by increasing the action of the skin, and thereby tending to cool the body; and therefore the two substances are applicable to different conditions of the system. He thought that both, and particularly tea, ought to be more commonly used as medicinal agents. Coffee-leaves he believed to be a valuable febrifuge medicine, and one particularly fitted for cases of nervous excitability.

The author then contrasted the effects of brandy and gin with tea, and showed that in all respects they were directly opposed; but coffee so far resembled them in action, that it lessened the action of the skin, and thereby lessened refrigeration. Rum and beer he regarded as restoratives, and the combination of rum and milk as the best restorative employed as food; whilst brandy and gin simply lessened waste. He regarded all alcohols as having their chief influence in sustaining the action of the heart; and recommended that they should be given in small quantities, and repeated every quarter or half hour in urgent cases, so as to accumulate their action, rather than allow reaction to follow each dose by permitting a long interval between the doses. He mentioned a case in which he gave six bottles of port wine in forty-eight hours, with the effect of saving the patient's life, and reducing the pulse from 150 to 90 per minute. He believed that alcohol increased the respiratory action indirectly through the nervous

system; and that, in fine old wines and spirits, the action is lessened by the volatile elements, which have a conservative tendency. He particularly cited the conservative influence of fine old port wine, and the disturbing influence of new and inferior spirits. The primary and secondary action of all alcohols, when taken in an amount to affect the sensorium, was always felt; and the author described the attendant circumstances.

In conclusion, the author stated that dislikes for food are indicative of lessened action, and that other foods of analogous properties should be provided in such cases; and also that it was probable that at least some kinds of azotized substances are more fitted for the hot season, when the chemical changes are greatly reduced, than has been heretofore believed.

Dr. Stallard said, however valuable the facts stated by the author, they should, nevertheless, be received with a certain amount of caution, and that a wider view of the case should be taken than that presented in the paper. The recommendation of bread, in preference to beef-tea, seemed so opposed to the general experience of the profession, that it ought not to be received without further confirmation. Dr. Smith's experiments, however, were upon a very healthy person, and this possibly might make all the difference. Beef-tea might be absorbed by the simple law of the percolation of fluids through membranes; but bread had to undergo a distinctly assimilative process, which in a weak patient might not be easily carried on. The author scarcely took into consideration the influence exerted by fat in the assimilation of non-nitrogenous as well as nitrogenous foods. It was possible that fat, instead of being assimilated, might pass out in the fæces. With regard to tea, it no doubt produced a greatly increased respiratory action; but the question was whether the increase was temporary, or whether it had a permanent specific effect, due to the composition of the tea itself. It might possibly have an ultimate depressing effect. He inquired whether the author's experiments showed any increase of the vapor expired after the use of alcohol?

Dr. J. A. Wilson said that the paper was of such an unreasonable length, that it precluded anything like an adequate discussion of its statements—an evil which, he said, was unfortunately growing upon the Society. He agreed with the author's remarks as to the importance of fat in supplying loss by perspiration. Liebig's statements had led to the inference that fat was not used in warm countries; but in the course of Captain Sturt's journeys in the interior of Australia, where the heat in the deserts was most intense, he and his followers ate fat with the greatest avidity, their desire for it departing only when the heat subsided. Other instances of a similar kind had been recorded. The utility of casein to aid digestion had been long known, since Shakspeare made *Ajax* address *Thersites*, "Come my cheese, my digestion." (A laugh.)

Dr. Smith, in reply, stated that when he recommended the use of starchy food and milk, instead of or in addition to beef-tea and brandy in cases of exhaustion, he was desirous to urge the distinction between food which can alone supply material to meet waste and that which simply tends to lessen the rate of the wasting process. Fat

when taken alone lessens the evolution of carbonic acid, and when taken with the cereals it also somewhat lessens (instead of increasing) their action on the same function. The action of tea is temporary; but if food is duly supplied to meet the increased waste, tea does not cause a subsequent decline of action. Inhalation of alcohol increases the amount of vapor exhaled by the lungs, probably from some local action; but when it is taken into the stomach, the doses taken by the author did not produce a very decided effect; but this subject was only incidentally examined in the present series of experiments. Casein, known as a digester, was proved to act in common with other nitrogenous substances to promote the transformation of starch and fat, or at least the evolution of carbonic acid from whatever source. *British Medical Journal*, May 28, 1859

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#### ART. XI.—*Pharmacopœias.*

[IN the last issue of this Journal, a few general remarks on pharmacopœias appeared; since which Dr. Headland, no mean authority, has published an extended paper on these subjects, in the *Lancet*, and in so far as the topics referred are the same, the opinions are identical. As the Convention for revising the Pharmacopœia of the United States, will assemble in January next, it may not be amiss to report some of Dr. Headland's opinions by giving a few extracts from portions of his paper in relation to the contemplated improvement and consolidation of the several pharmacopœias of the United Kingdom, so as to form a National one.

The interminable, cumbrous, and ever-changing classifications of the pharmacologists and formulary builders, are virtually antitheses to, if not burlesques on positive science. Excepting castor oil and a few similar bulky articles of the *Materia Medica*, the practitioner can, in cases of emergency, carry in his vest pocket enough of the really valuable concentrated medicines, as opium, quinine, calomel, and a few others to answer all the useful ends known to the practical man. These instead of being elaborately combined with a long array of other (*olla podrida*) articles, often incompatible, useless or worse, should generally be given alone, or in combination with the fewest possible number consistent with the indications of the case. Whether

a dose of quinine will cure an intermittent by acting on the solids or fluids, or whether this agent should be classed as chemical, vital, or mechanical, by a pharmacologist, is for the former not a scientific classification, but a hypothesis.

But it is time to let Dr. Headland speak. B. D.]

With regard to the important question of what remedies and what formulæ are to be excluded from the pharmacopœia, while I state my own feeling, I beg respectfully to invite the opinion of those members of the profession who may have given attention (and who has not?) to this important matter. The lists of the three pharmacopœias, if added together, would burden us with a number of old-fashioned names with which we would readily dispense. Some sacrifice must be made by each, and a spirit of friendly accommodation shown by their supporters. As the Roman triumvirs brought each a list of proscribed friends whose lives they offered up on the shrine of their union, so must the united pharmacopœias set an example of self-abnegation.

Thus, as a melancholy indispensable in our new pharmaceutical reform bill, I beg to present a list of rotten boroughs for disfranchisement—a catalogue of drugs drawn from the materia medica of the three pharmacopœias whose constituencies, or the doctors who prescribe them, have become so extremely limited that it seems scarcely necessary to retain them any longer.

My index expurgatorius would run much as follows:

Absinthium, acetum, acidum aceticum (omit eight of these and leave only a strong acid of 85 per cent., and a dilute acid of 5 per cent.), allium, althæa, anethum, angelica, anthemidis, oleum, aurantii fructus, balsamum Canadense, barytæ carb. et sulph., calamina (replace by pure carb. zinc), calamus aromaticus, canna, cannabis, carota, cassiæ cortex et oleum, castoreum, centaurium, chiretta, chincona cinerea, cocculus, cornu et c. ustum (replace by pure phosph. lime), cuminum, dulcamara, euphorbium, gossypium, hemidesmus, inula, lactuca, lactucarium, laurocerasus, lauri bacca, linum cath., lixivus cinis, lupulina, malva, marmor, matico, melissa, menyantes, mori succus, mucuna, origanum, ossa, petroleum, plumbi carb., plumb. ox. rubrum, potassæ bichromas, potass. carb. impura, rhanni succus, saccharum communc, sacch. lactis, sagapenum, salicis cortex, sassafras, simaruba, spigelia, spiritus pyroxilicus, spongia, stannum, tapioca, terebinthina veneta, ulmus, viola. In all seventy-four.

This would make a great clearance, after which we should be able to see our way better. Half of the list are superfluous—i. e., rendered unnecessary by the existence of better drugs of the same kind. The others are useless, or nearly so. I shall be glad, if called upon, to give my especial reasons for wishing to expunge any particular member of the foregoing list. \* \* \*

I contend that the Latin language should be adopted, as in the present London Pharmacopœia. An English translation might be published to satisfy those who prefer it, but the authoritative text must be in Latin; and this for the same old reason that induce us to con-



tinue the old custom of Latin prescriptions. Any arguments brought against the one system, must tell with equal force against the other. The arguments in favor of the Latin language are distinct and decisive. It is the language of science. Chemical terms are given more simply and unmistakably, botanical names are only recognized in Latin. It is a language understood by the learned throughout the world—the only universal tongue. Its adoption has given to the London Pharmacopœia an advantage above all European codes, which it might not else have obtained. Its abbreviations are precise, and are well understood by dispensers, which is found a great convenience in prescribing. And lastly, it is not understood by the common people, nor by the majority of the patients. With the English tongue the contrary is the case. There is no preciseness about it. Many of our medicaments have several names, and one appellation may indicate any out of several distinct substances. It may suffice to instance copperas, orange vinegar, vitriol, bark, almond, verdigris, camomile, nightshade, hellebore, poppy, turpentine—names which I have taken indiscriminately. The English language is but little understood abroad, but Latin formulæ are read and understood all over the world. The former is not capable of being much abbreviated, because these abbreviations are not recognized, and would be unintelligible. If written in full, it is no clearer than Latin to the properly-trained dispenser; whereas its being understood by the patient would be a great source of embarrassment, inasmuch as our endeavors to administer opium, colchicum, arsenic, mercury, etc., to nervous patients who understand their own cases, and “know that their constitutions will not bear them,” would often be ineffectual.

There should be no change in the present system of chemical nomenclature and equivalents. The introduction of the system of atomic weights, recommended first by Gerhardt, and adopted by Brodie, Williamson, and some others in England, would utterly revolutionize our formulæ from beginning to end. Fortunately for us, the habit of doubling the elementary equivalents of oxygen, sulphur, carbon, etc., is by no means generally adopted, and with other equally daring innovations, is likely to die out before long. It is hardly necessary to point out the danger of tampering with the accepted names of the chlorides of mercury, the compounds of arsenic and other powerful medicines. We prefer to leave things as they are, and decline to sit down and commence *de novo* our study of chemical names, at the bidding of every rash speculator who may choose to propose an alteration.

\* \* \* Mistakes would be next to impossible if all the solutions of the pharmacopœia were so diluted that the ordinary dose for an adult man should be just one drachm, and in no case more or less.

Along with this plan of drachm solutions, it would be highly desirable that tinctures for internal use should be brought to a uniform standard of two drachms for a dose, and all infusions, decoctions and mixtures to a dose of one ounce. The tinctures of cantharides, aconite, opium, conium, stramonium, etc., being thus diluted, would be no longer sources of danger. The solutions not intended for internal use should be kept in a separate part of the druggist's shop, and labelled *poison*.

ART. XII.—*Inhalation of Quinic Æther in the Treatment of Intermittents.*

IN the *Gazette Médicale de Strasbourg* (June 25, 1859), Dr. Eissen, the editor, has extracted from a Vienna medical journal (*Österreichische Zeitschrift für Practische Heilkunde*,\* No. 22, 3 Juin, 1859), an account of the remedial power of quinic æther by inhalation, in the treatment of intermittent fever—an outline rather than a translation of which will occupy all the space that can now be devoted to this subject.

Dr. Eissen justly remarks, that in many cases of obstinate intermittents the energetic administration of quinine is attended with gastric complications, and almost always with repugnance—inconveniences which the inhalation of quinic æther obviates.

Quinic æther, invented by M. Manetti, and experimented upon by M. Pignacca, of Milan, is a perfectly limpid, colorless liquid, less volatile than sulphuric æther, being, nevertheless, sufficiently so at the ordinary temperature, leaving no residuum. It has an agreeable odor, reddens tournesol-paper, and mixes readily with distilled water; the vapor from which escapes slowly, having an agreeable penetrating odor.

This substance prepared by the distillation of the quinate of lime (*quinate de chaux*) obtained incidentally during the preparation of the sulphate of quinine with sulphuric acid and alcohol of a specific gravity of 1.184, in the proportion of one pound of the quinate of lime to as much sulphuric acid and 44 ounces of alcohol, mixing slowly the alcohol and acid that these liquids be not overheated; then enclose the quinate of lime in a tubed retort of sufficient size to contain a little beyond that of the twofold mixture; decant little by little from this mixture of the two liquids, and finish by moistening completely the quinate of lime, stirring it cautiously. Afterwards it is to be distilled over a slow fire in a sandbath, until 20 ounces shall have passed into the receiver, which must be distilled a second time upon the chloride (*chlorure*) of lime, rendering it anhydrous.

Drs. Wurzian and Groh, physicians of the Austrian army, in 1855, at the military hospital of St. Ambrose, at Milan, experimented with this preparation, having chosen with this purpose, six patients suffering from intermittent fevers; patients who had not previously

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\* This Journal, one of the exchanges of the *N. O. Med. and Surg. Jour.*, has not been received since the commencement of the late war.

employed any remedy; also one, the seventh, who had been using unsuccessfully the compound tincture of cinchona.

The six patients inhaled from one to three grammes, placed on a compress folded in the form of a cornet or horn, so as to cover the mouth and nose. They were directed to make deep inspirations. The inhalation repeated at short intervals, always with the same dose, began some time before the expected access. If the inhalations were made during the apyrexial period, no remarkable symptom was noticed except an acceleration of the pulse and of the respiration. The patients experienced during this process, pleasure, having had no cough or other disagreeable symptom. If the inhalations were made at the commencement of the algid period, they produced immediately a feeling of comfort (*sentiment de bien-être*), so that it will be difficult to find a more agreeable febrifuge.

With a single exception, all who submitted to this medication, declared that the shivering and coldness instantly diminished; the access having been much lessened both in duration and intensity. The tumefaction of the spleen was dissipated. Six of the seven cases observed at Milan were promptly and radically cured; the unsuccessful case (the seventh) failed because the dose was insufficient and the inhalation imperfect.

Dr. Groh, subsequently employed this remedy at the hospital of Olmutz, as well as in his private *dientèle*, and always with remarkable success.

It is a matter to be regretted that the chemical constitution of this medicinal preparation has not been determined with exactness.

These practitioners have used upon other fever-patients inhalations of sulphuric æther in which the sulphate of quinine had been dissolved, which, however, had no other effect than that of increasing to an insupportable degree, the hot stage of the fever.

The reported cases illustrative of the above-named quinic æther inhalations are too long to be reproduced in the remaining limited space of this Journal.

B. DOWLER.

## ORIGINAL COMMUNICATIONS.

(Continued from page 687.)

ART. XIV.—*Ligature of the Common Iliac Artery for Aneurism—Use of Silver Ligature—Death on the 26th day from Exhaustion, by Dysentery:*  
By WARREN STONE, M. D., Professor of Surgery in the University of Louisiana.

CHARLES O'DONNELL, aged 36 years, was admitted into the Charity Hospital, December 18, 1858, laboring under aneurism of the external iliac and upper part of the femoral arteries. It was a plain unmistakable aneurismal tumor, extending from two inches below Poupart's ligament, to two inches and a half above it. The tumor was rather prominent, and the pulsation strong, but there was no inflammation or discoloration of the skin. He stated that he first perceived it about the middle of April previous, but he took little notice of it until July, when the leg began to swell below, and he suffered shooting pains down the leg, which disturbed his rest at night. His general health was bad; he could take but little food; stomach much deranged; bowels irregular, sometimes constipated, and sometimes loose, and of a dysenteric character.

As the tumor did not seem to be increasing very fast, it was thought best to give the patient rest, and endeavor to improve his health before resorting to any operation. Under rest, and opiates, the tumor remained nearly stationary for some time, and his general health improved; but about the middle of January, 1859, it suddenly enlarged and some inflammatory action was set up. The operation by ligature was offered to the patient, as the only chance of saving his life; but as I could not offer him very strong encouragement, he declined having it made. But his sufferings increased, and on the 26th he concluded to take the chance of an operation, which I accordingly made. An incision was made from the upper edge of the tumor nearly in the course of the *linea semi-lunaris* up to the cartilage of the ribs, and the abdominal walls carefully divided down to the internal common fascia, which was peeled up from its loose attachment to the walls, and with the peritoneum, pushed aside until the artery was uncovered. There was some difficulty in detaching these tissues at the lower part where they came in contact with the tumor and be-

came adherent, and a small rent was made into the peritoneal cavity, but no serious injury was sustained by it. A small portion of omentum protruded once, but it was returned, and did not appear again. A common silk ligature was carried under the common iliac, by means of an aneurismal needle, and the silver wire attached to it and drawn through. This was tied like a common silk ligature and the ends cut near the knot and the points bent down so as not to present to the soft parts. The knot was not drawn strongly as is usual; but only sufficiently so to stop the current of blood in the artery. The wound was brought together by the twisted suture, and the patient appeared in a favorable condition. The extremity became cooler than the other, and there was some pain in it, but by the use of artificial warmth and diffusible stimulants, the circulation was gradually established, and no symptoms of gangrene appeared.

The tumor subsided very favorably, and the patient was almost entirely relieved from pain in it, and did well for several days, when the dysenteric symptoms returned with increased violence and continued until his death, which took place on the 20th February, the twenty-sixth day after the operation. The operation was on the left side and it is probable that the displacement and handling of the descending colon, and the sigmoid flexure increased the diseased action which already existed.

Being unwell at the time of this patient's death, I did not attend the Hospital for a day or two, and the clerk delivered the body to the wife of the deceased for burial, and no post-mortem examination was made. This was an unpardonable blunder, for it would have been of great value to have observed the manner in which the silver ligature was disposed of, and the result of a moderately light ligature of materials that will not excite ulceration of the coats of the vessel, unless the tissues are completely strangulated. I am confident that the silver thread may be applied sufficiently tight to completely interrupt the current of blood through the artery, without strangulating the tissues of the vessel, so as to cause its division by ulceration. If this is correct, the danger of secondary hæmorrhage in tying large arteries, may be entirely avoided. The danger from hæmorrhage after the ligature for aneurism is so great that it has become the settled practice to use the painful, tedious and uncertain method of compression, where it can be made, and the ligature is only resorted to when this method fails. Setting aside the danger of

secondary hæmorrhage, the ligature must be allowed to be the best operation. It is soon completed, and the patient receives immediate relief from pressure and pain in the tumor, which often diminishes in size and leaves a smaller amount of coagulated blood to absorb than when the circulation is arrested by degrees and the blood in the sack is made to coagulate while it is fully distended. A common silk ligature if applied only just tight enough to the artery to arrest the current of blood, is slow in coming away, and obliterates it as certainly as if it was tied so as to cut the inner coat of the vessel, according to the directions of Jones. When I was a student, my preceptor (the late Dr. Amos Twitchell) made experiments to test the truth of the assertion of this surgeon, that it was necessary to draw the ligature so as to cut the inner coat of the artery in order to produce adhesive inflammation, and proved that it was mere theory, and not a fact. The truth is, that adhesive inflammation does not take place in the inner surface of ligated arteries under any circumstances; at least, I have never found it in the cases I have examined. When an artery is tied in its course, so as to arrest the current of blood, a coagulum is formed on the proximal side, but not on the distal side, of the ligature, and hence it is that hæmorrhage often takes place from the distal extremity, when the anastomosis and collateral circulation is free, in cases when the adhesive process has been perfectly good. The coagulum acts as a plug which is held in place or kept from being forced out by the heart's action, by the lymph that is thrown out in and around its coats so as to thicken them and hold them in the puckered condition in which the ligature places it. On the distal side, as no clot is formed, if the vessel is filled with blood, and the patient makes much exertion about the time the ligature comes away, hæmorrhage will often take place, and I believe is frequently mistaken for hæmorrhage from the proximal side. This mistake may cost the patient his life; for, if a ligature is applied higher or nearer the heart, it may not stop the bleeding, but lead to serious consequences; whereas, a very little compression over the sinus left by the ligature, until the granulations fill it up, renders it perfectly safe.

It is plain, however, that, when an artery is tied in its course, it is not necessary to tie it so as to make a division of its coats by ulceration, if the ligature is of silver or any material that will not cause ulceration by its presence. All that is required of the ligature in

the case of aneurism is to arrest the current of blood in the artery, and this can be done without strangulating the tissues inclosed in the ligature so as to cause a division. In the case here related, as the vessel tied was so large, and the organic functions were so feebly carried on, I feel confident that hæmorrhage would have taken place if the ordinary silk ligature had been used in the usual way. In this connection I will take occasion to express my sense of what is due to Dr. J. M. Sims, for introducing the silver ligature, as well as establishing a successful method of operating for vesico-vaginal fistula and analogous lesions, which is mainly due to the silver suture.

Up to the time Dr. Sims laid before the profession his mode of operating for vesico-vaginal fistula, it must be admitted that no successful mode of treatment had been established, and hundreds of females laboring under this most loathsome and painful affliction were left in hopeless misery. It is true that an occasional cure was effected under favorable circumstances, but these cases were rare exceptions to the general rule. Since the method of Dr. Sims has been adopted, all the bad cases that were formerly pronounced incurable are cured with great certainty. The attention of the profession was called a few years ago to the button suture of Dr. Bozeman, of Montgomery, Alabama, which, by many, is considered an improvement, and it has been quite generally adopted. It is not my object to discuss the merits of this improvement. Dr. Sims uses the plain interrupted suture, and no one operates with more success, I believe, than he does; but if others realize more favorable results by the button suture, than by the simple, it is their duty to adopt it. I see nothing in this improvement that detracts from the merits of Dr. Sims, however much it may redound to the credit and honor of the inventor. It would seem that Dr. Sims thought otherwise, and setting aside all his well founded claims to merit, for his indomitable perseverance, in inventing instruments and simplifying the operation, he bases them entirely upon the introduction of the silver suture. This he set forth in a paper he read before the New York Academy of Medicine, in which he may have indulged in some pardonable egotism, and unfortunately brought forward personal matters, that belonged to him and Dr. Bozeman alone. This gave the friends of Dr. Bozeman (who seemed to have been sharp critics) an excuse for a severe criticism, and they not only ridiculed his paper, but his claim to the discovery of the silver suture. Although there was in these criticisms an affecta-

tion of rendering some undefined merit to Dr. Sims, the effect with strangers was calculated to do him great injustice. It was urged (and with truth it must be admitted) that the silver and other metallic sutures were known long since. Dr. Levert, of Mobile, made experiments as long as long ago as 1828, and published the results (which were highly favorable) in the American Journal of Medical Sciences of that year, but no one acted upon the principle thus established.

When a principle is put to practical use, if it is important, many aspirants spring up for the honor of the discovery. Dr. Simpson, of Edinburgh, on reading the discourse of Dr. Sims, was reminded that he had read somewhere the report of a successful case operated upon in which the silver suture was used. He instituted a correspondence on the subject, and finally found that it was reported in a London journal in the year 1835. Dr. Simpson is a very ingenious man, and ready to adopt any improvement that gives promise of value, but this case passed him unnoticed, and would in all probability, never have been thought of again if he had not read the discourse of Dr. Sims. The remarks of Sidney Smith, in the defence of Hamilton, who established a system of education, when the critics denied the originality of the system, are very appropriate. He says, "whether Hamilton is or is not the inventor of the system that bears his name, or what his claims to originality may be, are questions of very second rate importance; but they merit a few observations. That man is not the first discoverer of any art, who first says the thing; but he who says it so long, so loud, and so clearly that he compels mankind to hear him. The man who is so deeply impressed with the importance of the discovery that he will take no denial, but, at the risk of fortune or fame, pushes through all opposition, and is determined that what he has discovered shall not perish for the want of a fair trial. Other persons had witnessed the effect of coal gas in producing light; but Wipdsor worried the town with bad English for three winters before he could attract any serious attention to his views. Many persons broke stone before Macadam, but Macadam felt the importance of the discovery more strongly, stated it more clearly, persevered in it with greater tenacity, wielded his hammer, in short, with greater force than other men, and finally succeeded in bringing his plan into general use."

When Civiale brought before the profession of Paris his admirable



surgical operation, called lithotrity, the critics attempted to write him down, and denied that the process of breaking down stone in the bladder was a new invention. After having proved that he was not the inventor of his instruments, they attempted to show that the instruments themselves were detestable, and farther, that Civiale did not know how to use them. Civiale, however, was happy in his associations. The eminent Chaussier and Percy concluded their report upon the subject of Lithotrity, to the Academy of Science, in these words: "Lithotrity is glorious for French surgery, honorable to its inventor, and consoling to humanity;" and a writer, in reply to these critics, who would deprive Civiale of the merit of originality, says: "in effect, the only true proprietor of a surgical improvement is he who applies it successfully, all theoretical reasonings and the cavilings of chronologists to the contrary notwithstanding."

Whether Dr. Sims bases his claims for distinction upon his indomitable perseverance under repeated failures and disappointments, ill health and other impediments, in endeavoring to establish the curability of vesico-vaginal fistula, or merely upon the introduction of the silver suture, as embracing the whole merit, he is clearly entitled to it, and these just acknowledgments no more detract from the merit of those who have made valuable improvements, than those improvements detract from the merits of the original inventor.

I have made these few remarks in justice to the profession, rather than from any partiality from any individual member. Whatever an individual member may do that redounds to his honor, sheds a proportionate lustre upon the whole profession, and nothing detracts from it so much as our own bickerings and injustice to each other.

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ART. XV.—*Tracheotomy for the Removal of a Foreign Body, with Remarks :*

By WARREN STONE, M. D., Professor of Surgery in the University of Louisiana.

A CHILD twenty months of age, was brought to me on the 24th May last, from Mississippi, by way of the Northern railroad, almost in a state of suffocation. Eight days before, while playing with some

corn, it was taken suddenly and came near suffocating. Dr. Little, a young, but intelligent physician in the neighborhood, was called in, and at once recognized the nature of the case (a foreign substance in the trachea), and advised the parents to take the child to me for an operation. The foreign body, however, became fixed probably in one of the bronchial tubes, the spells of extreme difficulty of breathing ceased, and they delayed their journey for several days, when, in a fit of coughing, the substance was probably again dislodged and thrown into the larynx, as the child had a new spell, and was thought at one time to be dead. It, however, revived, and they started for this city where they arrived as stated above.

The child's face was somewhat swollen and cyanosed, and on examination coarse crepitation was heard over the whole chest. It was plain that nothing but tracheotomy could afford relief, and it was accordingly made at once, in the usual manner, while the child was under the influence of chloroform. A free opening was made in the trachea and the divided cartilages held apart by two pieces of silver wire doubled and bent so as to form something like a spatula. The effect of the chloroform soon passed off; the handling of the trachea caused the little patient to cough, and a large grain of corn, of the kind called gourd seed, was thrown out upon the table, with complete relief from the suffocative cough. The respiration, which was hurried previously, gradually became slower and fuller, the child slept well, and the next morning the lungs were entirely relieved of the engorgement, and the respiration was natural.

There was some erysipelas in my infirmary (when the child was operated on), and some diphtheria in the neighborhood, and I thought it best for the child to be taken back to the country at once. I have not yet heard from it, but have no doubt but it did well. There was nothing very remarkable in the case; the child's neck was short and deeply covered with adipose; the isthmus of the thyroid bodies was somewhat in the way, but was easily pushed up so as to allow sufficient exposure of the trachea.

Notwithstanding the engorgement of the lungs, and the partial cyanosed state, chloroform was well borne, and did not seem to aggravate the symptoms in the least, but enabled me to make the necessary dissection with ease and precision; the thyroid veins were distinctly seen and the fascia divided between them and the trachea fairly exposed with scarcely any loss of blood.

There is not much to be said on this subject, since the admirable compilation of Professor Gross, entitled "Foreign Bodies in the Air Passages." It may be proper, however, to say something in favor of immediate action in all cases, where it is decided that a foreign body is lodged in the trachea, at least if it is loose, and against delay or waiting for urgent symptoms.

Some years since I was called in consultation, to see a child two or three years of age, who, it was thought, might have an orange seed in his wind pipe. The family were strangers on their way to California, and while on their way to this city, by way of the river, the child took cold and was suffering from catarrh, on their arrival.

While eating an orange, a more violent fit of coughing took place, and the child strangled and turned quite black in the face. A gentleman of the party stated that he dissected the orange and found all the seeds in it. (The child was sucking the juice through a small opening made in the end through the peeling.)

Upon examination of the chest a coarse mucous r le was heard over the chest which might have been from catarrh ; the child was comfortable, was taking a cough mixture under the direction of the attending physician, and as I was told that the symptoms had never seemed to threaten immediate death, I thought it best to await farther development.

I left with the understanding that I should be sent for if the symptoms grew worse. Thirty-six hours after the child died suddenly, and almost without warning. A post-mortem examination showed a part of an orange seed in the larynx, which no doubt had been thrown up from where it had been lodged in one of the bronchia, and produced suffocation.

There was nothing urgent, apparently, in this case when I saw it, and the history that I got left it doubtful if there was anything foreign in the trachea. I think, however, that a careful physical exploration would have furnished sufficient evidence, in corroboration of the reasonable suspicion of the existence of a foreign body in the air passages to have justified an operation, and the result showed that it ought to have been made.

Another case less urgent than this, I was called to see by a medical friend. This case was a girl about seven or eight years of age, who had been brought to the city by her mother from an adjoining State, who thought she had a portion of a pine burr in her windpipe.

The child was very nervous and excitable naturally, but the lungs were not much affected; and although it was evident that there was a foreign body in the trachea, which played up and down, the mother stated that she never, in her worst spells of coughing, seemed in danger. We thought it might be thrown out in a fit of coughing, and as the lungs were not suffering, and there appeared to be no immediate danger, we concluded to postpone the operation. The second day after this the child died suddenly from suffocation. No post-mortem was allowed, but I think there is no doubt of the true nature of the case and cause of death.

I believe that deaths have occurred in this way, and the true cause has never been suspected, or set down to the account of laryngismus or false croup. While on a visit in the country, a few years since, I was asked to see a child about four years of age, who had a singular cough, which seemed to be paroxysmal and sometimes spasmodic, and it was apparently asthmatic. The child appeared delicate and irritable, but no one had any suspicion that there was any foreign body in the trachea, and I saw nothing that led me to suspect it. About two months after this, and near six months after the commencement of the cough, the child exclaimed suddenly after a fit of coughing, "there mother! there goes that seed!" and on examining, a water-melon seed was found on the floor, covered with mucus, as is usual in such cases. This ended the cough, and all the hitherto unaccountable symptoms. This child was nervous, and it was supposed to have some thing like what has been termed laryngismus stridulus, and if it had died by the lodgement of this seed in the larynx, it would probably have been pronounced a case of this nature.

I remember a case in point that occurred in an adjoining town, when I was a boy, and before I was a medical student. A child was suddenly taken with what was supposed to be a fit, while playing with larger children in the garden. The family physician was sent for, but when he arrived the child was playing about, and there appeared to be nothing the matter. These fits, however, were frequently repeated, and the treatment for worms was instituted, but neither this nor any other treatment seemed to have any effect. In this manner a month elapsed, when the child suddenly died in one of these fits. A post-mortem was held, and the heart or centre of a small onion was found in the larynx. The children it was found had been eating and pulling the young onions that were growing in the garden, where the child was playing when first taken.

There is very little danger in tracheotomy in otherwise healthy subjects, as is usually the case in all recent cases of this nature, while the danger is very imminent if the foreign body is left to the efforts of nature. There may be some difficulty occasionally in determining positively the nature of the case; but I think generally there will be found no difficulty, and having determined the fact, I think no surgeon ought to be justified in delaying the operation. The auscultatory signs in these cases are different from those produced by any disease, and the history is always strongly corroborative, and the suffocating spells when cough suddenly takes place (which is always the case when the foreign body is loose), completes the testimony.

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ART. XVI.—*Singular Change of Skin—Albinism.*

DR. CHAILLÉ: Sir—You will please find enclosed a notice of a "spotted darkey," and I want your views in relation to the nature and cause of his disease.

Write me at length upon the subject, as myself and several others feel an interest in what you may say about it. Please return the printed notice, etc.

Yours hastily, etc.,

A. R. NEWSOM.

P. S.—Since writing the above, I searched for the notice mentioned above, and cannot find it, but enclosed you will find the substance of it.

A. R. NEWSOM.

*Singular Change of Skin.*—There is a negro in Franklin county, Mississippi, who is 34 years old, of large size, and was born and raised in that county. About three years ago he was severely attacked with disease, which rendered him helpless for four months. During that time the entire skin peeled off, leaving nothing but the flesh and bones. In a short time the skin commenced growing again, and soon covered the whole body, and now he is perfectly pied all over—the spots being of all shapes and sizes.

His hair and nails came off; and his hair in spots, some white and

some perfectly black. The negro is now perfectly well, and has been so since his recovery from the attack of disease.

This paper comes from my excellent associate, now summering upon the shelly shores of the Gulf, having the following endorsement: "Will Dr. Dowler please answer this?"

ANSWER—NOT AN EXPLANATION.—Pharoah, who forbid the taskmasters to allow the Israelitish brickmakers any straw wherewith to make brick, nevertheless exacted the "tale of bricks" under severe pains and penalties. A doctor who is required to diagnosticate a disease which he has neither seen nor has any symptomatic history of, is in a worse predicament than the brickmakers who had recourse to the stubble-fields when the Pharaonic straw was withheld. Hood, though no doctor, bewailed the sad fate of the diagnostician who had the patient actually in hand: "If any man," says he, "labors in the dark like a mole, it is the 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."

Dr. J. C. Hutchison, of Marshall, Missouri (*Am. Jour. Med. Sci.*, Jan., 1852), reports the case of Jo, a slave, perfectly black up to his twelfth year, who after that time began to turn white, first in spots, and then gradually for ten years growing whiter, until he became altogether white, "his skin presenting the healthy vascular appearance of a fair complexioned European." He lost the sense of smell: "Taste unaffected." His health appears to have been good, though he had had the ordinary diseases of mankind, while the Mississippi negro suffered more than Job, provided nothing was left of him "but the flesh and bones."

If the pathologist can believe that the Mississippi negro lost his entire skin, the physiologist may, perhaps, believe that he not only lost his *pigmentum nigrum*, but with his skin the power of the dermoid tissue to reproduce the pigment cells and perpetuate the coloring matter secreted by the same; so that his case, had he turned altogether white, would be of easier explanation than that of Missouri Jo, and many other Albinos who never lost their skins; but as the

former is spotted only and not completely *albified* coëxtensively with his loss of skin, this theory rests on a sandy foundation. This negro though not a perfectly white-black, is worthy of further investigation by the Caucasian physiologists of Mississippi. Is he white-eyed? Is the color of his ires altered? Would not the ophthalmoscope be useful in exploring this organ? Is the tactile sensibility of the skin, or the taste, or smell impaired?

Now if the severe illness and integumentary denudation which the Mississippi *nègre-pie* suffered should be alleged as a satisfactory explanation "of the nature and cause" of his piebald condition, it might, on the other hand, be urged that albinism often occurs without any apparent alteration of the health either antecedently or subsequently to the albification, so that one hypothetical explanation may be counterpoised by another of an altogether different character.

In this state of ætiological equilibration, a theory of albinism does not seem to rest on a sure foundation. It remains for the future to discover what it is which "changes the Æthiopian's skin," which causes the Caucasian to become unnaturally white, and which generally gives the eyes, particularly the iris, a roseate hue, with probably a diminution of the *pigmentum nigrum*.

The following cases from the *Æsculapian Register* are remarkable instances of albinism, though not reported with sufficient particularity :

"*Ethiopian skin changed*.—A man by the name of Tom Clinton, has been exhibiting himself in many parts of the United States: he was born in Essex County, Va., of black parents, both from Africa, and is now about sixty years old. He was always of a glossy *black*, till fifteen years ago, when a *white speck* appeared at the root of the nail of one of the fingers, which at end of fifteen months had extended over his hand and arm. At present, more than *nine tenths* of the surface of his whole body, is perfectly *white, smooth and clear*. His *hair*, grown grey by age, is still *woolly*. He enjoys good health, sweats easy, and has none of that *rancid odor* peculiar to negroes. His children, twelve in number, are all healthy, stout negroes.

"*Negro becoming white*.—A case is related by Mr. Brown in the transactions of the Medical and Chirurgical Society of Edinburgh, in which the change went on gradually but progressively; and at the end of eighteen months (the period to which the report extends), the

extremities and head were of a natural white appearance ; the breast, abdomen and back, speckled ; and the change making regular progress."

President Jefferson who has seen more Albinos in Virginia than perhaps any other writer, gives the following account which is doubtlessly altogether authentic :

"I will add a short account of an anomaly of nature, taking place sometimes in the race of negroes brought from Africa, who, though black themselves, have, in rare instances, white children, called Albinos. I have known four of these myself, and have faithful accounts of three others. The circumstances in which all the individuals agree are these. They are of a pallid cadaverous white, untinged with red, without any colored spots or seams ; their hair of the same kind of white, short, coarse, and curled as is that of the negro ; all of them well formed, strong, healthy, perfect in their senses, except that of sight, and born of parents who had no mixture of white blood. Three of these Albinos were sisters, having two other full sisters, who were black. The youngest of the three was killed by lightning, at twelve years of age. The eldest died at about twenty-seven years of age, in child-bed, with her second child. The middle one is now alive, in health, and has issue, as the eldest had, by a black man, which issue was black. They are uncommonly shrewd, quick in their apprehensions and in reply. Their eyes are in a perpetual tremulous vibration, very weak, and much affected by the sun ; but they see much better in the night than we do. They are of the property of Colonel Skipwith, of Cumberland. The fourth is a negro woman, whose parents came from Guinea, and had three other children, who were of their own color. She is freckled, her eye-sight so weak that she is obliged to wear a bonnet in the summer ; but it is better in the night than day. She had an Albino child by a black man. It died at the age of a few weeks. These were the property of Col. Carter, of Albemarle. A sixth instance is a woman the property of a Mr. Butler, near Petersburg. She is stout and robust, has issue a daughter, jet black, by a black man. I am not informed as to her eye-sight. The seventh instance is of a male belonging to a Mr. Lee, of Cumberland. His eyes are tremulous and weak. He is tall of stature, and now advanced in years. He is the only male of the Albinos which have come within my information. Whatever be the cause of the disease in the skin, or in its coloring



matter, which produces this change, it seems more incident to the female than the male sex. To these I may add the mention of a negro man within my own knowledge, born black, and of black parents ; on whose chin, when a boy, a white spot appeared. This continued to increase till he became a man, by which time it had extended over his chin, lips, one cheek, the under jaw, and neck on that side. It is of the Albino white, without any mixture of red, and has for several years been stationary. He is robust and healthy, and the change of color was not accompanied with any sensible disease, either general or topical."—*Jefferson's Works*, viii, 318–19.

A word more concerning the point of departure, that is, the Mississippi Negro :

The loss of the entire *cutis vera* includes necessarily the *epidermis* as well as the intermediate *rete mucosum*. Such loss, of course, leaves the whole surface of the body in the condition of an ulcer, which upon healing produces an universal cicatrix, which latter is whiter in the white race than the original skin. The new integument is accepted neither by the anatomist nor physiologist as altogether identical with the original. It is, in some respects, an inferior article, though a very acceptable reserve which Nature brings up in cases of emergency. Without dwelling upon the physiological anatomy of the regenerated integument of cicatrices, the physical appearances alone show a marked difference between them and the original skin. It is evident that the *rete mucosum* of cicatrices in the Caucasian, if not in the black race, fails more or less in restoring the original coloration. J. Hunter maintained that sometimes the *rete mucosum* is never reproduced. He says of a cicatrix: "In 'one black who came under my observation, a sore which had been upon his leg when young, remained white when he was old. However, in many cicatrices of blacks we find them even darker than any other part of the skin."

The tendency to contraction, in almost all scars, particularly in those following burns, giving rise to frightful deformities, requiring terrible and often dubious surgical operations, affords additional evidence of dissimilarity. It is not only difficult to conceive how the Mississippi negro could have lost his entire skin without having lost his life, but it is equally difficult to comprehend how the regenerated skin could have escaped contractions and deformities.

B. DOWLER.

## R E V I E W .

*Contributions to Midwifery and Diseases of Women and Children, with a Report on the Progress of Obstetrics and Uterine and Infantile Pathology in 1858:* By E. NOEGGERATH, M. D., and A. JACOBI, M. D. Pp. 466, 8vo. 1859. New York: Baillière Brothers.

THESE Reports, based on the current literature of the medical journals of two continents, will prove very useful to the student, not only for the retrospective abstracts and brief criticisms which they contain, but also for their copious references to a vast many original articles and monographs, a kind of *index rerum*, wherein fuller expositions of the nature and recent progress of obstetrical, uterine, and infantile medicine may be found.

The critical reader may discover grammatical constructions and ill-chosen words which will admit of improvement in order to bring them up to the utmost purity of the English language. Generally, the style is clear and satisfactory.

The first part of the work, consisting of about 100 pages, contributed by the authors, comprehends the following articles:

Art. 1—Three cases of induction of premature labor, performed in New York, after Cohen's method; with remarks: by E. Noeggerath, M. D. Art. 2—Four cases of injection of a caustic solution into the cavity of the womb, illustrative of the advantages and dangers connected with this proceeding: by the same. Art. 3—Remarks on the employment of pessaries; with the description of a new instrument: by the same. Art. 4—A contribution to the pathogenesis of uterine polypi: by the same. Art. 5—Invagination of the colon descendens in an infant; with repeated hæmorrhages in the colon transversum: by A. Jacobi, M. D. Art. 6—On the oxysulphuret of antimony as an expectorant in inflammatory diseases of the infantile respiratory organs: by the same. Art. 7—On the etiological and prognostic importance of the premature closure of the fontanels and sutures of the infantile cranium: by the same.

In the retrospective and critical portion of the work, the observant reader will be sometimes disappointed in regard to omissions; but on considering the difficulty and scope of the work, and viewing it as a whole, he will be surprised rather than disappointed at the depth of

its erudition, the expansive range of its research, and the comparative completeness of its execution. Few, if any, will read this volume for 1858, without a wish that the learned authors may give for each succeeding year, a similar summary.

Such are the impressions produced by a hurried examination of this work. Hurried examinations even by an abler critic, are, however, of little value in reviewing a new work of this character, consisting of numerous details and critical opinions. Hence, it is proposed briefly to remark upon one or two articles of the first part of this book, its original department, entitled *contributions, etc.*, as neither leisure nor space will permit of a notice of the entire work.

As will be seen above, the first article is devoted to the method of inducing premature labor and delivery.

Dr. Noeggerath, who adopts Dr. Cohen's method of inducing premature labor by injecting warm water through an elastic catheter into the uterus between the uterine walls and foetal membranes, has successfully performed this operation in three cases in New York. This method he maintains is far superior to the douche, rupture of the membranes, or the administration of ergot. These three cases, together with one reported by Dr. Blatchford, are, according to Dr. Noeggerath, the only instances "where this method was made use of in the United States."

The physician who is consulted by erring females, particularly those who are intelligent, and who implore with tears and the most pathetic language the procurement of abortion, have generally used repeated doses of ergot without effect, in that behalf, before applying in their despair for professional aid. Fortunately for foetal well-being, ergot is almost powerless as an abortive. The discovery of an agent of this character, though very desirable for the obstetrician who sometimes meets with deformed bones, or organic changes, rendering delivery at full term an impossibility without a fatal result or great danger to the female, would probably prove detrimental, at least to the externals of morality and the salutary restraints upon libertinage. The induction of abortion by the most skillful hands, directed by a thorough knowledge of anatomy, is a very grave procedure, in which the mother may and often does perish.

Dr. Noeggerath's second article consisting of four cases of injection of caustic solutions into the cavity of the womb, illustrative of the advantages and dangers of such procedures, are too few to establish

a general rule—too perilous to the uterus, fallopian tubes, peritoneal cavity, and life itself, to be acceptable to the conservative practitioner whose golden rule is not only to do no harm, but to do no act the result of which he can neither foresee nor control.

Uterine hydrostatics is not reduced to a mechanical science. The transmitted pressure, the actions and reactions of caustic fluids by forcing pistons and by gravity of fluids including the hydrostatical paradox and hydraulic pressure, undershot and overshot water wheels, are in no wise manageable, and therefore unsafe in the dark recesses of the womb.

The significant titles of Dr. Noeggerath's four cases are in these words : "Case 1 : Intractable hæmorrhage ; injection of iodine ; cure. Case 2 : Uterine hæmorrhage of twenty-three years' standing, cured by a single injection of the diluted liquor ferri sesquichlorati ; alarming symptoms after the injection ; slow recovery. Case 3 : Injection of a caustic solution into the uterus ; severe metro-peritonitis ; recovery. Case 4 : Application of a solution of nitrate of silver to the inner surface of the womb ; death on the sixth day."

In the second case, the symptoms arising from the injection, were, as he justly terms them, "alarming. It was not before ten weeks from the day the injection was made, that she was strong enough to leave her bed, and then only for a few hours at a time." In the third case, notwithstanding the "severe metro-peritonitis" from the injection, it appears that the patient wisely objected to further experiments, as the doctor says : "After this she could not be induced to continue a course of local treatment, being impressed with the idea that the first attempt came very near killing her." In the last case the author says : "Although no autopsy was made, it is clear that the woman died from metro-peritonitis, induced by application of a caustic solution to the inner surface of the womb."

Dr. Noeggerath's practice is repudiated by Dr. Noeggerath's conclusions ; for he expressly says "that we ought to abstain entirely from the use of caustic injections into the cavity of the womb." It is therefore incomprehensible in what sense these cases constitute a "contribution to the diseases of women." Although in the first case, the woman made a happy escape not only from impending death, but from any "inconvenience," yet cases like those of the residuè, might, if repeated, be worthy of the attention of the grand jury of New York, provided that uterine injections be generally as dangerous as Dr. Noeggerath's cases have been according to his own account.

There is a wide difference as to the necessity or expediency of uterine injections of caustics for medicating the uterus and those of simple water for the purpose of procuring abortion or premature delivery. The latter or some operation having the same in view, is carried into effect under a full conviction that it cannot be dispensed with, and, therefore, the risk, however great, must be incurred. In the former there is greater risk without any known necessity; if a beneficial rather than a harmful result follow, it is but a fortunate accident, rather a common recognized sequent. It may be affirmed that this local mode of treating the interior of the uterus, has not been, as yet, established by sufficient experience as an efficacious one, while, for the most part, the general or constitutional treatment presents a well established probability of a favorable termination of the very diseases for which these injections are used. Neither local nor general treatment can be relied on for the cure of some malignant diseases of the uterus.

When the condition of an organ is invisible, intangible, or otherwise insusceptible of accurate diagnostication, as is generally the case with the interior of the uterus, the difficulty of locally applying a caustic injection or other medicinal agent, is great, often insuperable, while the anatomical relations of the womb are such, that the normal parts may be injuriously affected.

When cathartics raged in the valley of the Mississippi there was a city doctor on the banks of that river, whose mode of salutation is said to have been—"How are your bowels to-day?"\*

The formula for salutation at present in the female world might be put—how is your womb to-day? Among white ladies and black wenches (and if letter writers abroad be not mistaken, their Majesties too), womb-complaint is spoken of as a very common occurrence, and it is not at all necessary to mingle with the illiterate or vulgar in order to hear this topic referred to with freedom.

Thirty years ago "the womb-complaint" either did not exist to the extent it now does, or if it did, neither patients nor doctors knew it in their happy ignorance.

There can be little doubt that womb-diseases have increased, are increasing, and might be diminished; if not, the matrons of this Republic as compared with those of American Revolution, must be content to

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\* Sir Astley Cooper mentions a physician with whom he consulted, whose standing preception was—"the fear of the Lord and the keeping of the bowels open."

stand on lower level in physical organization and vigorous health than their ancestors.

Whether this increase of uterine maladies has any connection with climatic, social modes of life, anatomical or physiological changes, or with uterine injections, cauterizings, pessaries, specula, probings novel manipulations so prevalent of late, "must give us pause." It is here, perhaps, if anywhere, that art has been too meddlesome and Nature's hygienic commandments have been too much disregarded.

It is unjust, however, to call the bad, bad, without taking into account the good. For example, thirty years ago, that terrible and repulsive disease, vesico-vaginal fistula, was rarely heard of; during later years it has often occurred (chiefly as a sequela of difficult child birth), five cases of which have been seen in New Orleans, by the writer of these lines—a small number compared with what many other practitioners have witnessed.\* Hitherto the boldest and most skillful surgeon had been struck with despair and disgust at the almost utter impotency of his art in vesico-vaginal fistula, cases of which continued to increase to an unaccountable extent. How is it now? "Watchman! what of the night? The morning cometh"—nay has already come. Read the discriminating estimate, the disinterested adjudication, and the triumphant result which surgery has recently achieved herein, as summed by Professor Stone in his luminous paper in the present number of this Journal. Such "contributions to the diseases of women" and the healing art, are the best answers to sciolists and satirists, who declaim against the utility of the medical profession.

B. DOWLER.

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\* P. S. Since the above was in type the *Medical Times and Gazette* of July 2d, 1859, has been received from which the following is copied :

IRON-THREAD IN VESICO-VAGINAL FISTULÆ.—Since last autumn Professor Simpson has operated on fourteen cases of vesico-vaginal fistulæ. The last two are proceeding favorably in all respects, but the operation in them is too lately done to admit yet of perfect certainty as to the ultimate result. Out of the remaining twelve, in three the operation was only partially successful, and will require to be repeated a second time. In the remaining nine the fistulæ were closed by the first operation. Dr. Simpson has always used the common blue, annealed, iron-wire (No. 32 of the ironmonger's gauge) as his suture thread. In his last six cases he has dispensed with the assistance of all clamps, buttons, or splints, and has merely brought together the vivified edges of the fistulæ, some of which were very large, with deep and closely-placed stitches of the iron-thread. All these six cases have proved successful. The two cases at present under treatment have been operated on in the same way.

## MISCELLANEA.

i. *Elastic Tubes instead of Tents.*—The use of small india-rubber tubes (*tubes de drainage* of the French) instead of lint tents after opening abscesses, etc., has not yet become nearly so general as it well deserves to be. They serve an admirable purpose, and do not irritate in the least. They are much easier to get in than lint or cotton wool; and when once in, instead of closing up the aperture, enable the matter to drain away as fast as secreted, and thus allow the sides of the cavity to remain in contact, with a good chance of speedy coalescence. In the treatment of post-mammary abscesses they are invaluable. Tubing of the size which is the most generally useful, may be had in the shops at sixpence a yard. To remove the sulphur it should be washed in warm water with a little soda. Dr. Savage, Mr. Price, Mr. Spencer Wells, Mr. Bowman, Mr. De Morgan, and Mr. Henry have all employed these tubes for some two years past, and speak well of their utility. Their use has already become very general in the Paris hospitals.—*Med. Times and Gaz.*, June 25, 1859.

ii. *Does the Liver Fabricate Sugar?*—In the Guy's Hospital Reports, Dr. Pavy has published a memoir on what he calls the pretended glycogenic function of the liver. He therein announces: 1st. That the blood taken from the right ventricle of living animals contains only an exceedingly minute quantity of sugar; 2d, and that the liver itself in a normal state contains scarcely any sugar. Dr. Pavy shows, moreover, that when the liver contains much sugar, it has been subjected to causes, which have altered and transformed the glycogenic materials which it contains. When much sugar is found in the blood of a living animal, the liver has been subjected to congestion, or compression, etc. From all which, it follows, according to Dr. Pavy that the liver has not the function of forming sugar. In another memoir Dr. Pavy shows, that the liver enlarges notably in dogs fed on amylaceous matters, or flesh and sugar, and that in animals the quantity of glycogenic material in the liver becomes greater than in animals fed on flesh. From which, it results that the glycogenic materials of the liver appear to be derived from feculent and saccharine foods.—*Journal de Physiologie*, *ib.*

iii. *Yellow Fever of 1859.*—1. Letter writers at Havana represent the yellow fever as having occurred frequently in that city though not in a severe epidemic form.

In June the yellow fever was reported as prevalent in Matanzas (Cuba), where a Russian ship lost her captain and ten of her crew.

2. The Board of Health at New Orleans issued the following bulletin on the 3d of August, 1859:

The Board of Health deem it their duty to state that there has been no yellow fever in this city up to this date. By order of the Board.

A. F. Axson, President Board of Health.

3. Texas and Mexico. The *Brownsville Flag* of the 21st of July, 1859, says :

*Sickness up the Rio Grande.*—We learn through a person just from Edinburgh, in Hidalgo county, that that town and its Mexican sister Reynosa, have been sometime suffering from a fever in most respects identical with the yellow fever which devastated our region last year. In Reynosa some 230 have died, and in Edinburg, 13. It is strange, but few or none of the deaths have been among females. Dr. Bowie, of Edinburgh, reports nine of the thirteen to have died of unmistakable black vomit. Three Americans have died, Messrs. Fowler and Davis, of Seguin, Texas, and Mr. James Robinson, attorney, formerly of this place. Also D. Salvador Dominguez, sheriff of Hidalgo county. The sickness is abating for want of subjects, and all now sick are getting well. It is said that but two persons in Reynosa had escaped an attack.

*From the Rio Grande—Yellow Fever in Brownsville and Reynosa.*—The Galveston Civilian learns from Mr. Savines, a passenger from Brownsville by the Arizona, that from the best information which could be had, over fifty cases of yellow fever had already occurred at Brownsville, one in about three of which had proved fatal; but up to within a few days of the Arizona's departure, it had been confined to the poorer classes of Mexicans, for which reasons mainly, the physicians of the place were unwilling to acknowledge that it was other than ordinary bilious fever. This belief, however, was dispelled as soon as it attacked the American population, several of whom had died—most noteworthy among which number, was a young lawyer of much promise, Wm. S. Lincoln, from Maine, and descendant of General Lincoln, of Revolutionary renown. A perfect stampede had taken place among the unacclimated of the city; some had started for Monterey, some for Corpus Christi; whilst several came passengers on the Arizona, en route North. It was estimated that no more than twenty or thirty persons were left in the city subject to an attack.

In the Mexican town of Reynosa, forty miles above Brownsville, over one hundred and thirty had died of the same disease, where it has been well ascertained the fever has been busily at work since early spring.

The *New Orleans Picayune* of to-day (August 6th), has the following statement:

*Yellow Fever on the Mexican Coast.*—We regret to learn from a gentleman direct hither, that the yellow fever (vomito) is prevailing as an epidemic not only on the Rio Grande, as before recorded, but also at Tampico and all along the Northern Mexican coast. The mortality is very great, especially among the Mexican population, and the people were fleeing in every direction.

iv. *The Wounded in Italy—Effects of the New Projectiles.*—M. Armand, of the Fourth French Army Corps, writes as follows: The battle fought on the Ticino was hotly contested on both sides, and attended with great losses. Everything in our department was organized for acting *cito citissime*, conformably to the urgency of the circumstances, and the injunction of inspector Baron Larrey: "Proceed direct to the bridge, dress the wounded as rapidly as possible, so as to put them in a condition to be evacuated on the Hospitals, and do not



loose time in the great operations, which may be deferred." Most of the injuries of the head only required simple dressings, for the reason that those which penetrated were usually fatal on the spot. Those of the face were accompanied by the most frightful injury, without affecting the intellectual faculties; the patients themselves, indeed, coming to have their wounds dressed. Wounds of the neck were usually immediately fatal or comparatively slight. One of the prisoners had received a wound in the mouth with a saber bayonet, which glanced off at the side of the jaw, and came out at the lateral part of the neck.

There was hardly any loss of blood, and a simple suture at the commissure of the lips, and a bandage were all that were required. Injuries of the upper extremities almost always, whatever their amount, allowed of the wounded repairing soon to the ambulances. Penetrating wounds of the chest and abdomen, usually so fatal, allowed in several instances their victims to survive for some time—usually to die, however, on the road to, or soon after their arrival at, the hospital. In some cases, however, vigorous reparative power of the economy, and a free use of antiphlogistic means enabled recovery to take place. Injuries to the lower extremities were numerous, and the fractures were often comminuted. The rule was here, as in other cases, to extract accessible foreign bodies, arrest hæmorrhage, and so put up the part as to enable the patient to be carried to the hospital, where the question of amputation would have to be decided. The military surgeon is thus compelled to resort to much temporizing surgery; and, for our part, we scarce know the case calling for immediate amputation on the field, except when some large projectile has carried away a limb with irremediable laceration.

v. *Secondary Syphilis*.—The space of this Journal will not admit of a more detailed translation of the late proceedings of the Academy of Medicine upon the contagiousness of secondary syphilis, than the following brief summary from the *Med. Times and Gaz.*, of June 25, 1859:

A commission, composed of MM. Velpeau, Ricord, Devergie, Depaul and Gibert, appointed to inquire concerning the contagious nature of the secondary accidents of syphilis, has decided (with the exception of M. Ricord, who reserves himself on certain points): 1. That there are secondary or constitutional accidents of syphilis manifestly contagious; and of these accidents in an especial manner the mucous pustule or flat tubercle. 2. That this rule applies to the nurse and the child she suckles, as well as to others; and there is no reason to suppose that the product of these accidents has different characters in the child at the breast of one what it has in the adult.

vi. *Vitality of the Tadpole*.—The tail of the tadpole lives after it has been separated from its attachment to the body; this has been long known. M. Vulpian, in addition, has lately observed, not only that this separated tail does not die, but that it grows, and is developed up to the ninth or tenth day after the section has been effected. He exhibits plates, which show the presence of muscular fibres and vessels in tails which had neither one nor the other in them at the time of their section.

vii. *Cutano-hysterical Anæsthesia*.—Dr. Aug. Voisin, a learned and rising young physician of Paris, has written an ingenious pamphlet of thirty-nine pages (*De L'Anesthésie cutanée Hystérique*), comprising fifteen cases laboriously observed at the Charity and Lariboisière hospitals, during his *internat*; including cases of hyperæsthesia, paralysis, etc.

viii. A death from injection of carbonic acid into the uterus is reported by M. SCANZONI. Air entered the abdomen, etc.

ix. Tracheotomy has been successfully performed in a case of chloroformic asphyxia (apparent death), by Dr. LANGENBECK, of Berlin.

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### OBITUARIES.

*Tribute of Respect to* DR. BENJAMIN F. HARNEY, *late Surgeon U. S. Army*: By JOHN B. PORTER, M. D., Surgeon U. S. Army.

DIED, at Baton Rouge, Louisiana, August 29, 1858, Doctor Benjamin F. Harney, Senior Surgeon of the Army. It is not yet too late to do justice to an old and faithful officer, by noticing some points in his medical history, for which he has not received due credit, to which he was fairly entitled, and which he has really merited.

Dr. Harney was born in Delaware, from which State the family removed to Tennessee when he was but a youth. On the completion of his medical studies he removed to the State of Mississippi, in order to practise his profession; and in 1814, during the war with Great Britain, he received the appointment of surgeon in the army. Early in 1815 he joined General Jackson's army at New Orleans, but arrived too late to participate in the glorious victory of the 8th of January, which the doctor always regretted. At the close of the war, when most of the army was disbanded, Dr. Harney was retained in service. He was with General Jackson during the contest with the Florida Indians, when Arbuthnot and Ambrister were executed. In 1840 he again went to Florida, where he was medical director of the army (then acting against the Seminole Indians) until 1842.

In 1847 he served in Mexico as medical director of General Scott's army at the siege of Vera Cruz, and in the valley of Mexico; and in one encounter with the enemy he was slightly wounded.

Dr. Harney possessed good traits of character; he was open and

straightforward in all his dealings ; brave, generous, and magnanimous ; without envy, jealousy, or hypocrisy ; in few words, he was an honest man. He was popular in Baton Rouge, and when a new hotel was erected in the place, it was named the "Harney House." On the day of his funeral the places of business were closed, and he was buried with the honors of war by the volunteer companies.

The services of Dr. Harney were especially valuable to the medical officers of the army, on account of his active agency in introducing the efficient use of quinine in the treatment of Florida fevers, which were principally of the intermittent, remittent, or congestive type, of a severe character. To the active treatment introduced by him may be ascribed the success of the medical officers of the army during the sickly year of 1841, and the army owes him a debt of gratitude. On joining the Florida army in 1840, Dr. Harney strongly advised the employment of larger doses of quinine than had been heretofore given—10, 15, 20, and 25 grs., and even more—which advice was immediately put into practice by the medical officers present (this was at Tampa), and subsequently by the whole medical staff in Florida, so that during the sickly year of 1841 the use of larger doses was general.

The writer joined the Florida army in July, 1839, at Fort White, East Florida, which was situated on a branch of the Suwannee river, in one of the most sickly parts of the country, where malarial fevers were prevalent and severe; and here, of course, quinine was essential, the dose having been gradually increased, until four or five grs. were given, generally combined with *infus. capsici* ; but no idea was entertained of using the medicine in the large doses recommended by Dr. Harney, until after his arrival and by his advice, and it is believed that my doses of quinine were as large as those of any medical officer serving in Florida. Previous to Dr. Harney's arrival we had been in the practice of using small doses—one, two, three, four and five grs., few daring to venture so high as the last named quantity.

Early in 1841, in the month of March, the writer took charge of the sick at Fort King, East Florida, where there was a large command, and where the sickness was very great. Here we were fully prepared to carry out the principles and practice of Dr. H. most successfully—as did many members of the medical department, at various posts, during that year.

Having been transferred from Fort King to St. Augustine in the autumn of the same year (1841) remittent and congestive fevers were prevalent, followed finally by unmistakable yellow fever; and through all of these fevers we had abundant experience in the use of quinine according to the plan of Dr. Harney.

It is not intended to enter into the details of practice, but simply to bear testimony to a particular fact; and no one is better acquainted with the facts, the writer having been among the very first to adopt the large doses advised by Dr. Harney. He is entitled to the whole credit. "Render unto Cæsar the things that are Cæsar's."

During the winter of 1856-7, Dr. H. visited New Orleans, where we had a full and free conversation on the subject. He spoke with indignation of the attempts of some few persons to appropriate the honors wholly to themselves, without giving the least credit to those who introduced this practice into the Florida army, in 1840; and at this time he proposed, at a future day, to give me his facts and experience for publication. Circumstances prevented another meeting. The Doctor, unfortunately, was not fond of writing, or he would have made his publication long before the period of our meeting. But the annoyance he experienced was none the less for his silence.

Those who have been in Dr. Harney's situation will appreciate his feelings. It may not be the importance of the transaction, but the meanness of the act, which excites indignation. In 1841, the writer was relieved from duty at St. Augustine, by an assistant surgeon of the army, after the yellow fever epidemic was entirely over, and his successor did not see a single case of the disease; yet he took the hospital records and made a report to the Surgeon-General, using the remarks as his own, as if he had treated the epidemic. Further, he subsequently spoke of the use of quinine in that fever as if he had administered it. The evidence is on record in the Surgeon-General's office. This man is now a professor in a flourishing Northern medical college. He should have the chair of Ethics also.

In reply to inquiries, Dr. Harney informed us that having settled in the Valley of the Mississippi, and having also been stationed in it during a large portion of his military service, he had treated a large number of cases of malarial fever; that when quinine was introduced as a substitute for cinchona, he commenced the use of it and gradually increased the dose; that an older physician than himself, in his vicinity, whose name cannot be recollected, taught him to use the medi-

cine in still larger doses than before; and, finally, that he had himself increased the dose to a larger quantity than the physician ever advised him to prescribe. Dr. Harney, in 1840, did not regard the use of large doses of quinine as an experiment, but as established practice; and with him it was established practice.

Almost without exception, the medical department of the army, particularly those members of it who served with Dr. Harney, in Florida, give to him the well-merited credit of strongly recommending to the medical officers the practice of giving large doses of quinine in malarial fevers. Most indubitably, he is entitled to the whole credit. *Palmas qui meruit ferat.*

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DIED, in Montgomery, Alabama, on the 4th of March last, after a lingering illness, Dr. William M. Boling.

Dr. Boling was born in Baltimore County, Maryland, July 14th, 1811. His father dying very soon after his birth, left him under the protection and guidance of his mother. What his early advantages were for an education, we are not informed. We know that he went to Greensburg, Pennsylvania, to reside when eighteen years old, with only an ordinary English education. While here he made choice of the profession of Medicine, and in 1833, entered upon its study in the office of Dr. S. P. Brown. He prosecuted his studies with great assiduity, applying himself not only to his text books, but to the Greek, Latin and French languages, a very good knowledge of all of which he soon acquired. He attended his first course of lectures at the Jefferson Medical College, 1835 and '6. Soon after the close of the session he set out in quest of a location to commence practice. Coosawda, Antauga County, Alabama, is the place he selected. Here he practised with much success until the fall of 1837, when he married and returned to Philadelphia to attend his second course of lectures at the Jefferson College. After graduating the following spring, he returned to Alabama and settled in Montgomery where he continued to reside up to the time of his death.

As to the success Dr. Boling met with in his profession and the position he occupied as a writer and teacher, we have only to refer to the periodicals of the day. His contributions to the medical literature of our country were always well received. Many of his papers possess great merit, and none perhaps more than the one entitled "Observations on Remittent Fever," published in the American

Journal of the Medical Sciences. In this essay he has shown most clearly the wide range of his thought, the shrewdness of his intellect, the force of his reasoning powers, and the knowledge he possessed not only of the resources of his art, but of nature herself. His views here set forth have been largely quoted by nearly, if not all subsequent writers upon the subject in this country.

In 1848 he was invited to take the chair of *Materia Medica* in the Memphis Medical School, which he accepted. He was connected with this school but one session. He gave, however, ample proof of his ability as a teacher, and endeared himself to a large circle of friends throughout the South and South-west. Soon after leaving Memphis, he received a call from the Transylvania Medical School to the chair of Obstetrics. In this school he delivered one course of lectures (1849 and '50), and as a thorough teacher of the branch assigned him, he gave entire satisfaction. After this session closed, he resigned his chair and returned to his home in Alabama.

There are other positions which he has been called upon to fill, highly complimentary: such as President of the Alabama State Medical Association, and Vice President of the American Medical Association. He never accepted a professorship in any other school after he left Transylvania, although several advantageous offers were made him. Having a wide circle of friends, he preferred remaining among them, to being a public teacher. The result of this was an overwhelming practice. For several years he bore up under his immense physical and mental labor, but at last his feeble frame began to give way under it. In the spring of 1853, paraplegia showed itself, from which, although better at times, he never recovered. Notwithstanding the gravity of his infirmity, he continued as long as he was able to get about, to do some practice, and never ceased to take that interest in medicine which had characterized the whole course of his professional life. His disease gradually grew on him, until last winter when he became almost completely helpless, and finally died from the prostrating effect of nausea and vomiting, occasioned by an attack of intermittent fever. Thus ended the short but useful career of Dr. William M. Boling; truly may it be said a martyr to his noble calling. Had he enjoyed the advantages of many, and been more fortunate in the selection of a wider field for his labors, we can safely assert, he would have been second to no one in the ranks of his profession.

The private character of Dr. Boling was spotless. His moral worth, coupled with the kind and generous impulses of his heart, rendered him one of the noblest of men. His intercourse with the world was always characterized by the strictest integrity. He loved truth and candor. To many his manner appeared austere, though not to such an extent as to render him unapproachable. Dignified and self-possessed, he always commanded respect. In the relation of husband and father the goodness of his nature was ever dominant; gentle and affectionate, he smoothed the asperities of life and diffused happiness around him. Long will the memory of so good a man be cherished by us who knew him well. His reward is that blissful immortality beyond the grave. B.

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DIED.—At Memphis, June 14, 1859, of gastro enterites, Dr. Wm. J. Tuck, a native of Halifax county, Virginia, and son of Dr. D. G. Tuck, now of Kentucky. Dr. T. was born February 22d, 1814; graduated at Kenyon College, Ohio, in 1834, and in the medical department of the University of Pennsylvania in 1838. Removed to New Orleans January, 1841; was licensed by the Medical Board of Louisiana to practise medicine; resided in that city until August; removed to Memphis, January, 1842; was elected Secretary of the Board of Health, October, 1852, and acted as such, with but a short intermission, till his death. Filled the chair of the Institutes of Medicine in the Memphis Medical College during the session of 1858-9, during which time, wherever he resided, he was beloved by and endeared to his friends by his uniform goodness and amiability of character.

In the practice of his profession he was kind and skillful—as a teacher of medical science, he gave high promise of success, having kept pace with its progress by patient study and dilligent research, he was an able exponent of its principles.

He was an occasional contributor to the press and to medical literature, and was the author of a *gem* of a work, entitled “Sabbath Reading,” which possessed decided merit. His mind, which was richly stored with general literature, was rather philosophical than poetical. His style of writing was chaste and concise in the extreme without being ornate, it was impressive and logical.

In his intercourse with his fellow men he was modest and retiring, yet social and dignified—with his professional brethren, courteous and honorable—in his business relations, an *honest* man.

To add to the many moral beauties of his life, he was a conscientious, practical *Christian*—his every day walk and conversation indicating the purity of his heart and the excellencies of his character.

Such is an imperfect sketch of the life and character of Dr. W. J. Tuck.

His virtues and his memory will long be held in affectionate reverence by his many friends.

COMMITTEE.

*Mortality Statistics of New Orleans, from June 5, to August 7, 1859, compiled from the Weekly Reports politely furnished by Dr. Baldwin, Secretary of the Board of Health. Population of New Orleans by the last census, 138,000.*

Time.	Total Deaths.	Children under 2 yrs.	Under 20.	U. States
June (4 weeks).....	525.....	215.....	297.....	347
July (4 weeks).....	563.....	213.....	302.....	350
August (1 week).....	131.....	36.....	60.....	65
Principal Diseases.	June (4 weeks.)	July (4 weeks.)	August (1 week.)	
Still born.....	17.....	28.....	3.....	
Trismus Nascentium.....	11.....	19.....	5.....	
Teething.....	26.....	36.....	5.....	
Cholera Infantum.....	18.....	15.....	4.....	
Infantile Convulsions.....	30.....	41.....	4.....	
Infant. Marasmus.....	21.....	15.....	4.....	
Croup.....	3.....	5.....	0.....	
Inflammation of Throat.....	23.....	0.....	0.....	
Scarlatina.....	3.....	7.....	4.....	
Rubeola.....	3.....	5.....	1.....	
Variola.....	1.....	0.....	0.....	
Tetanus.....	5.....	2.....	2.....	
Diarrhœa and Dysentery.....	52.....	42.....	18.....	
Gastro-Enteritis.....	12.....	9.....	1.....	
Inflammation of Liver.....	7.....	9.....	0.....	
Inflammation of Lungs.....	12.....	5.....	2.....	
Phthisis.....	57.....	63.....	14.....	
Apoplexy.....	7.....	10.....	2.....	
Congestion of Brain.....	18.....	10.....	5.....	
Fever, Typhoid.....	12.....	16.....	4.....	
" Miasmatic.....	21.....	33.....	11.....	
" Yellow.....	0.....	2*.....	0.....	

\* These two deaths were reported on July 28th and 29th. The Board of Health has officially denied them to have been of Yellow Fever. August 13. Although the report of the Board of Health for the current week is not yet received, it is believed that no case of yellow fever, or change in the sanitary condition of the city has taken place since the above table was made.

CHAILLÉ.

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.—*From the Medical Purveying Office, U. S. Army, N. O. New Orleans, La., Lat. 29 deg. 57 min. 30 sec. N.; Long. 90 deg. W. Altitude of Barometer above the level of the sea, 35 feet.*

1859.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
	Max.	Min'm.	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
June.....	9 P. M.	2 P. M.	30.262	9 P. M.	7 A. M.	81.54	2 P. M.	7 A. M.	82.11
	23th.	22d.		19th.	5th.		2d.	5th.	
	30.420	30.090		88	74		90	73	
July.....	7 A. M.	9 P. M.	30.266	2 P. M.	7 A. M.	83.68	2 P. M.	7 A. M.	83.35
	7th.	9th		27th.	11th.		24 & 27.	10th.	
	30.392	30.150		90	77		90	76	
1859.	HYGROMETER.			PREV'G WINDS.	WEATHER.		RAIN.		
	Max.	Min'm	Mean.		Fair.	Cloudy.	Days.	Quantity.	
June.....	2 P. M.	7 A. M.	78.46	E.; S. E.; & N. W.	20.	10.	16	7.12.	
	1 & 12.	5th.							
	83	69							
July.....	2 P. M.	7 A. M.	78.94	E.; N. W.; & S. W.	23.66	7.33.	8	0.9?*	
	Several.	10th.							
	82	73							

\*The smallest quantity of rain for one month, so far as this register extends, it having commenced in 1855.

T. HARRISON, Clerk.



THE  
NEW ORLEANS  
MEDICAL AND SURGICAL JOURNAL.

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NOVEMBER, 1859.

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ORIGINAL COMMUNICATIONS.

ART. I.—*On Inflammation*: By WARREN STONE, M. D., Professor of Surgery in the University of Louisiana.\*

This term is one of the oldest in the science of medicine ; it covers a large portion of the diseases that the practitioner is called upon to treat, and in its diagnosis there is a great degree of unanimity in the profession ; but in the treatment, notwithstanding our boasted improvements, there is as great a diversity as has ever existed, not excepting the days of Brown and Broussais. This diversity arises principally from two causes: first, the disposition each practitioner has to take one form of inflammation as a type for the whole, and second, our imperfect knowledge of the action of therapeutical agents. The term inflammation is a general one, and covers a great variety

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\* [Professor Stone, during a temporary absence from New Orleans, forwarded this paper on inflammation, accompanied with a personal letter, the publication of which will be unexpected but readily excused by him, as it will afford the readers of this Journal unalloyed pleasure to learn that the present is but the precursor of a series of practical contributions.]

NEAR ST. FRANCISVILLE, LA., August 29, 1859.

MR. EDITOR—According to promise, I herewith send you a small contribution, which I fear you will think hardly worth publishing. I have undertaken to give some of my impressions upon an important subject, and my method of analyzing it, but find that I have totally failed. I know I have observed much, and honestly, but with no object, except to learn the right use of medicine; but although my convictions, to me, have all the force of facts, when I come to put them to paper I feel myself in the condition of an ancient philosopher, who, to a difficult question, answered, if you don't ask me I know, but if you ask me I cannot tell. However, good or bad, publish it, and I will continue the subject, and try to make something intelligible on the subject of treatment or the application of therapeutical agents to pathological conditions.

Very truly your friend,

To DR. B. DOWLER,

WARREN STONE.

of pathological conditions, some of them quite opposite in their nature, and calling for very different therapeutic agents.

No one has been able to give an unobjectionable definition to inflammation ; but there is no difference of opinion as to what constitutes it. The pathology, so far as it is tangible to our senses, is thoroughly understood, and very uniformly appreciated, and the diversity of treatment arises mainly from the difficulty of adapting therapeutic agents accurately to the varying morbid conditions which must necessarily be found in a morbid action produced by so many causes and under so many different circumstances. Upon nothing can the action of therapeutic agents be so well studied as upon inflammation, when we have all the various disturbed conditions of nerve force, and abnormal conditions of the blood, and often upon the external surface, where the effects can be better appreciated than when hidden from view. The conditions of healthy nutrition, says Mr. Paget, are: 1st—a regular supply of blood ; 2d—a right state and composition of that blood ; 3d—a certain influence of the nervous force ; and 4th—a natural state of the part in which nutrition is to be effected. All these are usually altered in inflammation, and whatever may be the immediate cause, the character of it varies as these conditions vary from the healthy standard. When these conditions are natural and healthy, injuries and lesions are followed by only just enough excitement, or increased nutritive action for their reparation, or what Hunter called healthy inflammation ; and we have nothing to do but to place the injured parts in the most favorable condition for the performance of nutrition. But these healthy conditions are liable to be disturbed or altered, so that upon the receipt of an injury instead of a healthy recuperative reaction, excessive exudation of lymph takes place, which results in more or less mischief in proportion to the deviation from the healthy standard, and even these healthy conditions may be so far disturbed as to give rise to inflammation spontaneously, or from causes so slight as not to be readily appreciated. The majority of the cases that fall under the observation of the surgeon, are the result of mechanical injury or some tangible cause, and generally under circumstances approaching those of health ; while the physician sees mostly the cases that arise spontaneously, or from slight causes, showing some blood-poison or epidemic influence which modifies greatly the character of the reaction, and hence the difference between them in their views of the

nature and treatment of inflammation. The phenomena of inflammation, the changes that take place in the blood and tissues during the inflammatory process, are accurately described in all the modern works on the subject, and I shall only speak of them in connection with the action of therapeutic agents. It makes but little difference whether the morbid process begins with, or is the fault of the fluids or solids ; we first observe an increased sensibility of the nerves of the part and a flux of blood to it. This is the first stage, which is soon followed by the second, the exudation of lymph, which under the most favorable circumstances, organizes, and is finally absorbed, or it degenerates or changes into pus, and is finally discharged wholesale, or it may disintegrate the tissues, which, if on a small scale, is called ulceration, and if on a large scale, is called gangrene. Now, it is quite useless, in a practical sense, to attempt to determine whether this process is an increased action or a diminished action ; whether it is strength or debility, or whether the remedies which experience proves to be useful in mitigating it, are stimulants, revulsives, contra-stimulants, or sedatives, according to the views of Brown, Broussais, Rasori, or the more modern idea. To the physician who has no theory to maintain, who is simply engaged in the cure of disease, the inquiry is, simply, what will modify, and tend to prevent the disorganization which this morbid process tends to? It is an old but true saying, that extremes follow each other, and it is strongly shown in the use of medicine. Excessive medication evidently gave a foothold to homœopathy, which we know to be the other extreme—that is, no medication at all. The excessive employment of blood-letting has led to its almost total abandonment, and so of many of our most valuable remedial agents. There is no doubt another cause that has operated strongly in producing the general change in the treatment of this class of disease, and that is the change in the character of disease itself. This fact is denied only by those who are ambitious for the merit of founding a new doctrine. Watson, whose writings exhibit as little prejudice as any modern work, speaks of the change in the character of inflammation as a fixed fact, and attributes it to the influence of the cholera atmosphere, but practically the cause is of no consequence, since we can have no control over it, and scientifically it is not worth discussing, since the point can never be settled. Bennett, of Edinburgh, whose doctrines are likely to be adopted as extensively, perhaps, as those of his prede-

cessor, Brown, uses much special pleading to prove that no change has taken place, and that the old practitioners were in error. I will not pretend to argue the point. All those who have been close observers of disease since 1832, can have no doubts on the subject. This seems to be a law which is not confined to any one form of disease. When John Hunter described syphilis, and established the mercurial treatment, the prevailing variety was that of the indurated chancre, and the exceptions were evidently few; but although he considered mercury a specific in syphilis, he did not commit the common blunder of giving it in all cases of sores caught by impure sexual intercourse. The few cases in which mercury was not appropriate, he called pseudo-syphilis. These cases of pseudo-syphilis, however, began to increase, and the true Hunterian to diminish, until the former became the most prevalent, and mercury, in its indiscriminate use began to do more harm than good, when men possessing but little of the genius for observation of their great predecessor, instead of analyzing the disease, and determining the cases in which mercury was useful and those in which it was injurious, established an anti-mercurial school which divided the profession with the mercurialists. The results of farther and truer investigations are (or ought to be) universally known; and now, although the cases of pseudo-syphilis, are, in proportion to the true Hunterian variety, as three or four to one, mercury is the *sine qua non* in the treatment of true syphilis as much as it was in the days of the great surgeon. This is a strong illustration of the effect of the excessive and improper use of a valuable medicine, and shows how difficult it is to determine the precise mode of action of a remedy, and to adapt it to the precise pathological condition that it is calculated to relieve. There are not many medicines, the good effects of which are so marked as mercury, properly used, in true syphilis; and if it has been so difficult to give it its true estimate and reduce it to rule in practice, hence it is not singular we should entertain different opinions upon agents less marked in their effects. Again, if there was so much difficulty in establishing the true nature and varieties of syphilis, it is not singular that opinions on the subject of inflammation should be as opposite as those of Brown and Broussais, or Bennett and Alison.

I hope that some one with a genius for observation and description, will soon come forward and do as much towards systematizing the different forms of inflammation and establishing the proper treatment,

as Ricord has done for syphilis. There are many causes that modify and give character to inflammation beside the general one spoken of. Defective excretion leaves the blood impure, as in gout and rheumatism, and there are epidemic influences which operate upon the system, in a manner that can only be known by its effects. Pneumonia not unfrequently appears as an epidemic in the South and West, and its character varies very much as is shown by the fact, that the same treatment which is successful in one epidemic fails in another. What would be the value of statistics in support of one mode of treatment over another, unless the precise pathological condition is pointed out, so that the same treatment could be used under the same circumstances? Mr. Bennett, like every one who has a theory to maintain, has resorted to statistics, and in like manner has limited himself to such as favored his doctrine. If he had wished to advocate the doctrine that the character of inflammation changes, he might have found statistical proof without going far from Edinburgh, and these statistics would also have shown that his remarkable success depended upon the character of the pneumonia more than any remarkable influence of his treatment. Statistics from Glasgow and some other cities of Scotland, show that the results of the old treatment of pneumonia during the same period, were nearly as favorable as those shown by Mr. Bennett. I have no doubt but Mr. Bennett's management of the cases he reports was perfectly correct, and evinced great good judgment; but I object to his views that the disease is unchangeable, the same through all time, in all places and all seasons. The truth is, that it is the improper application of remedies, and not their inapplicable character, that renders them useless or injurious. I admit that it is better to look on and regulate diets and drinks, and aid nature with harmless things, than administer active therapeutic agents at random, or without a precise knowledge of the pathological condition to be relieved, and of the action of the agent administered; but if we are to abandon the use of active agents on the principle that we keep edge tools from children, it is time to abandon the practice of medicine to homœopaths and old women, and follow in the footsteps of Sir John Forbes and Jacob Bigelow. I have no doubt but they are conscientious men, and are the best judges of the evil they have done to their fellow-men. They are not the first men of note and talent that I have thought did more harm than good by their excessive and indiscriminate use of medi-

chine. Fortunately for mankind in general, and the medical profession in particular, all organized beings possess a certain recuperative power, which resists, in a great degree, the various causes that tend to their destruction, and consequently, if disease is left to the resistance of this power alone, the result could be made to show favorably by any partial statistician. This is a consoling fact to those who cannot avail themselves of the benefits of science ; but it does not lessen the confidence in the good effect of therapeutic agents in any one who is acquainted with the physiological laws that govern us, and the influence of the destructive elements that surround us and tend to the disturbance of these laws, and of the effect of the various agents that science can bring to the aid of nature, and counteract morbid effects, and thereby relieve human suffering and increase the sum of human life.

The science of medicine, studied and practised scientifically, is a noble and gratifying profession, and furnishes a wide field for the keenest intellect. The facts are many, but can be acquired by ordinary minds, but their application under all the varied circumstances that we are called upon to apply them, cannot be reduced to rule, but must be left to our judgment. This is a great responsibility, and in the use of active remedies it is well to act upon the principles which humane judges charge juries in criminal cases, that if they have any doubt as to the guilt of the prisoner, let him have the benefit of the doubt. If we have any doubt as to the good effect of an active medicine, let us give the patient the benefit of the doubt and withhold it. With this precautionary method we will not be likely to over-medicate. I have urged this in defence of the science of medicine, for, agreeable to the law of extremes, the world in general is becoming timid in the use of drugs ; homœopathy has obtained a partial foothold upon the strength of it ; some who have gone through a proper course of medical education, but who cannot obtain a livelihood by practising honestly, put on the homœopathic disguise and cheat the world : and even some of the better class of our profession find it easier to fall in with public prejudice, and palliate, than to labor for that accurate practical knowledge which would enable them to combat disease boldly with whatever means may be proper, without fear of comparison, or of acquiring censure for the result. Whoever, having some knowledge of medicine, shrinks from its proper use, may deserve to be called prudent, but no more than

if he were to look on and see a patient gradually perish from the loss of blood, because he is afraid to cut down upon the wounded vessel and tie it.

The truth is, that except in certain well-defined conditions, the effect of medicine is not so marked and well defined as to appear the same to every observer; and it is also true that in very many diseased conditions, the natural powers are quite sufficient for the cure, and the close observer will not discover any indication for his interference. Next in importance to knowing what ought to be done, is knowing what ought not to be done, or knowing when disease may as well, or better, be left to its natural course. The science of medicine has been brought to a great degree of perfection when considered in its separate branches; anatomy, physiology, pathology, symptomatology, and therapeutics. There is here a general agreement in the profession; but when we come to construct it, and apply it to the cure of disease, it ceases to be an exact science, and the fallacy of human judgment destroys the unanimity. It is plain, then, that the great difficulty in the practice of medicine consists in the correct application of therapeutic agents to the cure or relief of pathological conditions. It is plain that science alone does not make the physician, any more than it does the musician, and the administration of drugs under acknowledged scientific rules may be as far from the true practice of medicine, as the making of a noise on a musical instrument is from the scientific practice of music; but it is equally plain that observation and experience, unless guided by science, are not likely to lead to more exact practical results. True experience is the knowledge acquired in the application of scientific facts and rules. Who has not in his recollection some one, who is a perfect walking cyclopædia of medicine, whom he would not trust to treat a critical case? The fact is, the true cultivators of the science have little leisure for the practice of it; and those who faithfully practise it, have little leisure to pursue it. If the student makes himself master of all the undisputed facts in medicine, and applies himself diligently to learn to apply them accurately in practice, he will have little leisure time for the pursuit of new ones. It is to be regretted that the truly scientific men of our country, are not more encouraged and rewarded, and less encouragement offered to those who live by practice and are striving for the honors of new discoveries, while they are yet deficient in their knowledge of the already established and undisputed

facts. The prevailing idea is, that every one ought to communicate his experience for the benefit of the profession, but the truth is, that true experience, like revelation, can be felt and realized, but cannot be communicated to a second person by any power of mere human description, and the few tangible facts that may occur to one in a long life of experience can be recorded in a small space. If our experience has been valuable to us, we may be able to impart our method of obtaining it, but everything practical we must learn ourselves.

Under the head of inflammation I commenced this article with the view of giving my idea of the action of medicine under the various pathological conditions which are found under this head, or rather to reconstruct the treatment of inflammation, and bring to mind old and well established views which of late have been much discarded. Blood-letting, tart. emetic and calomel, are terms familiar to all who received their first lessons when the term antiphlogistic expressed a treatment called for in inflammation, but between the change in its character and their too general and indiscriminate use, these powerful agents have sunk into unmerited disrepute. Whatever the sophist may write, or whatever ingenuity may be exercised in the use of words to change our views of the nature of things, we do occasionally, and indeed very often meet with an inflammatory condition which calls for what has been termed antiphlogistic treatment, or the lowering of the vital forces. This condition is recognized by pain and heat, and an active circulation in the part affected, and by a quick and resistant pulse. This condition is dangerous to the organ in which it is located, and whatever tends to lessen the danger without injury to the system, should be opposed to it. Blood-letting, when properly done, acts as a sedative upon the nervous system, lessens the force of the circulation and causes a contraction of the capillaries of the affected part—in a word, diminishes the whole morbid process without really permanently diminishing the vital force. In very many cases there is a plethora, and the loss of blood is of use independent of the sedative effect; but in general it is best to take the blood rapidly, while the patient is sitting, so as to produce nausea and approaching syncope with moderate loss; when it should be necessary, it may be repeated.

The immediate effects of blood-letting, both local and general, will not be denied, and if the idea that has been earnestly inculcated of



its ultimate bad effects can be done away with, it will again be adopted in practice in appropriate cases. When the nervous force is active, the blood nearly of normal proportion and quality, and the digestive and assimilative functions tolerably good, blood-letting is not only safe but much less injurious than an active cathartic. This is shown by the rapid recoveries from severe injuries after great losses of blood. When I was House-Surgeon in the Charity Hospital, a patient was brought in at twelve o'clock at night, with both legs crushed from midway below the knees, and the right arm to its middle. He had been run over about eight o'clock, by the train of cars on the Pontchartráin road, and as he was not seen, and was rendered senseless by an injury to his head, the train passed over him three times before he was discovered. His system was as nearly drained of its blood as it was possible, without producing death; he was pulseless, and believing that there was no chance to save life, I laid him upon a bed to die. However, his respiration was good; he was thin, but a robust man, below middle age, and upon second thought I concluded to make an attempt to save the poor man. An injection of carbonate of ammonia was given, and as soon as its effect was produced, I commenced operating and removing all three of the mutilated limbs. Such nourishing drinks as his stomach could bear were given, and strong broth was given by injection. Digestion and assimilation were established in a few days, and he made a rapid recovery. Cases of a different nature have frequently occurred in my experience. A patient was brought in the hospital with a compound dislocation of his ankle, which was produced while he was intoxicated. *Mania à potu* followed. Gangrene set in, and in proper time I amputated the limb just below the knee. His condition was yet bad, as he had not renewed his blood, his appetite had not returned, and the flap sloughed; but finally his functions became established and he was beginning to nourish well, the stump assumed a healthy, granulating appearance, when the main ligature gave way and secondary hæmorrhage took place, and before it was observed he had lost a large amount of blood. I found him at first pulseless, pale, and covered with a cold sweat; the bleeding had stopped of itself. The femoral artery was immediately tied; nourishment was carefully and judiciously administered, and the patient recovered without interruption. These may appear as isolated cases and mere coincidences, but I could give, if it were profitable, a long

list to show the innocuousness of the same loss of blood while the powers of assimilation are good.

The powers of adaptation in the human system are very great. In some of the nations of the earth, the habit of low living through a series of generations, has reduced the amount of food, actually necessary to the support of a healthy condition, to the lowest rate, and it was estimated by an English writer, that a good substantial English dinner would serve a Hindoo for ten days. In our country the power of adaptation is tested in the opposite direction, and our systems are habituated to receive and dispose of the largest possible amount of nutriment consistent with health, and our systems are often taxed beyond this point. I can remember when the foolish and pernicious custom of bleeding for plethora prevailed, and the lightness and buoyancy, and evident increase of the vital force experienced from it, induced those who had once resorted to it, to continue the practice. No one is more fully impressed with the evils of the excessive and indiscriminate use of blood-letting of former days, but I am as strongly impressed with its great advantages in many important cases. When life, or an important organ is in danger, it is the imperative duty of the physician to use every agent that will lessen it. But I will refer to this subject again in connection with other antiphlogistics. In the early stage of inflammation, when there is an exalted sensibility in the nerves of the part, congestion of the capillary circulation, tartar emetic exercises a powerful influence in arresting or lessening the exudation that is impending. Bennett defines inflammation to be exudation of the liquor sanguinis, when in truth this is the immediate termination of inflammation, and the more remote evils—suppuration, ulceration, or gangrene, are in proportion to this exudation. If we are only to recognize inflammation after the mischief is done, Mr. Bennett's treatment may be very generally applicable. Happily we do see it in its early stage, and we can diagnose it in time to counteract, in a great degree, its ill effects. Antimony, in the proper stage, has more influence in arresting or mitigating inflammatory action than any one thing besides. It diminishes the force of the circulation, diminishes the sensibility of the nerves of the part, and causes a contraction of the dilated capillaries, and all this is effected and can be continued often for some time without producing any of the evils attributed to the loss of blood. Considering that this dilatation of the capillaries is considered as the result of debility, and that the action

of antimony causes contraction, it is not without reason that Brown classed it with his stimulants. Language cannot express the action of medicine, except in a very general way. It is only in connection with pathological conditions that it can be made intelligible, and even then we must have the case before us, or we will fail in our description. It matters not whether antimony is a stimulant, a contra-stimulant, or a sedative, so we do not allow the name to cause a misapplication of it ; but keep in view the exact condition in which its action is useful. This valuable medicine has in a great measure gone into disuse, and chiefly for two reasons : first, the proportion of cases in which it was appropriate, has diminished greatly; and second, because during a long period when cholera was prevalent, it was liable, when carelessly used, to excite the stomach and bowels and produced serous discharges, and thereby produced permanent depression. This effect is not often seen now, and there is no occasion to have it at all if it is properly administered in conjunction with a proper proportion of opium. The manifest action, then, of antimony is to soften the pulse, diminish morbid sensibility, and the blood in the inflamed part, without taking from the system any of its resources, or permanently depressing the vital forces. If it is so administered as not to produce any local irritation in the stomach, it may be continued as long as it exercises a favorable control over the morbid action, and when discontinued, the effects soon disappear, and the system is left natural, and with all its resources. Under the same circumstances in which bleeding and antimony are useful, calomel in sedative doses is proper and highly valuable. By a sedative dose, I mean a dose that will produce slight nausea and diminish the force of the circulation. In addition to the immediate sedative effect, calomel acts upon the whole excretory organs, producing in some measure a revulsive effect, and at the same time by exciting the excretions assist in carrying off the effete matter that is often retained in the blood. Blood-letting is only applicable to the early stage of active inflammation. Antimony is applicable to the first stage not only of active inflammation, but in the first stage of many forms that cannot properly be called active ; and calomel is not only valuable in sedative doses in the first stage, but it is of great value in smaller doses in many cases of the second stage, or after exudation has taken place. Whether inflammation is sthenic or asthenic, in the very outset there is an increased sensibility of the part, and a flux

of blood, which is an exaggeration of the natural state, and anything is proper that will tend to counteract it, without detracting from the resources of the system, that may be required at a later stage of the disease. I am sure that antimony may be so used. I will cite one of Mr. Bennett's cases of pneumonia, not after extensive exudation has taken place, but when it is going to take place. Inflammation has not always progressed to exudation when we first see it. During the prevalence of pneumonia, we are often called to a patient who has had slight rigors, followed with some fever, and a slight hacking cough which gives him a little uneasiness in one side of his chest. He says he is not much sick, but thought it prudent to ask advice lest he might be. The close observer will find that the respiration is a little humid, and he will examine the chest and discover the fine crepitation of beginning engorgement. Under these circumstances, antimony, combined with a little opium, given so as to produce its full sedative effect, will, in a large majority of cases, prevent the exudation that would most certainly take place if the case was left to nature. I know that we do not always see our patients in this early stage. I know also that we often overlook the real condition when we do see them early, and only recognize it when it is rendered unmistakable by serious symptoms, and then, by adopting what is called active treatment, do more harm than good. But when I speak confidently of the precise effects of antimony, I mean that it shall be given precisely at the right time, and not two or three days afterwards. Given before exudation has taken place, it may prevent it, or even while the process is going on it will check it, but after the process is completed, it can do no good, and probably will do harm. All active means must be used in anticipation. We must know what is to be, and guard against it, for after the mischief is done we can do but little. To illustrate farther the effect of antimony, I will refer to its effects in neuralgia and nervous delirium, combined with morphia, in equal proportions; violent nervous pain is subdued by half the quantity of morphia which would be required if given alone, and it seems to modify the action of the opiate, and in a measure obviate its unpleasant effects. In *mania à potu*, after the system has been cleaned of the alcoholic poison, and there is left an exalted state of the nervous system, delirium, and sleeplessness, the same proportions act with safety and great certainty when it would be dangerous to force sleep with opiates alone. Those who are familiar with the

writings of Stokes, of Dublin, will remember that he advises a similar prescription in small doses in the dry tongue and nervous delirium in typhus. His prescription, I think, is a grain of antimony, a drachm of laudanum, in a six ounce mixture, to be given in tablespoonful doses. The idea of giving it according to the Italian school, to get a toleration of it, and pustulating the stomach and bowels, is one of those extremes which so often brings into disrepute a valuable medicine.

Opium is a valuable remedy in inflammation, but it requires no recommendation to bring it into use, for it is becoming the fashion of the day to smother and disguise inflammation under narcotics ; but a few remarks may be proper on the best manner of using it. After a full bleeding, while the system is yet relaxed, and the sedative influence is still felt, a full opiate quiets the system, and if it is in the early stage of inflammation, it will assist materially in arresting it. Soon after injuries, and before any reaction has taken place that calls for treatment, a proper opiate either prevents, or greatly modifies the reaction and the inflammation that may follow. After acute inflammation has run its course, and left a condition of subacute or chronic disease, opium is valuable, and it may be useful by giving sleep, in any stage, but it is more particularly useful under the circumstances mentioned. The quack and imposter take advantage of the public prejudice, which the excessive use of mercury has engendered, and set up their opposition to mineral poisons, while they administer the most deadly vegetable poisons that are far more deleterious in their effect, and far more dangerous to use than any of the mercurial preparations. Calomel and antimony are exact preparations, very uniform in their effect; the dose necessary for the required effect can be calculated with great certainty, and when administered scientifically, the end is very surely accomplished ; no local injury is produced, and the nervous forces (the presiding influence over the organic actions) are left untrammelled and undisturbed. Aconite has been termed the lancet of the homœopath, and I begin to suspect that it is doing as much mischief as any of Evans's make ever did, and more than can be attributed to tartar emetic. It is well understood, that homœopaths in general have given up the idea upon which their system is founded—the smaller the dose the greater the effect—and give their concentrated narcotics in doses that produce effect, and their patients occasionally die of what they term brain fever, an antiquated unmeaning term

which has been applied when there is delirium, and general disturbance of the nervous system. When ignorant men practised homœopathy according to the principles laid down by its founder, if one had a friend under their treatment, he might feel consoled with the idea that he would have the full chance of nature, which might not be the case under a bold practitioner who has active means; but now that ignorant men, and men half educated in the profession have taken to the use of the most potent poisons in the most concentrated form, the case is quite different. Some of these active, acrid vegetable poisons, no doubt exercise a powerful control over the heart's action, but I have yet to learn that this has a favorable effect upon the capillaries and nerve force, in other words, upon the peculiar process going on in the tissues, which we call inflammation; I may be prejudiced, but I commenced very early in life to school myself against such a fault, in observing both men and things, in order that I might get as near the truth as possible. I have never used the *veratrum*, which is so highly lauded by some, first, because my observation has led me to the opinion that those vegetable substances that control the heart's action have no favorable control over inflammatory action; and, second, because it is admitted that its action is not uniform in its effect on different individuals, and sometimes the effect is unexpectedly severe, giving rise to alarming symptoms. However, I will urge nothing against its use, and although I do not now use it, I will be free to adopt it, if its action is ever fully and accurately established, and a precise, safe, and uniform method laid down for its use. Medicines may be classified beneficially, for the purpose of description, but it will be found that the different articles of each class differ very widely in their effect. Some of the narcotics used to control the action of the heart and allay excitement, seem to produce a kind of stasis of the capillary circulation; but opium evidently acts as a stimulant upon the same tissues, as is shown by its effect upon indolent ulcers, when properly given, and at the same time by allaying morbid sensibility, it controls, in some manner, reâction and the flux of blood to the affected part. Calomel is evidently a stimulant upon the capillaries when it is absorbed and comes in contact with the tissues, although its first effect in a full dose is best expressed by the term sedative; and tartar emetic, although its first effect is in the same sense, highly sedative, has some mysterious influence upon the capillary circulation, which may, in a reasonable sense, be termed

stimulant, although it is quite different from the influence of calomel. The effect of antimony can be had at once, and the exudation prevented or controlled to a greater or less extent, but its good effects cease at this stage; but the action of mercury, in many conditions, promotes the absorption of the exudation. Let us call these medicines stimulants, for the purpose of illustration; let us call all medicines stimulants, according to an old doctrine, and as good as any other, which considers the first action of everything brought in contact with the living tissues, as irritant, or stimulant, and let us take for granted that all inflammatory action is a state of debility, but varying in peculiar character, as much as the stimulants vary in their mode of action, and thus we may make something like a rational and useful classification. I would class together all the forms of inflammation that are characterized by excitement, local and general, by all those conditions, which we know by experience, if not checked tend to a large exudation of lymph, which may end in suppuration, or at best to induration and impairment of the organ involved, and I would add all the less active forms that tend to the exudation of plastic lymph, its organization and consequent thickening, and induration of the part involved; and this class may be styled the phlogistic, if you please, since it must be christened something. Medicines must be classified or ought to be, according to the pathological conditions they are calculated by their action to relieve. Opposed to phlogistic we may as well style the remedies for the above class of inflammations, antiphlogistic, and under this head I would arrange blood-letting, tart. antimony, calomel, and many others might be added, but my object is simply to generalize. For the next class I would take the other extreme when the state of the system and of the part affected may be termed passive, from some inexplicable cause; it may be some unwholesome atmospheric influence, changing the healthy condition of the fluids or the solids, in which the exudation deviates farther from the healthy condition, and instead of organizing and being carried off by absorption in the course of the nutritive process, is liable to break down and disintegrate the tissues, or degenerate into imperfect pus. This class may be termed the antiphlogistic class, and the class of medicines or agents best calculated to oppose the destructive process, might properly be called phlogistics. Under this class may be ranked carb. ammonia, beef tea, opium, alcoholic stimulants, and many others of similar character. An intermediate class might be

made, including the conditions that would not be benefited by either of the above class of medicines, but will do better if the subject is placed under the most favorable circumstances, and his diet and drinks regulated properly, in which the practitioner has not much to do but to watch and to render assistance if Nature, in her true course, should flag or overdo. A distinct class might be formed of those morbid conditions which must be ranked as inflammations, caused mainly by blood-poison, by reason of imperfect excretion and depuration of the blood; or it may be from imperfect digestion and assimilation, in which much may be done by the use of medicine, either to neutralize the offending matter, or by exciting the excretory functions promote its discharge from the system, or by purifying its source, the digestion.

I disclaim all intention of setting forth a doctrine or a classification. I am merely aiming to take inflammation to pieces, so that it may be seen to better advantage, and its composite nature better understood. As a whole, its appearance depends upon circumstances. One sees it under one condition, and pronounces it black; another sees it under quite different circumstances, and pronounces it white, and by a little change it appears brown, and by close observation all the intermediate shades between black and white may be seen. So long as we persist in considering it a unit, we will never agree as to its color or character; but, if we analyze or dissect it, and classify it, as we have the various tissues of the animal system, we may be able to see it nearly alike, at least we will not differ about the two extreme conditions, and if we can get to a general agreement on these two extreme points, we may be so far able to agree on all the intermediate points as to entitle us to call medicine a science, and not till then. A knowledge of the nature and treatment of inflammation, and of the precise action of therapeutic agents in all its various forms, is to the science of medicine what a knowledge of the four fundamental rules in arithmetic is to mathematics, which enables us to work out all mathematical problems. With this knowledge we can apply, with considerable accuracy, the appropriate therapeutic measures that may be required in all the varied morbid conditions we may be called upon to treat. In the present state of medical science, all those who do not follow a special and exclusive doctrine of their own, must, necessarily, be more or less eclectics, and hence our lack of uniformity in our treatment, which costs us much of the confidence and respect



of the public, which is really due to our profession for its character, devotion, and real usefulness, however much it may fall short of what it ought to be. Treatment that may appear quite opposite to the ignorant, may have the same general tendency, differing only in the degree of good they produce. Uniformity cannot be expected in any matter governed by human judgment; but the importance of the nice and exact application of well known therapeutic agents to well known and undisputed pathological conditions, is strongly shown by the fact, that one practitioner by nice tact, in the application of a remedy, will obtain satisfactory results, where another, of equal tact in the use of something else, will fail. The skill of the practitioner does not depend upon the multiplicity of medicines he uses, but upon the exact application of a few. I doubt whether it is within human power to apply accurately a multiplicity of medicines, any more than it is for the musician to execute perfectly on many musical instruments.

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ART. II.—*Typhoid Fever and its Treatment, with reference to an Epidemic which prevailed in Elyton and vicinity, during the spring of 1858*: By JOSEPH R. SMITH, M. D., of Elyton, Alabama.

FROM the great diversity of opinions which are now held by the different members of the profession, not only in regard to the true pathology but also the treatment of typhoid fever, we cannot fail to feel a deep interest in the history of every epidemic of this now too common and fatal disease of the South and South-west. And in giving a history of its symptoms and treatment, a paramount consideration is to divest ourselves of all prejudices in favor of or against any preconceived opinions we may have formed in relation to either, but give every system a fair, candid, and dispassionate hearing; never condemning any plan of treatment we have not tried or seen tried, merely from the fact that it might seem like endorsing the treatment of a rival brother in the profession. Should we suffer such motives to influence us in our treatment, we cannot but fail to be

unworthy of our high calling; for we should ever remember that, "Whatever the motives of the individual, all the *labors* of the *physician* are labors of love sanctified by the holiest of examples, and full of gracious dignity." And he who would condemn any system of practice *untried*, merely for the purpose of pecuniary gain, by pandering to the prejudices of a community wholly uninformed in relation to medical science, is entirely unfit for association with the members of a profession as high, noble and honorable as that of medicine. And again, of the physician who will compromise a prescription, the correctness of which he is *thoroughly convinced*, with a non-professional individual, however well informed that individual may be on other subjects, is devoid of the elements of firmness and decision of character necessary to success and ultimate renown.

When any epidemic of an ordinarily fatal character falls upon a community, much more such an one as typhoid fever, fear and consternation seize the mind of the community; hasty, vague and indefinite conclusions are arrived at, both in regard to the remote and proximate cause of the disease; ill-digested and hasty plans of treatment are adopted; consequently, quite a large proportional number of those first attacked of any epidemic die.

That such was the case with the fever that prevailed here from the middle of March to the first of August, 1858, I am not prepared to say; at least it was not the case in my practice, having gone through two other epidemics less extensive, and experienced the effects of the disease in my own person. My plan of treatment was matured from experience, not in a large number of cases it is true, but sufficient to satisfy me of its correctness when sufficiently early called into requisition.

To go into a lengthy detail of all the external manifestations and symptoms, and to prove that this epidemic was typhoid fever, would be a labor of supererogation, and extend this article to a length far beyond the limits we allotted ourself in the outset, without adding an iota of interest of a practical character; it is sufficient to say that one and all, professional and non-professional, clerical and laity, those interested and those not, the sophist and philosopher, all united in pronouncing it *pure typhoid fever*; there seemed to be no dissentient voice on this head. We wish we could say as much for its treatment. Here the physician had to contend against the opinion of Mr. this one or Mrs. that one who had got a letter from an uncle, brother, or

cousin in an adjoining State or County, and that Dr. — was treating successfully all his cases with blisters, or calomel, or Dr. that, another was curing all the cases that came before him with Dover's powders, blue mass, and so on.

That one and the same plan of treatment for every and all cases of typhoid fever is the merest empiricism, no one is more thoroughly convinced than ourselves; and that age, sex, temperament, and stage of the disease, with various other modifying causes, should govern us in the application of our remedies we are equally ready to admit. And again, on the other hand, that the do-nothing practice of giving merely colored drops, with an occasional inert powder without having in view the fulfilment of some pathological indication, merely for the purpose of keeping in the confidence of the patient or retaining a situation in an influential family, is virtual acknowledgement on the part of the physician of his total disqualification to practise the honorable profession to which he aspires; and should consign the author of such low chicanery to that depth of ignominy that such duplicity merits, and will ever prevent him from taking that high and honorable stand among the members of the profession that every one should be emulous of attaining.

Notwithstanding, no one has probably met with the embarrassing circumstance of combatting these outside and interested influences more than ourselves. Under such circumstances we should meet their own plans of treatment and objections to ours with good logical reasons, and never ridicule their opinions however absurd they may seem; then, should we fail to secure their confidence, we should yield the field to—possibly a quack, and make our retreat, with the proud consciousness of having, to the best of our ability, upheld the dignity and usefulness of our profession.

Notwithstanding the humble source from whence this system of the treatment of typhoid fever emanates, we feel sure that, in the progress of improvement, and the advancement of medical research, it will prove to be true, because it is the most successful. And we do hope the profession generally will be liberal enough to give it a fair and impartial trial before condemning it; justice requires this of them and candor will compel them to admit its truth. It is to the practical thinking portion of the profession we appeal.

Between the 15th of March and the 20th of June, 1858, we treated fifty cases of pure, non-complicated typhoid fever, of an epidemic

character, no other disease prevailing to any extent at the same time in town or neighborhood. And here it is well enough to state, as it is well known, that we treated a great many more after this latter date, and the reason they are not taken into this statistical list is, because after the 20th of June, the disease assumed more the character of remittent bilious fever, and yielded to the quinine treatment almost without an exception, where sufficiently early called into requisition.

Of the fifty cases occurring before the 20th of June, thirty-six were seen and prescribed for from the second to the eighth day of their sickness; fourteen were not seen until after the eighth, and some as late as the fifteenth day of sickness. Of the thirty-six prescribed for before the eighth day, eighteen were cut short by large (fifteen to thirty grains) doses of quinine; sixteen cases ran from fourteen to fifteen days, and recovered, and two died—one from a relapse, and one after a fair trial of the quinine. Of the fourteen cases seen after the fourteenth and fifteenth day, twelve recovered, and two died. Of the twelve recoveries we succeeded in keeping the bowels closed from four to ten days. Of the two fatal cases, the bowels continued to be relaxed, notwithstanding the persevering use of all the remedies in such cases, until death closed the scene.

We lay no claim to the originality of treating typhoid fever in its earliest stages with large doses of quinine; although in an article that appeared in the *Western Lancet*, for February, 1846, in describing an epidemic fever that prevailed in the fall of 1845, in this town, we made these remarks, in speaking of the use of quinine in the treatment of this fever: "Quinine I also found to effect nothing unless perseveringly used in large doses, and that too before the seventh day." At that time we do not recollect of ever having read a word in relation to the cutting short of typhoid fever, by large doses of quinine, except from T. D. Mitchell, then the distinguished Professor of Materia Medica and Therapeutics, in Transylvania University, who says that "by the use of large doses of quinine in the early stages of all our fall miasmatic fevers, we would fail to see any cases of typhoid fever, for it was his opinion that the majority of cases of typhoid fever was caused from neglected cases of remittent or intermittent, which if properly treated by large doses of quinine, would yield in a few days." Of the correctness of this opinion, a large number of the profession, South, who have given the plan a fair trial, are now thoroughly convinced.

We by no means wish to be understood as discarding all preparatory treatment for the quinine, such as emetics, cathartics, or even in some cases, the lancet, still we cannot too strongly urge on the physician the propriety of, even where the intermission is short, almost imperceptible, pressing the quinine in large doses, without ever waiting for emetics or cathartics, as such delay might prove hazardous to the patient. Too often have we seen cases of pure, undoubted typhoid fever cut short to even doubt the propriety of such a course.

We never give but two doses (fifteen to thirty grains each) at one interval of three hours between doses. This we prefer giving in a mixture of Bateman's pectoral drops (made double the strength of the dispensatory), and spirits nit. dulc., a drachm of each. When thus given, it seems to act finely on the skin, at the same time it soothes and quiets the patient. This course, if unhesitatingly persevered in, will, in four out of five, prevent a long spell of typhoid fever.

This, like everything human, is fallible in a few cases, and it is in these few cases we now propose to the profession our plan of treatment. In a former article\* on the treatment of typhoid fever, we, in our haste, neglected to be sufficiently specific.

We wish our readers to understand we are now treating typhoid fever in its latter stages, or wherein quinine is out of the question and offers no hope for relief. Picture to yourself a case of hot, dry skin; flushed cheeks, parched, fiery, red tongue; subsultus tendinum; a hard, rigid state of the abdomen, with occasional borborygmus, the diarrhœa six to eight times a day, and probably twice as often during the night, the discharges being thin, watery, and muddy, with occasional streaks of blood; low, muttering delirium; picking at the bed clothes, or at imaginary objects in the air; thirst intolerable; pulse ranging, owing to the time of day, from 120 to 140 beats per minute.

Where is the physician at all conversant with the treatment or symptoms of typhoid fever, but will say such a case will prove fatal? at least his prognosis would be unfavorable, and so we say, unless the bowels be checked. But if the physician can succeed in checking the bowels (for the diarrhœa, which has been probably gradually increased unheeded by the nurse and physician for the last three or four days, which is the sole cause of all the other unfavorable symp-

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\* Our first article appeared in the *N. O. Med. and Surg. Jour.*, for July, 1856.

toms), he will find a general amelioration of all the symptoms, and his patient will be snatched, as it were, from a premature grave. He will be able to note the improvement from a slight moisture of the tongue and skin; his naps of sleep will be longer, and even awake will seem more rational; the tongue becomes more pale; the intolerable thirst and burning heat are less; abdomen becomes more soft and less painful on pressure; the borborygmus almost entirely ceases; and the longer we can maintain this quiescent state of the bowels the greater the improvement up to perfect convalescence; after this nature generally acts sufficiently early for the well-being of our patient, without the assistance of cathartics.

We have again and again kept the bowels closed as long as ten days, and the patient improved in all his symptoms during the time; the fact is, we never have lost a patient whose bowels were closed and so maintained *in pure non-complicated typhoid fever*, and we wish our readers to understand that such *alone* are the cases in which we recommend this practice.

And here we wish to name a circumstance which may deter, and no doubt has deterred the timid and wavering physician from putting into execution this mode of practice, even though his better informed judgment had already dictated it to him. During this diarrhœa, while the patient is rational, he or she may say, and no doubt will, if you speak of checking the bowels, that he feels relieved at every operation and thinks it proper and right for the purging to go on; now all this relief is deceptive and only momentary; and the morbid lesion in the mucous follicles of the bowels, of which the patient, and possibly his physician, are entirely ignorant, is gradually, by every operation of the bowels, augmenting, and ultimately will, if not checked, result in hæmorrhage, which is the crisis of the disease, and, in nine cases out of ten, a fatal one too. That some escape this hæmorrhagic crisis, we are willing to admit, but that they have more cause to congratulate themselves on a vigorous constitution and the recuperative powers of nature than on the skill of their physicians, we *equally as well know*. This hæmorrhagic tendency of the bowels is almost invariably brought about by the free use of drastic cathartics in the early stages of the disease, prescribed for the purpose of putting in operation the hobby—"a torpid liver."

When we reflect upon the pathology of typhoid fever fully developed, we can but know the well-being and safety of our patient mainly de-

pend on a quiet state of the bowels; for certainly there is no physician who has made this disease his study at the bedside, but must have observed that the oftener the bowels act, the higher the fever, the drier the skin; in fact, all the symptoms will grow worse; and why? because we are, by suffering the bowels to run, feeding the disease, by increasing the irritation, and consequently inflammation in the mucous follicles of the bowels. The only rational and sure way to prevent this state of things from running on and proving fatal to our patient is to check the bowels, thus removing the cause.

In accomplishing this very desirable object, our sheet-anchor is opium and tannic acid, and these we give for this purpose, regardless of quantity; if we give the articles in their solid state, we prefer making them into pill, one grain of opium, with four of tannin, given at every other operation of the bowels. A better form is to saturate the tinct. opii. with tannic acid, giving from forty to sixty drops at a dose, repeated in proportion to the frequency of the discharge. If the pulse is too frequent, we combine with the laudanum and tannin, tinct. verat. virid. in such doses as will bring down the pulse to near its natural standard. With this prescription we are highly pleased; it rarely fails to accomplish the object, that is, checking the bowels and bringing down the pulse. In some instances, the stomach may reject the above, even without the verat. virid.; if so, we have found it answer equally as well to give it *per anum*, with starch water, directing the patient to retain the mixture until after the first feeling of irritation passes off. When sufficiently long retained, it seems to soothe that irritation so peculiar to typhoid purging. Should there be much abdominal tenderness, a warm bran poultice, slightly moistened with spirits of turpentine, and spread over the abdomen, will effect much in allaying irritation. For a common drink, nothing answers so well as a weak solution of chloride of soda; in fact, we consider this as an important adjuvant in the treatment of typhoid fever. Soda powders, in solution, we suffer our patients to drink at pleasure, with the happiest results; they allay thirst, relieve nausea, and, by the addition of one-sixth of a grain of morphia produce quietude and promote diaphoresis; the morphia may be added to the glass containing the blue powder, with a little sugar, and when poured together and effervescing the draught will be tasteless.

Now we do not wish to be misunderstood in relation to the astringent treatment of typhoid fever. We, by no means, advocate the

locking up the bowels from the beginning, where there exists no necessity for such a procedure; but, on the contrary, we advise the opening and cleansing of the whole alimentary canal in the beginning of the disease, both by emesis and catharsis; and after this be very careful to avoid hypercatharsis, by withholding drastic purgatives, and give Nature her course as regards the bowels; and if by so doing your patient has one operation on the bowels once every three or four days, the physician may be content; for such a patient will invariably recover unless the bowels run. Nor do we ever attempt to lock up the bowels if our patient has one operation of a *natural consistent* character; but so soon as they assume a thin, muddy appearance, with occasional borborygmus preceding the operation, the physician should anticipate danger; for it will surely come, if such a state of the bowels be suffered to continue twelve hours; now is the time to give the tannin and opium with the addition of oil of turpentine, in such doses as will immediately allay the irritation of the bowels, or the life of his patient will be in great danger; and as we have before said, we do not hesitate under such symptoms to keep the bowels closed as long as eight or even twelve days, where no untoward symptoms supervene—a thing of the rarest occurrence. Upon these subjects I expect to make additional observations hereafter.

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ART. III.—*Treatment of a Tarantulated Patient.*

DR. B. DOWLER—*Dear Sir*: If the following case, which occurred some years since in my practice, be deemed of sufficient interest, I hope you will find a place for it in your Journal.

I was called, September 15, 1844, at two, A. M., to visit Dandridge Bibbe, distant two miles from the town of Washington, on the Brazos river, in this State, at which place I then resided. The patient, ætat. 20, was a young man of sanguino-bilious temperament, whose general health was pretty good. His habits were active, his vocation being that of an ox-teamster,



He stated that about midnight he was bitten by a tarantula on his right hand—that the first unpleasant sensation was a burning pain at the wound, and that this painful sensation, which became more and more acute, seemed to course up his arm until it reached his spine, when, as if by an electric shock, it dashed over his entire system.

I saw him about two hours after he was bitten, and found the wound to consist of a few small punctures on his right hand. There was no swelling of the wounded part, or other evidence of inflammatory action. But the distress of the poor man exceeded my powers of description. He was shivering and cold, and bedewed with a cold clammy perspiration, his countenance sunken and expressive of the most exquisite agony which one could suppose the human organization capable of enduring, his voice sepulchral and with looks and language imploring relief.

Mustard was at once used externally, with great freedom, and warm or rather hot and dry applications also employed. I gave largely of sulphate of morphia, ether, carbonate and spirits of ammonia, also camphor. I used the above remedies as freely as I dared; but what was the effect? The man sank progressively under this plan of treatment. His agony became greater, with less ability to complain. The recession of the blood from the superficies of the body was rapidly and progressively going on. Then, in such a state of affairs what was I to do? Would not my patient die, and that speedily too, if not relieved? Could I send two or three miles for a consulting physician? and if I did, would not the patient be beyond hope before he could be obtained? Discouraged by my exertions hitherto so unavailing, should I attempt nothing more for his relief, and merely look on while the man died? or should I continue my efforts to save him? The latter of course. Therefore I had to act, and differently from my former efforts, and that too promptly or uselessly. The pulse of the patient was a mere thread, and quite slow. I felt satisfied that his pulse was a depressed one, and that if a proper relationship between the blood, which in a normal state freely circulates and is the bearer of vitalizing elements to the economy and the powers of vital resistance, were reestablished, that the system would respond to the remedial means heretofore employed, and might yet triumph over the deadly lethean agent which seemed to weigh down and oppress as if irresistibly its powers. I made a large orifice in a vein of one of the arms of the patient, but for five minutes I

was compelled to literally *knead* the blood from the vein. After the lapse of that time, the blood flowed sluggishly, being dark and thick. But in a short time the flow increased and the stream became large and free, the blood-fluid and the pulse more full and frequent.

The poor fellow looked up with an expression of returning hope, and remarked, "Doctor, I feel as though the pains were flowing out with the blood." In ten or fifteen minutes from the time I opened the vein, he was easy. I stopped the bleeding, and the patient dropped asleep in five minutes thereafter. Thus in about three hours I had the gratification of seeing the poor young man redeemed from apparently inevitable death. At half-past five, A. M., I left him sleeping quietly. At twelve, M., I saw him again just as he awakened. He was without pain, and comfortable, though slightly feverish. I ordered him a saline aperient and left him. He was entirely recovered by the following day.

I shall make no attempt at explanation of the *rationale* of the blood-letting in this case. The fact is such as I have related. I leave comments and deductions to others.

T. J. HEARD, M. D.

GALVESTON, TEXAS, Aug. 17, 1859.

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ART. IV.—*Remarks on the Natural History of the Tarantulidæ and other Arachnidans, and on the alleged Poisonous Effects of their Bites*: By BENNET DOWLER, M. D.

He who uses the word *tarantula*, without having any imagination or idea of what it stands for, pronounces a good word; but so long means nothing at all by it. He that in a new discovered country shall see several sorts of animals and vegetables, unknown to him before, may have as true ideas of them, as of a horse or a stag; but can speak of them only by a description, till he shall either take the names the natives call them by, or give them names himself. He that gives the name *horse* to that *idea* which common usage calls *mule*, talks improperly and will not be understood. He that thinks the name *centaur* stands for some real being, imposes on himself, and mistakes words for things.—LOCKE. Human Understanding. (B. iii, c. 10, § 32.)

THE dangerous effects from the bite of the tarantula, reported in a preceding article, naturally suggest the propriety of further investigation into the natural history of this famous arachnidan. As there is no Natural History Journal in the South, and as this subject is not foreign to the legitimate scope of medical journalism, especially in so

far as it involves the question of animal poisoning, it is hoped that the reader, for reasons which will more fully appear in the sequel, will be in no haste to condemn these few remarks as being necessarily misplaced or ill-timed, notwithstanding their incompleteness, as their chief aim is not to communicate but to elicit scientific information from others in the South, who are more competent and have better opportunities to make observations in the premises.

The natural history of the arachnidans, especially such as are alleged to be poisonous, appears to be confused, contradictory, or fabulous to a greater extent than in almost any other branch of natural science. The fabled toxical powers of the tarantula find no parallel in the history of the dark ages. The bites of these animals, as learned historians relate, caused and perpetuated the great epidemic called tarantism, which raged in Italy and Germany for several centuries subsequent to 1350—a disease which either soon killed the patient, or took a chronic form, causing him to dance violently and repeatedly until exhausted or cured by music.

Professor Hecker, of Berlin, in his history of epidemic tarantism, often cites Baglivi's statements, one of which implies that music (a pleasant and efficacious remedy, truly) had been much neglected; for, he expressly declares, that "many bitten by the tarantula, perished miserably because the *tarantella* [music], which would have afforded them deliverance, was not played to them." *Diss. de anatome, morsu et effectibus Tarantulæ* (written in 1595).

Among philosophers, doctors and historians contemporary with the dancing millions of southern Europe who affirmed that they had been bitten by the tarantula, there were, perhaps, always some dissenters. Hecker says:

"The learned Nicholas Perotti gives the earliest account of this strange disorder [tarantismus], and that nobody had the least doubt that it was caused by the bite of the *tarantula* (*Lycosa Tarantula*), a ground spider." (P. 34.)

Hecker quotes authors, who, in the sixteenth century began to distrust the honesty of some of the *tarantali* [tarantulated patients], but "they were able to distinguish impostors from those who were really bitten." In 1770, an account appeared in the Philosophical Transactions, charging that tarantism was "a common imposture;" in Berlin, 1772, statements were published on the tarantula "which suffice entirely to set aside the prejudice respecting the venom of its bite." On

the other hand, Mead, as late as 1747, "contends for the reality of tarantism." Hecker gives a long list of writers, chiefly upon the affirmative, in the years 1560, 1600, 1612, 1614, 1650, 1658, 1666, 1668, 1672, 1685, 1687, 1706, 1738, etc.

MM. Bayle and Gibert, in their *Dictionaire de Médecine* (1835), say (what has been long and still is generally asserted by European writers), that "the wounds inflicted by the tarantula are not dangerous." Dr. Livingston, in his recent travels and researches in Africa, mentions that his men were bitten by spiders, "but that no effect but pain followed." The question of innocuity will, for the present, be postponed in order to offer some remarks on the natural history of this arachnidan.

The very name or orthography of the tarantula has been and continues to be unsettled. Hecker designates this spider as the *lycosa tarantula*, but immediately after says, "the word tarantula is apparently the same as terantola." Clavigero, in his history of Mexico, calls it tarantola. Webster says, "terentula is the most correct orthography." Dunglison gives tarantula, tarentula, tarentule (F.). These and other nomenclatural discrepancies which might be mentioned derive importance from the fact, that they have led to mistakes concerning the identity of this arachnidan.

Perhaps half of the writers who have described the tarantula, say it stings!—the other, that it bites. As it is stingless, while another and more dangerous arachnidan, the scorpion, has a sting, the biting and the stinging as well as the animals themselves, seem often to be mixed up confusedly. The scorpionidæ are described as having "a sharp curved sting, which analogous to the poison fang of the serpent, instills a venom into the wound it makes. They run very actively, arching the tail over the back, and in attacking their prey seize it with their claws, and instantly pierce it with the sting. They spare not even their own species. Maupertuis put a hundred scorpions together, and a murderous conflict immediately ensued; almost all were massacred in a few days and devoured by the survivors.

"The sting of the common scorpion of Southern Europe and Barbary (*Bathus occitanus*, Leach), or the six-eyed European species (*scorpio Europæus*), is not fatal except to small animals and insects. But in the hotter regions there is some degree of danger. In South America, the sting of some kinds has been found to occasion fever, numbness of the limbs, and dimness of sight, lasting for two or three

days, and death is said to result sometimes from the wounds of the black scorpion of Ceylon. Mr. Kirby states that the only means of saving the lives of our soldiers who were stung by these creatures, in Egypt, was amputation." (*Museum Nat. Hist.*, ii fol. 331; *ib.* 331.)

Clavigero, in the first volume of his *History of Mexico*, says: "Scorpions are common throughout the whole kingdom, but in the cold and temperate countries they are not numerous, nor very hurtful. They abound in the hot parts, or where the air is very dry, although the heat is but moderate; and their poison is so active as to kill children, and to occasion terrible pain to adults. It has been remarked that the poison of the small yellowish scorpion is more powerful than that of the large brown one, and that their sting is the most dangerous during those hours when the sun gives most heat."

Spiders do not sting like the scorpion, but bite with their antennæ-pincers or hooked fangs with which they can seize and pierce, and, as some affirm, poison their prey. It is said that all arachnidans "are venomous, at least to insects. Each hook or tooth is perforated by a tube leading to a poison-sac, whence exudes a deadly fluid, fatal to flies and the ordinary prey upon which the spiders feed; this fluid is thrown into the wound in the same manner as in the case of venomous snakes. \* \* \* Many of the hunting spiders, as the huge mygale of South America; the thomisus venatorious of the West Indies; the cteniza or mygale cæmentaria, and the lycosa tarantula, construct singular nests or dwellings in the ground, to which, as the lion to his lair, they return with their victims." (*Mus. Anim. Nat.*, ii, 330-1.)

During the last summer, I observed a small house-spider upon a smooth wooden, perpendicular pillar, between which and the animal's body, was held a house-fly. I supposed at the time that the fly was dead, as it was entirely motionless, and that the spider was sucking the juices or eating out the intestines of its captive. My observations lasted sometime, perhaps two or three minutes, neither animal, in the meantime, having moved. I then made a motion as if to strike the spider, whereupon it moved away rapidly, and at the same time, the fly, to my surprise, flew away as if uninjured; whence I concluded that probably the fly was neither poisoned nor mutilated, but had been held by the superior compressing force of the spider's eight legs. Is it probable that venomous animals poison their prey or food as they do their enemies?

Cuvier says: "It is certain that the bite of the great American spiders, called crab spiders, belonging to the genus *mygale*, kill small vertebrated animals, and may even in man cause a violent fever; even the wound of some of our southern [French] species has proved fatal. Without believing all the fables of Baglivi and others as to the powers of the *tarantula*, we may dread the bite of the larger species of spiders, especially those of warm climates. South America and the Antilles also, furnish other species which are known to the French colonists under the name of spider-crabs, and of which the bite is reputed dangerous. \* \* \* The largest species, in a state of repose occupy a circular space of six or seven inches in diameter." (*An. King.*)

Clavigero, the celebrated historian of Mexico (born in Vera Cruz in 1720), says: "The name of tarantola [tarantula] is given very improperly in that country [Mexico], to a very large spider, the back and legs of which are covered with a fine, soft, blackish down like that upon young chickens. This spider is peculiar to the hot countries, and is found in houses as well as in the fields. It is supposed to be poisonous, and it is generally believed that if a horse tramples upon one he very soon loses his hoof; but I have never known a single instance of this happening, although I was for five years in a very hot country where those spiders were in great numbers. The casampulga is a small spider of the size of a chick-pea, with short legs and a red belly. This spider is venomous, and common in the diocese of Chiapa, and elsewhere." (I. 95-6.)

Tarantula, as a scientific term, is sometimes used to designate a genus, sometimes a species. Professor Owen, who edited the Zoological Department of Brande's Encyclopædia, applies the name generically: "Tarantula: The name of a Fabrician genus of Pedipalpus Pulmonary Arachnidans infesting the torrid regions of Asia and America. The group is now divided into the genera *Phrynus* and *Thelyphonus*. The term is also applied to a genus of spiders found in some parts of Sicily whose bite produces a train of symptoms long believed to be curable only by music;" while the French Academy's dictionary (sixth edition) defines it as "a species of large spider found principally in the neighborhood of *Terante*." Cuvier, under the term *tarantule* (tarantula) passing by Italy, finds the large and dangerous spiders far off: "Those arachnidans," says he, "only inhabit the hot countries of Asia and America."

In the National Cyclopædia (London, 1850), a doubt is expressed whether the bite of the tarantula be at all venomous. M. De Wailly, in his French dictionary, says "*tarentule, tarantule*, is a large, non-venomous spider, common in Barbary and Italy.

"The bite of the tarantula as it is called, is not properly a bite, but a wound inflicted in a very peculiar manner. The creature pierces the skin with its forceps, and at the same instant ejects from its mouth a poison into the wound. The bite occasions a pain, which at first seems much like that felt from the stinging of a bee or an ant; but in a few hours the patient feels a numbness, and the part affected becomes marked with a little livid circle, which soon after rises into a very painful tumor; a little after this he falls into a profound sadness, breathes with much difficulty, his pulse grows feeble, and his senses fail; at length he loses all sense and motion, and dies, unless relieved," etc. (Rees's Cyc. xxxv.) This article compiled at great length from European authorities, enumerates other symptoms of tarantismus worthy of witchcraft and demonology, and then proceeds to develop some of the profound theories explanatory of the *modus operandi* of the poison, admitting, at the same time, that some writers considered the bite of the tarantula to be free from any poisonous properties.

The following case of convulsions from the bite of a spider, published nearly the third of a century ago, in the first volume of the *Transylvania Journal of Medicine*, reported by Dr. J. C. Cross, of Courtland, Alabama, possesses a singular in itself, while it coincides in some degree with the fabled tarantismus already mentioned, which was characterized by convulsions as its fundamental symptom.

"*Convulsions from the bite of a spider.*—I saw in company with my friend and partner Dr. Gray, Jacob B., Esq., æt. 35, of thin, infirm habit of body, sallow complexion, sanguineo-bilious temperament, convalescing from chronic liver disease, who had been bitten on the glans penis, near the verge of the meatus urinarius, while at the temple of Cloacina, by a small black spider, having a very small red spot on its back. The pain produced by the bite did not exceed in intensity a smart prick of a pin, and even this soon subsided, and did not return again. The part did not swell, redden, or exhibit any other mark of having been injured, except a small black spot resembling a musquito bite. An hour elapsed before any unpleasant sensation had been experienced, and then the first premonitory intelligence he received of the approach of his disease, was by a violent,

sudden, and very painful contraction of the cremaster muscle, which soon extended successively to the abdominal, particularly the recti, to the glutei, intercostal, and diaphragmatic muscles. Those of the extremities, both superior and inferior, in about half an hour after the contraction of the cremaster became convulsed. The only parts of the body that enjoyed an exemption from convulsive action, were the muscles of the neck and face, and even those at a more advanced period, though not so violently, began to contract irregularly and quickly, which induced us to apprehend the occurrence of tetanus. Respiration was excessively embarrassed, being short, quick, and asthmatic; the skin was cold, and from the whole surface poured an abundant perspiration, which stood in large drops on different parts of the body, and on the forehead particularly; the pulse was small, thready, and vermicular; the mind was prodigiously alarmed, though it retained its usual clearness and penetration.

“Emesis was produced but without benefit; a circumstance, however, of a character a little singular should be mentioned. Previous to the commencement of vomiting, the stomach was the seat of a very unpleasant sensation of cold, though warm water had been exhibited in considerable quantity to promote emesis; the antimony produced no nausea, and while the patient was under its operation, the same cold sensation extended from the cardiac orifice of the stomach to the pharynx.

“In about an hour after the commencement of the convulsions, and when they appeared to be steadily increasing in intensity, the warm bath was employed, in which he was kept about forty minutes, which succeeded in producing a pretty general tranquillity throughout the muscular system. In less, however, than half an hour after he emerged from the bath, they returned with redoubled violence, and threatened the most alarming consequences. The muscles of the cheeks and neck appeared now for the first time slightly affected. Thinking that if we could benumb the sensibility of the part originally injured, and from which this strange train of muscular irritability seemed to radiate, we might succeed in subduing the convulsions; upon the suggestion of Giannini we introduced a large pill of opium into the urethra, which soon moderated the symptoms and contributed considerably to encourage the belief that it would succeed in subduing the convulsions altogether. The hope, however, was fallacious and unfounded; the narcotic soon lost its influence and the convulsions resumed all their former intensity. Our chief, indeed only



reliance, was now in large doses of laudanum, which we determined to push to any extent that the object to be attained might seem to require. Half a drachm of liquid laudanum was immediately exhibited, but without obvious benefit; in thirty minutes the same quantity was repeated; still but inconsiderable moderation of the symptoms followed; in thirty minutes same dose repeated; the impression made was now very manifest, the convulsions being considerably reduced in violence; in thirty minutes the same dose was repeated, the disease moderating so rapidly that its repetition was unnecessary, and in an hour there was no vestige of convulsions. At twelve, at night, about seven hours after the infliction of the injury, two drops of croton oil were exhibited to counteract the constipating tendency of the tincture.

“ On the ensuing morning he was free from spasm, but experienced great soreness of the muscular system; skin comfortably warm and covered with a moderate and pleasant perspiration: pulse feeble, but fuller and less frequent; bowels costive, no operation having been produced. The same dose of croton oil was repeated. At one, P. M., pulse full, frequent, with some hardness, but no intestinal evacuation. Bled to sixteen ounces, and three drops of croton oil ordered. Seven, P. M., no alvine discharge; rectum examined and found choked with hardened impacted feces; an enema was exhibited which dislodged the obstruction, and the bowels became sufficiently active. In less than twenty-four hours he was entirely recovered.”

Professor Brainard, a few years ago, performed numerous experiments on animals, with the venom of the rattlesnake, from which he concludes that this poison produces “ a spasmodic action of the whole muscular system, but more particularly of the larynx, and a certain change of the blood corpuscles.”

The following recent account, apparently by a non-professional person, is sufficiently difficult of relief, but still more creditable than many accounts formerly given by the faculty upon this subject.

Mr. Francis C. Woodworth, in his work on the insect world (N. Y., 1856), gives an account of the tarantula, copied from a letter addressed to himself by a gentleman of Texas, whose history of this insect is at least extraordinary: “ It [the tarantula] is an enormous spider, concentrating in itself all the venom, and spite, and ugliness of all other spiders living. Its body is some two inches long, black, and bloated, has eight long, strong legs, a red mouth, and an abundance of stiff, brown hair all over itself. When standing, it covers

an area of a saucer. Attack it with a stick, and it rears on its hind legs, gnashes at the stick, and fights like a fiend. It even jumps forward a foot or two, in its rage, and if it bites a vein, the bite is death. The first I saw was at the house of a friend. I spied it crawling slowly over the wall. Excessively prudent in regard to my fingers, I at last, however, had it safely imprisoned in a glass jar, unhurt. There was a flaw in the glass, as well as a hole through the cork by which it could breathe, but in ten minutes it was dead from rage. Soon after I killed three upon my place. A month after I killed a whole nest of them. Had he (my boy) seen one of them, he would, of course, have picked it up as a remarkably promising toy, and I would have been childless. I was sitting one day upon a log, in the woods, when I saw one slowly crawl out to enjoy the evening air and the sun-set scenery. He was the largest, most bloated one I ever saw. As I was about to kill him, I was struck with the conduct of a chance wasp. It, too, had seen the tarantula, and was flying slowly around it. The tarantula recognized it as a foe, and, throwing itself upon its hind legs, breathed defiance. For some time the wasp flew around it, and then, like a flash, flew right against it, and stung it under its bloated belly. The tarantula gnashed its red and venomous jaws, and threw its long hairy legs about in impotent rage, while the wasp flew round and round it, watching for another opportunity. Again and again did it dash its sting into the reptile and escape. After the sixth stab, the tarantula actually fell over on its back dead; and the wasp, after making itself sure of the fact, and inflicting a last sting to make matters sure, flew off, happy in having done a duty assigned it in creation. In an hour more a colony of ants had carried it down piecemeal, and deposited it in their catacombs." (p. 91, *et seq.*)

This writer, relates, upon hearsay evidence, several wonderful exploits of the tarantulas of Texas, which seem altogether fabulous. This account of the size of the tarantula which "covers the area of a saucer," is within the marvellous limits which Cuvier assigned to some of the arachnidans, whose bodies "in repose, occupy a circular space of six or seven inches in diameter, and can seize humming birds, pigeons, etc.! They dart forth, use all their efforts to wound their captives with their murderous darts, and discharge into the wounds an active poison."

In the late finely illustrated but mutilated edition of Cuvier's Animal Kingdom, edited and annotated by Professor Carpenter and Mr.

Westwood (London, 1851), among other blunders is the following which purports to be a translation of Cuvier: "*L. tarentula* (*Aranea tarentula*, Linn.) is about a foot long," etc. On referring to the original, I find the text is as follows: "*La Lycose tarentule* (*Aranea tarentula*, Linn. Fab.). *Longue d'environ un pouce*," that is, eleven inches short of one foot. Cuvier's description of arachnidans was provisional and purported to be confined to the principal ones, and bears internal evidence of being a compilation rather than the result of original investigation for which he was in general so justly distinguished. Under one order he mentions the tarantula as poisonous—under another harmless, varying the orthography and the classification, and commentators have warned the reader not to mistake these. Of the terrible varieties which he mentions as inhabiting the hot countries of Asia and America, he says, "*their habits are unknown to us*."

If the tarantulas of Texas\* were to write the natural history of man, they would probably charge the Texas writer, above quoted, with prejudice, and Cuvier with exaggeration. The latter calls these pigeon-killers "American spiders." Mr. MacLeay, in a communication in the *Annals of Natural History*, for 1842, denies this statement concerning these celebrated bird-catching arachnidans. Several living Naturalists are now ready to lay the blame of these fabulous accounts ("falsehoods" they are called) at the door of a woman, namely, Madame Merian, a celebrated zoölogist (born 1647—died 1717), who, in 1699 went from Holland to Surinam expressly to study and draw insects.

Professor Dunglison's excellent table of poisons, in his dictionary, arranges Venomous Insects thus:

VENOMOUS INSECTS.	SYMPTOMS.	TREATMENT.
Tarantula.	In general the <i>sting</i> of these insects occasions only a slight degree of pain and swelling; but occasionally the symptoms are more violent, and sickness and fever are produced by the intensity of the pain.	Hartshorn and oil may be rubbed on the affected part, and a piece of rag, moistened in the same, or in salt and water, may be kept upon it till the pain is removed. A few drops of hartshorn may be given frequently in a little water, and a glass or two of wine may be taken. The <i>sting</i> may, in general be removed by making strong pressure around it with the barrel of a small watch-key.
Scorpio. Scorpion.		
Vespa <i>Crabro</i> . Hornet.		
" <i>Vulgaris</i> . Wasp.		
Apis <i>Mellifica</i> . Bee.		
Culex <i>Pipiens</i> . Gnat.		
Æstrus <i>Bovis</i> . Gad-fly.		

\* It is remarkable that the U. S. Army Surgeons, in their official reports to the Government, generally mention the prevalence of tarantulas in the department of Texas, and there alone, if I remember correctly. Their natural history seems to have been neglected however in these reports.

In his Elements of General Pathology, M. Chomel says, “*venomous secretions* are peculiar to certain species of animals. When deposited by the animal secreting them, in the wounds which it inflicts, they occasion the same sort of affection in every instance. The viper, the wasp, and the bee, in our [French] climate, are the only creatures provided with apparatus fitted for the secretion and deposition of these poisons. In warmer climates, the scorpion and various kinds of serpents possess far more deleterious qualities.” The natural history of animals, and of diseases, too, must be studied, compared, and utilized in their native climates. The mortal venom of the rattlesnake (*crotalus horridus*), and the copperhead (*trigonocephalus*\*) of the United States, as well as yellow fever, congestive fever, cholera infantum, etc., should be studied where they are *in esse*.

One of the objects contemplated at the commencement of this article, was to offer some remarks on Cuvier’s classification of certain arachnidans. But this paper is already long, and I will not venture to trespass further on the patience of the reader. If what has been written neither settles anything in, nor adds anything to science (which is conceded), nevertheless, if the uncertainty and contradictions actually extant upon the topics alluded to shall have been indicated, and attention called to the same, it is not unreasonable to expect that competent persons will make and communicate more exact observations upon the natural history and venomous or non-venomous properties of the arachnidans of the South, instead of implicitly relying upon conflicting, fabulous, or even true observations and descriptions made in and taken from remote and very different climates.

The student of Nature, that is the physician, finds in every living organization, even in the minutest insect, direct or analogical information illustrative of the anatomy and physiology of Man, the nobilitation of the whole animal kingdom.

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\* Dr. Holbrook the distinguished herpetologist, says, of this venomous and dangerous snake, that “it is found from New England to middle Florida, inclusive, and from the Atlantic to the Alleghanies;” he might have added, West of these mountains. In Western Virginia, on one occasion, in the harvest season, while I was standing within a yard or two of a lad who was handling sheaves of new wheat, he was hitten on the hack of the hand by a copperhead. I seized his hand immediately, at once found the wounds by the blood issuing from them, applied my mouth to the same, and made strong and repeated suction. I ordered a dose of glauher-salts to be taken. Neither local nor constitutional symptoms followed.

ART. V.—*Malum Egyptiacum, Cold Plague, Diphtheria, or Black Tongue*: By SAMUEL A. CARTWRIGHT, M. D.

I PUBLISHED some remarks in the May Number of the *New Orleans Medical and Surgical Journal*, on *Malum Egyptiacum*—an old disease at present attracting much attention under the new name *diphtheria*, which was first given to it by Bretonneau, of Tours. The new name is an unfortunate one. It is derived from the Greek word *diphthera*, *skin* or *membrane*, and would do very well if it were the only disease of the throat attended with pseudo-membranous formations. But there are many diphtheritic affections, which have nothing in common except the adventitious membranous concretions, and are very different in their causes, requiring different treatment. Even the false membrane itself arises from different causes—internal or external, constitutional or local. Bretonneau used the word *diphtherite* as a synonym of *angina membranacea maligna*, but if he did not deceive himself by the new name, he led Guersent and many other pathologists into the belief that diphtheritic diseases are more alike than clinical experience proves them to be. The new name for the old Egyptian sore throat or malignant sore throat, *diphtherite* or *diphtheritis*, implied that the false membrane is the effect of an inflammation, which is a gratuitous assumption.

No name should be given to a disease covering up an hypothesis in the name, as it is sure to lead to error and great confusion. We have only to look into the medical literature of the day to see the Egyptian darkness into which this new name, for an old disease, is leading our most conspicuous medical writers and teachers. Dr. Samuel Wilkes, in the *London Medical Times and Gazette*, asks: "Is the disease, strictly speaking, a malignant sore throat, and the formation of a pellicle an accident?" He regards it as arising from a parasitic vegetable fungus, supposed to be the *oidium albicans*, but cannot tell whether a previously existing nidus is essential or not for its development. He mentions the *porrigo lupinosa*, a pustulous eruption, in which a parasitic fungus is detected by the microscope; "but the question is not settled, whether a previous irritation of the skin be necessary or not to form a suitable soil for the sporules to take root in." "In *diphtheria*, whether the plastic or concrete membraniform exudation in the throat be primary or secondary, it is important to know whether its presence is an essential part of the disease." Dr. Wilkes has examined the mouths of patients afflicted with various

discases, in which films are formed on the mucous surfaces, and says, "he cannot distinguish, by the microscope, any difference between the fungous growth in such affections and that of diphtheria." He made these examinations to meet the objection, which pre-supposes that the parasitic growth in diphtheria is different from that of aphthæ; observation at the bedside proves that the character of the pellicle and its rapid extension over the fauces are totally unlike ordinary aphthæ.

The report of the London Sanitary Commission on Diphtheria is even less satisfactory than that of Dr. Wilkes. It jumbles a great number of distinct species of disease together under the common name diphtheria, thereby changing species into mere varieties. The simple diphtheritic angina and the croupal diphtheritic angina of that report embrace a number of diseases very different in their ætiology, symptomatology and morbid anatomy from malignant diphtheritic angina. All those maladies, lately bunched together under a new name, were known to the physicians of Great Britain, Ireland, France, and Germany previously to the invention of the new name, with the single exception of one of them, the *malum Egyptiacum* of the ancients, the garrotillo of the Spaniards, the cold plague or black tongue of the Mississippi Valley, and the putrid sore throat of the West Indies. Dr. Corcoran, in the *Dublin Hospital Gazette*, of February last, assures us that this species of malignant sore throat is a *new disease* in that part of Europe where he resides. The English, Scotch, French and German physicians of the present generation also regard it as a *new disease*, or at least one which has not fallen under their observation until very recently. The text books of their immediate predecessors give no description of it. We do not find it described by European writers, until we go one hundred years back to Fothergill, who wrote in 1748, describing the disease as it appeared in the fenny districts of Great Britain, and in some parts of Spain. For more than half a century it has prevailed in various parts of the United States. Professor John Bell, one of the most learned and distinguished physicians of Philadelphia, acknowledges the important fact, that the disease has prevailed in some portion or other of the United States ever since the year 1813, when he first observed it, while a medical student; yet the medical students of America ever since have been depending upon European medical books to learn how to treat a disease which the writers never saw, nor their fathers be-

fore them. They did not go back to such old writers as Huxham, Fothergill, and Heberden, but were looking for the last edition of the latest system of European practice, written by authors who had never seen one of the most fatal diseases which American physicians had to treat. In the first volume of Stokes's & Bell's Practice (p. 93), Professor Bell says, "The epidemic which prevailed during the winter months, in successive years, from 1813 to 1816, in so many parts of the United States, presented numerous examples of diphtheritis, in which the pneumonia and bronchitis were sometimes apparent, but in other cases they were completely masked by the anginose symptoms. Then, however, although but a student, I remember very distinctly, that adults and those advanced in life are the greatest sufferers and most numerous victims; in some of the oldest persons the anginose symptoms were most predominant." "The mortality from this disease," says Dr. Bell, in the above-mentioned work, "under all modes of treatment, is excessive. In some instances it caused death in the course of a few hours." (p. 93.) But Dr. Bell's "all modes of treatment" were all drawn from modern European authors, who had never seen it. He reminds his readers that "in scarlatina and in the angina maligna of European writers, there are often deep ulcerations, which cannot be confounded with any crust or coat formed and spread in the mucous membrane itself;" and on the same page (94), makes the important confession, that in the disease which fell under his observation in America, "there is no ulceration nor gangrene, the removal of the membrane leaves the parts beneath entire, with some of the mucous lacunæ larger and more open than usual."

In the remarks I made on the Malum Egyptiacum, in the *N. O. Med. and Surg. Jour.*, I state, "I had never seen a gangrenous throat, properly so called, in this disease, in any of its forms. The throat may be apparently but little affected, or the whole fauces may be covered with what appears to be a gangrenous, putrid sloughing sore. But there is no sore at all under the *molluscum* (as Professor Meigs would call it), which covers the mucous membrane of the throat. The membrane is always found to be whole and entire as soon as the *molluscum*, which covers it, is killed and removed." These remarks were made to prove, what Professor Bell admits, that the disease is not a scarlatina maligna or any of those inflammatory, gangrenous or scorbutic ulceration of the fauces, occurring from constitutional

causes in adynamic states of the system, but is a disease *sui generis*, invading the throat without, inwards; and not from within, outwards. There is ulceration surely, but it is *exulceration*, not an ulceration of the mucous membrane of the fauces, but an ulceration of the false membrane itself, properly called exulceration. Hence, Aretæus advises such remedies as will cause the ulcerations to fall off in scabs; which would be impossible if they were seated in the true membranes instead of the false. The fœtor attending the ulceration of the membraniform growth is much greater than that of ulcers in the natural parts. No matter whether the ulcerated pseudo-membranous patch be large or small, a worse than gangrenous odor attends it. Indeed, in many cases it is so small as to pass unnoticed, if the extremely nauseous odor of the breath did not prompt the physician to look for it. When the malign pustule of the throat is small in extent, it is apt to be overlooked entirely, and the disease supposed to be a typhoid pneumonia. Misled by British text books, the malady has generally been regarded by American physicians as a pure asthenia, or a violent phlegmasia, and treated accordingly. The practice has been so unsuccessful, that no disease has brought so much opprobrium on medicine, as a science, in these United States, as this. Long ago I called the attention of the medical profession, particularly of the South, to the fact that many of the diseases daily met with in practice were very different from those of the North of Europe, and required a very different treatment. Experience proved that the closer the American practitioner followed the latest authority of London, Edinburgh, Dublin, or Paris, the more unsuccessful he was likely to be in his practice. I called in question the qualification of any one of Queen Victoria's physicians to prescribe for an American afflicted with bilious fever, cold plague, or milk sickness; or for any negro, no matter what the disease might be; for the very good reason that they had never seen these diseases, and had no practical knowledge in regard to them; and as for negroes, ninety-nine in a hundred of her subjects know nothing about them psychologically or physiologically, having mistaken them for white men with black skins; being taught to believe that there is no radical difference in the types of mankind, just as American physicians are taught to believe that there is no radical difference in the types of British and American fevers, and other diseases. Although many diseases are similar all over the world, there are others which differ as radically as the white man differs from the



negro. When, some years ago, I called attention to the fact that diseases, like animals and vegetables, have their peculiar zones, beyond which some can be carried and others not, Professor Bell, of Philadelphia, ridiculed the doctrine by calling it *States Rights Medicine*. But until ridicule has the power of making the cosmic, meteorological and climactic conditions of America the same as those of the North of Europe, where the medical schools are chiefly located, the diseases of the two regions must, from necessity, differ so much in their types as to make the Professors of those schools unsafe guides for American physicians to follow. Nor will it mend the matter to locate branches of the European schools in the United States. The name American will not make them so. The so-called American medical journals and medical schools, as a general rule, set a higher value on European chaff than on American wheat. The wheat comes mostly from our country physicians. Whoever would treat the diseases of our mixed population successfully, should visit our country physicians and lay in their stores of medical knowledge from them. They will find the most successful practice governed by rules, differing as the several localities differ from one another, and will learn why Hippocrates required more time to be devoted to the study of medical geography and topography than to all the other branches of medicine combined. If a few men in Europe, or elsewhere, could give rules to guide the physicians of the whole world into the safest and most successful method of treating diseases, there would be no use in their wasting much precious time in studying medical geography, which is but another name for what Professor Bell calls *States Rights Medicine*. Indeed, he has, in the last edition of Bell's & Stokes's Practice, gone so far into *States Rights Medicine himself* as to make a good text book, by omitting to incumber the work with diseases engendered in European hospitals, camps, work-houses, and jails, and in place thereof wisely adding some matter of his own. In his preface, he calls "upon American physicians to send him carefully prepared histories of the fevers they meet with in practice to answer the wants of American medical students." If, in the next edition, he were to drop Stokes entirely and substitute American *cane-brake practice* for European *hospital practice*, he would improve his book wonderfully. His chapter on angina membrenacea maligna shows the hand of a master, but he has spoiled it by shackling it in the types of European sthenic and asthenic fevers. He trimmed the foot to make it fit the shoe,

and padded the body to make it fit the coat. At first I had some difficulty in recognizing my old acquaintance, the *malum Egyptiæcum* introduced under a name that does not belong to it. There is not only the old danger of mistaking the *malum Egyptiæcum* for some other disease, and treating it improperly by bleeding and antiphlogistic remedies, as actually happened in the case of the great and good Washington; but there is the new danger of mistaking some mild diphtheritic affection for that formidable malady, and maltreating the little patient, if a child, by vigorous applications of the solid nitrate of silver, as advised by Dr. Monkin, notwithstanding "remonstrances, biting, scratching, screaming and shouting." To prevent such evils, the disease should be stripped of all theory and false coverings, and introduced to the inquirer after truth under a name belonging to no other disease.

#### NOMENCLATURE.

Of all the names which have been given to the disease under consideration, *malum Egyptiæcum* seems to be the most appropriate and least exceptionable. It has been called *ulcus Egyptiæcum*, but the name is exceptionable, because there is no ulcer in the normal tissues, the ulceration being confined to the abnormal growth or false membrane—properly speaking an *exulceration* and not an ulceration. Pestilential, ulcerous, putrid and malignant angina, are names which have been given to other diseases as well as to the disease under review. The same objection applies to *cynanche maligna*, and to *angina membranacea maligna*. It is a different disease from *pharyngitis ulcerosa* or *gangrenosa*—the gangrene and ulceration being confined in the *malum Egyptiæcum* to the false membrane and not extending to the natural structures. Besides, it is not an inflammatory disease in the sense that the term *pharyngitis* and *diphtheritis* would imply. It is different from *aphthæ* or *stomatitis*. The French have named it *diphthérite couenneuse*—a name which implies an inflammation forming a skinny substance like that on the sea-hog. The analogy is far fetched and not true to nature. The membraniform substance varies greatly in its density and appearance in different cases, and in different stages of the disease. *Diphtheritis* and *diphtheria* have been lately applied to so many different diseases, mild and severe, as to be a medical nomenclature, what John Smith is in our city directories. *Typhoid pneumonia*, *typhus malignus novus*, black

tongue, black thrush, and cold plague, are terms too vague and indefinite to designate a specific disease. The Spanish name *garrotillo*, expresses very well its worst forms, which destroy life in a few hours by suffocation; but does not convey a correct idea of the great majority of cases, which are more protracted, wherein life is destroyed by toxic influences and not by emphysematous choking. There would be no objection to one of the names the French have given it, *charbon angineux*, if the disease partook more of the nature of *charbon* or *carbuncle* than of *malign pustule* or *puce malign*. But it is so intimately associated with *charbon* and *malign pustule*, that it cannot be understood unless they be understood.

#### WHAT IS CHARBON? WHAT IS MALIGN PUSTULE?

The ancient medical writers seem to have confounded the *charbon*, *carbuncle* or *anthrax*, with the *malign pustule*, as most of the moderns do. The symptoms of both diseases are somewhat similar, but there is a very distinct line of difference between them. In *charbon* proper, the general symptoms precede the local affection. In the *malign pustule* the local ailment is the first morbid phenomenon observed. On its first appearance it looks very much like a mosquito bite or the bite of some insect. It is a veritable inoculation with poisonous matter. *Malign pustule*, or the *Persian fire*, as it has sometimes been called, is an inoculated *charbon* or *carbuncle*, and should not be confounded with spontaneous or symptomatic *charbon*.

Both diseases are common enough in new settlements in our Southern States and territories. *Charbon*, from constitutional causes, is often epidemic among quadrupeds. It seems to be due to the development of a similar disease in the vegetable kingdom in unfavorable seasons and under peculiar circumstances. The grasses are liable to be infested with the *puccinia graminum*. It is found on all parts of these plants, even on the glume and beard. It consists of oval or linear pustules, of a black color, at the moment they break through the epidermis. The microscope shows that these pustules are composed of miniature plants, having a whitish filiform pedicle, surmounted by a black capsule divided into two lodges. The *linear uredo*, commonly called the *smut* or *rust*, in wheat, is of the same nature as the *puccinia* of the meadow grasses. The hay or straw infected by the *puccinia* or the *uredo* is a very unwholesome nourishment for beasts; even the manure which is formed by it is regarded of a bad quality.

The disease of rice, called the *carolo*, is of a similar nature with the *smut* in wheat. It consists of reddish spots on the leaves and stalks, covered with a yellow powder, which soon turns to a green color. Plants infected with the *uredo* or *charbon*, abound in so much black powder as to discolor the clothing, face and hands of the field-laborers. The *charbon* of Indian corn is called the *uredo maydis*. It attacks the male flowers and the grains themselves. The part affected swells and forms a tumor of a white, red, or ash color, which is soon filled with a black powder. Very dry weather, as well as very wet weather, is favorable to the production of this disease in Indian corn. Something likewise seems to be inherent in the soil which tends to produce it. I have frequently observed that plants growing on little patches of ground of a few rods square, from the same kind of seed, and cultivated in the same manner, will, year after year, be diseased with *charbon*, while those in the surrounding fields are entirely healthy. Nor could any difference in the soil be discovered.

The affection called *charbon*, in cattle, is observed most commonly in those seasons and localities where a similar disease has preceded it in some one or other of the plants on which they generally feed. *Entozoöns* or parasitical animals, in great numbers, bred within the body, have been observed to be the forerunners and concomitants of *epizooties*, as they appear to be of epidemics both of man and beast. Whether worms should be regarded as one of the causes of epidemics, or as useful scavengers to remove the unwholesome or toxic elements from food and drinks of bad quality, is an unsettled question. The *local* affection, in the disease called *charbon*, is, perhaps, an effort of nature to deplete the system from the poison in the mushrooms swallowed with the food. What is called the *charbon* or *uredo* in the cereals, and the *puccinia* in the grasses are *cryptogamia*, similar in almost every respect, chemically and microscopically, to *poisonous mushrooms*.

I have often observed, when *charbon* prevails among cattle, that many sudden deaths occur without any local affection at all. No doubt if they had lived longer, the carbuncle, *charbon* or local affection would have manifested itself. One of my carriage horses died very suddenly a few days ago. I found, upon examination, that he had, for a week or more previously, been fed upon *cryptogamic* corn which was badly weevil-eaten. I took a specimen of it to Dr. Cawcours to examine with the microscope.

The entozoä, which live on the juices of plants and animals, are all provided with a proboscis or sucking apparatus of some kind. The weevil of the Indian corn has a very long proboscis for its size. The tumor or local affection in the disease called charbon in cattle, at a very early period (how soon I do not know), becomes fly-blown, as it is commonly called. Various species of flies and insects belonging to the orders *lepidoptera* and *diptera* deposit their eggs in it. The larvæ, hatched from these eggs, have the external appearance and mechanical structure of a worm. They have the same annular structure of rings, the same apparatus of short feet with claws or bristles or tufts of hair. Yet they inclose in this vermiform organization the rudiments of all the organs of a butterfly, or some other perfectly formed winged insect. The term *larva* literally means a mask. Under this mask in the infantile stage of life they mock at science. No less do they continue to mock in their *pupa* or chrysalis condition when the mask is thrown off. They then appear as if not only dead, but buried and wrapped in a shroud, hiding all appearance of limbs or organs. After remaining in this shroud an indefinite time it bursts asunder, the dead awakes to life, and under the name of imago, leaves the earth, rising above it on two wings, or a beautiful butterfly on four gilded wings. Solomon, in all his glory, was not arrayed like one of these; nor was his temple composed of substances so unalterable in the chemist's crucible as the singular substance, called *entomoline*, found in large quantities in their wings and elytra. It is this which gives the gorgeous metallic lustre to the scales of the body and wings by reflecting and decomposing the rays of light. The imago plays a more conspicuous part in the ætiology of diseases of plants and animals in the Mississippi Valley than is generally supposed. The imago, or perfectly formed insect, is often armed with poison, which it seldom uses, except in self-defence. I have been led by observation to conclude that the *larvæ of no insect whatever are poisonous*. This is an original idea drawn from nature and not from books. It is possible there may be exceptions to it. I was about to include all insects, in the larva and pupa state as innocuous, when some facts of Reaumur (of thermometer memory), quoted by Van Swieten, in his Commentaries on Boerhaave, vol. vi, page 430, occurred to my mind, forbidding it. "Reaumur, on handling some caterpillars's nests, was attacked with a troublesome itching betwixt his fingers, and his face and eyelids were swollen for four days."

“Nor did the exuviæ or cast-off skins offend only by touching them, but if they were agitated with a stick, they produced nearly the same effect. As this is observed throughout a large tribe of common caterpillars which may be thus of mischievous consequences, it seems probable that the hairs of the exuviæ may be carried by the winds to divers places and produce the like disorders. Four women who were present when these nests were handled were soon afflicted with exanthemata on the neck.”

The larvæ or worms often found in the sores on cattle, and even on the human subject, are believed to be void of any toxical property. They feed on the confervoid growths, which the microscope discovers to exist in ulcers and can only be injurious from mechanical irritation by preying on the sound parts when the algoid filaments and the debris are insufficient—as in the entozoä in children, which, perhaps, are healthful rather than hurtful, until their numbers are disproportionate to the crudities they are intended to consume. In many cutaneous affections, it is probable that the parasites, found therein, are effects and not causes of the disease, as I am sure the larvæ of insects are, which are observed in the ulcers called *charbon* in quadrupeds, sometimes also in those of man. The ætiological conditions, requisite to produce charbon, consist in the previous consumption of unwholesome elements containing fungoid or other poisonous substances. The poison may be so virulent, as in milk-sickness, to destroy life before the tumors and ulcers, called charbon, have time to form. In other cases it may be eliminated from the system by vomiting, purging, and sweats. But when not thrown out of the system by the excretory organs, charbon is one of the modes which Nature adopts to free the system from it.

#### MALIGN PUSTULE.

*Some Facts from the Field of Experience touching the Ætiology, Pathology, and the Therapy of Malign Pustule, and the Diagnosis between it and Diphtheritic Bilious Fever, Charbon, and Malum Egyptiacum.*—At the present day it is generally admitted that malignant pustule is caused by an inoculation with poisonous animal matter; but whether by poisonous insects or by common insects with the virus of charbon adhering to them, is an open question now, as it was many years ago, when I entered the field of practice. The disease was very common in newly settled localities, and was attributed to the bite or sting of

some poisonous insect. A black fly, like the anthrax of Meigen, inhabiting the wilderness in the North-eastern territory of the State of Mississippi, was supposed to be so poisonous to horses, that travelers, to avoid them, would hasten through the localities they inhabited in a gallop, or await until night to pursue their journey. Carcasses of dead horses were very common on the way side, a good portion of the distance between Columbus and Jackson, the present capital of Mississippi, their death being attributed to the black fly, which was due, no doubt, to hard riding and to the unwholesome provender they were fed with. Having located as a practicing physician in Monroe County during the excitement about the black fly, supposed to be so poisonous to horses and cattle, it was necessary to go to Columbia, on Pearl river to get a license to practice medicine in Mississippi from the medical board of censors who met there. In returning, I passed through Jackson, containing only two or three houses, and along the road, in the Indian nation so much infested with the much dreaded fly. I rode leisurely along in the day time, as I did not believe that that or any other fly killed the horses on the road side, or the cattle in the woods. I observed, however, that the Indian's *tonsha* (the name the Choctaws give to the grains of maize), was weevil-eaten, and that the *tonhisha* (the blades of maize) were mildewed. My horse and myself both got sick on the road. An account of my own case is published in the *American Medical Recorder*, 1825, vol. viii, p. 449-50. The symptoms: "Violent pain in the stomach, constant vomiting attended with a constipated state of the bowels, cold skin, and small pulse." I described it graphically as above, and gave it no name, not knowing by what name to call it. It was, no doubt, caused by poison in the shape of confervoid growths taken into the stomach with the food the Indians called *tomfuller*, after the digestive organs, had been impaired by great exposure. The symptoms differ according to the vehicle containing the poisonous fungoid or mushroom; if in milk, obstinate constipation is one of the symptoms; in other substances, it is apt to purge. It is also apt to purge and to affect the respiratory organs more than the stomach when it enters the circulation through the agency of a malign pustule, any where on the surface of the body, or on the mucous membrane of the throat. The effects of the poison are essentially the same, differing only in degree, no matter in what way it may have got into the system. The cold skin and weak pulse invariably attending it, has caused the disease to be designated as

the cold plague, or congestive fever. The case of the Prince Albert of the Empire of Beauty's Queen, Dr. Henry Levert, of Mobile, is reported by me in the same medical work above referred to, (pp. 455-6) as a bilious fever, for the want of a better name to designate it. There was so much diphtheritic matter covering the mucous membrane of the mouth and throat, that two of the physicians, who were called in consultation to see the case (one of whom was a graduate of the University of Edinburgh), mistook it for the disease now called diphtheritis mercurialis or stomatitis mercurialis, then called erethismus mercurialis by Pearson, of Lock Hospital. "He shuddered when I strenuously urged the propriety of giving immediately fifteen or twenty grains of calomel," "as he was of the decided opinion that I had already given Levert too much calomel," and was sure that the exudation matter was caused by mercury and not by the original disease. While discussing the case with four doctors, who had been called in consultation, the report as published in the *Medical Recorder*, says, "a difficulty of breathing and heaviness at the stomach came on, and alarming syncope took place, from which the patient was aroused by volatiles and stimulants only to pant a few minutes and to faint away again. I took the responsibility on myself alone, and gave the calomel and jalap, and assisted it by injections. The medicine operated freely, and as it operated the pressure about the præcordia became more and more relieved, and the fainting fits subsided. As soon as I had given the patient the forbidden dose of calomel, I sent twenty-five miles for Dr. Butler." "In a few days Dr. Butler and myself gave the patient four hundred grains of calomel, and he recovered without having a sore mouth or the least increased salivary discharge." The diphtheritic incrustation on the mucous surfaces accumulated so rapidly, as, if not purged off, to cause syncope and a failing of the vital forces. Calomel was the best remedy I then knew, or *now know*, to loosen it and bring it away. In the same article (p. 454), I state, "I am no advocate for the indiscriminate use of calomel in any form, as the states of the system which indicate it I believe to be few in comparison to those which contra-indicate it." The states of the system which indicate it are those in which the mucous surfaces are plastered over with a tough, tenacious coating, characteristic of certain diseases bearing the objectionable name bilious, but which are now called diphtheritic, a term more objectionable, because it draws off attention from other morbid



phenomena of equal or greater importance; as for instance, the congestions of the vital organs; the torpidity of the capillaries of the skin; the neurosis of the cerebro-spinal and ganglionic system of nerves, as proved not only by the pains, spasms, convulsions and deranged sensibilities, but by an abnormal deficiency or excess of animal heat—in unequal distribution, the sensational cognition of burning heat in a part that may be actually below the uniform thermal standard, and shivering with cold when the actual temperature is exalted; symptoms too apt to be regarded with very little attention, although they point unerringly to important lesions, occurring periodically, in the nervous centres, which the toxic agent in malarious districts has produced. The case of Dr. Levert is a good type of diphtheritic bilious fever, which every practitioner in malarial localities in our Southern States has had to treat as well as he could without help from books, as it is no where well described, or the mysteries in its pathology explained. The quantity of diphtheritic or aphthous matter, exuded upon the lining membrane of the alimentary canal, would astonish those who have never seen the disease, and who erroneously attribute the cause to the abuse of purgative medicines. The diphtheritic exudation precedes the use of purgatives and forces the physician to purge the patient or let him die. But the case of Dr. Levert has been alluded to merely to draw the dividing line between diphtheritic bilious fever, attended with large quantities of pseudo-membranous exudation matter, and the malum Egyptiacum. The latter, although a diphtheria, is a different disease, either from that or from diphtheritic apoplexy; being caused by the direct action of poison introduced into the system from without, which it may gain in three ways: 1st—by the absorption of the virus in that poisonous diphtheritic affection of the skin known as the malign pustule; 2d—from a diphtheria of the throat or some other mucous surface of the same poisonous nature as that on the skin; 3d—from milk or any other food taken into the stomach abounding in cryptogamic substances possessing the same deleterious properties as the poisonous mushrooms.

Diphtheritic affections of the skin, as also of the mucous membranes, are not poisonous unless inoculated with some specific poison. There are various parasitical insects which deposit their eggs in the skin of animals, causing those pseudo-membranous formations, tumors and ulcerations to form, called by the French, cutaneous diphthérite. The

animals are in the mean time but little annoyed by them, and their general health continues good, although these sores on their body are filled with worms. Humboldt, we are told by Cuvier in his account of the *Cestrus*, met with Indians in South America whose abdomens were covered with little tumors produced by the larvæ of that insect.

In such cases the pseudo membranous substance is evidently not an exudation produced by any internal or constitutional cause, but is entirely due to the local irritation of the insect when the ova are deposited in the skin and to the larvæ, which continue to dwell in their diphtheritic nest, until prepared for metamorphosis. On the field of experience I observed that the tumors and sores, which were so frequently seen on the skin of both man and beast were of little or no consequence and were rather regarded as a sign of good health than otherwise, except in those localities where one or more animals had died of charbon. Then, indeed, they required to be looked to, and carefully covered to prevent them from being converted into malign pustules by being inoculated, through the agency of flies or other means, with the virus of charbon. The entire body of an animal dying of charbon is a mass of virus; every part of which is endowed with the property of reproducing the disease if it comes in contact with an irritated spot, either on the skin or mucous surfaces; or if taken into the stomach with the food, in those cases where any cause has impaired the digestive powers; otherwise, if not innocuous, its effects are not perceived. The last cases of milk sickness I ever had, occurred in a family where only about half the whites and none of the negroes were attacked with the disease. It is asserted on good authority that even the hair or the tanned leather of an animal which has died of charbon, if applied to an irritated surface, is sufficient to cause that poisonous diphtheritic affection, known as the malign pustule. Hence the wisdom of those French laws, which require that every animal dying of charbon, shall be buried, whole and entire, in the earth eight feet deep. The pustule itself, caused by the inoculation of charbonous matter, may be so small and inconsiderable, whether situated on the mucous or cutaneous surfaces, as to pass unnoticed for several days until the poison reaches the circulation and those constitutional symptoms occur, which constitute the disease in the human subject called *malum Egyptiæcum*. The first effect of the virus is shown by an obscure red prominence or salient pimple in

the centre of a small areöla ; the pimple being surmounted by a little vesicle or phlyctæna, which breaks and discharges a redish fluid within forty-eight hours after its first appearance. Then a hard, circumscribed, flat tubercle can be recognized by the sight and touch. The breaking of the vesicle leaves a livid or black spot surrounded by a shining surface, sprinkled with isolated phlyctæna on its margin. The margin of the original areöla becomes, in a few days, so much tumefied as to rise above the central black spot and a new areöla is formed around the first. The subcutaneous cellular tissue is then invaded with an *adematous emphysema*, as might be called, as it seems to consist of neither air nor liquid. This peculiar kind of bloating is characteristic of malign pustule. There is another characteristic symptom. Although the tumor may be, and generally is, very painful to the patient, the central portion, within the limits of the original areöla, is void of sensibility. The actual cautery applied to the circumscribed surface within the first areöla gives no pain, but immediate relief. Nor is there any cicatrix following the use of the cautery, indicating that there has been a loss of substance in the part cauterized. There is no loss of substance on the healing of the ulcer ; for the very good reason that there is no ulcer in the cutis vera, but only in the diphtheritic or spheroidal membranous substance, which forms between the true skin and the cuticle. If that false membrane be permitted to slough, constitutional symptoms will arise, and great prostration of the vital forces will take place, with a foul breath. If the lymphatics are numerous, in the part of the body where the pustule is formed an erysipelatous tumefaction, holding a middle place between œdema and emphysema, will invade the eye lids and the thinner and looser parts of the cutaneous surface. Anon the throat—then death, if the proper remedies are neglected. That the cause of malign pustule is due to an inoculation with poisonous matter and not to any vice in the general system, is proved by cauterization putting an immediate stop to its progress and curing the disease speedily without any other remedies, if used before the secondary or constitutional symptoms have occurred. Even after they have occurred, it has the same beneficial influence in relieving the constitutional symptoms, that the stomach pump has in relieving the constitutional symptoms arising from arsenical poison swallowed.

I concluded that malign pustule was not caused by the bite or sting of any venomous insect ; because in all such cases the poison

acts immediately, causing great pain and swelling ; whereas, the virus which produces malign pustule, like the vacemo, syphilitic, hydrophobie, pusulan, and other animal poisons, require a stage of incubation before they manifest themselves in any other manner than by a slight irritation, and it is some days afterwards before they produce constitutional or secondary symptoms. As these secondary symptoms are not much unlike those of milk sickness, or the disease produced by eating poisonous mushrooms, and as the symptoms of the latter disease are nearly identical, differing only in degree, from the disease caused by eating any kind of food abounding in confervoid growths, the presumption is that the virus is of a similar nature in all. Experience proves that when charbon occurs spontaneously in animals, it is among those which have been fed on damaged grain or provender. Experience also proves that an animal dying of charbon is essentially poisonous. Nevertheless, when left to putrify in the open air various insects prey upon the cadaver or deposit their ova in it, the larvæ of which ultimately consume it. Observation teaches, that while this is going on, that malign pustule is sometimes observed on the face, hands or exposed parts of the body of some of the neighboring inhabitants ; but more often some one or more of them are attacked with a putrid sore throat, a typhoid pneumonia, a phlegmonous erysipelas, a cold plague, congestive fever, or some malignant disease, not only baffling the skill of the physician, but baffling nosological writers to give it a name. It is not epidemic, properly so-called, as it occurs here and there without any regularity ; but it is most common while some epidemic disorder is prevailing. Sore throats of one kind or other are apt to be very prevalent, or eruptions of divers and anomalous kinds on the skin, or fevers of different grades may be prevalent. Contemporaneous with these milder affections ash colored or black spots, the sloughs of superficial ulcers, appear on some part of the lining membrane of the mouth or throat, attended with a very nauseous odor of the breath, the offensiveness of which hourly grows more intolerable. There is a noise in the throat, in severe cases, conveying the idea of strangulation. A sanious matter distils from the nostrils of an acrid nature, which, if it suddenly stops, the anginous symptoms become more threatening of immediate suffocation. Some exanthematous affection of the skin shows itself *before* the sore throat in some cases, and others *afterwards*. But perhaps in a moiety of cases there is no eruption of any kind on the skin,

either before or afterwards. Giddiness of the head, anxiety, sickness at stomach, vomiting and purging, difficulty of breathing, small rapid pulse, prostration of the vital forces, foul odor, not only of the breath, but of the sweats and excretions, with internal heat and cold surface, close the scene of this cold plague in a day or two, or it may pass into a protracted typhoid form of fever. In my former article I have particularly described that variety of Egyptian evil which assumes the form of typhoid pneumonia.

A question arises. Is not the affection of the throat in the Egyptian evil a malign pustule on the mucous membrane similar to the malign pustule on the skin? After the skin is inoculated with the virus of charbon, there is a period of inertia, then a period in which a diphtheritic substance is formed, which sloughs in the centre, and soon constitutional or secondary symptoms occur. The same period of inertia, then primary and then secondary symptoms are observed as in other poisonous inoculations. Under this view of the subject the diphtheritic affection of the throat in malum Egyptiacum is not an exudation from any constitutional cause, but the mere effect of the zymotic germ or particle of animal poison, which has found a nidus in the throat. The diphtheritic web is a mere domicile constructed by the toxic dynamics of the germ itself. It is well known that the larvæ of many insects are found in sheep, horses and horned cattle in localities of the body where it would be impossible for the fly, which furnishes the eggs, to reach. The fly called the *œstrus* ovis deposits her eggs on the internal margin of the nostrils of sheep. The larvæ, when hatched by the warm breath of the sheep, hasten to insinuate themselves into the posterior nares and in the frontal sinusses to which they cling by means of two hooks. The *œstrus* equi, *veterinus* and *hæmorrhoidalis* deposit their eggs on the shoulders and inside of the legs of horses, where they hatch and are licked off by the tongue of the horse and then descend into the œsophagus and stomach, where they remain until ready for metamorphosis, and escape with the fæces. Clarke thinks they are rather useful than injurious to the horse. All those ova which are deposited on parts of the body of animals, or in places where the larvæ are compelled to change their domicile, are inclosed in a diphtheritic cradle of glutinous matter, which keeps them together and secures them to the part the animal is in the habit of licking. The flies, which deposit their eggs on animals, are about half an inch long and more or less pilose.

They are carnivorous. They feed on the bodies, not only of dead animals, but living ones, by sucking their blood. After feeding on a charbonous animal and afterwards alighting on a living one, either to suck the blood or to deposit their ova, they are saturated with sufficient poison to inoculate the animal with the virus of charbon, particularly if it has a thin delicate skin. The ova are not only deposited on animals, but on vegetable substances used for food, and may thus gain admission to the fauces where they adhere by virtue of the glutinous matter or diphtheritic cradle inclosing them. The ova of the musquito are fastened together by diphtheritic matter in the form of a canoe or cradle and committed to the waters as was the cradle of Moses. It is not uncommon to see the carcasses of animals, including those which have died of charbon, literally covered with various insects, including the *culicides*, as the musquito family are called, from their flying around in a circle singing dithyrambics, like the worshippers of Bacchus. Glutted with charbonous matter, they arise from their repast and the females deposit their ova in water used for drink and on various substances used for food; every female enclosing her eggs in a diphtheritic web, secreted from her own stomach filled with poisonous matter. As these envelopes are very glutinous there is nothing to prevent a great many of them from adhering to the mucous coat of the mouth and the isthmus of the fauces but the rythmical ciliary movement, which is constantly in activity in a healthy individual, propelling the fluids over the surface into the stomach; thereby preventing poisonous substances from tarrying there by driving them rapidly into an organ, which has more power over poisons than any other of the body. If, however, any thing has happened to impede or impair the vibratory movement of the cilia, which fringe the margins of a good portion both of the tessellated and cylindrical epithelium of the mucous membrane of the fauces, there is nothing to prevent any tenaceous substance, like the diphtheritic web, inclosing the ova of insects, from adhering to the velum pendulum palati, to the tonsils or some other part of the mouth and throat, and inoculating the particular spot in the mucous surface they adhere to, with the virus of charbon. The inoculation would be more apt to take effect if the throat be in a raw irritable state from an existing cold or some other accidental malady.

TREATMENT OF MALUM EGYPTIACUM.

In closing my remarks on the malum Egyptiacum, published in the

May Number of the *N. O. Med. and Surg. Jour.*, I briefly alluded to some of the remedies, which experience has proved to be the most successful in its treatment. In the same article I touched upon the causes which had deprived American physicians of the benefits of the medical experience of all the world, ancient and modern, except a few writers and teachers mostly occupying professorships in medical schools, and deriving their experience from the observation of diseases in hospitals from six to ten degrees north of our Vermont and New Hampshire.

The pupils of the great medical school of Edinburgh, established a branch in Philadelphia, and it is chiefly through that branch school, that medical knowledge has been disseminated through the United States. As the malum Egyptiacum, bilious fever, bilious pneumonia, and many other diseases, common enough in Southern latitudes, were seldom or ever met with in the hyperborean regions of Europe, they were gradually dropped out of the new editions of the medical text books. Thus all knowledge of these Southern diseases was lost to the new generation, who trusted to the new books for information. The last medical text book giving any information of consequence in regard to them and the accumulated experience of centuries touching the best methods of cure, is Van Swieten's Commentaries on Boerhaave, published in London in 1747, upwards of one hundred years ago. When that great medical work, in fourteen volumes, was superseded by other text books, omitting the Southern diseases that it contained, a generation grew up which knew not Joseph. The American physicians were every day meeting with Joseph in the field of practice, but their British text books which they still cling to with colonial subserviency, contain no information in regard to him—the writers having none themselves. The object of the modern English text books is merely to teach beginners how to treat the diseases met with in the United Kingdom of Great Britain and Ireland. When British physicians go to the Indies, or into the army and navy, they prepare themselves for treating diseases of other climes by studying medicine *de novo*. They do not depend upon the latest text books in the English language, but upon monographs of southern writers and old books, mostly in the Latin and Greek. We find them in the West Indies, and elsewhere, treating malum Egyptiacum, or putrid sore throat, by a remedy, which a large portion of American physicians would be afraid to use, even if they had not imbibed strong theoretic-

cal prejudices against it, lest they might be confounded with Thompsonian doctors, although it was used in the West Indies by the most learned British physicians before Thompson was born, and is recommended in the works of Hippocrates, the father of medicine as a science. Need I mention that the remedy alluded to is *red pepper*? Upwards of thirty years ago (see *Medical Recorder*, vol. xiii, p. 212), a physician confesses, under an anonymous name, that "he has long been in the habit of prescribing *capsicum* very freely, both externally and internally, in a great variety of toxic diseases, including pneumonia typhodes and cynanche maligna," and goes on to state that he knows no substitute for it in such affections, in the materia medica, and that it may be safely and advantageously given in large doses in alarming hæmorrhagies, hæmoptysis, hæmatemesis, etc. In the same volume (pp. 120 to 127) we find Drs. Stephen Brown and Lewis Belden, treating the malum Egyptiæcum, called by them cynanche maligna, with lunar caustic, though not very successfully; because regarding it as a high grade of phlegmasia, they used leeching and evacuating medicine. "In two very dangerous cases the capsicum gargle was used" by Dr. Belden—one recovered and the other died. Now, if these doctors had looked into Aretæus, or even into Van Swieten's Commentaries on Boerhaave, vol. viii, pp. 90 to 191, they would have seen that they ran no risk of being regarded as quacks for making such applications to the throat as capsicum and the caustic in the disease they were treating. They would there have learned from the highest authority, that "the indications of cure consist in the use of *such medicines which like fire are able to restrain the eating ulcerations, and to make them fall off in scabs.*" The question of choice between the lunar caustic and capsicum would be a question which is the most like fire? Medicine in America is making progress, and releasing itself from the fetters which the miserable theories in British text books, based on sthenia and asthenia, phlegmasia and cachexia, solidism, humoralism, and other isms for more than half a century have bound it. The most obscure physician in practice can now stand in the face of these theories, and apply lunar caustic to the throat without making any apology to the profession for violating theory, as Drs. Brown and Belden had to do thirty years ago, but they must take care how they use *red pepper*, if they would escape the imputation of quackery. That remedy is so perfectly antagonistical to the entire batch of new theories imported from northern Europe,



and so unsuitable to northern diseases, that it scarcely finds a place in the late editions of the materia medica. The myrica cerifera or candle-berry, although its virtues were lauded in medical works, one hundred and fifty years ago, for dysentery and other affections, being very pungent and prickly, causing the saliva to flow very freely, has been discarded from practice on theoretical grounds, by those who did not know that the membrane which lines the nasal cavities, frontal sinuses, maxillary antra, lachrymal and salivary ducts, the posterior surface of the fauces, eustachian tubes, larynx, trachea and bronchi, is paved with a cylindrical epithelium furnished with cilia, whose perpetual motion is essential to health, or they would not have rejected, on theoretical grounds, one of the most efficient articles to restore ciliary action when suspended. They must have forgotten that the hydrastis canadensis or yellow pucoon, which grows so plentifully in America, is similar in its properties to the sarratula tinctoria (the sariette of the French, entering in those celebrated compositions the *vin aromatique* and the *alcoolat vulneraire*, and also recommended by Hippocrates in diseases of the throat), or they would not have deprived American physicians of the remedy by excluding it from the materia medica. Although they did not reject myrrh from the materia medica, they almost theorized it out of use. The French are not altogether satisfied with lunar caustic in the treatment of diphtheritic diseases of the throat of the malignant kind, as they do not think it powerful enough, not enough like fire to cause the diphtheritic incrustation to fall off ; besides, it is difficult to apply, in the solid form, to the entire disease. Hence they have invented a remedy they call *pyrothonide*. Dr. Banque introduced it into medical use. It has some analogy to creosote. It is made by burning hair in copper vessels until an oily brown matter is obtained. I have never used it, being well satisfied with a tincture made by infusing for several days, in diluted alcohol one pint, finely powdered hydrastis root (or the sariette of the French), and the powdered bark of the root of the myrica cerifera, of each one ounce; three ounces of gum myrrh, and two drachms of capsicum. A piece of cotton or wool (the former the best), fastened to a probang, saturated in the above tincture, is the best thing I have ever found to swab the throat with in cases of malum Egyptiacum. I prefer making the probang by doubling a piece of stiff wire upon itself, and then bending the double wire to form a suitable curve to gain the isthmus of the fauces. Raw cotton can be wrapped around

the end of the wire without the trouble of tying it on. The wire is better than wood or whalebone to make the probang of, as it takes up less room and is much easier introduced. According to my experience the tincture, above mentioned, is as effectual as the lunar caustic in substance, and possesses the advantage of being readily applied to the whole diseased surface. A little of it should also be swallowed. The mouths of children had better be previously *buttered* to prevent the burning sensation they complain of.

There is another tincture made of three ounces of myrrh and half an ounce of the seed of the *xanthoxylum fraxineum* or prickly ash, half an ounce of Cayenne pepper and half a drachm of gum camphor in a pint of alcohol ; but it is rather too pungent. Prof. Merrill's syrup, made by mixing one drachm of hydriodate of potash, half a drachm of iodine, half an ounce of syrup of morphine and an ounce of water, applied to the throat by a probang is an excellent application in many diphtheritic affections. But I have never trusted to it in any formidable case of *malum Egyptiæcum*, confining its use to bronchitis, quinsy, etc. When the tincture above mentioned is not at hand, I have been in the habit of using the compound tincture of myrrh, sold in the shops under the name of No. 6, or the capsicum simmered with salt and vinegar. The advantage of these gargles is that they can be swallowed and constitute about as good an internal medicine as can be given.

In cases where calomel is indicated I give a dose of my cholera powder, which consists of twenty grains of calomel, twenty grains of Cayenne pepper, and ten grains of gum camphor, rubbed together in a little mucilage. The dose may seem large, but it is not too much. It quickly checks watery purging. I introduced it into practice in the year 1832, during the first visitation of the cholera, and it was found to be a very effectual remedy in arresting the disease, if timely administered. It is a good internal remedy in *malum Egyptiæcum*. It is an imitation of the *theriaca andromachus*, being, like that celebrated antidote, a slow purgative, an antispasmodic and a pungent stimulant. The *theriaca* is composed of seventy-five different articles. Some years ago it was voted out of the Medical Pharmacopœia by the London College of Physicians, by a majority of one. The objection against it was that it was an unscientific, many-headed monster. Yet it was acknowledged by the very men who rejected it, that it had enjoyed the confidence of the

medical world ever since the earliest records of science, and that that confidence was based upon experience and observation. The ostracism of that valuable southern medicine is a clear instance of the violation of that fundamental principle of medical science, which maintains that truth, derived from experience, shall not be gainsayed on theoretical grounds. The theorizers, who in pursuit of simplicity in medicine, entered the apothecary shops to simplify the compounds used as medicine, ought not to have stopped at them, but to have gone into the restaurats and cook shops and simplified the compounds used for food. Not less than three or four hundred different articles enter into the composition of a good dinner—and it would not be regarded as a good dinner by an epicure with a fewer number—yet we find human reason in its vanity discarding medical compounds, approved by experience, because they contain more articles than it could see the necessity for. The homœopathists have simplified medicine still further than the London College of Physicians, but like them, not their dinners. The *theriaca*, so named from its being an antidote for animal poison, is a very convenient and useful medicine, particularly in country practice where the services of the apothecary cannot be had. It is difficult to obtain the genuine article. I was in the habit, when practising in Mississippi, of importing it from Europe. It is composed of a few ingredients which act like calomel on the liver ; a few astringents ; some pure bitters ; a long list of aromatics ; a few balsamic substances ; half a dozen foetid ingredients ; a few gummy and amylacious substances, together with honey, wine and opium. A combination of one or more substances from each of these classes is a *theriaca* in Greek, an *antidote* in English, and will be found more effectual in sustaining the vital powers in malum Egyptiæcum and other diseases when they are greatly depressed, than any one simple stimulant or tonic. Every one knows that aromatics correct the harsh quality of acrid purgatives, and the depressing effects of opium. In all such matters experience is everything ; reason nothing. We need not be very particular or waste time to procure the special medicines we would prefer using in treating a case of malum Egyptiæcum ; especially in its severe suffocating form.

Twenty years ago, on a visit to Biloxi, I was called in great haste to see M. Vallé, a cotton broker, now residing in this city, office No. 10 Royal street, who had the garrotilla, or black tongue, and was

almost suffocating. I snatched up some red pepper, tincture of assafoetida and paregoric—mixed them together, twisted a piece of cotton around a bonnet wire doubled upon itself, saturated it in the mixture, thrust it down his throat, and then gave him some of the same to swallow. The black diphtheritic incrustations soon fell off, and the breathing was greatly relieved in a few moments after the first application was made to his throat. It excited a copious flow of thick tenaceous mucus, and was repeated so as to keep up the flow of saliva and mucus. Under the treatment, which had been previously used, the mouth and throat had become dry, and as soon as the discharge stopped he began to be threatened with immediate suffocation, making a noise in his throat like a person strangling. It is not the inflammation, swelling, ulceration or gangrene, which produces death in such cases, but *emphysema*.

Emphysema is often caused by poisons of various kinds. The sting of insects will sometimes produce it. It may be local, or more or less general. It consists of a gaseous secretion. In malign pustule, as before observed, a swelling of the subcutaneous cellular tissue, holding a middle place between œdema and emphysema, occurs. It is caused by a spontaneous disengagement of gas in different parts of the body. The exhalation of aëriform fluids into some parts of the body, is a natural phenomenon in many kinds of animals. In fish, for instance, there is an air-bladder or air-tumor. Air-tumors occur in *malum Egyptiæcum* and may be mistaken for apostemes. They are paroxysmal in some cases. They occur in *malum Egyptiæcum* when the throat and mouth become dry, and disappear when the discharge of the sanious matter is made to flow. Assafoetida or the foetid gums are as useful in such cases as in hysterics. When the pepper or any other gargle tends to arrest, instead of increasing the discharge, it should be discontinued, and mucilaginous substances used in its place. Sponges wet in hot water, as advised by Hippocrates, should be applied on each side of the throat, and the patient, as directed by Hippocrates, should inhale the steam of honey, vinegar, nitre, organum, serratula, apium gravcolens, mentha and pulgium, mixed together and brought to the boiling point. He directs the steam to be inhaled through a tube. He also advises the patient to be cupped about the ears. This old practice may be improved by a little honey and ipecac put in the mouth to assist in restoring the discharge. I used to use vomits in such cases, but am inclined to

think that a thick syrup of honey and ipecae rubbed on the gums or held in the mouth is preferable.

I have not space in this article to treat at length on that form of malum Egyptiæcum caused by poisonous cryptogamic substances taken in the food. The poisonous matter, which gains the circulation through the digestive system, I have reason to believe is often cast out by boils, sores, cutaneous eruptions, whitlows, and carbuncles, without producing any serious constitutional disturbance. I infer that this is the case, from the fact that in all such cases, bark and wine or some other tonic to strengthen the digestive powers are much better remedies than anti-phlogistics. Change of diet and locality will often very quickly cure such affections.

NEW ORLEANS, September, 1859.

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ART. VI.—*Therapeutical Memoranda*: By F. STRUBE, M. D., of New Orleans.

i. *Uva Ursi in Cases of Extremely Tedious Labor.*

It is very desirable to have a choice of remedies in tedious labor and not to be confined to the use of secale cornutum alone—a remedy which is too frequently abused, and with which, in the hands of inexperienced practitioners, a great deal of harm is done. The ergot generally produces such intense and violent contractions, that its exhibition is not without danger, particularly if the neck of the uterus is not sufficiently dilated, and the woman be a primipara. Some physicians even pretend to say that the ergot is always more or less injurious to the infant, and statistical researches made on this subject, and read before the Academy of Medicine, in Paris, by Dr. Deville, seem to prove this assertion. Conceding, however, that these statistical observations are correct, we do not think quite so badly of the ergot, if it be only exhibited at the right time and in the proper indications by skillful attendants, but we do not doubt, that in the

majority of cases, where it is employed, it is given by inexperienced persons, and for this reason, has so often proved fatal to the infant and also highly injurious to the mother. An experienced practitioner will always be very careful in exhibiting this remedy, considering it as an "*ultimum refugium*;" and we do not doubt that if used at the right time it is an extremely valuable remedy, with which we cannot dispense in our therapeutics. But it is highly desirable that we should discover some other remedial agent of an identical action with the ergot, but less powerful—a remedy, whose action would be sufficient to give to labor the same energy which it would present in normal cases, without accelerating the same in any case in a too sudden and injurious manner.

Dr. Gauchet, of Paris, has made some experiments with the *uva ursi*, and highly recommends it for said purpose, expressing the opinion that the same fulfills all the above-mentioned conditions, without doing any harm to mother or child. He relates a case in which the action of the *uva ursi*, on the contraction of the uterus, proved very successful. The first symptoms of labor, in this case, commenced at six o'clock, A. M., being very weak and at long intervals; the waters soon flowed off, and this state continued until four o'clock, P. M. At this time Dr. Gauchet seeing no progress whatever in the labor, prescribed a half ounce of *fol. uv. ursi* to be infused in a litre of boiling water during one hour, and ordered a teacup of this infusion to be taken every half hour. After the second dose the pains increased, the neck of the womb slowly dilated, and at eight o'clock in the evening the woman was delivered of a healthy child. (See *Bulletin Général de Thérapeutique*, June, 1859.)

A single case of course cannot decide the value of any remedy, for "*post hoc, ergo propter hoc*," is often a false logic in medicine. But eight other cases are mentioned where the *uva ursi* in tedious labor was followed with the very same success, as above, and we express our hope, that this remedy may enjoy a larger reputation for this purpose than it had before, and may do away, in some degree, with the too free and often injudicious employment of *secale cornutum* and its bad consequences.

## ii. *Treatment of Pulmonary Consumption with Lead.*

We cannot pass over, unnoticed, the recommendation of lead in pulmonary consumption, which we find published in the *Paris Hospital*

*Gazette*, and which found its way from there into the *Gazette Hebdomadaire*, Paris, June 24, 1859, and the *Medical Journal*, of Bordeaux, June, 1859. This remedy has been known and used for many decenniums, for this purpose, in the preparation of acetate of lead, and has failed like so many other antiquated remedies. But as each of them are, from time to time, dug out of the darkness of oblivion and become fashionable again (for we are sorry to confess that in therapeutics we also have got our fashions), so the lead once more makes its appearance in our journals. This time, however, not in the shape of plumb. acetat., but as the carbonate (cerussa).

Dr. Beau, who has experimented with it in a Paris hospital, prescribes it in pills of two grains, commencing with one per diem, gradually increasing with the number, till the patient takes eight *per diem*. He discontinues them when the first symptoms of poisoning show themselves in yellow coloration of the skin, the peculiar bluish stripe on the gums, and affection of the spinal marrow, consisting in partial anæsthesia, or even paralysis.

We have only too often an opportunity in common life to observe the physiological effects of lead on the human system, and certainly find, that in whatever form or preparation it be introduced into the economy, it acts as an astringent on the different mucous membranes of the same, checking their secretions and producing a contraction of their tissues. It is very natural, that based upon these physiological observations, we should employ this remedy as a powerful agent in such cases, where an abnormal secretion has taken place in any of the mucous membranes. It is, therefore, much used in this country in dysentery, and even in cholera, although great care must be taken not to exhibit it as long as there are symptoms of acute inflammation. The same is true of the lungs, as long as they are in a state of inflammation, high irritability or hyperæmia, we consider this remedy to be highly contraïndicated, although Dr. Beau, of Paris, makes no such difference. We would particularly recommend it in cases of chronic bronchial catarrh, with copious discharge of matter and mucus, but are rather opposed to use it where tubercles have already formed, or where they are in the second stage of dissolution. Here, generally, preparations of lead will increase the great difficulty of breathing, the coughing, and produce hæmoptysis. But as every remedy used "*cum grano salis*," may prove beneficial in the hands of an experienced physician, so also the lead preparations may do well in some cases of

pulmonary consumption, and we only hope, that these our lines may not lead to any extensive abuse of this powerful remedy.

Dr. Beau recommends the carbonate of lead as *allerans*, not only to stop the profuse night sweats and colliquative diarrhœa, but with the well expressed intention of really curing the patients. He says that anæmia is the proper characteristic of lead poisoning, as well as of intermittent fevers, which are also more or less incompatible with phthisis. Further, he contends that the workmen in lead mines very rarely are attacked with pulmonary tubercles. This latter observation has already frequently been made. Tanquerel des Planché\* reports, that "among 1,217 cases of lead poisoning, he only counted 55 consumptives. Phthisis has equally seldom been observed among the workmen in silver mines, in which the silver is generally found combined with lead and sulphur. In the silver mines of the Harz, Northern Germany, neither Dr. Sander nor Dr. Brockmann† ever observed in their practice any case of phthisis, not even in men, in whom a strong hereditary predisposition might produce the same under the most unfavorable external circumstances. Diseases of the skin have also very rarely been observed by them."

### iii. *A Treatment of Mentagra.*

Mentagra, as every practitioner knows, is very intractible, and usually continues to annoy the patient for some time. Legion is the number of remedies recommended and employed, every new remedy pretending to be superior to those hitherto known. If we add another formula to those already extant, it is to give our readers a chance of trying one which is highly recommended in the *Bulletin Général de Thérapeutique*, Paris, June, 1859. The crusts of the diseased parts must be softened and be frequently washed with the following solution:

℞	Sulphate of zinc .....	ʒv;	
	Sulphate of copper.....	ʒiiss;	
	Water.....	ʒiii,	
	Aq. lauroceras .....	ʒv.	M.

\* *Gazette Hebdom.*, Paris, June, 1859.

† *Die Metallischen Krankheiten des Oberharzes*, 1851.



ART. VII. — *Researches on Typhoid and Typhus Fevers*: By BENNET DOWLER, M. D.

POSTPONING for the present, symptomatic, clinical, and post-mortem histories, as well as any positive opinion upon the identity or non-identity of typhoid and typhus fevers, I purpose in the following pages to offer some general remarks, principally on the history, nomenclature, characteristics and prevalent opinions concerning the former malady, without attempting to give a systematic form to this investigation, hoping to suggest without dogmatizing, to show the existing uncertainty and confusion without materially increasing either, and to diagnosticate the evil that one and all may unite in discovering the proper remedy. Which is right? that moiety of the medical world which affirms that typhoid and typhus are identical, absolutely one? or that which says they are different, distinct, two absolutely? Let there be light!

As already indicated, it is not within the humble aims of this desultory paper (which will be composed so as to keep up with the printer), to pronounce judgment in the premises wherein so many gigantic minds have differed; neither is it intended to follow some of the most luminous but extended routes by which conclusions more or less satisfactory might be established; as, for example, the parallelism, affinity, identity, or differentia between typhus and typhoid, as recorded under various names in the too much neglected medical annals of the last two centuries. The candid reader will admit that instruction may be derived from the records and reasonings of past ages as well as from the reports of isolated cases with which the medical press daily teems at present. May I not be allowed to say that, although this is a book-making age, the cause of science would be greatly advanced by the republication of the medical classics of the seventeenth and eighteenth centuries, particularly those of Continental Europe, some of which, it is believed, have not been translated into English?

An impartial and philosophical history of epidemics since the revival of Letters and the Reformation in Medicine, including their topographical, social, hygienic, and other external and internal conditions, connections and dependencies, is, I venture to say, a desideratum. The materials for a splendid edifice exist. The master builder alone is wanting.

The general History of Epidemics by the late Noah Webster, the great lexicographer (in two volumes, published near the close of the

last century), the only extended American work, is an attempt to connect epidemics with the remarkable physical phenomena, particularly the catastrophes (if one may so speak) which have occurred "in the heavens above and in the earth beneath;" such as the planetary, cometary, volcanic and atmospheric commotions; drought, rain, conflagrations, inundations, tornadoes, cold, famine, insects, and other accidental cosmic coincidences in earth, air, and sky, some of which, however, instead of ushering in epidemics served as but harbingers of the death of great personages—a work valueless for its ætiology, but very useful for its chronological and historical data, both cosmical and medical.

M. Ozanam's *Histoire Médicale Générale et particulière des Maladies Epidémiques* (4 vols., 1835), though desultory in narrative, vague in individualization, generalization and criticism, and unsystematic, is, nevertheless, a valuable contribution to medical history.

In whatever point of view typhoid may be considered, there is no good reason to conclude that it is a new malady or of modern date only, as has been virtually assumed, although the group of symptoms which are supposed to characterize it, has been analyzed of late by diagnosticians with an apparent accuracy unknown in the last and preceding centuries. Under various names this disease was formerly described, as *febris stomachali* (Haller, Arnold); *febris mesenterica* (Baglivi); *febris pituitosa* (Stoll); *morbis mucosus* (Ræderer and Wagler); *febris mucosa* (German); *adéno-méningée* (Pinel); *entero-mésentérique* (Petit); *fièvre muqueuse ou pituiteuse*, etc. (See Buserius, Ozanam, and many others.)

"Professor Jackson observes that the advent of the typhoid fever in the United States dates about the years 1817-18. At that time its pathology was unknown. It was supposed to be a form of remittent fever, complicated with a typhus type." (Trans. *Am. Med. Assoc.*, v. 307.)

Although M. Louis ardently sought to find out a suitable name for this disease, and finally baptized it "*affection typhode*," yet the writers of the last century, to go no further back, gave it several names, most of which were more happy and significant both as it regards its symptoms and alleged special pathology. JO. BAPT. BUSERIUS, in his *Institutionum Medicinæ Practicæ* (vol. ii, Venice, A. D., 1782), *Tab. ccclxxiii*, calls this disease *febris gastrica acuta*—a name by no means so significant as some others, which, however, he subjoins in a foot

note. As his work has not been translated from the Latin, so far as I know, the original of his synonymy, will be subjoined without entering into the views of the author.

“Febris a putredine orta ALEXANDRI TRALLIANI, lib. XII. cap. 12. Febris putrida Monspelliensium, FIZESII Tract. de febr. LE ROY second, Mémoir. sur les Fievr. p. 243. Febris putrida quorundam *Recentiorum*. Febris mesenterica BAGLIVII, Prax. Medic. lib. 1. De feb. malig. et mesenteric. § 3. et 4. Febris acuta mesenterica BURCHARDI, Diss. de Febr. acut. Mesent. Febris acuta stomachica, et intestinalis HEISTERI, Compend. medic. Pract. cap. V. Febris stercoralis QUESNÆI, Des Fievr. T. 2. p. 367. Febris intestinalis, et mesenterica a cacochymia atrabilaria RIEDELLI, De Febr. Intestin. § XV. Febris putrida, aut biliosa TISSOTI, Avis au Peuple Premier, Part. chap. XVI. pag. 263.”

Upon the supposition that typhoid fever has a special lesion, which some writers utterly deny, the above terms are more significant of the same and of its chief symptoms than the term typhoid affection, as putrid fever, mesenteric and intestinal fever, and, finally, intestinal fever, the very term (a little abbreviated), which is now daily gaining ground, that is to say, *enteric fever*. Its name, if not legion, is at least sufficiently multiform.

While writing this page, *The Lancet*, and the *Ohio Med. and Surg. Jour.* were received, from which it appears that Dr. Wm. Budd is contributing papers to the former under the head of “*Intestinal fever essentially contagious*,” in which, he says that he prefers to call “typhoid intestinal fever”—which is a simple translation of the *febris intestinalis*—a term more than a century old, given to the same malady.

The latter Journal for September, 1859, opposes the substitution of the name *enteric fever* for typhoid fever, and holds the following language concerning this malady:

“If the student gets the name *enteric* in his mind, he will naturally suppose that the disease is one of the bowels—that the bowels are primarily and constantly affected. *Enteric* (from *enteron* an intestine) relates at once to the bowels; and applied to the fever under consideration, locates it in the bowels. Now, there is no evidence at all conclusive, that typhoid fever has *any special* location. The evidence is just as strong that it has not—that it is purely *idiopathic*; that in the language of the older pathologists, with reference to a cognate disorder, ‘it has its seat every where and no where.’ For fifteen

years we have seen more or less of the complaint, and we may say that we have seen nothing to induce us to believe that structural lesions, of any kind, are necessary as elements of the '*pathos*.' Their existence, when present, has the relation of *effect*, rather than of cause; and it is this fact that explains their diversity, one epidemic having them mostly located in the head, another in the chest, another in the abdomen. Broussais and Louis have made the impression that the disease is not only symptomatic in character, but due to structural lesions of the digestive tube. Andral, however, and others have shown that such is not the case—that we have very many cases of what Louis would call the '*typhoid affection*,' or Broussais, chronic gastro-enteritis, in which there is no lesion whatever in the bowels. Such being the state of our knowledge with reference to *typhoid fever*, we do not see the propriety of changing its name to *enteric fever*—a name that implies not only that it is *symptomatic*, but that it is essentially one of the intestines. We like the term *typhoid* (from '*typhos*,' stupor, and '*eidōs*,' like) the best. If it signifies but little, it is not likely to mislead. Proper nomenclature can only be attained by consulting carefully every element. A term that expresses but an occasional one, or one that, although constant, is insignificant, will amount to little in the advancement of our knowledge of disease."

With regard to the alleged lesions of typhoid, it is an open question, whether the vascularity, injection, hypertrophy, induration and softening of the mesenteric glands, are not equally worthy with the glands of Peyer to give the name to this disease, especially upon the basis of pathological anatomy. By falling back upon past centuries for a name at once broad and characteristic of lesions, provided this fever be assumed to be not idiopathic but local and intestinal, none can surpass that of "*febris intestinalis et mesenterica*." The remittent type probably prevailed more generally in this fever in former times, than at present, or remittent and typhoid may have been oftener confounded.

The writers who flourished a century or two since have, as already indicated, under various names described typhoid fever as it is now called. There is nothing new in it but the name.\*

In tracing typhoid, however, under numerous nomenclatural phases

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\* So new is this term that it is not found in Good's Nosology (1825); Hooper's Medical Dictionary (1832), annotated by Ackerly; Thomas's Practice, annotated by Hossack (1822); Gregory's Practice by Potter & Calhoun (1832); not to name later works.

during the last century, a multitude of non-essential symptoms or rather symptoms common to it and other maladies are detailed and mixed as well as those now deemed characteristic or peculiar: It is described as insidious in its invasion; chills, algidity; heat; debility, uneasiness, lassitude; dejection, mental dulness; eyes injected; headache, delirium, stupor, deafness; trembling (*subsultus*); convulsions; pulse small, hard, quick, intermittent, etc.; respiration difficult, accelerated, etc.; catarrhal symptoms; bronchitis, sore throat; thirst, difficult deglutition, mouth dry, aphthous; tongue at first white, often red, dark, dry, black fuliginous, tremulous; vomiting, præcordial and *abdominal pain*; *colic*; *bomborygmus*, *meteorism*; *diarrhœa copious, mucous, watery, bilious, bloody*, etc.; fœtid sweats, boils, abscesses, supurations of the parotid glands; hæmorrhages, *red or dusky color of the skin, flushed face*; *petechiæ*, chiefly in the second week, often insignificant; also *rose-colored spots*; fever lasting about three weeks, sometimes continued, but often remitting, etc.

The lesions found after death were not wholly confined to the small intestine, but involved the stomach and large intestine, and consisted in the hypertrophy, tumefaction, ulceration or gangrene of the mucous follicles; the mesenteric glands were tumefied, indurated or softened, etc.. The follicular and mesenteric lesions appeared in every fundamental particular, to run parallel with those now witnessed in many cases of typhoid, and now described with more minuteness as occurring oftenest in the lower third of the ileum in oblong patches more or less developed, pustular or ulcerated. The former anatomists, however, did not claim for these lesions the universality which are now unwarrantably claimed.

Upon the whole, the most remarkable difference between the old and the new descriptions of typhoid, appear to consist in the predominance of the intermittent and remittent types, particularly in the early stages. The late M. Chomel, in his work on *General Pathology* (1842), says that, "in Paris where nothing is more rare than essential pernicious fever, *typhoid is distinguished for several successive days, by chills, which may occur at the same hour, and give the appearance of intermittent or remittent type*; but this is only the exceptional course, and after some days the disease resumes the *continued type peculiar to itself*. Of all the affections where continued and prolonged febrile action is observed, *without local phenomena clearly indicative of its source*, typhoid fever is indisputably the most common. The greater part of

those affections described by writers of former ages under the name of essential fevers, are, evidently, as we have proved (*Leçons de Clinique Médicale sur la Maladie Typhoïde*), only various forms of typhoidal disease, since post-mortem examination reveals identical lesions in all these fevers, which are not met with in any other disease. From this fact it follows, that typhoid fever is one of the most frequent affections noticed in our climate, and if we consider that the general symptoms which accompany it are often very grave, *while the local phenomena connected with the anatomical changes are most frequently obscure, and sometimes almost null*, we shall by a comparison of these two conditions, have the natural explanation of the opinion often expressed in our clinical instructions, viz.: that an acute fever persisting beyond the eighth day, and presenting up to that period after methodical and repeated explorations, no indication of local inflammation sufficiently intense to explain the fever, should be considered typhoidal, at least in our climate."

Ozanam says that in 1816, there prevailed in Paris an epidemic (which according to his history of the symptoms, appears to have been the *muqueuse* or typhoid fever), which, as proved by post-mortem examination presented in the intestines, more especially in the lower portion of the ileum, livid or brown spots, together with more or less engorgement, and often suppuration of the mesenteric glands. Although this fever was called *fièvre entéro-mésentérique* by MM. Petit and Serres, it was, according to M. Bouillaud the same *maladie muqueuse* described by Rœderer and Wagler, and by Sarcone in 1760 and 1761, 1764, etc.

About thirty years ago, M. Bretonneau called this disease *dothinteritis*, M. Cruvelheir, and others, follicular enteritis. Bretonneau, however, maintains that the lesions of the ileum or the isolated and agminated glands, is an intestinal eruption which accompanies the malady, and that the malady is not caused by that eruption.

If a special alteration of the mucous follicles in the ileum constitutes the anatomical lesion characteristic of this malady, and if this lesion be primordial and not secondary it would be allowable to name it *intestinal fever* or *enteritis*, as being not an idiopathic but symptomatic fever. The geometrical figure of the lesion whether elliptical or circular, might have been important in the estimation of the iatromathematical pathologists some centuries ago. In Hoffman's volumes on fevers (1746), nearly all of the latter are named according to their supposed anatomical seat, as the stomach, liver, brain, bowels, etc.

The differential diagnosis of typhus and typhoid fevers is one of the problems in febrile pathology not yet solved. The number of writers and teachers who consider these so-called diseases as one and identical is, perhaps, progressively diminishing. French pathologists have been instrumental in building up typhoid fever as *sui generis*. In the British Isles, where typhus is the prevalent fever, typhoid is comparatively infrequent; or, otherwise, it appears to be little recognized as different from the former, although of late the doctrine of the non-identity of these fevers is gaining ground among British writers as well as among those of America. It is probable that the most zealous supporters of this doctrine are now to be found beyond the limits of France, and within those of the United States and Great Britain.

It is remarkable that the very existence and supposed lesions of typhoid fever have been recently wholly denied and unsparingly ridiculed in the capital of European science, where a few years ago this splendid monument was erected to pathology upon a basis supposed to be imperishable, but which was soon attacked as vigorously if not as destructively as was the Bastille in 1789.

M. Cayol, formerly Professor of Clinical Medicine to the Faculty of Paris, not to name others, thus treats the anatomical test of typhoid: "Since autopsies have been made carefully, and especially since late theories have called the attention of pathologists to the intestinal canal, nothing is more common than to find more or less numerous ulcerations of different sizes in the bowels; they are sometimes superficial, and sometimes deep enough to cause perforation of the bowel, a melancholy accident, *always* followed by fatal peritonitis, which is fortunately very rare. These ulcerations may affect the isolated mucous crypts of Brunner, and the agminated follicles of Peyer, as well as other portions of the mucous membrane. They are found not only in subjects dead from acute diseases, but also in those who have succumbed to chronic effects, particularly phthisis. These ulcerations depend upon various causes. Some appear to be primary; others are evidently the result of intestinal exanthemata. They are generally cured easily, where the causes which produced them no longer subsist; at least we must judge so from the cicatrices we find in the bowels taken in connection with pathological facts. All this was public property. Now here is the NEW FACT, in all its immensity. \* \* \* For, except the alteration of the elliptical patches, all the lesions of

the enteric mucous membrane observed in cases of typhoid affection, are found in individuals who have succumbed to other acute diseases; indeed, the frequency with which they are observed in the two orders of cases presents very little difference. For the anatomical fact to be true, it is obviously quite necessary that the alleged alteration of the glands should be found in every case of fever, from the mildest to the most malignant form, and that it should never exist in cases of local phlegmasiæ or other acute non-typhoid diseases.

“We will admit the existence of an alteration of the glands of Peyer in all continued fevers: does it necessarily follow that all these fevers, differing in their causes, symptoms, tendencies, in the accidents they are apt to produce, and especially in their curative indications, are one and the same disease? Yes, perhaps, for the mere anatomist absorbed in the examination of the dead body, and unobservant of the phenomena of the living one, but certainly not for the physician who studies diseases with a view to their treatment and cure. What! because, in all these fevers, you suppose a lesion of the elliptic patches, do you think that inflammatory, bilious and nervous symptoms are no longer to be taken into account? Will these various symptoms represent the same condition of the organism in your eyes; will they not call your attention to one organ suffering more than another, or aid you in the choice of a remedy? Will you keep your eyes stupidly fixed upon the *plaques elliptiques*, and do nothing for fevers until you have discovered some specific for the lesion of these glands?”

Among even the most competent and truthful historians of diseases, there is a bias more or less prevalent which influences them to select not the average typical forms, but the more salient and strongly developed examples of morbid action and anatomical alteration for the purpose of illustrating, fixing, and making them known, omitting unintentionally the less prominent, more numerous, and often doubtful—doubtful relatively to the human understanding. Such descriptions so apparently clear and striking being more easily understood and remembered, are, consequently more acceptable than descriptions less obvious, and less easy of comprehension. In actual practice, the physician will often find it difficult to level upward or downward so as to realize written or pictorial descriptions either at the bedside or in the dissecting room.

For example, let the student accustomed to the examination of dead



bodies turn to the splendid colored engravings of M. Cruveilhier's *Anatomie Pathologique* illustrative of follicular enteritis (typhoid), and he will, if long experienced in the post-mortem appearances of this malady, recognize in this picture (plate 4) its varieties, all of which are seen sometimes in the same cadaver, namely: 1, *la forme granuleuse et gaufrée*; 2, *la forme pustuleuse*; 3, *la forme ulcéreuse*; 4, *la forme gangreneuse*;\* but the probability is, that he may have performed many, perhaps dozens of dissections (a number rarely witnessed by a practitioner) in typhoid, before he will have encountered one fully responding to those of this eminent savant, and the *artiste* whose care, skill and genius for gorgeous embellishment, this engraving displays.

The hypertrophy or ulceration of the Peyerian patches occurs in protracted diseases, especially in fevers, diarrhœa, cholera, consumption, etc. Their development is by no means peculiar to typhoid fever, and even in this malady, they, in a majority of fatal cases, may be absent or so slight as to appear insignificant as an exponent of its nature and pathological anatomy. If this fever really had a local origin in hypertrophy, or ulceration of the mucous glands of Peyer and Brunner, it would be extremely difficult to prove the fact anatomically several weeks later, that is, after death from this disease, which is a very protracted one.

A quarter of a century ago the fevers (in the vague nomenclature of that period) termed autumnal, winter, typhus, etc., were not characterized by either the remittent or intermittent types, at least in Western Virginia, but I have no doubt that both of these types are now met with in the typhoid of Southern States of this Republic. In such cases especially, it is supposed, that the exhibition of quinine is virtually a collateral test of the type as well as often efficacious in the cure, notwithstanding the positive affirmations of some writers, and the insinuations of others, that no case of typhoid can be cut short by any remedial means whatever. Setting aside the positive assertions to the contrary, based on the experience of all times and places, there is much analogical evidence amounting to strong probability, that there is no fever foreordained to run a definite course so as not to admit in any case of being controlled, and cut short by the healing art.

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\* In his elaborate Treatise on Fevers, M. Boisseau (1830), says of these plates: "In four plates of remarkable accuracy, M. Cruveilhier has represented the varieties of alteration occurring in the small intestine. I would call the attention of my readers, and particularly of physicians who possess but limited opportunity for post-mortem examination, to this work, remarkable for the exactness of the descriptions and drawings." (215.)

This hypothetical fatalism is not only discouraging, but is a negation which can not be proved.

Febrile nomenclature, as already stated, was a quarter of a century ago, variable and vibrated from the general term fever, to typhus including typhoid, to bilious, inflammatory, autumnal, winter, etc. The term typhoid, and its alleged pathognomonic symptoms and anatomical characters were little if at all alluded to in medical books or lectures. But the observer who then faithfully recorded his cases, may, perhaps, nevertheless, have been able by the lights of notes, subsequent experience, and the progress of science, to distinguish among them the features of many cases which would now be called typhoid and figure in its medical and mortuary statistics, whether such classification be a valid one as establishing this as an idiopathic fever, or as a mere phase of typhus.

Fracastor, who wrote the history of the malignant fever (typhus) which prevailed in Italy, in 1505 and 1528, mentions, in his account of that fever, the most marked symptoms of typhoid, and says that those which were the most indicative of danger were, great debility, copious colliquative diarrhœa, hæmorrhages, livid petechial points and spots, purpura, etc. Other writers of the sixteenth century call the diarrhœa of typhus colliquative, and mention tympanites, or meteorism, petechial points (vibices) and spots of larger size and different hues. Subsequently, for three centuries the fundamental symptoms of typhoid often appear in narratives of epidemic typhus; but the accepted significance of these symptoms was various, often hypothetical. Thus, the colliquative diarrhœa was interpreted by some as favorable—being a critical discharge—a throwing out of vitiated humors, etc., by others unfavorable, always aggravating the disease: The lesions most frequently noticed were in the head, or in the bowels; and here is an example of the latter class: The great epidemic typhus of 1754, which ravaged the city of Rouen, was attended with either bilious or serous diarrhœa, tumefaction (meteorism) of the abdomen, epistaxis, purpural eruption, etc. The following are the post-mortem appearances taken entire from Ozanam: “L’ouverture des cadavres fit voir une partie de l’estomac et des intestins grêles enflammée, le surplus couvert d’une éruption miliaire cristalline, les glandes mésentériques tuméfiée, les gros intestins distendus par des gaz. Dans ceux où le délire avait été long, l’estomac avait des ulcérations gangreneuses, le cerveau était sain.” (iii, 158.) Has any modern epidemic

of typhoid exhibited its alleged lesions (here virtually described) in degree or extent comparable with those of the typhus of Rouen?

The inherent difficulties, the masked characters, and inconstancy pertaining to morbid phenomena, are often so great, that the observer is compelled to doubt the nature, seat, and classificatory position of individual cases. The march of a disease, its termination in recovery, or in death, with, in the latter event, the additional light resulting from post-mortem examination may still leave the medical investigator occasionally, nay often, in doubt whether the case should be classed with typhus or typhoid—with remittent or yellow fever, etc. The certificate of the cause of death written thus doubtingly by the physician is, at most, presumptive, not positive evidence.

A considerable number of patients get well or die, not of one, but of a complication of diseases, or of such as are intercurrent or secondary, and no one can positively define the precise rôle of each, nor which determined the fatal result.

Who among the competent, has not sometimes felt an embarrassing dubitation in writing a mortuary certificate, particularly when the cause of death has been from ill-defined fevers? Hence the records abound with non-specified fever (and what is no better) with country fever, jungle fever, swamp fever, lake fever, Panama fever, Chagres fever, coast fever, stranger's fever, bone fever, pernicious fever, winter fever, fall fever, ship fever, etc., to the great scandal of nosological nomenclature, diagnosis, and medical statistics, much of which, however, is due to incompetency or absolute indifference on the part of medical men.

Post-mortem examination though of the highest importance, is often incompetent to teach the primary character and lesions of a malady, especially of one that has been chronic. Anæmia, hypertrophy of the spleen, and dropsy, so often the attendants of intermittent cannot be always, if ever viewed, as the primary and peculiar lesions of that fever but as sequelæ, or accidental eventualities.

If by any accident, as from explosion, war, or the falling down of a hospital, a dozen of patients diagnosticated as being in the first or second day of typhoid fever, should be killed, and be examined after death, probably no definite lesions of the "elliptical plates" would be found, a supposition not altogether improbable, and in such event, could this fever be justly referred to these plates in the absence of any alteration? Under such circumstances, there can be little doubt

that dysentery, peritonitis, croup, pneumonia, or pleurisy, would exhibit more or less anatomical evidence of the morbid changes usually attributed to, and characteristic of, these affections, even in their inception as well as in their full development, or fatal termination.

The failure, however, of the anatomical test does not necessarily imply, much less prove, the non-existence of typhoid, more especially if it be considered as an idiopathic fever. On the other hand, if it be a primordial dothinteritis, that is, as Bretonneau would say, a pustular exanthem, inflammation and ulceration of the follicles of Peyer and Brunner, the anatomical test cannot be dispensed with in any case, the fever being of local origin, symptomatic, secondary, sequent. The defenders of typhoid upon the special, local and invariable anatomical lesion which they have fixed as the basis of their pathology, are in conflict with observers equally able, and, perhaps, more numerous. There are some diseases, the distinctive characters of which must be chiefly drawn from symptoms, with little aid from post-mortem investigation. If the differential diagnosis of typhoid is often doubtful, it is not the only disease in that category.

The statistics of typhoid fever together with the ratio of its prevalence and mortality as compared with other fevers is, in a great degree conjectural, owing greatly to a bad nomenclature, and especially to the common belief that it is identical with typhus. This belief written and unwritten, whether true or false, exists in a practical point of view in the nomenclature of mortuary statistics, so that typhoid is called typhus, and typhus, typhoid, thereby creating confusion which every earnest and competent observer should endeavor to remove, if that be possible, as the present state of uncertainty is scarcely tolerable not to say disgraceful. If typhoid be a distinction without a difference, as has so often happened in nosological classifications, it is high time that it were expunged from the medical vocabulary. The mere possibility of its real existence, so long as its positive or differential diagnosis is unknown, would require its amalgamation with typhus. Judging from mortuary statistics, it is scarcely possible now to differentiate typhoid from typhus any better than it was more than a century since, under the varied nomenclature already mentioned.

If the old names and the new had been replaced with that of typhus mitior or gravior as the title of the treatises and monographs

on, and reported cases of mucous, mesenteric, intestinal, typhoid, etc., would they not generally pass current as varied phases proper of typhus, notwithstanding the occasional prevalence of diarrhœa, "lenticular" petechiæ and follicular lesions in some epidemics or sporadic cases? Does not the history of medicine, especially that part relating to nosology, show that names, though evanescent, have been all powerful? Is it not now an admitted truth, that the most gigantic minds have always failed in establishing a scientific, acceptable and enduring system of nosology? and that such a system is either beyond the reach of the human intellect, or that pathological phenomena really are in themselves or in their perceived relations to the senses, so variable and inconstant that they cannot be classified upon a permanent basis, as in natural history or physics?

Without admitting with Juvenal, that there is nothing a man will not believe in his own favor, it may be assumed that each physician thinks himself a diagnostician. If this pretension is not true of all, it is of many. If typhoid and typhus be distinct and distinguishable diseases, it is reasonable to suppose that the mortuary statistics of a country in similar latitudes, climates, topographies, and conditions, would afford data of great significance for determining this question. Admitting what writers on typhoid suppose, namely, that typhoid is now more or less prevalent generally in the United States, it is important to refer to the mortuary records, which, independently of their bearings on this subject, are always suggestive and instructive, and will not, therefore, be deemed intrusive or irrelevant to the subject under consideration. Although memoirs and cases of typhoid and typhus fevers are frequently reported in the medical journals, the most elaborate being from the pen of an able diagnostician, Professor Austin Flint; yet these are but the individual observations of a few—one in the thousand. Now, the identity or non-identity of typhoid and typhus may not be in itself at all doubtful. The doubt may be due to the imperfection of our knowledge—in the inability of observers, however able, to give satisfactory and indisputable criteria, whether on the affirmative or negative sides of the question. Hence the question is virtually one of degrees of probability, and therefore more amenable to many than to few observations.

The modern progress of diagnosis in some diseases is unquestionably great, in others inconsiderable. But it is not intended to inquire what *might*, or *ought to be*, but what *is*; and with this view it may be

useful to offer a few data which will not be selected to prove the identity or non-identity of typhoid and typhus, but to show that they are generally not distinguished in mortuary statistics.

Upon what principle can the following data, taken at random from the most accessible documents, be explained? Have the physicians of some States tacitly abandoned all distinction between typhoid and typhus, calling both by one or the other name indifferently? Is it credible that in some of the States, especially the maritime States, many hundreds should die annually of typhoid and not one from typhus or ship fever, while in other contiguous States many hundreds die of typhus and none from typhoid? If the diagnosis of these fevers were obvious, clear and satisfactory, as in consumption, cholera, scarlatina, yellow fever, intermittent and the like, would such anomalies occur in the mortuary statistics upon a scale so extensive? Laying aside the popular names which are very numerous and often different in different districts—names which may be reduced, perhaps, to one-fourth of the existing number, it may be assumed that diseases whose diagnosis is clear and salient, will generally have the same or a synonymous nomenclature among medical men at least.

The Mortality Statistics of the United States, for the year ending June 1, 1850, show an aggregate of 323,023 deaths from all causes—from all fevers, excluding scarlatina, 38,339.

The fevers not specified give the following list: Alabama, 585; Arkansas, 279; District of Columbia, 10; Connecticut, 223; Delaware, 13; Florida, 101; Georgia, 693; Illinois, 615; Indiana, 684; Iowa, 156; Kentucky, 986; Louisiana, 748; Maine, 363; Maryland, 139; Massachusetts, 489; Michigan, 231; Mississippi, 758; Missouri, 601; New Hampshire, 262; New Jersey, 163; New York, 799; North Carolina, 484; Ohio, 1,479; Pennsylvania, 1,015; Rhode Island, 55; South Carolina, 555; Tennessee, 834; Texas, 313; Vermont, 194; Virginia, 519; Wisconsin, 123; Minnesota, 2; New Mexico, 177; Oregon, 6; Utah, 11; California, 81; the aggregate of which is 12,776, being nearly one-third of the whole mortality from fevers, having no specified character other than simply, fever.

Deaths from typhus and ship fever for the same year and from the same official source: Louisiana, 807; Texas, 66; California, 11; Arkansas, 39; Mississippi, 257; Tennessee, 180; Georgia, 557; Florida, 9; North Carolina, 335; Virginia, 747; Kentucky, 814; Maryland, 365; Pennsylvania, 751; Delaware, 41; District of Columbia, 151;

Rhode Island, 51; New Jersey, 114; New York, 1,172; Connecticut, 151; Vermont, 74; New Hampshire, 124; Massachusetts, 572; Maine, 289; Ohio, 766; Indiana, 384; Illinois, 293; Michigan, 58; Missouri, 457; Iowa, 51; Wisconsin, 111; Minnesota, 13; Utah, 1; Oregon, 5; New Mexico, 13. Alabama, none! South Carolina, none!

Now, it is difficult to believe that Alabama and South Carolina should not have had a single death from typhus, while the conterminous or adjoining States had so many, as Georgia, 557; Mississippi, 257; North Carolina, 335; Tennessee, 630. On the other hand, South Carolina had 410 and Alabama 323 deaths from typhoid, while, as already stated, these contiguous and intervening States had none.

The Legislature of South Carolina has, within a few weeks, published a statistical work of ninety-eight pages, compiled by the State Registrar, Dr. Robert W. Gibbes, Jr., for the year 1857, showing an aggregate mortality for that year namely, 11,657. The deaths from typhoid fever amounted to 707 (or 1 in 14.41 of the total mortality); congestive fever, 117; remittent, 110; intermittent, 6; yellow fever, 13; *typhus*, none; fever not specified, 226. Thus it appears that, with the exception of pneumonia,\* typhoid is the most fatal disease of the year, exceeding even consumption, as 2.47 to 1 of the latter. Here, eight years after the taking of the United States census, the State census seems to confirm the former, while the Registrar of the city of Charleston, for the same year (1858), reports no death from typhoid, but reports 39 from typhus, 19 whites and 20 blacks.

According to the official returns of the United States' Mortality Statistics, for the year ending June 1, 1850, the deaths from fevers specified and non-specified, amounted to 48,099—a calculation I have taken much trouble to make by counting the returns for each State and territory, which gives a mortality from fevers as 1 in 6.66 to the whole mortality of the year. The non-specified fevers amount to 18,108 (which is not a flattering aggregate for the science of diagnosis). The returns from the individual States give an aggregate of only 733 deaths from typhoid, all of which were in South Carolina and Alabama, and not one elsewhere. Nevertheless, the government, "in condensing the State tables from the returns of the marshals, according to the *best authorities*," has given a compendium in which the science of diagnosis and medical statistics fare badly; for, the first

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\* Pneumonia caused 1,052 deaths; nearly one-eleventh of the whole. Total deaths, 1,922; or 1 in 26.14 for the whole population.

table in which all the State returns are summed together, typhoid grows from 733 to 18,099, and ship [typhus] fever dwindles to 240.

No explanation of these strange diagnostic and statistical metamorphoses is given by the census office. A learned physician, formerly of New Orleans, was, I believe, employed as a clerk in the preparation of these Mortality Statistics of the United States, and it appears on the face of this book, that others were consulted by letters as to the nomenclatural, and classificatory construction of the compendium or general table, and it is, therefore, more than probable, that the government officer concluded from what he believed to be "*the best authorities,*" that typhus was identical with typhoid, and that the returns of typhus from each State should be merged in the latter in order to insure accuracy. Omitting non-specified fevers, yellow fever and scarlatina, all non-typhoidal fevers amount to 19,406, exceeding typhoid by the inconsiderable number of 1,361. Now, it will be seen by the individual State returns which I have reproduced above, that the mortality was generally very considerable from typhus in the States, reaching in some 760—747—807—814—1,172, etc.

No satisfactory explanation of these strange nosological, nomenclatural, diagnostic and statistical metamorphoses is given with the exception of one offered in the last page of the work, where it is stated that "the terms found upon the returns of the marshals, and appearing on the State tables, or [are] condensed under other terms according to the *best medical authorities,*" "which appear in the United States tables." For example, "spotted, synocha, synochus and typhoid, are enumerated as "*fever, ship!*"

The only medical authority appearing on the face of the book itself, is that of Dr. Edward Jarvis, of Massachusetts, who was consulted by the government upon the Classification of Diseases for the United States Mortality Returns. His letter, dated September 22, 1855, is accepted and published in an appendix to the work. He instructs the census Bureau as follows: "19. *Fever, bilious,* is a most vague and uncertain term. In some regions it is used for all sorts of fevers, etc. 20. *Gastric fever* is probably intended for inflammation of the stomach. 21. *Winter fever* may mean bilious, typhus, or synochus. 22. *Congestive fever* is merely a form or phase of typhus," and so on to the 56th instruction.\*

\* One object in analyzing this work, the first attempt of the United States Government to give the mortality statistics of the nation, is to call attention to errors, that they may be obviated in the decennial census which will be taken in 1860. The importance of *correct* medical statistics cannot be easily overrated.



Dr. J. Wynne reported to the *Am. Med. Association* (ii), the causes of death in Baltimore, for thirteen years, 1836 to 1848, inclusive, which, on being summed up, give for fevers, excepting scarlatina, the following figures: bilious, 355; catarrhal, 1,192; congestive, 66; gastric, 194; intermittent, 59; nervous 9; remittent, 6; typhus, 327; ship, 113. To this aggregate of 2,321 deaths from fevers, typhoid contributed nothing.

Philadelphia, the next door neighbor to Baltimore, diagnosticates typhoid and typhus in the following figures: "Deaths from typhoid fever for the years 1848—eighty-one; 1849—one hundred and twenty-eight; 1850—ninety-four; 1851—one hundred and fourteen." During the same years respectively, the mortality from typhus in that city, was, 205—117—75—85. (*Trans. Am. Med. Asso.*, v, 308-9.)

Dr. J. Curtis, in his report to the *Am. Med. Association* (ii) gives the mortality in Massachusetts for 1846-7-8, from typhus, which also *includes typhoid*, in the following figures, 2,826; for 1848, 1,202, or twelve per cent. of the whole mortality; in Boston, 1846-7-8: intermittent, 1; remittent, 0; typhus, 1,093. Here typhoid is either ignored or merged in typhus.

The object in view in giving these data, is to show what *is*, not what the refinements of learned nosologists and infallible diagnosticians say *ought to be*. Is the diagnosis of fevers as exhibited in authors of the last two centuries less exact than that of the present day, as exhibited in documents emanating from our State and city authorities and medical publications?

It will be seen in the following paragraph, that in the voluminous reports and mortality statistics of the medical officers of the United States Army, for thirty years, that typhoid fever is wholly unrecognized:

Nomenclature of fevers as adopted in the Army of the United States: *Febris continua communis*; *febris intermittens quotidiana*; *febris intermittens quartana*; *febris remittens*; *febris typhus*; *febris typhus icterodes*. This nomenclature has obtained for thirty years, with the exception of typhus icterodes which had no separate place assigned it in the earlier reports. The eruptive fevers form a distinct class in army statistics, thus: erysipelas, rubeola, scarlatina, variola, varioloid. This official classification, for brevity and simplicity, forms at least a strong contrast to that of medical civilians, but whether it can be accepted by the latter as sufficiently comprehensive for the require-

ments and exigencies of febrile nosology for civil practice let others decide.

From a wish not to extend this article, further mortuary statistics illustrative of the actual nomenclature of the large cities of the United States will not be reproduced.

The mortuary statistics of the South show the prevalence of typhus, a disease which has been, heretofore, generally regarded as being almost wholly confined to countries having a low temperature. Formerly, it seems to have been comparatively rare in hot climates. Its increase in the great maritime, commercial cities of the South, is due chiefly to the influx of recent immigrants. This fact, perhaps, may serve in some degree to separate it from the so-called typhoid which is supposed to be a disease of temperate and warm climates, being repelled by cold; but this opinion, perhaps never sound, is losing ground, but whether owing to the influence of a more accurate diagnosis, nomenclature and statistics remains more or less questionable.

Dr. Griscom, of New York, in a communication to the Special Committee of the U. S. Senate (Jan. 14, 1854), says that "in 1852, there were treated in the Marine Hospital, on Staten Island, 3,040 cases of ship fever, of whom seventeen per cent. died. These were all emigrants" [immigrants].

Immigrants during the last fifteen years have been almost exclusively the subjects of typhus, although the disease never has spread among the citizens of New Orleans. From the foundation of the city it appears never to have prevailed even in its jails.

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## PROGRESS OF MEDICINE.

### ART. I.—*Diphtheria*.

#### i. *Diphtheria*.—By ROBERT S. CROSS, Esq., Petersfield.

I beg to submit the annexed remarks, with the idea of fulfilling one great purpose of our Association—the recording of individual observation, with a view to building up experience deduced from facts.

Throughout these last eighteen months, this town and neighborhood have been visited by throat-disease in various forms; considerably

above a hundred cases, of a diphtheritic character, having occurred under my observation, varying in character from the mild form, in which small spots of white easily detached lymph or exudation membrane, on more or less enlarged and inflamed tonsils, was the type; little or no constitutional disturbance being present; to the most severe kind, suddenly ushered in by severe symptoms of exhaustive febrile action; the tonsils, uvula, and whole of the parts at the back of the mouth as low down as could be seen, were coated with a dirty white or brown grey, firmly adhering membrane; life being destroyed in a few days, sometimes by extension to parts necessary for respiration, but as often by apparently a state of exhaustion of the system being induced, in which death occurred sometimes very unexpectedly.

Two cases (in one family, of six children, the oldest eleven years, and all of whom, with the exception of the infant, had the disease) were sufficiently remarkable to deserve a separate notice. In two boys, aged respectively eight and nine, the throat symptoms were very severe from the first. About the fifth day healing set in, so rapidly, that by the end of the seventh, a sloughing ulcer, which had embraced the whole of the parts at the back of the mouth within the reach of observation were quite well. One day only of apparently perfect convalescence intervened; and vomiting set in, with pain and tenderness over the epigastrium, extending particularly to the hepatic region. There were constipation; total loss of appetite; prostration; rapid emaciation; thirst; and, in one case there was intense pain in the head for the last forty-eight hours. One boy died at the end of three weeks of such condition; the other, at the end of ten days.

A very careful *post mortem* examination of the last case was made. The tonsils appeared gone; *i. e.*, their site was occupied by some loose irregular granulations, which had the character of remaining, the interstitial structure being absent. There was no extension of the disease into either œsophagus, or larynx and trachea. The lungs were healthy. The liver was enlarged and pale. The stomach contained about half a pint of greenish viscid fluid. The mucous membrane, particularly at the edges of the rugæ, which were very prominent, was of a deep chocolate, almost mahogany color; the color was not distributed in patches, although there were degrees of depth of shading, varying from this to deep rose color and red, but pervading the whole and extending a long way—I should say, throughout the whole of the small intestines, most intensely in the duodenum. The mucous membrane was softened and thickened, and presented at the two orifices an appearance of a separation, as though it might readily be detached.

The disease in question has occurred in all situations and among all classes. The mortality has been about ten per cent. of the whole. This ratio, I should remark, includes as well those cases in which application has been made too late to offer any reasonable chance of doing good, as it does those in which everything has been done from the very first moment of attack. As to contagion, I have been unable clearly to trace this cause in more than one case; as in those families where two, three, or more children were the subjects of the disease, the cases have occurred at such intervals as to preclude the idea of

such being the exciting cause; and, moreover, often only one has had it, even where the discovery was made too late for any attempt at isolation. One adult has died; and one who had it, had been nursing a child with angina, she herself having been the subject of scarlet fever since adolescence. Albumen has been sometimes present in the urine; sometimes not. I should say, that on the average the subjects of the malady had not been remarkable for their apparent vigor of constitution, but rather the least robust and healthy of the community.

The treatment has consisted of sulphate of zinc or ipecacuanha emetics, with a mild calomel purge, followed by mineral acids, with bark or quinine; sometimes citric acid, chlorate of potass, tincture of sesquichloride of iron, etc.; with applications of nitrate of silver, solid and in solution (five grains to one drachm); solution of chlorinated soda, hydrochloric acid, etc., diligently applied by sponges, and gargles of like character; externally, an embrocation of compound camphor liniment, soap liniment, and laudanum. Leeches were applied in one case, followed by a blister, which caused a most unhealthy sloughing sore, which undoubtedly accelerated the end. And here I may mention one unfavorable symptom always attendant on the severer forms, *i. e.*, a filling up from ear to ear under the chin, with a hard, tense, brawny kind of shining swelling, corresponding to a similar infiltration of all the internal parts.

The diet has consisted of beef-tea and nutritious bland nutriment, with wine, according to its apparent necessity. On the first and second day, if given, wine appeared to aggravate the constitutional irritation already severe; and in like manner, about the third day, when the exudation membrane was beginning to separate, allowing the escape of a highly offensive discharge. Caustic or any pungent application to the throat seemed to do harm from the same cause.

ii. *Diphtheria*.—By GEORGE BOTTOMLEY, Esq., Croydon.

Many valuable records of cases have appeared almost weekly in the medical periodicals for months past, mostly differing from each other as to the nature of the epidemic, as well as to the mode of treatment. It would, therefore, be highly desirable that some better defined pathological character of the disease should be agreed upon by the medical profession, and also a general plan of treatment laid down, as in all other diseases.

It appears that, at present, we cannot do more than give our individual opinions, founded upon practical experience.

In the first place, it may be remarked that the disease assumes very nearly the same train of symptoms in all places.

From what has been written upon the subject, it appears that some believe it to be a new disease; others, that it is accompanied with scarlet fever. It therefore becomes our duty, if possible, to find out which view is correct.

In discussing the nature of this disease, its pathological character should be settled; but, having had but one fatal case in my own practice, my experience from *post-mortem* examinations will not do much. I can, therefore, only offer my opinion from the cases I have had under treatment.

It appears to me that at the commencement of the attack there is but slight congestion of the mucous membrane of the pharynx, accompanied with slight constitutional disturbance; but, in a few hours, the membrane puts on a livid appearance, and runs rapidly into the gangrenous state; and that the false membrane is a deposit of layers of lymph in the early stage of the disease, which soon loses its vitality, and acts as an extraneous body, thereby preventing the parts from performing their natural functions. Accompanying this change, great depression of the vital powers of the system takes place.

Now, whence arises the rapid and fatal change in a few hours? Is it not from meteorological causes? for it is certain that locality has but little to do with it. It has been as severe in high and dry situations as in low and damp; in isolated dwellings, and in crowded cities; in clean and well drained places, and the reverse. Therefore, ought we not to look to the atmosphere as producing those rapid changes, acting upon the blood, occasioning the gangrenous condition of the throat, accompanied with an extremely low and depressing type of fever, from which the patient rapidly sinks into a state of extreme and fatal exhaustion? It therefore appears consistent with pathology to administer a most powerful antiseptic remedy, to act promptly both locally and constitutionally. After taking the above view of the disease named by Bretonneau *diphtherite*, I beg to submit to my medical brethren the plan of treatment which, I am happy to say, I have found very successful.

I had under my care several severe cases in the autumn of 1857, and again in the autumn of 1858. Those in 1857 occurred in the town and neighborhood of Croydon, and those in 1858, in a new building, an asylum for the reception of children whose parents died young; consequently their offspring were not constitutionally the best fitted to resist so formidable a disease.

The treatment I adopted in all cases under my care was as follows, for *children*: R Solutionis chlorinii, ℥ss; syrugi simplici, ℥ss; aquæ destillatæ ad ℥vj. M. Fiat gargarisma sæpe utendum. R Solutionis chlorinii, gtt. iv; syrugi aurantii, ℥j; aquæ destillatæ ad ℥ss. M. Fiat haustus 2ndâ quâque horâ sumendus.

The dose was increased according to age. Calomel was given in doses of one grain and upwards, according to age. The diet, too, consisted of concentrated jellies, strong beef tea, wine, etc.

The same mode of treatment was adopted in adult cases; except that, instead of calomel, I gave the hydrargyrum cum cretâ.

For many years I have ordered the chlorine solution in malignant scarlet fever accompanied with a diphtheritic state of the throat, with marked success. In the *Cyclopædia of Practical Medicine*, published in the year 1835, Dr. Tweedie states that, in an epidemic sore-throat which made its appearance in Tours, in 1818, and to which Bretonneau gave the name of *diphtherite*, hydrochloric acid was found most efficacious.

The orphan asylum to which I have before alluded is a new building erected on the top of a hill, open to the South-west; the rooms spacious and lofty; every attention paid to ventilation. It stands alone, in a most healthy situation; and the children, before

admission, undergo a medical examination; so that, at the time of admission, they are perfectly healthy. So that does away with the supposition of its being confined to ill-drained, low, and swampy situations, and densely populated, poor localities. In the months of July and August, 1858, there were fifty cases in the above asylum, of the most malignant form. They were all placed under the treatment before named. After the third day, quinine was added to the chlorine solution. Many were covered with the scarlet eruption; eighteen had a severe form of measles; and some few were free from eruption; but all had the diphtheritic throat, and but one case out of the fifty proved fatal—a child four years old. The disease made its way through the various tissues of the throat and neck, and broke externally, giving vent to a most offensive discharge, the patient sinking from exhaustion, not being able to take sufficient stimulant and nourishment.

iii. *Diphtheria*: By J. C. S. JENNINGS, Esq., Malmesbury.

There are a few points in the summary of cases published in the *British Medical Journal* of June 25th, to which I wish to advert, while completing a tabulated report of cases of diphtheria attended by me for the last two years, although I can, in truth, add but little as regards its treatment to the outline which I gave in the *Journal* of March 27, 1858.

*Presence of Contagion.* Of this I have had such clear proof ever since the first outbreak of diphtheria in the parish of Shipton Moyne, that I have had no hesitation in declaring its contagious character; in fact, in the first case which came under my care in that parish, the disease was clearly contracted by nursing and attendance on an adult who rapidly sank under it. Moreover, two of his brothers-in-law, residing a mile distant, contracted the disease either from a casual visit, or by attending the funeral.

*Scarlatina.* At the first outbreak of the disease, no cases of the above had appeared in the neighborhood; nor were there any until the second outbreak, during the month of January of this year, when a few cases of diphtheria occurred; but scarlatina maligna ran through several families. In those cases, however, in which the rash was well developed, and not suppressed, there was little or no throat affection; and *vice versa*; and when the tonsils were affected, there was not the peculiar leathery exudation of diphtheria.

*Treatment.* The plan I have invariably adopted, regardless of age, sex, or incubation of disease, has been to give an active emetic of antimonial wine, from half an ounce to an ounce, according to age; to freely cauterize the throat with solid nitrate of silver; to have a mustard poultice applied from ear to ear; the feet and legs plunged in a hot bath, and the patient confined to bed. After the emetic action has ceased, from three to five grains of calomel with five of compound extract of colocynth were given (or, for a child, two grains of calomel with two grains of compound antimonial powder); and, four hours afterwards, the following mixture:  $\mathcal{R}$  Quinæ disulph.  $\mathfrak{zss}$ ; potassæ chloratis  $\mathfrak{zj}$ ; acidi hydrochlorici diluti  $\mathfrak{zss}$ ; aquæ  $\mathfrak{zviij}$ . *M.* Fiat mistura cujus sumatur pars sexta 4tis horis.

A gargle of chlorine solution was directed to be used frequently,

prepared by impregnating water as much as can be borne with the protoxide of chlorine, generated from two parts of chlorate of potass, one of hydrochloric acid and one of water, and the fauces to be sponged out frequently with the same. The emetic I have rarely repeated more than once; but when the inflammatory stage has been severe, the fauces tense and shining, and the throat œdematous, spirit of nitrous ether and liquor of acetate of ammonia, or nitrate of potass, has been added to the mixture.

The diet has been at first farinaceous, and afterwards consisting of strong broths and jellies. Stimulants have been very rarely administered, and then only as sherry whey, alternately with the quinine, which I have trusted to as the sheet-anchor. For infants, quinine may be given in jelly, washed down with a mixture of tincture of sesquichloride of iron.

Upon this plan all my cases have been successfully treated, with the exception of one fatal case in a stout young man, where much valuable time had been allowed to elapse; the mushroom-like exudation having extended over the soft palate, completely blocking up the fauces and chink of the glottis; besides which, the emetic was not rightly administered, or failed to produce effect; neither was the purgative given as ordered, and the patient died from apnœa, suffocation being produced from spasm of the glottis when turning to lie down in bed.

Too much stress cannot be laid upon tartar emetic, quinine in large doses, and the avoidance or guarded use of alcoholic stimulants.—*Brit. Med. Jour.*, July 16, 1859.

iv. *Diphtheria*: By THOMAS HECKSTALL SMITH, Esq.

In the year 1857, I had in my practice three cases of diphtheria, each a mile or two miles distant from the other. They were sporadic cases: no others occurred. All were in the better ranks of life. One was a governess in the family of a clergyman; one was the wife of another clergyman; the third was a gentleman of rank well known in the county. They presented three distinct characters of the disease.

CASE 1 was that of a delicate young lady, aged 23, subject to relaxed sore-throat. There were many patches, somewhat elevated, on each tonsil, as if greyish peas had been split and pressed into the part; the surrounding parts having a blush only of red. There had been no rigors; the pulse was quiet, very feeble; the countenance and skin were anæmic.

CASE 2. A healthy lady, aged 30, of somewhat full habit, but who once had erysipelas, presented the membrane formed on both tonsils, the velum, part of the tongue and cheek, of a deep ash color. On removing a portion of the membrane, which was loose, from the tonsil, the parts under were found to be nearly of chocolate color—so intense was the congestion. The breath was foetid; there had been slight rigors; the pulse was now quiet, easily compressed.

CASE 3 was that of a healthy man, aged 53, with some tendency to gout. In this case, both tonsils were partially covered with patches, but with acute tonsillitis, one tonsil ending in quinsy and suppurating.

These cases present a fair type of three of the forms in which the disease has shown itself as an epidemic in my district during the latter half of the year 1858, up to this date (May, 1859). Up to this time, I have had under my care upwards of eighty well marked cases; excluding many others, to which I shall presently allude, of a less distinctly marked character. These cases have occurred at all ages, extending from two weeks to fifty-five years; not beyond that age, except in one case. They have occurred in all ranks of life, but in a very much larger proportion amongst the middle and upper ranks than amongst the poor.

The exemption of the pauper class is remarkable. I have medical charge of a large district, and also of the union house. Out of three hundred and fifty recorded cases of illness in the out door poor, only eighteen were diphtheria; in the union house there were only two cases.

The cases have been quite unconnected with scarlet fever; of that I speak confidently.

Some cases of erysipelas have occurred during the epidemic of diphtheria; many of urticaria; an unusual number of eczema; several severe and unusual cases of pompholyx, or pemphigus; many of pleuropneumonia, of an asthenic type; very many of neuralgia, of an intermittent type; and more cases of ague than I have seen in the district before at any time during a period of thirty years.

The furunculoid epidemic, with whitlows, thecal abscess, and necrosed bone, which had gone on during the three years previously to the advent of diphtheria, almost entirely disappeared when the latter epidemic commenced.

I have already described three forms in which the disease presents itself, viz: simple ash-colored diphtheria membrane in patches, with very slight congestion of the surrounding parts, and without fœtor. Secondly, a deeper color, and more widely spread membranous exudation, with fœtid breath, and intense engorgement of dark hue. Thirdly, the membrane with much tonsilitis, in a few cases resulting in quinsy. But there has been a fourth and more formidable state of things to contend with, namely, an extension of the membrane in either of the above forms, to the larynx and trachea, the symptoms of which I need not describe. In the three cases of 1857, and in the earlier of this epidemic, there was no appearance of ulceration. If the membrane was peeled off, the part under presented a clean patch of smooth surface, distinguishable from the surrounding parts, but in no degree abraded. But lately, there has been more tonsilitis, and frequently superficial ulceration, in several cases in the last few weeks, excavating more deeply into the tonsil; the membrane being still distinctly marked. During this period, east winds have prevailed, and more feverish constitutional disturbances have been observed.

With reference to the more general symptoms, the prominent feature is depression of the vital powers. The pulse is not always quick, but when so, is that of an irritable circulation. More frequently the pulse is slow, waving, and very compressible; the skin readily cools down (as when a hand is exposed out of bed), is moist and soft, almost clammy to the feel. Where fever has been observed, it is of



a remittent or intermittent type. Some cases have been ushered in with slight diarrhœa, with discharge of blood. The nose, and sometimes the passage of the ears, become involved; and, in the former case, epistaxis has occurred, not readily arrested. In some cases, blebs of serum have arisen, especially on the fingers; and, in some cases, urticaria has come on as convalescence approached. Debility remains in a marked degree, even in milder cases, after the local symptoms have disappeared, and I know that fatal cases of syncope have occurred, even when convalescence was supposed to be advanced; but I have not seen this in my practice. \* \* \*

We have abundant evidence of this depression of vital power in the general symptoms of diphtheria. We have also a low type of local inflammation in unison with the general type; but why it should just now seize the throat as its local seat instead of showing itself as boils, carbuncles, whitlows, thecal abscess, necrosed bone, and in kindred forms, I do not know. But this I may venture to say, if I attack the malady with the local appliances, and the general treatment I should employ for erysipelas, the cases recover.

*Treatment.* The principles that have guided my treatment of this disease, are: *first*, to arrest the local inflammation by exciting another of a different character; *second*, to employ elimination according to the individual case; *third*, in all cases to sustain vigorously the vital powers.

To accomplish the first indication, I prefer the employment of a strong solution of the nitrate of silver. Having first cleared the fauces, etc., as far as practicable by gentle means, I paint every affected part and *beyond* it, with the solution of the strength of fifteen grains to a drachm. In mild cases I have frequently tried one of milder strength, say five grains; but I am satisfied that in all cases an efficient application of the full strength is the best. It is perfectly safe, and has at once a marked effect. It is more efficiently applied by a full-sized camel-hair pencil than a sponge. Severe cases must be seen again in twelve hours, and the application repeated should the so-called membrane spread. Later in the treatment, a weaker solution may be used, or Bretonneau's application, one part of hydrochloric acid to three of honey. And later still, when the membrane has disappeared, but much fullness and puffiness of the parts continue, a gargle, containing the sesquichloride of iron, or tannic acid. Where, as in my second case, there is much fœtor, the chlorate of potass is applicable. And where, as in my third case, there is more tonsillitis, we may, with advantage, employ inhalation of steam, or warm milk gargle. After the membrane is removed, and the tendency to diphtheritic deposit supposed to be arrested, the throat must be carefully watched; for until the endemic condition of the system is conquered, we may have a relapse of diphtheria.

I commence the treatment of almost every case with a purge, varying with the state of the tongue, pulse, etc.; but by far the most frequently, calomel and rhubarb, carefully avoiding salines. In some cases, with loaded tongue and suffused countenances, I have given with the greatest advantage, emetics. Indeed, I am now so satisfied of their value, that I shall, for the future, employ them more frequent-

ly, especially where the congestion is marked, or there is unusual tonsillitis. The further general treatment is of *great importance*, namely, that directed to sustain the vital powers and remove anæmia.

I need not dwell upon the necessity of wine, beef tea, etc. In the severe cases these are most urgently required, and must be liberally supplied. In the more trifling cases, if well marked, convalescence will be delayed, and danger of relapse continue, if these, or their equivalents, are not employed.

Of all the medicines that may present themselves for our choice, there is one far superior, in my experience, to all others; and upon which, I indeed, chiefly rely: tincture of sesquichloride of iron. I have tried others that were obvious; but none sustain the vital powers, steady the pulse, lessen its frequency, and give potency to it; none remove the soft clam of the skin, steady the action of the kidney, and remove the anæmic pallor of the face, as does this. My confidence in its employment, and also in the use of the nitrate of silver, is fortified by their effects in erysipelas, in which they are almost specific. Cases will occur in which this treatment must be deferred, or modified, as where the tonsillitis is severe. In those cases, with the appropriate local treatment, I have first used the decoction of cinchona, with liquor of acetate of ammonia, or the latter with ammonia; but we afterwards come to the steel.—*Ibid.*

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ART. II.—*The Formation of Clots During Life*: By GEORGE MURRAY HUMPHREY, M.D., F.R.S., Surgeon to Addenbrooke's Hospital; Lecturer on Surgery and Anatomy.

[THE following extracts from an unfinished series of papers (accompanied with cases) are taken from recent numbers of the *British Medical Journal*, the cases being generally omitted.]

i. *Formation of Clots in the Veins.*

The obstruction of the veins, by clots forming in their interior, has, of late years, been the subject of investigation by several pathologists, who have pointed out clearly the conditions under which it most frequently occurs, and the changes which take place in consequence. Still, doubts appear to exist, respecting the causes of the phenomenon, and the starting point of the mischief; and, though the affection is one of very frequent occurrence, and may usually be diagnosed with facility, it certainly has not attracted the attention of practical men so much as it deserves. Very commonly it is suffered to pass unnoticed during life; and, after death, the vessels concerned are seldom examined to a sufficient extent, and with sufficient care to enable the observer to form a correct opinion upon the matter.

In all the cases that I have seen, with the exception of one of the two, the patients have been in a feeble state, most of them having been previously reduced by some other disease. The most frequent causes of the accompanying debility, were: some chronic disease, such as phthisis, or a discharging abscess; old age; low fever; or an acute inflammatory affection, more particularly of the serous membranes, or of the lungs. In no instance has the condition of the veins appeared to be the cause of death, either directly or indirectly; though in many cases the patients died of the diseases which preceded that condition, and the state of the blood which was induced appeared in some to accelerate the fatal result. It is, moreover, a very important fact that in no case, which has occurred within my observation or reading, has this affection been productive of any of those alarming and much to be dreaded symptoms, which attend occasionally upon traumatic inflammation of the veins, and occur under other circumstances, and which are supposed to depend upon the admixture of purulent, and other morbid fluids, with the circulating blood. In some instances, as in that first related, the affection is attended with uneasiness, or pain, in the early stages. More commonly, it comes on insidiously and does not attract attention till the swelling of the limb is observed, when some tenderness in the course of the vein may generally be found. Not unfrequently, we are called upon to treat an œdematous state of one of the lower extremities, which commenced during an attack of fever, or some other illness, and which may be traced to an obstruction of the vein that had escaped notice. In several cases the first suspicion of any obstruction to the circulation has been excited by the observation, after death, that one of the limbs was swollen; this has led to an examination of the veins, and to the discovery in them of clots, which must have existed many days.

The circumstances under which the disease occurs, and the fact that it often affects several parts of the circulatory system at the same time, or consecutively, in the same person, are quite in accordance with the supposition that it depends, primarily, not upon a morbid condition of the vessels; but upon a preternatural tendency to coagulation in the fibrine; and this view derives confirmation from several of the following phenomena, which may be observed in the origin and progress of the malady.

Thus, the obstruction most frequently commences in the parts of the venous system which are most favorable to the coagulation of the blood, viz: in the great veins, particularly those of the lower extremities, where the current is more feeble than in other regions. The points of selection in the lower limbs are: first, at or near the junction of two large veins, as the external and internal iliacs, the superficial and deep femorals, the anterior and posterior tibials; the projecting angles between the confluent trunks, furnishing favorable spots for the settling of the blood; secondly, in the neighborhood of the valves. These present loose, free edges, to which the fibrine may readily adhere; and they also have the effect of shutting off from the circulating current the small quantity of blood which lies above them, included in the retiring angle, between the upper surfaces of the valves and the adjacent wall of the vein. The blood so situated must be

almost at rest when the circulation is feeble and the limbs are kept quiet, because the valves will be then only partially opened, and, being at rest, it has a favorable opportunity to coagulate and become the nucleus of a larger clot. That this is no imaginary cause is proved by a case in which I found small dry clots lying above—that is, under shelter of—the valves of the femoral vein; the remainder of the vein being free from clots, or nearly so. However, the veins just above the valves often present slight bulgings, or dilatations. Their walls are here a little thicker than at other parts, and they exhibit a faintly reticulated appearance upon the internal surface.\*

It is to be remarked, that the valves are more numerous in the lower limbs than in the upper, and in the deep veins than in the superficial.† They are also often placed in the main veins near the points of junction of large branches; so that a number of causes combine to facilitate the coagulation of the blood in these situations.

Thirdly, the formation of the clot often begins in the popliteal vein. This has relation, not merely to the fact that the trunks of the anterior and posterior tibial veins, and the saphena minor are here united, but also to the fact that the internal surface of the popliteal vein is often remarkably uneven, presenting quite a reticulated appearance from the interlacement of opaque strengthening bands which form projections in the interior.

In the upper part of the body, the clots form most frequently at, or near, the junction of the jugular and subclavian veins, where there are always large valves, and in the cerebral sinuses.‡ In the latter the peculiar construction of their walls prevents much variation in their calibre (see my *Treatise on Human Skeleton*, p. 200), so that there must be considerable variations in the rate at which the blood traverses them; and they present, at the points of junction of the branches, many and marked projecting angles favorable to the settling of the fibrine.

The clots form not unfrequently in the venous plexuses around the prostate,§ and in the hæmorrhoidal veins.

It appears that, as a general rule, the formation of the clot commences on the outside, that is near to the coats of the vein, where the current must be somewhat slower than in the axis of the tube; and the first stage in the process is the settling of a patch or layer of fibrine upon the inner surface of the vein. This is increased by the addition of successive layers upon the interior, whereby the channel for the blood is diminished. Soon the tube is completely obstructed;

\* In a man, aged 76, who died of senile gangrene, I found a reddish brown clot, which was evidently of many days standing, closely adherent to the valves of the femoral vein, near the junction of the profunda. The rest of the veins, in both lower extremities, were healthy, and contained no peculiar clots.

† I have found the distances at which the valves are placed in the superficial veins of the lower limbs to be about equal to those at which they are placed in the deep veins of the upper limbs.

‡ They were found by Virchow *Froriep's Notizen*, xxxvii, 30, in the cerebral sinuses, in six cases out of eighteen.

§ In a man, aged 67, who died with sloughing of the nates, after fracture of the thigh, I found several short thick firm clots, with stunted branches, in the veins near the prostate. They were smooth, quite unadherent, and tumbled out of from the divided vessels. A section of each showed a central cavity containing red fluid, surrounded by a wall composed of tough, laminated, reddish or mottled fibrine. The fluid exhibited red corpuscles and a great number of pale nucleated cells.

this result being commonly accelerated, more or less, by the clotting of the blood, in addition to the settling of the fibrine. The two processes—fibrinous deposit and blood-clotting—which differ, probably, only in the circumstances that the greater rapidity of the latter causes the entanglement of the red globules with the fibrine, go on somewhat irregularly, whence the mottled appearance of the coagula; but, as a general rule, the clots are firmer and more fibrinous near the exterior, softer and darker in the middle. In a young woman, who died of fever, with peritonitis, excited by approaching perforation of the ileum, we were led to examine the veins by observing some œdema about the left ankle, and found the external and internal iliacs, at and near their junction, on both sides, occupied by coagula, which, on the left side, extended down below the popliteal vein, and, on the right, terminated in an ordinary clot at Poupert's ligaments. Sections of these clots showed them to consist of laminated fibrinous tubes, moderately firm, and enclosing central cylinders of dark, soft blood. The thickness of the fibrinous tubes varied. In some places, more particularly near the junction of the iliacs, where we judged the affection had commenced, it was so great as to leave little space for the dark central portion of the clot. In other parts, the fibrinous layer was thin; and in one place it was separated from the internal surface of the vein by a layer of soft, dark, clotted blood, resembling an ordinary recently formed coagulum in consistence and appearance. This was probably formed from blood, which had insinuated itself between the clot and the vessel, and had coagulated shortly before, or possibly after, death. A transverse section of one of these clots showed very clearly the central dark soft coagulum surrounded by a circle, or tube, of laminated fibrine, which again was enclosed by a more recent dark external layer.

The exterior of the clot is usually smooth, sometimes having quite a polished appearance, except at the points where it has become adherent to the sides of the vein. These adhesions are not usually very extensive; they are most commonly found where the clot began to form, and vary in their firmness with the period of their duration. The smooth character of the external surface of the clot is important, inasmuch as it rather militates against the view so much advocated by Virchow and some other pathologists, that portions of the clot are very liable to be detached, and to be carried along in the blood-current, till they cause obstruction and give rise to secondary coagula in distant vessels.\*

The extension of the clot in the direction of the heart is usually limited by the junction of some large vein which is sufficient to maintain the current of the main trunk. Often the clot does not reach quite so far as this. Thus, when the clot commences at the junction of the iliacs, on one side, it commonly extends about half way up the common iliac; and, in a case of cancer of the uterus, in which the iliacs, on both sides, were obstructed, the clot extended up the vena cava nearly as high as the renal veins. Sometimes the clot

\* Mr. Hewett, *Medico-Chirurgical Transactions*, xxviii, 74, found the clot in one case "enveloped in a perfectly distinct, transparent, smooth, polished membrane, presenting the appearances of serous tissue, with arborescent vessels in its structure."

reaches further, and terminates in a round or conical end on the *cardiac* side of the point of junction of some large trunk. In the peripheral direction, it is prolonged to a variable extent into the tributary branches, but does not usually reach the small veins; indeed, the latter are very rarely obstructed, either primarily or secondarily, in this affection.

The clot not only fills and chokes up the vessels, so as to prevent the passage of blood through it, but also distends or stretches it, and this distension, together with a certain amount of irritation resulting from the presence of a solid body in its interior, soon produces an effect upon the walls of the vein, the results of which are exhibited chiefly, or almost exclusively, on the *exterior* of the vessel. Thus, we soon find that there is inflammation of the investing cellular tissue, causing an effusion of serum, lymph or pus: whereas, in the *interior*, there is commonly little change beyond a removal of the epithelium from the lining membrane, and more or less intimate adhesion of the clot to it. There may be also an increase of redness at some parts, which is evidently due to staining by the contiguous blood, inasmuch as it is commonly proportionate to the color of the contained clot, being deepest where the clot is darkest, and less marked, or quite absent where the clot is composed chiefly of fibrine. I have never seen lymph or pus, or any inflammatory product, formed from the interior of a vein. This proves that the inner coats of veins are by no means easily excited to inflammation, and is quite in accordance with the results of experiments made upon the veins of animals by Lee,\* Mackenzie,\* and Virchow (*Handbuch der Speciellen Pathologie und Therapie*, i, 161). It accords also with the general results of my experience, which by no means indicate a liability to inflammation in the inner coats of veins. I have, in many instances, applied a ligature to the chief vein of a limb after amputation, without any ill result in a single case; and I have never seen any mischief caused by the ligature of a varicose vein or a hæmorrhoidal tumor, though I have employed that method of treatment very often. It is not improbable that where unfavorable symptoms have ensued in cases of this kind, they have been caused, not so much by inflammation of the vein itself, as by suppuration in the surrounding cellular tissue.

When examining a vein which is plugged by a tough and adhering coagulum, one can scarcely be persuaded that the circulation could ever have been reëstablished through it, if the patient had survived; yet there can be no doubt that this does take place, and that a vessel may, in process of time, resume its functions, and be restored nearly, if not entirely, to its natural condition, after its channel has been completely, or to a considerable extent blocked up by a clot. The perfect restoration of the limbs in several instances, assured me of this; and it is in accordance with the great difficulty which I have

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\* *Medico-Chirurgical Transactions*, xxxv and xxxvi. Dr. Mackenzie infers, from the results of numerous experiments on the venous system, that the origin of *obstructive phlebitis* is to be sought for in a vitiated state of the blood, that this causes an irritation of the lining membrane of the veins at various points, which, in turn, leads to coagulation of the blood. I do not, however, discover sufficient evidence of this irritation of the lining membrane of the veins, and think there are many reasons against admitting that it is a necessary, or even the ordinary, intermediate link between the vitiated and the coagulated condition of the blood.

experienced in effecting the permanent obliteration of varicose veins by temporary ligatures, or by other means which had for their object the formation of coagula in the vessels. It appears that the blood is almost sure to revert to its natural channel, in process of time, unless the vein be completely destroyed.\* The dissection in the following case illustrated the condition to which the clots became reduced. A man, aged sixty-three, died, of erysipelas and pleuro-pneumonia, ten days after resection of one ramus of the lower jaw, performed on account of extensive necrosis and suppuration, which had continued for several months, and had reduced him to a very low state. His health had long been bad; but he did not mention that he had suffered any particular affection of the lower limbs. I was led to examine the veins in consequence of the condition of the pulmonary arteries, presently to be described. In dissecting out the femoral and popliteal vein of the left side, I remarked that the investing layer of cellular tissue, usually so delicate, was more coarse, tough, and closely adherent than natural. With this exception there was nothing to attract attention on the exterior of the vessels, or in the structure of their walls. In the interior were numerous delicate, but tough, white bands or strings, extending across or along the vessels; some were adherent in their whole length, and others only at their ends; also small, firm lumps of pale yellow, or gravel, or golden color, smooth on the surface, and more or less adherent to the inside of the vein. In some places there were merely yellowish stains in the lining membrane of the vein. The nature of these stains would have been scarcely recognizable, had they not been in most instances continuous with the threads or some other evident remains of the clots. These veins contained also coagula, which appeared to have been formed recently, probably after death. The popliteal and lower part of the femoral vein, on the right side, presented appearances similar to those on the left. The upper part of the femoral vein was occupied by a firm, dry, mottled clot; and the profunda was tightly plugged by a continuation of the same, of white color. Above the junction of the profunda, the vein was distended by a clot of comparatively recent formation, which in the centre, was semifluid and of dirty cream color. This soft part contained red corpuscles, and larger pale cells having indistinct nuclei.

It is no uncommon thing for the middle part of the clot to be, as in this instance, softened and converted into a dirty pultaceous or creamy substance, in which corpuscles are found, varying in size and shape, less regularly formed than pus-cells, and having less distinct nuclei. These are intermixed with oil-globules and red corpuscles, which may be natural in appearance, or more or less misshapen and granulated, and in various stages of dissolution. The changes which the blood thus undergoes are, as it would appear from the experiments of Mr. Gulliver (*Medico-Chirurgical Transactions*, xxii, 138), similar to those which take place when it is subjected to concoction after its removal from the body. They seem most frequently to occur when the clot has been quickly formed.

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\* Hence the treatment of varix, where it seems desirable to resort to operative procedure, my practice is to pass a needle or silver wire beneath the vein, and to allow the metal to find its way out by ulceration through the vessel and the superjacent skin.

In all the cases that I have seen, except one, the soft central part of the clot was walled in by the firmer exterior portion, so that there was no opportunity for any of the *débris* to enter the circulating current. We come next to inquire what are the conditions of the blood which predispose it thus to coagulate in the veins during life. It has been already remarked, that the clots are most liable to form in persons who are in an enfeebled and cachectic state. Now, in this state, it is well known that the fibrine of the blood exceeds its normal proportions; and it appears that its tendency to coagulation is increased by their being also in the blood an excess of water, which dilutes the saline or ammoniacal elements, and thereby renders them less able to hold the fibrine in solution. Nevertheless, persons often remain in cachectic and anæmic states for a great length of time; and they may, in addition, suffer several and prolonged attacks of syncope, without any coagulation of the blood taking place. Indeed, the cases in which the latter occurs are quite the exception; and we therefore search for some other cause to explain the phenomenon in these exceptional instances. It is most frequent when the cachexia has been induced by some inflammatory affection; and we know that the effect of inflammation, more particularly when it attacks the serous membranes, is to increase the amount and the coagulability of the fibrine. The parturient state, which, especially in the early period of lactation, is productive of a similar effect upon the blood, is also marked by a great tendency to clotting of the blood in the venous system. The researches of Dr. Richardson (*The Cause of the Coagulation of the Blood*: 1858), continued with great assiduity and care through a long period, give strong reason for his view that the fibrine is held in solution by the presence of ammonia; and that its tendency to coagulate in the body is increased, and its coagulation out of the body is accelerated, by a diminution of the volatile alkali of the blood; and it is quite probable that, in the cases which we are discussing, an insufficient quantity of this solvent medium is one of the proximate causes leading to the clotting of the blood in the vessels.\* There may be, in addition, some alteration in those relations of the corpuscles to one another and to the walls of the vessels, which, though not well understood, have an important influence in facilitating the circulation of the blood.† Certainly, there seems no good reason to attribute the affection to an introduction of pus or other morbid fluid into the circulating current.

The diagnosis of the disease is easy. Œdema of the limb is often the first symptom. This may be attended with, or preceded by, un-

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\* The researches of Scherer and Lehmann (*Physiological Chemistry*, i, 97), show that the blood sometimes exhibits an acid reaction in the puerperal state; the acid present is supposed to be the lactic. This may be associated with the fact that I have often observed the skin to be remarkably dry in cases in which the blood coagulated in the vessels during life. An incipient decomposition of the blood is regarded by Zimmermann to be one of the chief causes of its coagulation.

† The commencement of the clots must, doubtless, be attributed to influences affecting the fibrine, rather than the corpuscles, because at the parts where they are first formed they are usually composed almost entirely of fibrine. Nevertheless, it is most probable that changes in one of the constituents of the blood are attended with corresponding alterations in the others; and the analogy drawn from what is observed in inflammation and in pregnancy, suggests that an increase in the coagulating tendency of the fibrine is accompanied by an increase in the adhesive qualities of the corpuscles, which would materially contribute to the formation of clots, and which may, indeed, sometimes be the immediately originating cause of them.



easiness in the course of the affected vessels ; and there is enlargement of the superficial veins, with, perhaps, induration of some of them. When the femoral vein is affected, the inner side of the thigh is sometimes swollen in a marked manner. The integuments usually remain white ; occasionally they are inflamed in patches, or in a more diffused manner ; and occasionally they are mottled with purplish spots, like petechial spots, or like those resulting from ecchymosis. Mortification rarely or never results from this cause alone. (Virchow, *Handbuch der Speciellen Pathologie und Therapie*, 1, 171.)

I have already said that the affection is rarely attended with any serious consequences. It does not commonly seem much to aggravate the patient's condition, or to diminish his chance of recovery : indeed, I have sometimes observed an amelioration in the general condition of the patient to be coincident with the swelling of a limb which indicated an obstruction in the great vein ; as though the general mass of the blood had become thereby relieved of a certain quantity of its redundant fibrine, and was consequently better fitted to minister to the healthy nutrition of the body. If the obstruction of a vein occur during the course of an inflammatory disease, it generally takes place when the disease is subsiding : it may, therefore, be regarded as an attendant on recovery, though it is an evidence of a low or cachectic state of system. I may again remark, that it seems to have no relation to the malady called "pyæmia" ; and though necropsies prove that it is sometimes associated with the formation of clots in the pulmonary arteries, I have not in any case had clinical evidence of its being followed by that formidable affection.

Nevertheless, it is a dispiriting, tedious malady ; it retards the restoration of the patient, keeps him confined to his bed, and causes much annoyance and apprehension. The liability to its occurrence is to be borne in mind as a reason against having recourse to depletion or purgation, or other measures which may exhaust the patient, or draw away the saline and watery ingredients of the blood, in the latter stages of an inflammatory or febrile affection. I have given ammonia in some cases in which I thought there might be a predisposition to the formation of clots ; and, where the general condition of the patient is likely to be benefited by the use of such a medicine, we should not ignore the evidences which have been afforded of its influence in retarding coagulation of the fibrine. If given in a pure form, it is probable that some of it will enter the blood ; and that it will operate in the living vessels, more or less, in the same manner as it is found to do when mixed with blood which has been removed from the body. When the blood has begun to clot in the veins of a limb, I do not think that much good results from any particular local treatment. The affection runs a certain course, and, if the disease upon which it is an attendant do not prove fatal, will gradually subside spontaneously ; and the veins usually become clear again. Considering the nature and cause of the malady, we should be unwilling to resort to the use of leeches. Nevertheless, I have known decided relief follow their application when the pain and inflammation around the vein was considerable. Fomentations are sometimes attended with comfort.

Of the instances which I have seen, the greater number have been in males.\* Nevertheless, it appears to be the same condition of the veins which in women, after delivery, usually constitutes the disease known by the name of "phlegmasia dolens." The cause of the disease, and the results disclosed by dissection, appear to correspond very closely, whether it be developed after parturition, or in the course of an illness; and the name "phlegmasia dolens in the male" has, accordingly been applied to it by Sir H. Halford (*Medical Gazette*, x, 172) and others. Phlegmasia dolens most frequently occurs *after* parturition at that time when the fibrine of the blood is found to be most abundant, viz: during the early period of lactation; and it is most common in women who are weak at the time of delivery, or whose strength has been reduced by flooding, and especially in those in whom there has been, in addition, peritonitis or some inflammatory affection of the chest. Moreover, when a fatal result has followed, it has commonly resulted, not from the condition of the veins, but from some other cause.† It must, however, be observed that, in phlegmasia dolens, the pain is usually more severe, and the disease, on the whole, is of a more acute nature than in the ordinary obstruction of the veins: it seems sometimes to originate in a morbid condition of the uterine veins, and is sometimes attended with, or productive of, those inflammatory and suppurative affections in distant parts which are attributed to a morbid condition of the blood.

Indeed, phlegmasia dolens would seem to occupy an intermediate position between the simple and comparatively innocent obstruction of the veins, which we have been considering, and the more severe and dangerous affection of the veins, which has been described by Arnott,‡ and others, as an occasional attendant upon wounds and injuries. The changes which occur in the veins, and in the blood contained in them, seem to be much the same in the three classes of cases, except that in the traumatic variety the inflammatory symptoms are commonly more severe. And there is the further important difference, that in it we are liable to encounter that alarming, and commonly fatal train of symptoms which is supposed to depend upon a purulent infection of the blood; whereas, in the ordinary obstruction of the veins, as I have already said, such concomitants are little to be apprehended.

The purport of the foregoing remarks may be condensed into the following summary:

1. The great veins are very liable to become obstructed by clots forming in them when the patient is greatly debilitated, and when the

\* Of forty non-puerperal cases tabulated by Dr. Mackenzie (*Medico-Chirurgical Transactions*, xxxvi, 235) about two-thirds were females, and one-third males.

† See paper by Dr. Davis, who first pointed out the true nature of this disease, *Medico-Chirurgical Transactions*, vol. xii; also papers by other writers in the same *Transactions*, and in various medical journals. Dr. Simpson, in his lectures recently published in the *Medical Times and Gazette*, calls attention to the fact that in some well marked cases of phlegmasia dolens the veins have been found quite healthy.

‡ *Medico-Chirurgical Transactions*, xv, 46. Mr. Arnott remarks, and I think he was the first to call attention to the fact, that the inflammatory changes "are usually limited by the passage of a current of blood; where a trunk is concerned, the boundary line being the entrance of a branch, and where a branch is concerned, the boundary being the junction of this with the trunk,

circulation is enfeebled—by inflammatory affections, by discharging abscesses, difficult labors and other causes.

2. The clots result from an altered state of the blood, disposing the fibrine to solidify in those parts of the veins which offer the greatest facilities for its so doing.

3. The inflammation of the veins is a consequence of the presence of the clot, and is chiefly confined to their outer coats, and to the surrounding cellular tissue.

4. The clots may soften and become intimately connected with the walls of the vessels, and may lead to the complete and permanent obliteration of their canals: more commonly, however, they are removed, or shrink into delicate bands or fibres, which offer little or no obstruction to the circulation.

5. The affection rarely leads to any serious result. It may be associated with so-called pyæmia; but has no necessary or frequent connection with it.

## ii. *On the Formation of Clots in the Pulmonary Arteries.*

There can, I think, be little doubt that the formation of the clots in the pulmonary arteries, in these, and other like cases,\* is due to the same causes as the formation of the clots in the great veins; that is to say, it is due, primarily, to an increase in the coagulative tendency of the fibrine of the blood, and, secondarily, to some facilities which the vessel offers for that coagulation to take place. With regard to the primary cause: the pulmonary clots have been found chiefly, if not exclusively, in cases where the vital powers have been lowered by some other disease; in short, in cachectic states, especially where an inflammatory affection was superadded; and after confinements; that is, in precisely the same conditions as the venous clots. Moreover, the clots are often found in both situations in the same patient, as in Cases IV, V, VI, and VII.†

With regard to the secondary, or immediately inducing cause: it has not been satisfactorily shown in any one instance that the clot was preceded by, or attributable to, disease in the coats of the vessel. The discoloration and roughening, where it was present, was evidently due, as in the case of the veins, to the presence of the clot, and was not the cause of it.‡

It appears, from the above related cases, and others which have been recorded, that the clots begin to form, in some instances, in the smaller branches of the vessels; and that in these instances there is often some obstruction to the circulation in the artery, caused by in-

\* Baron, *Archives Generales de Medecine*, ii, p. 1, appears first to have directed attention to the subject. Paget, *Medico-Chirurgical Transactions*, ix and x. Ormerod, *Medical Gazette*. Virchow, *Archiv*, x, 225. (In the case related here, the blood in the heart was fluid.) And *Handbuch der Speciellen Pathologie*, Bd. i.

† Virchow, *Froriep's Notizen*, xxxviii, 35, remarks that in only one case out of eleven, in which clots were found in the pulmonary arteries, did he fail to discover them in some other part of the venous system.

‡ Kidd, *Dublin Journal of Medical Science*, xxii, 376, attributes the affection to inflammation of the pulmonary artery. Baron alludes to the absence of an inflammatory condition of the vessel in the case described by him. Nevertheless, in some other cases, to which he refers, he conceives that inflammation of the wall of the vessel led to the formation of the clots.

flammation, pulmonary apoplexy, or other cause which must tend to promote the settling of the fibrine. In other cases the affection commences in the larger branches, or in the main trunk; and in these the spot at which the first deposit takes place is usually at or near the root of the lung. In this situation the pulmonary artery breaks up at once into a number of branches, which radiate off from it, at different angles, to the several parts of the lung. Consequently, a large extent of surface is presented to the blood, and there are numerous angular projections into the current; both which conditions are calculated to induce the coagulation of the fibrine. It must also be remembered that the rate at which the blood travels through the pulmonary arteries is subject to considerable variations, depending, partly upon the alternating contraction and repose, and the varying force of the contraction, of the right ventricle, and partly upon the vicissitudes of respiration; and both these sources of disturbance become more marked in those enfeebled states, with tendency to fainting, in which we have found that the fibrine has a peculiar tendency to settle. In such states the circulation in the vessel must always be feeble; and, probably, the current is sometimes absolutely suspended in certain portions of the artery, or in the main trunk, for short periods. Moreover, the venous blood seems to exhibit an increasing tendency to coagulate in the body as it approaches the lungs, and therefore may be presumed to acquire the property in the greatest degree in the pulmonary arteries.

At any rate, the clots which we find in ordinary *post mortem* examinations are more common and larger in these vessels, and in the right cavities of the heart, than in any other parts of the body. Frequently they are of firm consistence, while the blood in the veins, and in the left side of the heart, is quite fluid.

Virchow attributes the formation of these plugs in the pulmonary arteries to the lodgment there of small clots, or fragments of clots ("emboli") which have been formed in the veins, and have been wafted with the blood through the right cavities of the heart, towards the lungs. These fragments, he thinks, become detached from the ends of the clots which project into the great venous trunks; thus, in any case where the end of a clot, formed in one iliac vein, projecting into the vena cava, a portion may be washed off by the blood flowing against it from the other iliac vein, and, being carried into the pulmonary artery, may lodge upon one of the projecting angles of the vessel, and constitute a nucleus for the formation of a plug. It is not improbable that this may sometimes occur. It must, however, be remembered that the surface of the venous clots is usually quite smooth, and therefore not very likely to be dismembered by the slowly flowing current of blood; secondly, that in many cases, as in No. I, II, III, there was no reason to suppose that the pulmonary clots were preceded by clots in the veins; and thirdly, the effects of a preternatural tendency of the venous blood to coagulate are, for the reasons just given, likely to be exhibited in the pulmonary arteries as well as in other parts of the system.

The pulmonary clots undergo the same changes as those in the

veins, provided the patient survives. They may soften (Case III and v)\* or become firmly adherent to the vessel and disappear, leaving scarcely a trace behind (as in Case VII); or be converted into threads or bands, stretching from one part of the tube to another. I do not know an instance in which they have caused obliteration of any of the pulmonary vessels; though it is probable that this may take place occasionally in the smaller branches.

The plugging in the pulmonary arteries does not appear so easily to induce inflammation on the exterior of the vessel as it does in the case of the veins of the limb. It is not usually attended with pain or uneasiness, or any symptoms which lead, with certainty, to a diagnosis. Hurried, oppressed breathing, with faintness, occurring without any other obvious cause, would make us suspicious of this affection, and should induce us to auscultate in the situations in which a bruit, originating in the pulmonary arteries, would be most likely to be distinguished. I am not aware that a bruit, produced in this manner, has yet been recognized, though it probably would have been discovered had attention been directed to the point during the life of any of the patients.

It is indeed a remarkable feature in the affection that the pulmonary arteries, one or both, in the main trunks, or in the larger branches, may be blocked up to a considerable extent without causing any sign of obstruction to the circulation, or of affection of the lungs, or, indeed, without producing any symptoms whatever. In Case VII, it was clear, from the size of its remains, that a clot must at some time have occupied nearly the entire calibre of the main trunk of each of the pulmonary arteries; yet there had been no symptom of such condition observed during the life of the patient. In Case I, the patient appeared to be in her usual health till the moment of the fatal seizure. In this, and other parallel cases that have been recorded, there can be no doubt that the clots were forming for some time before death, and that sufficient blood found its way by the side of them into the lungs to maintain the circulation and supply the wants of the system.

The sudden death is probably caused by a slight exertion following a period of repose. During the repose we may judge the clots are increasing; and the ensuing exertion, by causing a greater demand for oxygenated blood than can be supplied through the impeded pulmonary vessels, induces fainting, which is fatal. The extreme suddenness of the fatal seizure in these cases has suggested the idea that it may have been caused by some displacement of the clots, producing more complete occlusion of the vessels; but this is opposed by the facts that the clots are usually more or less adherent to the walls of the vessels, and show no sign of such displacement having taken place.

iii. *Sudden Death from Occlusion of the Pulmonary Arteries after Par-turition*: By DRAPER MACKINDER, M. D.

Two cases were detailed which had recently occurred in Dr. Mac-

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\* In the case related by Dr. Kidd, *loc. cit.*, one of the clots was softened in the middle, and the tissues around the artery, on one side, were condensed and indurated.

kinder's practice. In the first, the patient was thirty-two years of age, and had been delivered of her second child after a natural and easy labor. Seventeen days afterwards, while apparently in good health, she rose up convulsively, said she was choking, and died. On subsequently examining the body, a large, branching, fibrinous plug was found completely stopping up the right pulmonary artery, and its immediate ramifications; while the entrance to the left pulmonary artery gave lodgment to a large and tolerably firm concretion. The heart was rather thin, and the lungs were slightly congested; but there was no further trace of disease about the body.

In the second instance, the patient had an easy labor, and, for a few days afterwards, all appeared to progress favorably, when she imprudently left her bedroom and exposed herself to cold. Shortly afterwards she was seized with difficulty of breathing, gasping, and cold clammy sweats, from which death relieved her in twenty minutes. Permission to make a *post mortem* examination could not be obtained, and hence it could only be surmised that the fatal event was due to the plugging up of some important but smaller vessel than those found obliterated in the first example.

Dr. Graily Hewitt stated that an elaborate essay on sudden death during the puerperal state had been recently published in the *Memoirs of the Imperial Academy of Medicine, of Paris*; but the author of that essay had not thrown any considerable light on the interesting question of the cause of death under these circumstances. The case of the Duchess de Nemours, who died from plugging of the pulmonary artery, would be in the recollection of the Fellows of the Society. From personal inspection of the clot he was able to state that in that case the clot occupied the pulmonary artery and several of its ramifications, and was so firm that it could not have been formed subsequently to death. Respecting those cases in which sudden death during the puerperal state was connected with the presence of coagula in the pulmonary artery, he would hazard the following supposition as to the causes which lead to the coagulation: The blood was so altered in the pregnant woman as to favor coagulation, in the first place; and, in the second place, the maintenance of the recumbent position, usually rigidly enforced by the medical attendant during several days after labor, favored the stagnation of the blood in the heart and chest. It was not unreasonable to suppose that these circumstances had much to do with the occurrence of this fatal accident.

Dr. Priestly recommended that in all cases of sudden death from occlusion of the pulmonary artery, an attempt should be made, not only to give an accurate account of the thoracic organs, but also of the condition of the uterus and appendages, more especially of the blood-vessels and lymphatics. The researches of Virchow on this subject had conclusively shown the connection between emboli formed in the uterine veins, and plugs found in pulmonary arteries; the value of reports on such cases would therefore be greatly enhanced if the investigation were carried further than the immediate seat of obstruction. He thought it not improbable that in chloro-anæmic conditions of the system, when there is an increase of fibrine in the blood, a very small amount of acrid material generated in or near the uterus,

and added to the blood circulating in the vessels, might cause deposition of the fibrine, and consequent occlusion of the vessels.—*Obstet. Soc., London.*

#### iv. *Emboli.*

The following conclusions respecting the obstructions of vessels by solid bodies of fibrinous concretions, are well drawn up by Professor Schützensbergen:—1. Fibrinous concretions or solid bodies formed in the heart or great vessels, may be detached from their seat, carried along in the current of blood, and so obstruct different secondary branches of the vascular system. 2. This fact is neither absolutely rare nor exceptional; it constitutes a special and very peculiar affection of the arteries, which has been called by Virchow, “emboli.” 3. This affection was for a long time misunderstood; is now shown to exist both by scientific induction, and by clinical and microscopic observation. 4. It has been observed as a consequence of gangrenous inflammation of the pulmonary veins; of organic affections of left side of the heart; and of atheromatous degeneration of the large arterial trunks. 5. Its most frequent cause is derived from fibrinous or calcareous concretions, and polypoid excrescences developed on the mitral valve, and carried along in the current of blood. 6. When the patient does not succumb under a first attack, another generally follows. Thus the attacks are multiplied. 7. The arteries most frequently found affected are: the Sylvian artery, the internal carotid, the arteries of the upper and lower extremities, the splenic, renal, external carotid, and mesenteric. 8. The obstruction ordinarily occurs at contracted points of the arteries. 9. If in consequence of the obstruction, a collateral circulation is established, only temporary disturbance is produced. 10. But if no collateral circulation is established, then follow organic alterations, mortifications and gangrene, dry or humid, partial or general. 11. In the parenchymatous organs the obstruction of the arterial branches produces sanguinary or circumscribed fibrinous infarctus. 12. In the brain, the infarctus usually occasions yellow softening. 13. In the spleen and in the kidney, the infarctus produces a special lesion, exactly circumscribed ordinarily of a conical shape, varying in color according to its age, and often denser than the rest of the parenchyma. 14. Emboli in the cerebral arteries produce functional disturbances analogous to an attack of apoplexy. The symptoms do not differ from those of cerebral hæmorrhage, or acute softening.

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#### ART. III.—*Experimental Ligation of the External Iliac Arteries and Veins.*

[THE last volume of *Transactions of the State Medical Society of California*, contains an interesting case under the following head, namely,

“On Ligating the Satellite Veins in connexion with the Arteries which they accompany—Operation of Ligating the external Iliac Artery and Vein—Rapid Recovery of the Patient. By E. S. Cooper, A. M., M. D., of San Francisco, Cal.” The details of this case will be omitted in order to make room for the experimental portion at the end of the report.]

Dr. Cooper says, of fifteen dogs, I ligated the external iliac in six, one of which died, the balance recovered. In five I ligated the iliac artery and vein at the same time—all of these recovered. In the first six the extremity became cold in every instance, and the coldness was sometimes quite persistent for a day or two, in spite of externally stimulating applications. The sensibility of the limb was greatly impaired, so much so that the application of terebinthanic liniments would hardly disturb the animal during the first two or three days; but in the latter five the heat and sensibility of the limb remained nearly natural from the first.

In two of the dogs I tied the abdominal aorta and ascending vena cava simultaneously, and in the other two the artery alone, but the results were nearly the same in the four cases—all the animals dying on the third day.

I don't consider that the termination of the four latter experiments have any weight against the apparent advantages resulting from the ligation of the veins when the accompanying arteries are tied, so clearly shown in the case of the first eleven, because I think that an animal can seldom be found to survive the effects of ligating the abdominal aorta, for though the climate of San Francisco is as good if not the best in the world for recovery after deep-seated injuries and severe surgical operations, I have never been able to keep a dog alive over sixty-three hours after the circulation had been stopped completely and at once through this vessel, though I have tried the experiment very many times.

None of the animals, however, died of internal hæmorrhage, peritoneal inflammation, or gangrene of the lower extremities, the three assignable causes of death resulting from this operation as acknowledged by surgical writers.

We have much to learn in regard to the ultimatum of ligating large arteries of the lower extremities, because there is as yet no satisfactory reason to be assigned for the greater degree of fatality attendant upon these operations than upon those of the arteries of the upper parts of the body, of corresponding importance in size and anatomical relations.

Surgical writers throw not a ray upon the subject of the immediate cause of death in ligating the abdominal aorta, and it is full time that surgeons would consider in earnest this most important feature of practical surgery. The results of my experiments convince me that neither of the three modes of death, recognized by Surgeons, are often found the ones in this operation, because neither gangrene of the lower extremities, internal hæmorrhage, nor peritoneal inflammation were found by any post mortem examinations I made. The



right heart, however, was always engorged with coagulated blood to its utmost capacity, though some of the examinations were made in two or three minutes after the animals died.

Very little is known of this whole subject beyond the fact that Sir Astley Cooper and three or four others have ligated the aorta unsuccessfully, and that is it a question unsettled, whether it can be successful in any case. I am convinced that it never can unless the circulation is arrested gradually, because the system cannot accommodate itself suddenly to so great a change in the space allotted by nature to the circulating medium.

But I venture to predict that at no distant day the operation will be rendered successful by a gradual compression of the vessel behind the peritoneum, imitating to some extent the process of obliterating the channel of the artery, by the pressure of tumors, of which there are nearly forty cases on record found since Sir Astley Cooper first performed the operation of ligating this vessel in 1817. But as yet there is nothing in surgical writings giving the most remote hope of anything but death as the result of tying this artery in the usual way. The four last experiments above given, do not therefore carry any weight against the advantages resulting from the ligation of the satellite veins in connection with the arteries which they accompany, so clearly shown in the case of the first eleven.

This subject must be further pursued, as it opens a wide field for the investigations of the *dissectionist* and experimental surgeon.

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ART. IV.—*French Therapeutics, etc.* [Extracted from recent numbers of the *Medical Times and Gazette.*]

i. *Medical Electricity—Faradization.*

A few years ago a new sort of electricity was discovered by Prof. Faraday, to which he has given the name of "electricity by induction," and which has been found to possess properties peculiar to itself, serving to distinguish it from that derived from all other sources. The currents proceeding from this new source are known to produce, when introduced into the system, peculiar physiological phenomena, which have led to its employment in the treatment of a numerous class of diseases to the entire exclusion, except in one or two surgical affections, of the static electricity. A celebrated French physician, M. Duchenne, of Boulogne, has availed himself of the discovery of our distinguished countryman, and to him we are indebted for a full exposition of its value and its adaptation to the treatment of a great variety of diseases intractable in their character, and sometimes setting at defiance every other method of cure. By an in-

geniously devised apparatus, and by the adaptation to this apparatus of a variety of exciters, M. Duchenne has been enabled to direct the electric current to each particular organ of the body, and to limit their power at pleasure. By this new method, to which he has given the name of "local electrization," he can act either on the surface of the skin, or its deeper seated layers; or he can act on the muscles and internal organs, without interfering with the super-jacent tissues. The great advantage of this system over the more ancient method is at once recognizable, when we consider, that by the latter it was necessary, in order to act on the muscles or internal organs, to puncture and incise the skin, thus interfering with its integrity, exposing it in some instances to disorganization while the entire nervous system was rendered liable to preternatural stimulation. In the hospitals here, we observe, that besides the apparatus of M. Duchenne, another, the production of Légendre and Morin, is also employed in Faradization. The principle on which both are conducted is the same, although a preference is given to the latter by Briquet, and other physicians, owing, we believe, to its greater simplicity. In this apparatus the primary motor power is a galvanic current proceeding from a Bunsen pile, which by its influence electrifies small bars of soft iron, and so renders them magnetic. The iron having become a magnet, acts in its turn by influence on a very fine wire placed near it, and there determines the electric current called "current by induction."

Local electrization, or Faradization, M. Duchenne treats of under three great heads: 1. Electrization of the skin. 2. That of the muscles. 3. That of internal organs, the organs of sense together with the organs of sensation. We shall as briefly as possible notice these three divisions in succession, and our remarks will be a simple condensation of the views of this celebrated physician, as exposed in his treatise published some three years ago, and entitled "*De l'Electrization localisée et de son application à la Physiologie, à la Pathologie, et à la Thérapeutique.*" \* \* \* Different parts of the skin being endowed with different degrees of sensibility to the electric currents, it naturally follows that in Faradizing different regions some modification in the method or manner of performing it becomes necessary. For this reason Duchenne divides the different methods of cutaneous Faradization into three classes: 1. Faradization by the electric hand. 2. By metallic exciters; that is to say, by the application to the pole of globular, oval, or conical pieces of metal. 3. Faradization by means of metallic cords. The peculiar effects to which each of these methods gives rise are different in each case, and hence their application requires a special study.

Faradization by the electric hand is accomplished in the following manner: A wet sponge, enclosed in a cylinder, is attached to one of the poles of the machine; the sponge is then applied to a portion of the skin endowed with but a small degree of sensibility, as, for example, over the sacro-lumbar region; the second exciter connected with the apparatus is held by the operator, who, after having carefully dried the skin by means of some absolvent powder, passes his hand rapidly over those parts which he wishes to stimulate. This is

the mildest form of cutaneous Faradization, and, except when applied to certain parts of the body where the sensibility is exceedingly great, its effects are barely perceptible.

Cutaneous Faradization by solid metallic bodies is effected thus: The skin is carefully dried, as recommended for the electric hand, unless in those cases where the epidermis is thick and hard, as happens sometimes in those persons whose calling exposes them to much contact with the air, or substances calculated to harden it; in these latter cases it is even necessary to moisten the surface, in order that the electric excitation may penetrate the entire derma. This done, the metallic exciters, whether oval, conical or globular, are passed over the skin. The globular and the oval, or olive shaped, are intended to excite by their convex surface the skin of the limbs and the thorax, while the conical-shaped exciters are used in the Faradization of the scalp. All these solid metallic exciters must be passed with more or less rapidity over the affected parts; cases do occur, however, where it is necessary to produce a very powerful revulsion in a limited space, and to accomplish this the point of the olive-shaped exciter must be left in full contact with such spot until the desired effect be produced. From the extreme pain to which the contact of the olive-pointed exciter with the skin gives rise, it has been designated the "clou électrique," (electric nail), the patients comparing the sensation to that which might be produced by a burning nail being driven into the skin. This form of Faradization can be applied especially in the neighborhood of the vertebral column.

*Faradization by Metallic Cords*—The metallic cords are used in the form of small rods, or small brushes, which are fixed in cylinders, the latter of which are attached to the isolating handles by means of screws. Sometimes the Faradization is accomplished by whipping the skin with the metallic brush, and sometimes the brush is allowed to remain in contact with the skin as long as the patient can bear it. The former method is that most frequently in use; the latter can be with difficulty supported by the patient, and is had recourse to only in deep-seated affections, such as white-swelling of the knee, or other important articulations. This is called the "electric moxa."

## ii. *Quinine in Peritonitis.*

THE experiments made of late years by Legroux, Monneret, Andral, Trousseau, and other celebrities connected with the French school, have proved, beyond a doubt, the beneficial influence of quinine in large doses in the treatment of acute rheumatism, and other diseases of a similar type. Recently this same medicine has been vaunted in combating certain forms of peritoneal inflammation occurring in connection with the puerperal state, and more especially by M. Beau, who, during his official connection with the Cochin Hospital, employed it in an immense number of cases where that formidable disease prevailed in an epidemic form. So marked was the success which attended his experiments, that he ultimately arrived at the conclusion, that quinine in large and continued doses, if not a specific in that disease, is at least the most valuable, and at the same time the most powerful curative agent we possess in its management and treatment.

Entertaining such views in reference to the value of quinine in puerperal peritonitis, it was exceedingly natural that he should try the same remedy in a similar affection occurring apart from the puerperal state ; and hence we find, that in the idiopathic form of peritonitis he has recourse to the same mode of treatment, and urges on his pupils its decided superiority over every other with which he is acquainted. In a disease of this kind, which, in spite of the most active and energetic measures we may adopt, most generally pursues its fatal course with amazing rapidity, we are always disposed to accord a favourable reception to any theory, however novel and unsupported by facts it may be, provided only we be told that its practical application has been followed by merely partial success ; and we are always most agreeably surprised when, with preconceived notions unfavorable to the theory, we find ourselves confronted with a single fact which, though it may not completely establish the new doctrine, at least enlists our sympathies in its favor. Such was the surprise which we experienced in reference to the quinine treatment recommended by M. Beau, when we witnessed, for the first time a fortnight ago, a genuine case of idiopathic peritonitis treated and cured by this medicine, to the entire exclusion of every other therapeutical measure. To those who have been taught in early life that inflammation of the peritoneal membrane demands, in the majority of instances, the abstraction of blood—if not generally, at least locally—until a powerful impression be made on the system, and that this heroic plan of treatment must be followed by the assiduous exhibition of calomel until the patient be brought under the influence of this powerful and dangerous mineral, the solemn declaration of a man of high authority and great experience, to the effect that such treatment is not only useless but positively pernicious, cannot be viewed with any other feeling than that of amazement ; and yet, reluctant though they be to abandon a system which has been inculcated in their youth, and which they have been accustomed to regard as strictly orthodox, they must resign themselves to seeing it demolished by another diametrically its opposite.

As a sequel to the foregoing remarks, we cannot do better than place before our readers the following case, the details of which we extract from our medical notes taken at the bedside of the patient. A girl, 24 years of age, was admitted into the service of M. Beau, at the Hôpital la Charité, and on examination the following symptoms were recognized. Over the lower part of the abdomen there were great pain and tenderness, which were increased on the slightest pressure ; the pulse was small, hard, and quick, averaging about 120 beats in the minute ; the skin was hot, dry, and harsh ; tongue furred ; constant nausea with occasional vomiting ; constipation ; the features were pinched, and the countenance indicated much anxiety ; respiration hurried ; complexion muddy, and the conjunctiva slightly yellow. The patient lay on her back, with the legs flexed on the thighs. Her previous history showed that for some weeks prior to the invasion of the disease she had been in an indifferent state of health, which she attributed to an unusual amount of bodily fatigue to which she had been subjected. In the presence of these symptoms,

M. Beau at once pronounced the case to be one of acute peritonitis. Treatment : an emetic, composed of ipecac. and tart. antimon. was ordered, to be followed up by a purgative enema. The feeling of nausea having thus been removed, and the stomach prepared for the administration of the sulphate of quinine, this medicine was prescribed as follows : two grammes (equal to thirty grains) were ordered to be divided into three doses, one of which was to be given every eighth hour, and a blister was applied to the lower part of the abdomen, over the spot where the pain and tenderness were greatest. After four or five doses of the quinine, its physiological effects, such as deafness, ringing in the ears, etc., began to manifest themselves, and consentaneous with these there was a manifest diminution of the original symptoms. The pulse from 120 was reduced to 110 ; the pain and tenderness of the abdomen were considerably subdued ; the febrile symptoms, generally, were greatly modified, and the countenance became more placid. The quinine was ordered to be continued, together with the use of refreshing drinks, and on the fourth day of the treatment the pulse fell to 100, accompanied by corresponding improvement in all other respects.

In a week or ten days from the commencement of the treatment, the full effects of the quinine having been produced, that is to say, the original disease having been, as it were, overcome, the medicine was gradually diminished, and ultimately discontinued. The pulse has fallen regularly, and the patient is now convalescent. Now, in a case like the above, implying the undoubted existence of inflammatory action of a most important membrane, the indication, according to the notions generally entertained, would be abstraction of blood, if not from the arm, at least by the application of leeches over the seat of the disease ; and we venture to say, that in a similar case such would be the practice adopted by a large proportion of medical men. But according to the theory of M. Beau, instead of curing the disease the abstraction of a single drop of blood would only increase the evil, and ultimately lead to a fatal result. In peritonitis, and in the phlegmasia generally, the blood, according to Beau, is poor in red globules, and consequently rich in fibrine ; in other words, the increase of the latter is in a direct ratio to the diminution of the former. It is not difficult, therefore, to understand his determined opposition to depletion, and other measures calculated to lower the vitality of the blood. He regards the excess of fibrine as the real pathological condition on which this disease depends ; that is to say, he regards the fibrine as the phlogistic principle, and whatever tends to augment the fibrinous portion of the blood must of necessity increase the fever and feed the disease. Consonant with these views he considers anemia, which is always characterised by diminution of the red globules of the blood, as one of the principal predisposing causes of the phlegmasia ; and when this condition of body exists, exposure to cold and wet, to sudden change of temperature, or to any of the other exciting causes of inflammation, may be followed by an attack of acute peritonitis.—Quinine being the sheet-anchor of M. Beau, as we have shown, in the treatment of this disease, it will not be out of place here to indicate some of the leading rules which he lays down in reference to the man-

ner in which it should be employed, and on the rigid observance of which the success will depend. It is necessary that a decided effect be produced on the system at once, and hence the earlier the quinine is exhibited after the malady has declared itself the better. But in every case he deems it advisable to premise the first dose of quinine with an emetic; to be followed up by a purgative enema. As soon as the stomach is prepared to retain the quinine, he gives the latter to the extent of eight grains every eighth hour, and this quantity of the salt must be continued for several days in succession. It is right, however, to observe that the dose must be proportioned to the sensibility of the patient; that is to say, it must be increased should the intoxication produced by it prove too feeble, and diminished in the event of its being too strong. M. Beau has never, in the course of his experience, had occasion to prescribe a larger dose than four grammes (60 grains) of the salt in the twenty-four hours. Even when the inflammation of the peritoneum is limited to one spot, and when the malady has been to a certain extent overcome, it becomes imperative towards the second or third day to increase the dose of the salt to a small extent, as the system becoming accustomed to it, the quinine in its original dose would not suffice to maintain the mastery over the disease. For the same reason the medicine must not be diminished too rapidly, nor discontinued too early. Beau states that he has seen cases in which, up to the eighteenth day of the disease, he could not suspend the medicine without such suspension being followed by a recurrence of the febrile symptoms. Should the stomach from excessive irritability reject the quinine (by no means an uncommon occurrence), it should again be administered in some other form, so that if one form does not suit another may. When, notwithstanding the form in which it is administered, the stomach obstinately refuses it, the quinine may be given in the form of enema, as experience has proved that its absorption readily takes place when given in this manner.

### iii. *Inoculation of Secondary Syphilis.*

[The following is a specimen of virtuous indignation expressed by the *Gazette Hebdomadaire*, and directed at M. Ricord.]

“What is glory, what is reputation, when it has error for its basis? What is a mere man in the face of science? What is the celebrity of this doctrine of his before an accomplished fact? The question here is one of false laws, which for more than thirty years have spread infection through the conjugal bed, and through the cradle; which have insulted virtuous women, fresh from the arms of husbands tainted with secondary symptoms, and then compelled to defend their honour against the accusations of his theory; which have sown discord in families: which have occasioned those numberless dead births, which frighten the accoucheur; which have poisoned the wretched mercenaries, who have fulfilled the duties of second mother; which have misled justice, and transformed the victim into the guilty. During twenty years, we say, despite of all the advertisements of

daily observation, contrary to the authority of the most experienced practitioners, contrary to multitudes of facts detailed in works, in theses, in pamphlets, in journals, led by systematic blindness, or by the impulse of a school, the author of this system has exposed science, humanity, morality, and the law itself, to these misfortunes, to these iniquities."

#### iv. *New Disinfecting Agent.*

M. Velpéau has laid before the Académie des Sciences some account of the results of the employment of a new disinfecting agent, discovered by MM. Demeaux and Corne. Its effect in the removal of the most disgusting odors emanating from wounds, etc. (as gangrene, cancer, etc.), has proved almost instantaneous, while its application is painless and innocuous. It is a cheap, greyish powder, exhaling a slight bituminous odor, and is formed of 100 parts of the plaster of commerce in powder, and from 1 to 3 parts of coal tar. Not only does it possess the power of removing all kinds of stench, but also of absorbing the morbid fluids. For surgical purposes it may be combined with oil, which binds it together without dissolving it. It has been abundantly tried in the wards of La Charité, the dissecting rooms, and elsewhere, and the most sanguine anticipations are entertained as to the results of the publicity now given, whether regarding it as an application for surgical or hygienic purposes.

#### v. *The Action of Gases Injected into the Tissues of Living Animals*

Has been investigated by MM. Lecomte and Demarquay. They found that air, nitrogen, oxygen, carbonic acid and hydrogen, do not occasion any injurious effect when introduced into the sub-cutaneous cellular tissue or into the peritoneum. That all these gases are absorbed after a longer or shorter period—carbonic acid in forty-five minutes, and nitrogen after several weeks. As regards rapidity of absorption, these gases stand thus: carbonic acid, oxygen, hydrogen, air, nitrogen. That any gas thus injected invariably determines an exhalation of the gases contained in the blood and the tissues; in fact, absorption does not begin until the injected gas is mixed with the other exhaled gases. That generally the exhalation of the gases of the blood is greater during digestion than during fasting. That of the gases injected hydrogen determines the greatest amount of exhalation of gases from the blood. That the rapidity of the absorption of gases into the blood is not always in relation to their solubility in water.

#### vi. *Alcoholic Fermentation.*

The beautiful experiments of M. Pasteur on alcoholic fermentation tend to show that this phenomenon is the direct consequence of a vital action. He has lately, in continuing his researches, discovered new facts, which throw further light on the intimate causes of this wonderful phenomenon. Every one knows that a very little yeast is sufficient in order to cause a large quantity of sugar to undergo fermentation. When the quantity of yeast is increased the trans-

formation of sugar goes on rapidly. But when the quantity is increased to 50, 100, or 200 times the minimum quantity fixed in his previous experiments, remarkable results ensue. First of all, the sugar disappears with surprising rapidity; and when it is entirely destroyed, the fermentation which (according to the purely chemical theory) ought to stop, still continues, carbonic acid is disengaged with great activity, and at the same time alcohol is formed! In this way the carbonic acid may reach to two or three times the volume that the sugar experimented with can furnish. How is such a result to be explained? Whence comes this invisible sugar, which even doubles in quantity after the first quantity is destroyed? Every one will answer by referring to the beautiful researches of M. Payen: that the glycogenous matter is very probably the cellulose of the globules. And the fact has been verified by M. Pasteur. He has discovered that in order to transform into sugar more than 20 per cent. of its weight of dry yeast, it is only necessary to boil for a few hours ordinary yeast in a very diluted solution of sulphuric acid, as indicated by Pelouze. Hence it is distinctly proved that the yeast is organized, and that the increase of the sugar is intimately connected with the life of the globules; or, in other words, the physiological function of the globules of yeast—true living cells—is to give out carbonic acid, alcohol, glycerine and succinic acid, in the same measure as they themselves are produced, and the different phases of their existence are accomplished. Is it not very possible that these investigations on fermentation and sugars may be destined some day to throw light on the function and uses of the glycogenous matter found in the animal economy, and of which we have heard so much in these latter days?—*Gaz. Médic.*

#### vii. *Mortality of Soldiers.*

M. Tholozan concludes a series of articles in the *Gazette Médicale* upon military mortality in the following terms:

The most important conclusions to be drawn from our own observations, as well as from the works which have been published of late years relative to the hygiene, statistics and diseases of armies are the following: The considerable increase of deaths which afflict an army in the time of peace is especially caused by pulmonary lesions of a peculiar character. These lesions are the effect of a special vitiation, a specific diathesis of the economy which results from the overcrowding, the agglomeration, and the life in common incidental to barracks. Thus far, science has not been able to apprehend the differences which prevail between these conditions and those amidst which variöla, rubeöla, scarlatina, typhoid and typhus are developed. The means which are suitable for the prevention or diminution of these latter diseases are also wonderfully appropriate appliances for combatting the endemic phthisis of the army. If the opinion I here advance becomes confirmed, we must in future regard the phthisis of armies rather as a specific infectious disease than as an organic, diathetic, hereditary affection. Pathology enlightened by hygiene



would thus modify one of its most absolute beliefs, and this reform, in its turn, would second and generalize one of the most important advances of hygiene.

#### viii. *Vital Statistics.*

During thirteen years of observation, Dr. Marc D'Espine, a most persevering statistician, has collected 16,856 cases of death from 80 different kinds of diseases. Each of the principal causes of death has been divided into twelve periods of ages, and at the same time separated according to sex, and habitation in town or country. A second table gives a distribution of deaths through the twelve months of the year, with a similar division of sex and habitation. Finally, in the text, which accompanies each of these instructive tables, the author gives particular details, which he has not introduced into the tables, such as the powerful influences of fortune. This work of comparison and criticism, which must have cost him great labor and researches, is very precious. It enables us to discover the meaning of the differences met with here and there; and it strongly confirms the conclusions offered in England, Belgium and Geneva. The author, moreover, is a true statistician; he well knows the importance of a critical examination of statistical documents, and he very seldom omits subjecting his own materials to it. Here is one example of his method: *Cancer*—The cancerous diathesis has a decided preference for the upper classes of society. This cruel disease, which between 40 and 70 carries off more than one-ninth of the population, is a cause of death *twice* more frequent in the upper classes than among the general population (of 1000 deaths 111 are of the upper class from this cause, and 52 only of the general); and as regards sex, 57 women to 32 men. In cancer of the stomach, 52 deaths in the upper classes to 23 general; of the breast, 14 to 5; of the uterus, 13 to 8.

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#### ART. V. *Cellular Pathology—Cutaneous Absorption.*

##### i. *Virchow's Cellular Pathology.*

The object of Dr. Virchow's work on Cellular Pathology is to establish a new foundation for the creation of a Philosophical Medicine. Dr. Virchow desires to put an end to all the one-sided theories now in vogue—to the theories of the humorist and the solidist; to the crisis, exudation, and blastema, iatro-chemical, and iatro-mechanical, theories. In the place of all these partial stories, he proposes a knowledge of the fine organic processes of cell-life as the only foundation of a rational doctrine of disease. The cell-theory, and its relation to practical medicine, however, is not altogether a novelty;

but according to Virchow it has as yet only reached a certain stage of progress. What has been heretofore done, stands as a mere forerunner to his cellular pathology,—just as ideas stand to deeds, and imagination to investigation. The foundation upon which he fixes his deductions are actual histological facts, which he places before his disciples in the shape of microscopical preparations and drawings. It will be said by some that there is nothing new in the founding of pathology or histology. And on this score reference may be made to the last edition of Rokitsansky's work, which is filled with numerous microscopic representations of diseased structures. It may be indeed asserted that Rokitsansky even laid the axe to the root of his earlier, and somewhat humoral style of pathology. He has rejected the doctrine of Crases. In his third edition Rokitsansky says: "The origin and increase of cells and nuclei take place, either immediately out of a blastema, as free free cells and nuclei, or in mother-cells and mother-nuclei, endogenously;" and he adds, "the extra-cellular origin of these elements out of a free blastema cannot be doubted, in my opinion, at the present moment." Virchow, on the other hand, utterly rejects the theory of the origin of free cells in a structureless blastema; just as elsewhere men reject the theory of equivocal generation. He asserts that only out of ready-formed structures arise cells; and that out of old cells there is a continual growth and development of new cells, *omnis cellula ab cellula*. He substitutes a cell-growth theory for the blastema theory. He does not consider exudation as a peculiar product of the capillary vessels, containing plastic and other constituents; but he regards it as an ordinary transudation mixed with the products formed in the diseased structures outside the vessels, among which products may be mentioned fibrin, pus, mucus, etc. Hence, then, according to Virchow, the inflammatory process does not take place in the capillaries, but in the elements of the tissues themselves—in the cells which through an increased activity attract the blood to them, and become thickened and swollen. In this manner, by cell-growth and by attraction of the juices, likewise arise all other new formations; the homogeneous, as well as the heterologous and malignant, so also new areola, or bony tissues, pus, tubercle, sarcoma, and cancer, the disease of plants, etc. The history of heterologous forms is this: In a particular place, or at a particular time, or in a particular degree, there arises a growth which is not proper to the part. Some other structure normal in some parts of the body is substituted at the part for the structure normal to it, forming what Virchow calls a pathological substitution of tissues. The malignancy of heterologous growths consists in this: that the tissue affected is, by its vascularity, disposed to form a fluid, which is injurious to the structures around. The dyscrasiæ, also, can only arise through the propagation by means of the blood, of the products of certain existing local affections. For example, the so-called phlogistic or fibrinous crisis depends originally upon a local production of fibrin in an inflamed tissue—fibrin being always a local product, and never arising in the blood. Many other blood diseases, also, depend upon diseased states of the spleen and lymphatic glands—those being glands in which white and red blood corpuscles arise.

The disease which authors call pyæmia consists partly of thrombosis and emboli, partly of ichonhæmia, of leucocythemia; the substance which forms the obstruction in phlebitis not being pus, but a purulent-like detritus. Pus arises only out of tissues. Life and disease, in fact, cannot be explained by physical or chemical actions, but are the developments and processes of organized elements.—*Med. Times and Gazette*, July 8, 1859.—*Schmidt's Jahrb.*

ii. *Cutaneous Absorption*: By A. WALLER, M. D., F. R. S., Professor of Physiology at Queen's College, and Physician to the Queen's Hospital, Birmingham.

\* \* \* In a memoir recently laid before the Royal Society, I have examined into the action of various agencies which influence cutaneous absorption; and as it will shortly be published, I will only mention here some of the leading results at which I have arrived, and there described.

My principal experiments on absorption have been made by applying to the skin solutions of different alcaloids, as atropia, strychnia, morphia, etc., watching carefully the symptoms of absorption, and noting particularly the effects produced on the pupil. By these means the influence of age, of circulation, of innervation, etc., may be ascertained and accurately measured. I found a still more powerful influence than either of these on the rate of absorption existing in the nature of the menstruum, or vehicle of the alcaloid. The menstrua, with which I have experimented, are chloroform, alcohol, water and turpentine. Chloroform I find to exert the greatest accelerating influence on cutaneous absorptions, of all those yet tried by me. Alcohol, on the contrary, I find to possess the greatest retarding action.

With chloroform, for example, I find that atropia will dilate the pupil after its simple topical application, in the space of from two to four minutes; whereas the same proportion of atropia in alcohol will produce no influence on the pupil, even after the lapse of thirty minutes and upwards. Water and spirits of turpentine, as vehicles, hold an intermediate place between chloroform and alcohol, or spirits of wine.

Atropia is particularly well adapted to ascertain the exact influence of the various menstrua and other agencies on absorption. I have likewise found the same laws to obtain with aconite, morphia, strychnia, and many other alcaloids, and a solution of equal parts of these in chloroform, possess so great an influence on absorption through the skin, that the simple application of some of the former will cause death in a few minutes; whereas an alcoholic solution of the same may be applied from thirty to forty minutes without any effects whatever. Thus, the same agent which is perfectly innocuous in one case, becomes rapidly fatal in another.—*Ibid.*

ART. VI. *Antiphlogistic Treatment.*i. *Two Cases Illustrating the Benefits of Antiphlogistic Treatment after Severe Injuries to the Lungs.* Under the care of Mr. HILTON and Mr. BRYANT.

In the present period of prevailing skepticism as to the influence of many modes of treatment which held undisputed sway among the older surgeons, it is well every now and then to draw attention to unequivocal instances of prompt benefit obtained by decided measures. More especially is it so when these measures, instead of being novel, are of old established, but now somewhat waning, character.

Many of our readers may probably have noticed in the newspapers the account of a little boy who had been knocked down by one omnibus, and run over by another, and had received most severe injuries. The accident occurred in Shoreditch, and the poor little fellow was taken to Guy's Hospital, where his state appeared so hopeless that what was believed to be his dying deposition was taken, to be produced in evidence against the drivers of the vehicles. When admitted his ribs were found to be fractured in the most frightful manner; his chest had been, as it were, pounded, and it was difficult to say which of the ribs had not been broken. He was in collapse, and there was much emphysema of the sub-cutaneous cellular tissue. Of course no stethoscopic examination could be thought of. He remained very low indeed for some time, in spite of the cautious use of stimulants; and it was at the end of twenty-four hours that his evidence was taken under the circumstances above mentioned. He had expectorated a good deal of blood, but his respiration remained throughout much embarrassed. As this embarrassment of breathing was, however, accompanied by a cold skin and failing pulse, it was not thought justifiable to abstract blood. As soon, however, as there were signs of rallying, tartarized antimony in full doses was ordered. The effect of this remedy in quieting the breathing was most marked, and the boy's progress has been from that time steady and good. He is now considered out of danger.

The above case impressed all who saw it with a strong conviction that great benefit had resulted from the free use of the antimonial remedy; but the following offers evidence which will be still more convincing to the reader; it also illustrates a yet more decided adoption of antiphlogistic measures: A young man, aged nineteen, was brought into the hospital, having been run over by a cart, and having suffered an extensive fracture of the ribs on the left side. The five upper ribs were believed to be broken, and the lung was evidently wounded. There had been rather profuse hæmoptysis. The chest was ascertained to be resonant in all parts, but his state was such as altogether to preclude any detailed examination. The lungs appeared to be loaded with fluid, but the pleura did not, as far as could be made out, contain either air or blood. The chest was put up with strapping, as is usual at this hospital. Within the twelve hours following the accident, the man's difficulty of breathing greatly increased, and he seemed indeed to be rapidly sinking. Still, however,

the pulse had rallied, and he was not in collapse, but dying rather from pulmonic embarrassment. Under these circumstances, Mr. Bryant determined to bleed him. Antimony in large doses had already been commenced. The first bleeding was at one o'clock at midnight, and was to the extent of ten ounces. It was followed by much relief; but by degrees subsequently the difficulty in breathing returned. About noon the same day a second bleeding to the extent of fourteen ounces was practised, and with, again, the most marked benefit. Within twenty-four hours of this second bleeding, the man might indeed be considered well. He could sit up in bed, and talked cheerfully. The antimony had, of course, been kept up. At present the man is fast becoming convalescent, having experienced no drawback whatever.—*Med. Times and Gazette*, Aug. 20, 1859.

ii. *An Argument for Antiphlogistic Treatment:* By Dr. HENRY KENNEDY, Physician Extraordinary to Sir P. Dun's Hospital.

In a paper "On the Change of Type Theory in Disease," in which it is maintained that the type is always changing, and necessitating in consequence corresponding changes in treatment, we meet with the following passages in favor of antiphlogistic measures:

Have those, however, who are now so ready to assert that nature is the curer of disease, ever fully considered her plans? I doubt it; else, it strikes me, they would not be so ready to find fault with the antiphlogistic treatment. For then they must have observed that nature's operations have ever a tendency—supposing them to go on to cure—to reduce the system. Take an affection of very common occurrence, and more frequently left to itself than, probably, any other—a common cold; and what do we observe? A general *malaise*; some loss of appetite and strength; a disinclination to follow one's usual pursuits; occasionally some slight loss of flesh; very commonly paleness of the face, and then recovery. But if we go a step farther, and suppose a case of fever, left to itself, what will be observed? An utter loss of strength, appetite, sleep, and flesh (in cases where the latter is not observed, a very unfavorable sign it is); a persistence of disease for two, three, or more weeks; often during this period the occurrence of hæmorrhage, of one form or other, more frequently from the nose than elsewhere, and much more frequent at certain periods than others; the occurrence of diarrhœa, quite independent of the typhoid type of fever, sweats, and, lastly, crises of different kinds. I have supposed the fever not to have been complicated at all. But who does not know how rare this is; and that the disease is ever presenting complications of the head, chest, or abdomen? One point, at any rate, is beyond all dispute—that the final result leaves the patient much reduced in every way; and, speaking generally, some weeks elapse before perfect recovery takes place. In other words, nature's cure is by direct and unmistakable antiphlogistics. But now-a-days, and when nature is made all in all, these facts, though so obvious, appear to be entirely overlooked; and yet I cannot but think they prove, most unequivocally, the direction which art should or may take; and that the present cry-out against antiphlogistics is not only

utterly groundless, but that this line of treatment may be followed with the greatest advantage, when the case requires it.—*Ed. Med. Jour.—Ranking's Abstract.*

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#### ART.—VII. *Radical Cure of Hernia.*

i. *On the Radical Cure of Hernia:* By REDFERN DAVIES, Surgeon to the Birmingham Workhouse Infirmary.

In cases of scrotal and femoral hernia where the tissues are relaxed and the rings of large size—say, capable of admitting easily two, three, or even more fingers—considerable difficulty and disappointment are experienced in endeavoring to effect a cure by the first operation; and it has occasionally happened to others, and to myself, to be obliged to repeat the process.

From a practical acquaintance with the subject on the living, and repeated trials and experiments upon the bodies of the dead who have been affected with hernia, I have been led to believe that the cause of such failures is not attributable to any fault in the theory of Wutzer's method, but to a defect in his instrument, and upon the following grounds submit to the decision of practical test that they will be obviated by the adoption of the accompanying mechanical improvements in the instrument.

Upon examining a case of scrotal rupture in which the operation for the radical cure has failed (supposing, of course, that it has been properly managed, together with the proper after treatment), the rings and canal will be found to be obliterated probably for some three-fourths of its extent, or there may only remain an aperture which will with difficulty admit a crow quill; and thus, though the patient may be greatly benefited, and with the aid of a truss resume his duties, a radical cure has not been effected.

That portion of the canal and rings which have been blocked up is invariably that which is nearest the abdominal walls. "The gut slips down behind the plug," are the terms in which both surgeons and patients express the mishap which has occurred, and the reasons for this, I believe, are as follows:

The anterior or superior layer of the invaginated integument is subjected to not only the pressure of the wooden plug to keep it in apposition with the opposed surface of the rings and canal, but also to the direct pressure of the compressor. The compressor exerts its influence exclusively upon the parts included between it and the upper surface of the said wooden plug, and in no wise affects the posterior parts, viz: the posterior layer of invaginated integument and posterior surface of canal and ring, whose only chance of being kept in

apposition depends upon the accuracy with which the plug fits the canal, etc., as a whole.

The floor of the canal, etc., especially where the tissues are lax, as generally occurs in cases of old and large ruptures, does not present in the same manner an opposing resistance to the wooden plug as does the compressor, and thus should the two former be not very accurately adapted the one to the other, adhesion cannot even be expected to occur.

The mouth, or internal opening of the canal, is funnel-shaped, with the posterior surface the more sloped. Consequently, if there be any weak point it will be there, and it is sure to receive all the shocks of the gut during the process of cure.

Besides, it is evident that a cylinder, even closely applied to the rest of the extent of the canal, cannot fill up its funnel-shaped mouth; but must leave an interspace, which will be on the posterior surface.

And, again, it is not always practicable to introduce to a sufficient extent a solid plug, which would best fit the internal ring, by reason of the resistance of the other structures to its passage, besides entailing an endless variety of such plugs.

By the adoption, however, of the principle I now propose, viz: a plug, whose lower half is capable of expanding, these difficulties are severally overcome.

Mr. Davies gives an account of forty cases, which embraces the three varieties of oblique, ventral, and femoral hernia, occurring in ages varying from one year to sixty-five; and in both sexes it will be seen (that thirty-seven are cured, five requiring the operation to be repeated, and two complete failures (in one of which smallpox appeared on the day after removal of instrument). In this list, however, the more recent cases I have operated upon are not included. They will appear at a subsequent period.—*Med. Times and Gazette*, Aug. 6, 1859.

ii. *The Radical Cure of Reducible Inguinal Hernia*: by OLIVER PEMBERTON, Esq., Surgeon to the Birmingham General Hospital.

[Extracted from an extended and tabulated paper which appears in the *Brit. Med. Jour.* July, 1859.]

Owing to the ability and perseverance of Mr. Spencer Wells, the lecturer on surgery at the Grosvenor Place School of Medicine in London, an operation for the radical cure of reducible inguinal hernia, on the principle of the invagination of the scrotal integument in the inguinal canal and rings, has at length taken firm hold on the attention of the profession.

The effect, more or less, of all protrusions of the intestine through the canal and rings, will be to approximate the latter; and in some instances, where the hernia has descended into the scrotum, to so shorten the former that the internal ring entirely disappears, and a wide external one communicates at once with the abdominal cavity in an almost straight direction from the pubes to the sacrum. My experience of the examination of ruptures, some thousands in number,

during the past twelve years, tells me that these features are those most commonly to be recognized in the cases applying for trusses. To this class of cases, pretty much as they presented themselves in the out-patient room of the hospital, I have applied the radical cure—with what amount of success I shall presently detail—but I may here state that it has not been by any means “uniform.” As I have no wish to cast a doubt on the value of this operation—for it must even now be ranked amongst the most promising that has ever been laid before us for the cure of an almost universal malady—but I do desire that we should be in possession of more of the attendant characters of the cases treated, in order that its individual usefulness may be really estimated. I cannot but fear that certain cases only have been selected for the proceeding; so that, at best, we possess only a partial knowledge of its applicability to the most frequent and most disabling forms of herniary protrusions.

I shall now offer a tabulated statement of eleven cases in which I have performed the radical cure at various times previous to the last three months, leaving for a future report more recent instances. The table, with the single addition of the occupations of the patients, is on the plan suggested by Mr. Wells. \* \* \* Of the preceding instances, then, five have resulted, at present, in the closure of the inguinal canal against all protrusion of the intestine. Five have so lessened the width of the passage that the amount of bowel escaping has been diminished considerably; so that a truss in some instances for the first time for many years, could be worn. One failed altogether, from the slipping down again of the plug of scrotum, on the removal of the instrument.

In the cases in which the bowel came down, the plug of scrotum yet remaining attached, the escape invariably occurred behind its position, at the anterior margin of the ring.

One case alone evidenced during the treatment some alarming symptoms; these were, however, accounted for by the advent of a slight attack of erysipelas.

The sloughs, which generally formed at the point of exit of the needle, were limited in size, and soon healed; whilst the suppuration in the canal of the plug, and in the inguinal one also, were throughout of the most manageable character.

Taking into consideration, therefore, the varieties of reducible inguinal ruptures in especial regard to the utility of this operation, my opinion is, that we may fairly estimate their prospects of cure, improvement, or absolute failure, according to certain characteristics which place them in three divisions.

In the first, we number the small, firm, well defined internal ring in the narrow canal, not much shortened. The more muscular and youthful the subject the better.

In the second, a large ring, relaxed, with a broad short canal and surrounding flabby textures, as in so many old ruptures.

In the third are the protrusions of persons in very advanced life, the features of which are marked by vast size, by long continuance, and by their constant enemy, the bronchial cough.



## ORIGINAL COMMUNICATIONS.

(Continued from page 842.)

ART. VIII.—*Cases of Vesico-Vaginal Fistule Cured by the Button Suture.*

By T. G. RICHARDSON, M. D., Professor of Anatomy in the Medical Department of the University of Louisiana.\*

ALTHOUGH it is now an established fact that the great majority of cases of vesico-vaginal fistule can be cured by some one or other of the various operations now in vogue among surgeons, I am happy to be able to add my humble testimony to the value of the procedure devised by Dr. Bozeman, and herewith subjoin an account of some unusually severe cases in which the button suture proved entirely successful. In a future number of the Journal, I hope to be able to add histories of others of a still more serious nature, now under treatment, which it will be necessary to illustrate by means of engravings, in order that their peculiar difficulties and the modifications of treatment adapted to each may be fully understood.

CASE 1. *Two Vesico-Vaginal Fistules of Nine Months' standing: Three Operations with the Button Suture: Recovery.*—Mrs. S., a free woman of color, of the Parish of St. Mary, Louisiana, came to the city in December last, to consult my distinguished colleague, Professor Stone, of the University of Louisiana, who having ascertained the nature of the case, was kind enough to place her in my hands for operation.

The patient was quite fat, and apparently in good health, but somewhat flabby in her muscular development, and suffering, as usual in such cases, under great mental depression. She had been married two years, and was confined with her first child in April, 1858. The labor was difficult and prolonged, lasting according to her own account, eight days, and was only brought to a termination by the use of instruments. The urine was perceived to come from the vagina seven days subsequent to delivery, after which time none passed the natural outlet.

*Examination.*—Upon exposing the vagina with a lever speculum, I found the anterior wall of the canal only about an inch and a quarter

\*I am indebted for the notes of these cases, as well as other most valuable assistance, to Dr. Wm. M. Mercer, of Houma, Louisiana.

in length, and immediately in sight; occupying the centre of the vesico-vaginal septum, a transversely elongated opening large enough to admit the end of the fore-finger. Farther search disclosed a similar rent, of about half the size at the junction of the septum, with the anterior lip of the uterus. The latter opening was situated a little to the right of the median line, just behind the corresponding half of the former, and separated from it by a narrow band not more than a fourth or, at most, a third of an inch in width. Through these two openings all the urine escaped into the vagina, keeping it in a constant state of irritation, and passing out at the vulva it had encrusted the labia and inner sides of the thighs with a firmly adherent layer of phosphatic deposit.

*Operation.*—The patient was admitted into our Infirmary, where she could obtain every convenience necessary to her condition and be more immediately under my control, and on the 24th of January I proceeded to operate upon her, having on the previous evening ordered a dose of laxative medicine for the purpose of thoroughly clearing out her bowels, and followed it in the morning by a full opiate in order to put them completely at rest.

Not deeming it necessary here to describe the successive steps of procedure, the details of which are familiar to the profession, I will only mention that in this case the band of tissue separating the two fistules being too narrow to be of any material service, I determined to try to close both openings at once by paring the anterior edge of the larger and the posterior edge of the smaller, connecting the two raw surfaces at each of their extremities by a free denudation of the intervening parts. In doing this I was obliged to shave off a small portion of the anterior lip of the uterus, the smaller opening being situated, as already mentioned, in the *cul de sac* formed by the junction of the vesico-vaginal septum with the neck of the uterus. Five silver ligatures were employed, two of them passing through the anterior lip of the os tinæ, and notwithstanding the wide interval between the anterior and posterior edges, the parts were brought together by very moderate traction. A leaden plate of a semi-lunar shape, and bent to conform to the irregular surface upon which it was to rest, was passed down upon the wires, and the whole secured in the usual way by means of perforated shot.

The patient was placed in bed upon her back and a Sims's catheter introduced, the latter having a long gum-elastic catheter attached,

for the purpose of conducting the urine into a vessel. And here I would take occasion to caution inexperienced operators against allowing the silver catheter, when a tube is attached, to bear with its whole weight upon the inferior wall of the urethra, as sloughing and consequent fissure of the canal are thereby likely to occur. The best mode of supporting it, according to my observation, is by means of two strong but light threads tied around the junction between the silver catheter and elastic tube and fastened above, not to a band carried around the loins, as is frequently done, but to a couple of short strips of adhesive plaster placed upon the abdomen just above the anterior superior spinous processes of the ilia. By this method, which did not originate with me, a sufficient support is given to the instrument, while the natural resilience of the skin of the abdomen prevents any serious strain upon the parts when the patient turns upon her side.

No leaking occurred for five days, but about that time urine began to escape again from the vagina, and upon exposing the parts four days afterwards I found that most of the sutures had ulcerated out, and the plate hanging from the anterior edge of the larger opening. There had been no union whatever, so far as I could discover.

March 3d, I operated again, and this time, previously to paring the edges, I cut away entirely the band separating the two openings, and thus converted them into one large irregular chasm which extended from the root of the urethra to the anterior lip of the uterus. Seven ligatures were required, three of them penetrating the anterior lip of the neck of the uterus. But notwithstanding every care in paring the edges freely and in bringing them accurately together, and in the after treatment of the case, I was again disappointed in the result. The sutures gave way, and only a slight union occurred at each extremity of the rent.

March 30th, operated for the third time under more unfavorable circumstances than ever, the margins of the fistule being so soft and tender as barely to sustain the traction necessary to bring them in contact. Moreover, the hæmorrhage was profuse, amounting to not less than sixteen or eighteen ounces. Suspecting that the failure of the previous operations might be due in a great measure to a want of a sufficient tonicity in the patient's system I ordered for her, immediately after the operation, stimulants and rich diet, allowing her daily a little brandy toddy, a quart of good Édinburgh ale, and a liberal supply of beef tea and chicken soup. The result was a perfect

success, and upon removing the sutures ten days after the operation the parts were found to be in a most admirable condition, for not only was the fistule entirely closed but the septum presented a perfectly healthy appearance. The os tinæ, it is true, was within a very short distance of the meatus urinarius, but I have no doubt that it will finally return to its natural position, if it has not already done so.

In ten days after the removal of the sutures the patient returned to her home, having in the mean time regained sufficient control over her bladder to enable her to retain her water for four or five hours during the day, and a longer period at night.

*CASE 2. Very large Vesico-Vaginal Fistule of Eleven Months' standing, with Complete Extroversion of the Bladder: One Operation with the Button Suture: Recovery.*—I am indebted for this case to my much esteemed friend Dr. Tacket, of Holmes County, Mississippi, from whom I also obtained the following brief history:

Jane, a negro woman, belonging to Col. C. J. Bates, of Holmes County, eighteen years old, and of a robust constitution, was confined with her first child in June, 1858. The labor was protracted and difficult, and was not brought to a close until a resort was had to cutting instruments, and the fœtus removed piece by piece. The cause of the difficulty was then fully explained, for the fragments being put together showed a twin monster of very large dimensions. Dribbling of urine from the vagina followed three or four days after the delivery.

*Examination.*—On exposing the parts in the usual way, I found the vagina completely filled by a large, soft, florid tumor which protruded even beyond the nymphæ, and which I immediately recognized as the bladder in a state of inversion—or more properly, extroversion. Directing the woman, who was resting upon her knees and elbows, to empty her lungs and then hold her breath without straining, I grasped the tumor in my fingers and gently compressed it, while I at the same time endeavored to return it to its natural situation. This manipulation being kept up for a few minutes, reduction was accomplished, but only for a moment or two, as the tumor was reproduced so soon as the slightest straining occurred. I succeeded, however, in getting a fair view of the opening and estimating its dimensions, and I must confess that at the first sight the large size of the gap rather staggered my faith in the capability of the surgical

art to supply a remedy. The opening was quadrangular in shape, oblique in its direction, and measured at least an inch and a quarter in its longest diameter. Its anterior margin lay about half an inch behind the root of the urethra, and the posterior trenched slightly upon the anterior lip of the os tinæ, so that its antero-posterior diameter was scarcely less than the oblique. As unpromising however as the case appeared, I determined to give the poor creature the benefit of an operation, and for that purpose had her conveyed in due time to our Infirmary.

*Operation.*—Having previously, upon several occasions, exposed and handled the parts, in order to accustom the patient to the necessary position and manipulation, and thus enable her to afford all possible aid, by restraining the action of the abdominal muscles, I proceeded to operate upon her on the 8th of May. To keep the bladder in place I employed a sponge mop with a curved staff which was entrusted to an intelligent assistant, and I was surprised at the comparative ease with which it was effected. I had apprehended great difficulty upon this score and am convinced that if I had not frequently practiced the patient in the way just mentioned, my fears would have been in a great measure realized.

In this case, as in the proceeding, I was obliged to pare a narrow strip from the anterior lip of the uterus in order to get a sufficient extent of raw surface. The introduction of eight silver ligatures, two of which were carried through the anterior lip of the uterus, and the application of a crescentic leaden plate bent upon its long axis and confined in the usual way, completed the procedure.

The patient having been somewhat weakened by her long confinement I ordered a free supply of ale and beef tea, to be given to her daily.

The day after the operation she had a chill, followed by decided fever, which latter continued without remission until the fourth day, when the characteristic eruption of measles appeared upon different parts of her body. During the progress of the fever the stimulant was suspended, but the opiate, which is always used in these cases to quiet the peristaltic action of the bowels, was continued, and teaspoonful doses of acetate of ammonia given every hour or two. However, on the eighth day there was a copious alvine evacuation in spite of the opiate, which latter I then ordered to be stopped. No escape of urine followed this disturbance which thereafter occurred daily.

I did not remove the suture apparatus until the 11th day, when I was delighted to find the fistule completely closed. My pleasure was however slightly moderated upon observing a drop of urine escape from one of the orifices made by the ligatures, the occurrence of which was evidence that the vesical mucous membrane had been perforated by the needle. This new opening continuing to permit the passage of a few drops of water for several days, I pared its edges and closed it with two very delicate ligatures, which proved entirely satisfactory.

(To be continued.)

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## REVIEW.

*Anatomy, Descriptive and Surgical*: By HENRY GRAY, F. R. S., Lecturer on Anatomy at St. George's Hospital: The Drawings by H. V. CARTER, M. D., late Demonstrator of Anatomy at St. George's Hospital: The Dissections jointly by the Author and Dr. CARTER: With 360 Engravings on wood. Pp. 754, royal 8vo. Philadelphia: Blanchard and Lea, 1859.

THE transient gleam of individual life is insufficient to receive and reflect the increased and increasing lights from the vast expansions of universal science. In the primitive and dark ages the horizon of human knowledge was comparatively limited. The law-giver, judge, patriarch, warrior, priest, doctor, "philosopher and friend," were often concentrated in the same individual, and the amount of contemporaneous science having been little, was soon mastered. It is no longer so. The sciences have made vast progress, their number has been multiplied, and the principle of subdivision has been applied to nearly all. As neither individual genius nor knowledge is universal, it is important to economize in science by selecting that which is adapted to individual aims and to the brevity of life. To the medical man anatomy is "first, midst, last," in physiology, surgery, and clinical practice, even though it may as yet throw in some cases only a glimmering and uncertain light, yet still the surest light on his

pathway. The anatomist stands on the pinnacle of medical science where the rays of discovery are most likely to appear.

From the modern progress of anatomy and the art of engraving, daguerreotyping or photography, the means and the materials exist by which anatomical text-books, far more complete than any now in the hands of students, might be prepared. Although the best works on anatomy never can be substitutes for actual dissections, they are nevertheless of incalculable benefit, by reviving and perpetuating the knowledge gained during student-life in the dissecting room, when, as often happens, the practitioner has no longer in after life the opportunity or time to refresh his recollections of practical anatomy. Whether Mr. Gray's book has realized the high ideal which the state of anatomical science justifies and demands, it may not be improper to inquire. This inquiry, however, must be very limited, as the book did not reach the latitude of New Orleans until the present number of this journal was nearly through the press. The work already launched upon a Mississippi stream of eulogy, will doubtlessly have a prosperous voyage and good market. In glancing at this book, several remarks have been suggested, which, independently of their bearings on the work, may possess more or less value to the diligent student. Some of these topics which do not materially affect the merits or demerits of Mr. Gray's book may be noticed before proceeding further. But first of all, Mr. Gray's and the American publishers' prefatory platform will be announced as follows :

Mr. Gray says in his preface, "This work is intended to furnish the student and practitioner with an accurate view of the anatomy of the human body, illustrated by numerous accurately lettered engravings. A brief account of microscopical anatomy has also been introduced." Of his assistants and engravers he speaks in terms of the highest commendation, and of the drawings as having been made with great care and fidelity. The book is dedicated to Sir Benj. C. Brodie.

The American publishers give notice that "the London edition contained numerous typographical errors," and that "the passage of the work through the press has therefore been superintended by a competent professional gentleman, whose watchful scrutiny, it is hoped, has secured corrections so essential to a work intended both for study and reference ; no additions to the text have been found necessary."

Mr. Gray says that "the mesenteric glands vary in number from

one hundred and thirty to one hundred and fifty, and are about the size of an almond" (437). His compatriot, Professor Quain, in the fourth edition of his *Anatomy* (647, Lond.), says: "The mesenteric glands vary in number from one hundred and thirty to one hundred and fifty, and in the *healthy state are seldom larger than an almond.*"

Correctness in descriptive anatomy, desirable in itself, is very important in both surgical and pathological anatomy, especially in relation to the normal figure, color, consistence, size, and other conditions, so that the student may know any abnormality, fundamental alteration, or even slight departure from the healthy standard. Mr. Gray's anatomical postulates concerning the mesenteric glands (often the seat of serious disease) are calculated to mislead, and are therefore deserving of scrutiny.

In hypertrophy, mesenteritis, tuberculosis, scrofula, fever, etc., it is rare, except in *tabes mesenterica*, to meet with half a dozen, or even one of these glands as large as an almond. It is difficult to conceive how one hundred and fifty of these almond-sized bodies could be even packed, not to say disseminated, as they really are, in the peritoneal duplication of the mesentery, as it may be safely estimated that three of them would weigh at least an ounce, which would form an aggregate of three or four pounds for each healthy adult, being nearly the same as that of the brain—a ponderosity, which, after having made post mortem examinations of hundreds of Her Majesty's subjects, has not been witnessed in New Orleans. It is questionable whether the mesentric glands of the adult will average two ounces in the normal condition. Dr. Richardson, Professor of Anatomy in the University of Louisiana, says: "The mesenteric glands vary in number from fifty to a hundred, or more, and in size, from that of a grain of wheat to that of an ordinary white bean. (*Anatomy*, 295.)

Mr. Gray says that the arachnoid membrane "is almost destitute of [blood] vessels." (449.) He says again (444), of this membrane in the spinal canal, that "the arachnoid is not *very vascular.*" It is surely desirable in the reports, engravings and lectures on meningitis, cerebro-spinal-meningitis, arachnitis, etc., that the normal and pathological characters of this membrane should be better understood and more accurately fixed by definite criteria. Mr. Gray's statement implies that the arachnoid *is* vascular, but not *very* vascular.

Bichât denied, or wrote doubtfully as to the vascularity of all of the serous membranes. He says, "I think it probable that the serous



membranes have no blood vessels and am almost convinced of this. When these vessels are injected, they are easily raised with the scalpel from the external surface of these membranes, without affecting their continuity, which it is impossible ever to do in the mucous membranes. In examining these membranes (the serous) where both their surfaces are free, no blood vessel is perceptible; of this the *arachnoid* at the base of the cranium is an example." (*Treat. on the Membranes*; subsequently repeated in his *Anat. Gén.*) Since Bichat's time, statements conflict on this subject, particularly in regard to the anatomical condition of the arachnoid in arachnitis. Kölliker says that "the arachnoid membrane, either of the spinal cord or of the brain contains *no proper [blood] vessels.*" (*Mic. Anat.*, 1854, p. 398.)

The arachnoid is generally described as non-vascular, both in its normal and pathological conditions. Its morbid alterations consist chiefly in thickening, opacity, exudates, false membrane, and adhesions, but vascularity, injection, and redness never occur so as to be evident to unassisted vision, according to most observers of reliability. From its transparency, its inner or visceral surface next the highly vascular pia mater, seems as thus seen, almost always highly vascular, and being sometimes described as such in reports of post mortem examinations, in cases of arachnitis, etc.; on the other hand, its parietal portion consolidated upon the interior of the dura mater, may, in like manner, when the latter is morbidly injected, and vascular, give rise to a similar illusion.

Mr. Gray says: "It [the cerebro-spinal fluid] varies in quantity from two to ten ounces, being most abundant in old persons." (449.) Todd and Bowman's estimate is the same. As this estimate, on the one hand, does not appear to have been made by actual weight or measurement, so on the other hand, an adverse approximate estimate made by other authorities, and by the writer of these lines, that the average implied by the above statement is too high, also falls short of absolute demonstration. Two ounces as the minimum, with a range as high as ten ounces, is, it is believed after much observation and many estimates, to be quite above the normal standard. Few pathological anatomists would regard ten ounces of cerebro-spinal serosity in any other light than that of a grave morbid effusion, cerebral dropsy, hydrocephalus, or perhaps serous apoplexy. This sometimes is a question of high import in forensic medicine and always in pathological anatomy. A man receives a trifling blow on the head

and dies comatose in a few minutes or hours. At the inquest, an effusion amounting from two to ten ounces is found, and, perhaps, the medical witnesses may testify that the blow which caused no marked wound was apparently insufficient, of itself to cause death, and that the effusion must have been the effect of previous disease, and this testimony has actually been relied on and has had an important bearing where only two or three teaspoonfuls have been found. Physicians who have made no post mortem examinations may, though otherwise skillful, fall into grievous errors both pathological and forensic in this respect, and the more so because anatomists themselves differ so widely in the premises. That the cerebro-spinal fluid is most abundant in the aged, independently of disease, may be doubted. In the absence of satisfactory statistics, the contrary opinion seems most probable. Opportunities for the examination of the normal quantity of this fluid in the human subject, restricted as they are to deaths from accident, must be rare, and examinations for physiological purposes in such cases, almost unknown; consequently the physiological and pathological elements are blended, or differentiated with difficulty and uncertainty.

Mr. Solly, in his work on the Brain, says: "Magendie, in his estimate of the quantity of this fluid, agrees pretty closely with Catunnius. It varies according to the age and size of the *patient*, and usually bears an inverse proportion to the volume of the encephalon; seldom less than two ounces, and often amounting to five; in old age with *atrophy* of the brain, to eight, ten, and twelve ounces." Mr. Solly quotes and adopts this statement as illustrative of the normal—not the pathological anatomy of the brain, although upon its face the latter is inscribed as the words, "*patient, atrophy*," indicate. Professor Rokitansky says of the cerebro-spinal fluid: "The normal quantity of serum in adults amounts to three or four drachms—may often be estimated at an ounce, an ounce and a half, two ounces, or even more." (*Path. Anat.* iii. 258.) Andral (in his *Pathological Anatomy*, ii. 474-5) says: "In the human subject, the cerebro-spinal fluid usually amounts to about two ounces; its increase is the cause of serous effusion. M. Magendie is of opinion that the quantity of the fluid cannot much exceed two ounces without producing some bad effects." Kölliker says: "In the ventricles of the brain there exist, under normal conditions, an extremely small quantity of clear serous fluid; a second fluid, the *liquor cerebro-spinalis*, contained in the sub-arachnoid spaces." (*Mic. Anat.* p. 401—1854.)

It has been already stated that the materials exist for the great book of anatomy which is for the medical student, the book of books. "The good is coming"—or has it already come? and does it usher in the illustrious and the illustrated anatomy "descriptive and surgical?" If the book has not come, come it will. It will be a work which will defy alike indiscriminate praise and censure. It will be a work in a few royal octavos or small quartos. Some of the folios already extant, such as Bourgeroy and Jacob's\* are probably as complete as art can make them, but they are too ponderous for daily use, and too expensive for any but opulent individuals, or institutions. The coming book will have the illustrations the size or half the natural size with a few exceptions, such as skeletonic views of the entire osseous, vascular, and neurological systems, etc. It will be, to a great extent, even in discriptive anatomy a duality up to the adult period, that is, it will present the parallelism and differentiæ of the infant and adult bones, etc.; also, the parallelism and differentiæ of the male and female, black and white—normity and abnormality. Monstrosity, which is limited in its deviations and transpositions, has types and a uniformity of its own. The so-called *Lusus naturæ*, a fortuitous play of Nature, is no play at all, but is, as Gœthe said of hell, governed by laws. The coming book, besides its microscopic, morbid, surgical, comparative and philosophical anatomies, will contain embryological anatomy. In fine, it will be a book which Nature herself will look upon with admiration, if not with envy.

It may be proper to glance, in a rapid manner, at the first part of Mr. Gray's book or rather illustrations, offering a few occasional remarks on other portions. The point of departure for student as well as the book, is osteology. It is the osseous system, which being best known and at all times most accessible to the author in his study, and the artist in his studio, that may be supposed to show the greatest accuracy. It is here that the author and reviewer, book and bone, must be tried in the august presence of the skeleton. Skeleton, however, Mr. Gray has none; nor infant, nor adult, nor male, nor female skeleton has he! nor has he even a death's head, or pelvis either among his osteological engravings. The student must engrave in his own imagination articulated skeletons, etc., with front, posterior, lateral, superior, and inferior views. These general synthetic compositions

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\* Allusion is here made to vols. I and II. The other volumes have not been seen by the writer; if there be any work equal to this, he has not seen it.

he must imagine. He must be content with the separated bones, a fragment here and another there. The pieces are labelled, but are they artistic? are they accurate?

"A man who has never seen the light of the sun, cannot be blamed for thinking that no glory can exceed that of the moon." Persons who have seen wood engravings no better than those in Mr. Gray's Anatomy may innocently hold and publish the same opinion. But if they will look into even the cheap duodecimos, such as Jamin's *Anatomie Descriptive*, or the elementary French school books, as Milne-Edward's *Zoölogie*, Beudant's *Géologie*, or the octavos, such as Morton's Anatomy, Draper's Physiology, not to name a thousand other works, including the illustrated newspapers and magazines, they may find more elegant and accurate engravings than Mr. Gray's, or rather Dr. Carter's, as reproduced in America. They may have been better or worse in the English edition, which is not present for comparison. In fact, these anatomical illustrations are sometimes neither recognizable in themselves nor by the aid of the text, without reading the labels, which, like Pharaonic cartouches, are placed upon objects otherwise dubious. This novelty, which claims to be the great peculiarity of the work, may possibly be very useful where the objects are sufficiently large to allow of the lettering of the locality where the muscles arise, or are inserted, as well as the course of vessels, etc. This might generally be done by means of lines drawn on the object, the names being placed along and outside of its margin, as has hitherto prevailed. But the substitution of Pharaonic cartouches or apothecaries' labels for much of the engravings of the things themselves, almost covering the latter, is not that short and royal road to knowledge which has long been sought but never found. Nor is it edifying. When the objects are small and numerous, and patched over with a multitude of names huddled together, "confusion is worse confounded" thereby, and both artist and anatomist may well exclaim with Lady Macbeth,

"Out, damned spots!"

The utilitarian cares little, however, for execrations æsthetical. But when the novelty of the lettered labels has had its day, good taste and real utility will, probably, alike reform this innovation or put it away altogether.

Even in some of the larger figures of this work, the label is almost indispensable to ready recognition of some, such as fig. 69, represent-

ing the os innominatum, not to name others of still more "questionable shape."

Several of the osteological engravings, instead of being copied from Nature (that is, the bones themselves, which are always accessible to the artist) seem to be copies from Quain's Anatomy, but so far as examined they are copies of neither. For example, the 22d illustration, representing the internal surface of the parietal bone, one of the simplest and most easy in execution (in osteology) is unlike Quain's (fig. 38, 4th ed., Lond., 1837), which is bad enough, and will not bear a comparison with Bourgery and Jacob's (fig. 4, pl. 21). Here the chief features, whether anatomically or surgically considered, are the grooves or canals of this bone, in which the middle meningeal artery and its branches lie. In Quain's figure these are, of course, represented as dark depressions, but in Gray they appear as raised white ridges, like large, salient, injected arteries, so that both Nature and Quain are reversed. This bone in the skeleton is about five inches in its transverse and a little less in vertical diameter. The figure, which is not exactly proportioned, is reduced about one fifth, which, however, the student must find out for himself.

The surgical anatomy (announced in the title page) of Mr. Gray's book is of the most meagre and unsatisfactory character, and, therefore, it is not surprising that he should here omit to impress upon the student the danger attendant on the fracture of this bone, which often occurs—danger not directly to the brain, but secondarily from hæmorrhage—not hæmorrhage within the brain, but between it and the inner surface of the cranium, owing to the rupture of the meningeal artery imbedded in the canals or grooves of the parietal bone, causing, if left to itself, a sure, gradually increasing compression, coma, and death. Usually a large, symmetrical saucer-shaped disk of blood forms around the lesion of the artery as its centre, which, augmenting hourly, compresses the brain, although no depression of the bone occurs.

The 77th fig., an anterior view of the humerus (one of the simplest of the bones), limits the space for the insertion of the deltoid very near the middle of the bone—in fact, a considerable portion of it extends beyond the middle, on the distal moiety. According to the text, "the muscular fibres [of the deltoid] proceed downwards and are inserted, etc., into the *middle of the shaft*" (247). The insertion is not in the middle, as a whole, although the extreme distal portion is,

while the main central insertion is in the middle third of the bone in both whites and blacks. The place of the origin of the mastoid muscle (fig. 72, p. 85) commences at a considerable distance from the sternal end of the clavicle and extends to near the middle of the bone distally. Now, according to the figure (136, text, p. 204), "the clavicular portion arises from the inner third of the clavicle." Here there is no exception made corresponding with the erroneous figure 72. The figures of the humerus are two-thirds of the natural length, which the student is left to divine. In a male skeleton of five feet nine inches, the humerus is twelve and a half inches; in a white female of ordinary height eleven and a half; in mulatress of average height and of seventeen or eighteen years, eleven inches; in white children aged four or five, four inches; in full sized infants at birth, three inches. At least such are the measurements made by the reviewer.

The figures (72 and 73) of the simple bone, the left clavicle, are not only rough and inelegant, but inaccurate, as will be seen presently. The view of the interior surface (73) is not recognizable as the same bone which it purports to be, having an enormous curve, amounting to deformity, provided fig. 72 be accepted as the normal figure, as it claims to be. These figures are nearly the size of life, being only about eight or ten lines less in length, although as usual the student is not apprized of this important fact.

These figures are, as already mentioned, inaccurate; 72 purports to be a view of the superior surface of this bone, presenting a deep downward curve or concavity, which is precisely opposite to Nature; for this bone is, upon its superior surface, nearly horizontal, though sometimes a little convex, which is therefore contrary to the figure. The anterior and posterior have been mistaken for the superior and inferior views of this bone.

On referring to the text, after having written the above, it was found that the author himself, in referring to the horizontal or superior configuration of the surface of this bone as represented, or rather misrepresented, in the figure (72), says that "the *horizontal* plane of the clavicle is nearly *straight*" (84), which is equally true of the inferior surface *in situ*, whilst the figures present a series of salient and retiring curves or bends, like the bends of the Mississippi.

The coarse figure (75), giving a posterior view of the scapula nearly as large as nature, gives an elevation to the coracoid process

equal to that of the acromion, whereas it should be much lower, and would be almost wholly concealed by the latter according to the assumed position of the bone in the engraving.

The second cervical vertebra (fig. 3), is not only of "questionable shape" but larger than nature. The view, though a lateral one, and not designated as such, exceeds two inches from the middle of the superior articulating surface to the end of the spinous process, which is about half an inch, or one-fourth more than is required. The odontoid process, which in its horizontal diameter is half an inch, and in its vertical diameter one inch, is colossal and withal hideous. If the vertebræ are to be studied in books, superior, inferior, anterior, posterior and lateral views, and harmonious proportions should be given, and so of all other anatomical structures. A contrary rule has been generally followed in this work. A number of magnified figures are given which are not mentioned as such at all. This must be attributed to the theory that the student was born a histological microscopist. The skeleton hand (the left), page 104, is, but of course accidentally, of the average, size of the adult, but the palmer view of the same hand, p. 106, is an inch shorter. The fig. 17, a view of the entire spine, eight and a half inches in its vertical diameter (about one-third of the natural length, which the student is left to guess, or measure at his leisure), shows the vertical capacities of the pages of the book. Here there is but one view (the lateral), instead of three, or at least two equally important, the anterior and posterior, or even one of them would show the position and configuration of the transverse process. Neither the adult nor foetal skull of the white or the black is figured, whereas front, posterior, superior, inferior and lateral views and horizontal, vertical and lateral sections are required for studying the great head and centre of human anatomy, and not merely separated bones. As there are no figures of the skull, the sutures are not represented in an intelligible manner by the separated pieces or sections. This work contains no figures of the foetal, infantile, or adult pelvis of either sex, normal or deformed, all of which might be represented of half the natural size. Nor are there any figures of the skeleton, as already mentioned, which the page might give reduced to one-eighth of the adult size. In osteology the sizes of the sexes and of the foetus, and of particular parts are often of high value in forensic medicine.

"It [the mastoid muscle] is," says Mr. Gray, "directed *perpendicu-*

larly upward." (204.) According to the figure (136), and nature too, this muscle is directed upward and backward with a marked *obliquity from the perpendicular*. The expansive page of this work affords sufficient area for illustrations often as large as the adult size. Three-fourths of the engravings might have been either thus large or uniformly half sizes. Yet this advantage has been thrown away very generally, or used capriciously so that the student has no guide as to the actual or comparative magnitude, even in microscopic figures. The cuts would almost always correspond with an ordinary octavo or even duodecimo as they appear in this book, and might be even larger than the normal size. The book builders and publishers however, sometimes issue engravings duly lettered illustrative of microscopic subjects in the quarto or folio instead of the 32 mo. form. A large book, though inconvenient, is, or may be more ornamental than useful, where the scientific aim of the work does not necessarily require weight and magnitude to represent a cell, fibre, capillary vessel, or an infusorial animal, invisible by the naked eye.

The proportional volume or magnitude is ignored throughout the work; some exceed nature or are reduced in size without notice. Microscopic objects are often mentioned as magnified, but *how much* the student must find out, if he can, as he is not informed, it is believed, in a single case. See figures 274, 275, 280, 292, 293, 294, 295, 300, 313, 314, 315, etc.

Figure 187 represents the aërta as nearly an inch, the pulmonary artery three quarters of an inch, and the heart two and a half inches respectively in their transverse diameters. The former, therefore, ought to be reduced or the latter enlarged about half. The proportion of parts in this complex figure has been disregarded in other details.

Figure 280 represents the lens in a novel duplex form—one in the other—both invested with strongly drawn capsules!

There is no representational dissection of the entire fascia, nor even of the fasciæ of the great divisions of the body, as the head and neck, the trunk and limbs. This department of anatomy, which is of great importance physiologically and surgically, is usually much neglected in dissections, but when well dissected or represented by engravings is easily understood and makes a strong impression on the memory. Mr. Gray does not allude sufficiently, if at all, to its surgical anatomy in reference to tumors, abscesses, etc.

Mr. Gray says that "the mucous membrane of the stomach is thick;



its surface smooth, soft and of a *pulpy consistence*. *During infancy, and immediately after death, it is of a pinkish tinge; but in adult life and in old age it becomes of a pale straw or ash gray color*" (605). "Thick, smooth and soft," though vague epithets, may be allowed to pass, but with these exceptions the description is altogether erroneous. *Pulpiness, softening or ramollissement* is a fundamental test in the pathological anatomy of the stomach. The residue of the description by Mr. Gray is visionary.

Pulpiness in its application to the mucous membrane, is virtual gangrene. It begins in softening—ends in disorganization. Many pathological anatomists consider this tissue morbidly altered when narrow strips cannot be peeled for half an inch or more without breaking. Softening or *ramollissement* is an innocent babe compared with a pulp.

Mr. Gray says the lachrymal or "nasal duct is about three-quarters of an inch in length" (567); yet his figure of it is very near two inches long being only a line or two less.

Mr. Gray says: "The peritoneum invests, more or less *completely*, all the viscera (599). The first or ascending portion of the duodenum is *completely surrounded by this membrane*. \* \* \* Ileum: The remaining portion is *completely surrounded by the peritoneum*," etc., (607). The fundamental idea and anatomical reality of the serous membranes, whether in the chest (the pleura) or in the head and spine (the arachnoid) is altogether irreconcilable with the phrases to "surround" and to "*surround completely*," which are repeated a number of times, and are the strongest possible terms. The peritoneum is a closed, empty sack. It neither encloses nor "*completely surrounds*" any organ whatever. It is like the circle, without beginning, without end. Its duplications and flexuosities overlies or cover the abdominal parietes and organs, with few exceptions, leaving those organs and parietes themselves *outside of it*. If it were peeled or dissected off from its areolar attachments, it would be found to be absolutely empty. Its duplications, flexuosities and reflections often approximate and are sometimes fixed near each other by intervening areolar tissue, without enclosing even a blood vessel. The interior of this closed sack is, as all know, free. Its exterior is soldered to the parietal and visceral surfaces. Some portions of its parietal reflections have very little, if any, adhesion on its exterior to the internal abdominal walls, etc.

Mr. Gray says that "the os uteri is *round* in the virgin." (636.)

Madame Boivin and M. Dugès' 2d pl. fig. 2, represent the virgin *os tinæ* at puberty, as having the form of a *transverse slit*, even a little curved or crescent shaped. Cloquet says: "It [the *os tinæ*] presents at its summit a *transverse slit*." (*Anat.* 816.) Todd and Bowman describe it as "a *transverse opening*, bounded anteriorly and posteriorly by two thick and rounded lips" (*Phys. Anat.* 842); "usually a *transverse fissure*" (Meckel. *Anat.* iii. 401).

"The uterus," says Mr. Gray, "measures about three inches in length and two in breadth at its upper part, and an inch in thickness. It weighs at puberty from seven to twelve drachms, and after parturition from an ounce and a half to two ounces (686). \* \* \* Each ovary is about an inch and a half in length, three quarters of an inch in width, and nearly half an inch thick, and weighs from one to two drachms" (689). These estimates of volume seem extremely liberal but not of lawful weight. If London virgin uteri at puberty be three inches long, two broad and one deep, and weigh only from seven to twelve drachms, they must be cork-like in ponderosity. How a very dense heavy structure of such a size should have so little weight it is difficult to understand. Seven eighths of an ounce!

After having written the last sentence, a uterus which long ago had been selected and preserved in alcohol (among others) as a type of the virgin state in a stout young woman, was taken from a jar and exposed to the air for an hour or two, so as to dry it sufficiently to approximate its original condition; it was then weighed, having attached to it the dissected ovaries and fallopian tubes, and, perhaps, a drachm or less of areolar tissue which had not been fully dissected away from its external parietes.\* The womb was first opened, in order to ascertain whether any fluid or other substance was in its cavity, before weighing. The weight was found to be three ounces and nearly one drachm. Nearly a drachm (by estimation) of cellular tissue which might have been dissected without interfering with the integrity of the organ, may be deducted, which will leave three ounces avoirdupois.

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\* A few notes from the post mortem history of this subject (one of an extensive series) are subjoined: Miss \*\*\*, aged 27, well developed, a little over the average size, of vigorous constitution, was seized with yellow fever attended with furious delirium, and finally coma, and died after an illness of three days and a half. Dissection about thirty minutes after death; body warm; free from rigidity; no emaciation; muscles massive, natural; fatty tissue abundant, firm, yellow; abdomen free from convexity; skin yellowish and marked with purplish spots, etc.

Prof. Meig's whose authority in matters of this character is inferior only to that of scales and weights, says: "The non-gravid womb weighs about two and a half ounces, according to the individual."

Even if Mr. Gray's maximum be assumed, that is, twelve drachms for the uterus and one for the ovaries, and another be thrown in for the tubes, the aggregate will be only fourteen drachms, or one ounce and three quarters.

Mr. Gray's statement that "the uterus after parturition weighs from one ounce and a half to two ounces," is still more unsatisfactory. Soon after parturition it will weigh about as many pounds. These data may often be important in medico-legal investigations.

"The conclusion of the whole matter" concerning the normal weight of the uterus is, that the student and courts of jurisprudence should reject provisionally all the authorities, until further and more authentic statistics tested by actual weight shall be obtained in a reliable form. Some great authorities assert that the womb at puberty weighs but five drachms, while their description of its volume cannot be reconciled with the extremely light weight which they mention. If Newton virtually weighed the solar system with accuracy, surely some industrious doctor might weigh a hundred *uteri*.

In the hurried examination made of this volume, numerous passages which were noted as worthy of remark, will be passed over. Upon first opening this book for review, it was under a bias in its favor that its ample pages were turned over. If the work be the reliable text book which it has been represented to be, and if the present review should underrate its excellence, its alleged faults are owing to the incompetency of the reviewer, and not to the wish or will to indulge in censure. If it be not the best possible work on anatomy, it is, on the whole, a good one; its descriptions being generally accurate and concise, and its engravings calculated to assist the earnest and diligent student.

B. DOWLER.

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REV. II.—*Brief Exposition of Rational Medicine*: By Jacob Bigelow, M. D. Pp. 73. Boston: 1858.

It is not intended to enter upon the details of this book, which is a reproduction from Dr. Bigelow's volume, "Nature in Disease." In ani-

madverting upon the fundamental principle of this book, no disrespect is intended to the author, whose scholarship and chaste style entitle his works to a place among the medical classics.

The unparalleled progress of the present time in certain branches of histology, chemistry, pharmacy, physiology, pathology and practical medicine, surgery and obstetrics, has been accompanied with an extraordinary revolution. Systematic Medicine is no more. The comparatively recent nosologies of Cullen and Brown, of Good and others are no longer the *incubi* of the medical mind. General theories of therapeutics are distrusted or repudiated. Rush's "Defence of Bloodletting" to the extent proposed by him has no defenders. Bouillaud's spoliative, syncopal venesections, *coup sur coup*, which he calls *formule, la nouvelle*, and which he puts as the antithesis of the *ancienne méthode*, has received its *coup de grâce*, as well as Broussaisism and Cookism. On the other hand, extreme scepticism in therapeutic agents and a supreme devotion to, and belief in, the healing powers of Nature have followed. Scepticism continues. It increases. If extremes meet, here is an example: too much and too little faith.

From this existing scepticism, as a point of departure, both experimentalism and rationalism proceeds to renewed investigations and to re-test facts and opinions heretofore doubtful or too confidently believed upon insufficient evidence—an example of which may be found in the late and almost exclusive reference of diseases to a local origin with solids as their primary source, to the exclusion of the fluids and the general system. Previous achievements stimulated hope and afforded encouragement for the future, as one improvement or discovery leads to others.

On the whole, medicine has been and is progressing (for even its scepticism on certain points is only an armistice preparatory to the better understanding of boundaries and principles). Nevertheless, amid this progress in knowledge and these salutary reforms, legitimate Medicine has been and is still menaced by the most formidable charlatanries ever known even in the dark ages. Among these, formidable for numbers, may be enumerated homœopathists, hydropathists, Thompsonians, Eclectics, Physio-medicalists, Mesmerists, Spiritual knockers, and Clairvoyant doctors, and other sects.

Among these charlatans are some renegades from the regular profession, who have probably obtained a larger clientship, if not an increase of medical skill by the change. But as sometimes happens,

“ a man's enemies are those of his own household,” so it has been in the medical profession, a few of its learned and influential members having taken it upon themselves to propagate the doctrine that “ all medication, at least all active medication must be abandoned.” These philosophers, whose publications have only of late attracted any marked attention, have not promoted, but have followed, yet in an aberrant manner the revolution that has been going on adverse to ultra-antiphlogisticism, for a quarter of a century. The reaction against Broussaisism, against syncopal venesection, against drachm and ounce doses of calomel, against excessive catharticism, and against polypharmacy in general, was the natural reactionary result growing out of experience and the progress of the medical sciences, which the sceptics, who labor to establish a fundamental antithesis between Nature and Art, have neither hastened nor retarded. Gaubius (to go no further back) has said all that the present writers can justly say in favor of the healing power of Nature in disease. In his work, *Institutiones Pathologiæ Medicinalis* (anno 1758), he devotes a considerable space to “ *The Medicative Powers of Nature*,” concluding with the following question : “ Is it, then, a matter of greater consequence whether, according to Hippocrates, Nature is called the curer of diseases, or, according to others, the soul or *archæus*, when they who perceive differently concerning these things, do however restore mankind to the same state of health, and can even agree in the method of cure ?” (§649.) His work abounds with extreme eulogy of the healing powers of Nature, but not with denunciations against medication.

The late M. Chomel says, “ The Institutes of Pathology by Gaubius is certainly one of the most remarkable works ; we discover in many portions ingenious thoughts, profound reflections and luminous comparisons, which justify the favorable reception accorded to the work, and its well deserved reputation at the present day. He was the first to give the name *General Pathology*.”

The gratuitous assumption of superiority has ever characterized charlatanry—the newest, most original, and most extraordinary form of which is that now advocated by several distinguished writers and their followers of the regular profession, namely, that active medication is not only useless but injurious. The question which they have raised is not, whether unskillful medication, but whether all active medication—not whether active medication in health, but in disease

—be not hurtful. The affirmative, yet unproven answer which they give, is based upon two questionable sources of *ex parte* evidence, namely, their superior, not proven sagacity in interpreting the powers of Nature, and on the undeniable healing powers of Nature in certain cases, which they erroneously generalize and array as antitheses of Art, as if medication however active, when skillfully directed, must be opposed to Nature, instead of being, what it really is, auxiliary to her healing, and opposed only to her destructive tendencies. Nature has even to an optimist, very often a dark as well as a bright side—abnormality as well as regularity—destructive as well as healing powers. Those who look solely to the latter and ignore the former, may be “wise in their own conceit” but not wise in the art of healing any more than those who, refusing to recognize the healing power of Nature, propose to expel her *vi et armis* from the sick room altogether. Inactive medication being no medication but a flat contradiction both in the abstract and concrete, it follows, that if all active medication must be abandoned all medication whatsoever must be abandoned, and that all medication is *per se* evil and evil only. If active medication be good in a single case, the whole theory falls to the ground, unless in the first place, this case shall be proven *sui generis* and in the second, unless skillful medication in all cases whatsoever shall be proven to be injurious and in conflict with Nature. The question is, therefore, whether skillful medication is injurious, that is to say, all medication however skillful. No one has doubted that bad and injurious medication is bad and injurious. Nor has any one doubted that excessive medication is dangerous, as well as excessive diet, or brandy. If it be admitted that there are medicinal agents which mitigate or remove certain morbid conditions or symptoms dangerous to life, the doses or quantities of these agents may, with some exceptions, be increased until the end for which they are given is attained, without being amendable to the charge of excessive medication. All beyond that desiderated finality is excessive, all short of its criminal omission, indirect homicide. Thus an acute enteralgia, colic, spasm, congestive, intermittent, etc., occasionally only curable by enormous doses of opiates, quinia, etc., including sometimes alcoholic stimulants, would often proceed unrestrained under minute or even the ordinary doses. Excess is a relative, and not a definite known proportion in posology. Those who denounce active medication carry their fanatic charlatany beyond that of homœopathy.

The latter maintain that the *activity* of medicinal agent increases with its infinitesimal division, dilution or attenuation. The former reject the large, and homœopathically speaking, the infinitely small alike. They rely (credulous souls!) upon the medical statistics of homœopathists to prove that the regular faculty are in the wrong path. They ought to adopt the homœopathic practice, if they adopt any as being less active than any other having no action whatever. They reject this system but accept the statistics of the homœopathists. Homœopathists need not concern themselves with arguments or ridicule against their theory, provided their claims to superiority in practice be conceded. He is the best doctor, all the world agree in affirming, who cures the patient.

B. DOWLER.

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## MISCELLANEA.

### i. *Materiality of Caloric.\**

Heat is readily communicated from one body to another; so that when hot and cold bodies are placed near each other, they speedily attain the same temperature. The obvious transference of heat under such circumstances impresses the idea that it possesses a substantial existence, and is not merely a quality of bodies, like color and weight; and when thus considered as a material substance, it has received the name of caloric.

It is convenient to adopt the material theory of heat in considering its accumulation in bodies, and in expressing quantities of heat and the relative capacities of bodies for heat. Indeed, everything relating to the absorption of heat suggests the idea of its substantial existence; for heat, unlike light, is never extinguished when it falls upon a body, but is either reflected and may be further traced, or is absorbed and accumulated in the body, and may again be derived from it without loss. \* \* \*

In adopting the material theory of heat, we are under the necessity of assuming that there are different kinds of heat, some of which are capable of passing through glass, such as the heat of the sun, while others, such as that radiating from the hand, are entirely intercepted by glass. But on the undulatory theory of the different properties of heat are referred to differences in the size of the waves, as the differences of color are accounted for in light. Heat of the higher

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\*Inorganic Chemistry; by Prof. T. Graham, late Prof. Chem. in University College, Lond. 1858.

degrees of intensity, however, admits of a kind of degradation or conversion into heat of lower intensity, to which we have nothing parallel in the case of light. Thus when the calorific rays of the sun, which are of the highest intensity, pass through glass, and strike a black wall, they are absorbed, and appear immediately afterwards radiating from the heated wall, as heat of low intensity, and are no longer capable of passing through glass. It is yet an unsolved problem to reverse the order of change, and convert heat of low into heat of high intensity. The same degradation of heat or loss of intensity is observed in condensing steam in distillation. The whole heat of the steam, both latent and sensible, is transferred without loss in that process, to perhaps fifteen times as much condensing water; but the intensity of the heat is reduced from  $212^{\circ}$  to perhaps  $100^{\circ}$  Fahr. The heat is not lost; for the fifteen parts of water at  $100^{\circ}$  are capable of melting as much ice as the original steam. But by no quantity of this heat at  $100^{\circ}$  can temperature be raised above that degree; no means are known of giving it intensity. If heat of low is ever changed into heat of high intensity, it is in the compression of gaseous bodies by mechanical means. Let heat of half the tension of the atmosphere, produced at  $180^{\circ}$ , in a space otherwise vacuous, be reduced into half its volume, by doubling the pressure upon it, and its temperature will rise to  $212^{\circ}$ . If the pressure be again doubled, the temperature will become  $250^{\circ}$ , and the whole latent heat of the steam will now possess that high intensity. When the air itself is rapidly compressed in a common syringe, we have a remarkable conversion of heat of low into heat of very high intensity.

The same author quotes and adopts from Mr. Hutchison a table showing the conducting power of different building materials, which in the hot climate of New Orleans is worth remembering: Slate is the standard— $100^{\circ}$ ; plaster and sand,  $18.70^{\circ}$ ; lath and plaster,  $25.55^{\circ}$ ; firwood,  $27.61^{\circ}$ ; oak,  $33.66^{\circ}$ ; stock brick,  $60.14^{\circ}$ ; fire brick,  $61.08^{\circ}$ ; slate,  $100^{\circ}$ ; lead  $521.34^{\circ}$ , etc. (33, 51, 96-7.)

### ii. *The Mortality incidental to Military Life.*

Perhaps the strongest or rather the most manifest proof of the fatal effects resulting from the absence of the physical comforts of civil life is seen in the armies. However well provided for, soldiers may be, as soon as they take to the field the work of death from exposure, etc., begins, and in a few campaigns, disease, rather than the most murderous battles, thins the ranks the most actively year by year, the victims being not the feeble, old or predisposed to disease, but the strong, young, or middle aged.

### iii. *The Microscope before the Anatomical Society of Paris.*—(Lancet.)

Dr. Gallard, secretary to the society, has, in his report upon the transactions of the society for 1858, examined the promises held out by the microscope. He finds that hardly any have been fulfilled, especi-



ally as regards the cancer-cell. Mr. Gallard does not concede that heteromorphous matter ever exists in the human frame, and does not believe that tubercular or cancerous deposits are substances differing entirely from normal tissues. He lays particular stress, whilst passing in review the pathological preparations which were brought before the society in the course of the year, or twenty-six cases of cancer, upon only six of which the cancer-cell was found. He sarcastically alludes to the fact that microscopists have gradually receded from the cell to the nucleus, and from the nucleus to the nucleolus, driven, as they were, by the difficulty of finding any characteristic element in morbid textures. The author concludes that, in the present state of anatomy, and especially histology, no criterion exists by which to distinguish malignant from non-malignant tumors.

#### iv. *Monographs.*

A practice began to prevail about the fifteenth century, which very materially contributed to advance the science of medicine, and especially the practical part of it—the publication of monographs of particular diseases and of individual cases, with the reports of hospitals or other public institutions. In each succeeding period we find this plan to have been more generally adopted, and at the same time, to have been much improved in its method. (Dr. Bostock, *Hist. Med.*)

#### v. *Arctic Regions. Cold.*

Sir John Barrow, secretary of Admiralty, (in his work entitled *Voyages within the Arctic Regions* (Lond. 1846, pp. 530), says of icebergs and glaciers, that in the vicinity of them, silence is necessary; the explosion of a gun scarcely ever fails to bring down masses of ice. Sir John quotes Beechy to show that on discharging a musket a glacier half a mile distant fell with a thundering noise head-long into the sea—the first wave of which was 96 feet broad and wrecked their boat throwing it on shore. Mr. Beechy confirms what has frequently been found and noticed, namely, the mildness of the temperature on the western coast of Spitzbergen; there being little or no sensation of cold, though the thermometer might be only a few degrees above the freezing point. The radiation of the sun in some sheltered situations, is so powerful during two hours on either side of noon, that frequently they observed the thermometer upon the ice in the offing at 58°, 62°, 67°; and once at midnight 73°, although in the shade at the same time, it was only 36°.

Capt. Parry, in his first Arctic Voyage, in 1819, mentions that a hunting party of three had been out on land (his ships were frozen in the ice for ten months, lat. 74° 47' 19" N.), one was left behind frostbitten: of the other two he says, "when I sent for them into my cabin, they looked wild, spoke thick and indistinctly, and it was impossible to draw from them a rational answer to any of our questions. After being on board a short time the mental faculties appeared gradually to return." Hence he concludes that "exposure to severe frost benumbs the mental as well as the corporeal faculties."

*Mortality Statistics of New Orleans, from August 1, to October 9, 1859, compiled from the Weekly Reports politely furnished by Dr. Baldwin, Secretary of the Board of Health. Population of New Orleans by the last census, 138,000.*

Time.	Total Deaths.	Children under 2 yrs.	Under 20.	U. States
August (5 weeks).....	628.....	203.....	294.....	350.....
September (4 weeks).....	531.....	135.....	228.....	270.....
October (1 week).....	156.....	54.....	74.....	85.....
Principal Diseases.	August (5 weeks.)	September (4 weeks.)	October (1 week.)	
Still born.....	32.....	27.....	10.....	
Trismus Nascentium.....	22.....	21.....	8.....	
Teething.....	20.....	18.....	0.....	
Cholera Infantum.....	8.....	5.....	2.....	
Infantile Convulsions.....	41.....	23.....	12.....	
Infant. Marasmus.....	17.....	12.....	2.....	
Croup.....	3.....	2.....	4.....	
Inflammation of Throat.....	0.....	0.....	0.....	
Diphtheria.....	24.....	7.....	3.....	
Scarlatina.....	18.....	8.....	1.....	
Rubeola.....	1.....	0.....	9.....	
Variola.....	0.....	0.....	0.....	
Tetanus.....	5.....	5.....	1.....	
Diarrhoea and Dysentery.....	57.....	38.....	10.....	
Gastro-Enteritis.....	5.....	3.....	2.....	
Inflammation of Liver.....	1.....	2.....	1.....	
Inflammation of Lungs.....	3.....	9.....	2.....	
Phthisis.....	66.....	60.....	21.....	
Apoplexy.....	11.....	8.....	0.....	
Congestion of Brain.....	18.....	26.....	6.....	
Fever, Typhoid.....	27.....	13.....	3.....	
“ Miasmatic.....	41.....	83.....	13.....	
“ Yellow.....	0.....	1*.....	9.....	

\*This, the first case of Yellow Fever reported by the Board of Health, is thus noticed by it: “If this be a case of Yellow Fever it originated on a steamboat at Vicksburg, coming down the river. The patient was sick two days, and was admitted into the Hospital when he reached the city.”

†It will be observed that ten deaths from yellow fever are reported to October 9th. Such doubtless is the number, as shown by the certificates of physicians, from which these weekly reports are compiled. However, a larger number of deaths, which were preceded by Black Vomit, have occurred at the Charity Hospital alone. In a future number some notice will be taken of these deaths. E. C.

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.—From the Medical Purveying Office, U. S. Army, N. O. New Orleans, La., Lat. 29 deg. 57 min. 30 sec. N.; Long. 90 deg. W. Altitude of Barometer above the level of the sea, 35 feet.

1859.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
	Max.	Min'm.	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
August...	7 A. M. 5th.	9 P. M. 11th.	30.157	2 P. M. 20 & 21.	7 A. M. 31st.	83.17	2 P. M. 20th.	7 A. M. 31st.	82.50
	30.306	30.070		89	77		91	75	
September	7 A. M. 6th.	2 P. M. 15th.	30.149	Several.	7 A. M. 22d.	80.77	2 P. M. 12th.	7 A. M. 21 & 22d.	80.43
	30.316	29.850		88	68		89	68	
1859.	HYGROMETER.			PREV'G WINDS.	WEATHER.		RAIN.		
	Max.	Min'm	Mean		Fair.	Cloudy.	Days.	Quantity.	
August ...	Several.	7 A. M. 31st.	78.95	S. E.; E.; & N. W.	19.66	11.33.	11	6.17.	
	82	71							
September	2 P. M. 13th.	Several.	76.47	W.; E.; & N. E.	24.	6.	7	1.19.	
	84	66							

The appearance of a blood red illumination of the heavens in the north was noticed between 8 and 9, P. M., on the 28th of August, and also slightly on the 29th, supposed to have been produced by the Aurora Borealis, an unusual phenomenon in the south.

September 2d, the red illumination was again observed between 12 at night and 3, A. M., more brilliant in variety and longer in duration.

T. HARRISON, Clerk.



